ROYAL COMMISSION ON VIVISECTION.

APPENDIX

TO

FOURTH REPORT

OF

THE COMMISSIONERS.

MINUTES OF EVIDENCE,

October to December, 1907.

Presented to both Houses of Parliament by Command of His Majesty.

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ROYAL COMMISSION ON VIVISECTION

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ROYAL COMMISSION ON VIVISECTION.

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MINUTES OF EVIDENCE

TAKEN BEFORE THE

ROYAL COMMISSION ON VIVISECTION.

Wednesday, 23rd October 1907.

TWENTY-NINTH DAY.

PRESENT:

The Right Hon. The Viscount SELBY (*Chairman*).

Sir W. S. CHURCH, Bart., K.C.B., M.D.
Sir W. J. COLLINS, M.P., M.D., F.R.C.S.
Sir J. MCFADYEAN, M.B.
Sir M. D. CHALMERS, K.C.B., C.S.I.
Mr. A. J. RAM, K.C.
Mr. W. H. GASKELL, M.D., F.R.S.
Mr. J. TOMKINSON, M.P.
Mr. G. WILSON, LL.D., M.D.

Captain C. BIGHAM, C.M.G. (*Secretary*).

Mr. JOHN GLAISTER, M.D., called in, and Examined.

Mr. John Glaister, M.D. 23 Oct. 1907.

12819. (*Chairman.*) You are a Professor of Glasgow University?—I am—Professor of Forensic Medicine and Public Health.

12820. And you have been desired by the Faculty of Physicians and Surgeons of Glasgow to give evidence here?—I have.

12821. Is that one of the licensing bodies qualified to give diplomas in medicine and surgery?—Yes, and also diplomas in public health.

12822. And you are a Doctor of Medicine yourself, I believe?—I am, of the University of Glasgow.

12823. And you are a Fellow of the Faculty of Physicians and Surgeons of Glasgow, and a Licentiate also of the Royal College of Physicians and Surgeons of Edinburgh, and you have other qualifications?—That is so.

12824. Have you yourself ever taken out a licence under the Act?—No, I have not had facilities until now. I have a place which has now been registered in my new buildings, in my new department. I have not done so before, because I had no accommodation for animals, as a matter of fact. Now I have a building which is registered under the Act for the purpose.

12825. (*Sir William Collins.*) Do you hold a licence?—Not yet; I have not applied for one.

12826. (*Chairman.*) Do you propose to do so?—Yes, immediately.

12827. The subject is one that you have studied?—Yes, for 25 years I have been a teacher on the subject.

12828. But you have never had a licence yourself?—I have never held a licence up till now.

12829. At the same time you are familiar with the Act of Parliament?—I know it, and I think I know all about the certificates.

12830. And you are familiar with experiments on animals?—Yes, very familiar. I may add that on several occasions I have had to sign the certificates of others. Being one of the statutory certifiers under the Act I have had for some years to act as a signator of the certificates of others.

12831. Have you found any difficulty in carrying out your duties as a certifier under the Act?—No, I have never had any difficulty on the question as to the need for experiments upon animals. Before signing a certificate I have always satisfied myself of two things. First, that the applicant was a capable person to perform the experiment; and, secondly, that the experiments, in my mind, ought to be performed.

12832. And you have always been able to deal satisfactorily with those points?—Yes.

12833. In that respect, as regards certifying, is there any amendment that you would like to suggest in the Act?—The only suggestion that I would make is this. So far as I am able to see, the certificate "A," which is usually confined to inoculation and feeding experiments, is not large enough for such work as my own, for example. As Medico Legal Examiner to the Crown in Glasgow and the West of Scotland, charged with the duty of examining *post-mortem* the bodies of those who die sudden and violent or suspicious deaths, I am required to report to the Crown as to the cause of the death. In some cases the death has been due to poisoning; it may be from food or ordinary poisons, or more occult poisons, and, if one in Certificate "A" laid oneself down to ask for a certain number of experiments, on account of the uncertainty of the amount of work to be done in the year, one might have some difficulty in having the number of animals named in the certificate not exceeded; that is to say, I might require more animals in one year than in another, and if I put down a certain number, or, if the number were laid down for me by the Home Office, I might not be able to conduct the work. I would like to say further that I understand there is a certificate, which has not, I think, been mentioned in the evidence, so far as I have read the three volumes, called "A 2," which has been issued by the Home Office to a small number of investigators. I have only seen it scheduled in the last few days. The terms of that certificate would be quite satisfactory to me, inasmuch as the number of experiments is not named, the time of carrying out the work is not laid down, and the range of work would cover all that I personally would require.

12834. (*Mr. Ram.*) Have you got a copy of it?—I have only got a copy in writing.

12835. (*Sir John McFadyean.*) May I ask is it quite the rule that in Certificate A the number of experiments is limited?—I cannot say. I have only seen one of these certificates, and the only certificate that I saw did not bear any number of experiments.

(*Sir Mackenzie Chalmers.*) Have you ever applied for a certificate with regard to your duties and been refused?—As I have already explained to his Lordship, I have never applied for a licence or certificate at all, because up till now I have not had facilities. I have now, and my place is registered, and I am just waiting to get a comprehensive enough certificate to make an application.

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12837. (*Chairman.*) Your attention has been particularly directed, I presume, from what you have said of your duties, to the question of discovering poisons in the human body?—To the discovery of poisons in the human body and making investigations there anent.

12838. And as to the symptoms which those poisons develop?—Yes; and I may add further that there is a range of causes of poisoning due to food, of which I have had several cases in the last few years, which are more difficult to deal with than many cases of mere mineral poisons. My assistant, for example, is Medical Officer of Health of a burgh, and he works with me in any epidemic disturbance that there is. There was an epidemic of milk poisoning in which 600 people were involved, and I should like, of course, to be able to include in my work such investigations as that kind of work would demand—food poisoning.

12839. You mean in the new registered building which has been provided?—Yes.

12840. That is specially what you would like to experiment upon?—Precisely.

12841. Other things besides milk, of course?—Yes, any kind of food.

12842. What kind of poisons are developed there. How do you speak of those?—The cases which come under one's investigation are, for example, in the case of shell fish, the case of the typhoid fever bacillus, the bacillus coli communis, which is a type of bacillus, Gaertner's bacillus which is called the bacillus enteritidis, and a third group, the sausage bacillus, the bacillus botulinus. These form groups of meat poisoning micro-organisms, some of which form toxins, that is to say, the products of their growth, which of themselves are poisonous, apart from the existence of the organism itself. I had a case, for example, within the last three months in which the criminal authority of a neighbouring county sent me the internal organs of a girl, one of a series who were made ill and had died after eating sausages.

12843. (*Sir William Collins.*) Could you not deal with that case then?—I could only deal with that case chemically, and I got an experiment made, which I adopted, by a colleague whose licence covered such an investigation.

12844. (*Chairman.*) Do you consider that it is necessary to make experiments upon animals for the purpose of making these investigations about poisons?—I have no doubt about it. I think it is essential. I have mentioned in my précis the very well-known Dr. Lamson poisoning case, in which aconitin was the active poison, of which the poisonous doses are an infinitesimal part of a grain, and in which case Sir Thomas Stevenson discovered the poison along with Dr. Dupré, partially by experiments upon himself and upon mice.

12845. (*Sir Mackenzie Chalmers.*) Everything that was required was done in that case, was it not?—Absolutely. I am only pointing out that you require animals to test whether you are dealing with aconitin or not. I say without this physiological test you cannot detect your poison.

12846. (*Chairman.*) You cannot detect it by a chemical process?—No, you cannot prove it by chemical process.

12847. (*Mr. Tomkinson.*) The effect must be watched upon living animals?—Precisely.

12848. (*Sir Mackenzie Chalmers.*) Dr. Stevenson, in fact, experimented upon himself as well, did he not?—I have just said so. No chemical test would reveal that poison, but a physiological test would.

12849. (*Sir John McFadyean.*) Was that before the passing of the Act?—No, it was in 1882, I think.

12850. (*Sir William Collins.*) What animals did he use?—Mice.

12851. (*Chairman.*) That investigation of food, I suppose, is of growing importance, there being so much preserved food—tinned food?—I think it is absolutely essential—that we must have animals to experiment upon in such cases as those. The subject is yet in its infancy; it has to be opened up yet.

12852. Are there any other aspects of public health which make experiments of this kind necessary?—If you take a disease, such as cerebro-spinal meningitis, we have been having a large outbreak of it in Scotland during the past two years. I have the official figures here, and during this present year, 1907, from February to September in Scotland there have been 1,611 cases reported to the Local Government Board. Of these 1,028 occurred in the county of Lanark, in which Glasgow is situated. During 1906, up to March of this year, in Glasgow there were 312 cases and 230 deaths, a death rate of about 70 per cent., 73.7, to be quite accurate, and in the present year, between the 29th of March and the 11th of May the number of cases was 229. The question at issue with regard to that disease is how is the disease propagated. We have great difficulty in knowing how the disease is propagated now. An investigation has been started with regard to an examination of the air passages of apparently healthy persons acting as passive carriers; that is to say people who have been in contact with a case of the disease have been found to carry passively in their upper air passages this organism, and it has been found in these. That subject must be elaborated, and the nature of the organism itself has to be discovered. This disease simulates certain cases of food poisonings. In 1902 I was asked by the Corporation of Paisley, an adjoining burgh to Glasgow, to make an investigation regarding the causes of illness of several persons; I forget whether six or nine, three of whom had died, to make a *post mortem* examination on the body of one of the victims, and to report. I was able in that case to discover and isolate and photograph the organism of this disease. We do not know much about it yet, and one must make experiments upon animals in that direction.

12853. When you say an organism of this disease you mean a microbe?—A microbe. Of course we know the microbe, and we know how to grow it, but it is in its relation to its spread and other things that we want to make investigation.

12854. (*Sir Mackenzie Chalmers.*) Are animals susceptible to this disease?—That I cannot tell you. That is one of the things we want to find out—whether animals are the vehicle of contagion or not.

12855. (*Chairman.*) Have you experimented on animals?—No, I have not. I am eager to begin.

12856. Has it been done with regard to this disease?—Not so far as I know up till now. It has not been done in this country much so far as I can make out.

12857. (*Mr. Ram.*) It could be done to-day?—Yes, it could be done under the Act.

12858. (*Sir John McFadyean.*) When you say that it has not been done in this country have there not recently been some experiments carried out by Dr. Stuart McDonald in Edinburgh with regard to monkeys?—Yes.

12859. (*Mr. Tomkinson.*) Do you hold that this microbe must be either communicated by infection or caused by some specific food or other, and is never a natural growth?—I did not say that. All microbes, of course, must proceed from previous microbes, but the question of their communication from one person to another is the question in which difficulty is experienced just now—how was it spread? If we knew that we might devise preventive measures.

12860. But it does spread?—Yes, undoubtedly it does spread.

12861. By infection?—I think so.

12862. (*Chairman.*) Then at present all that you have to say about that is that you look forward to experiments upon animals to assist you in discovering how it spreads?—I think investigators will require to call in the help of animals to elucidate these points.

12863. You are not in a position to say that any experiments have been made which have thrown any light on the subject at present?—No, because the information which one of the Commissioners kindly referred to just now with regard to monkeys would not throw any light upon the possible spread, as of course we do not have monkeys in this country except as caged animals.

12864. Then as regards the wilful poisoning, the criminal poisoning of people, what is your experience?—I think I can safely say that there are a certain number of alkaloids which are so poisonous that I do not think any chemical means could possibly detect them, of which we know comparatively little now, and which must be investigated by the agency of animals.

12865. You do not think that any improvement in the knowledge of chemistry would do it?—I do not think so, for this reason, that it is a more difficult task than even looking for the proverbial needle in a haystack.

If the poisonous dose, for example, of a drug be half a grain (and that is an exaggerated case with regard to some of them), and that is taken into the body and causes the death of the individual, the most accurate chemical processes would not be able to isolate from the remains of that body anything like half a grain, and even if rescued, as in the case of aconitin, chemical tests would not enable one to say what it was; physiological tests might, and probably would.

12866. Have experiments increased your knowledge of those poisons that you speak of—aconitin and alkaloids?—They have materially, regarding their action, and it is because of their action upon animals having been ascertained by experiments that one is able by taking an alkaloid, of which we do not know the precise nature, and putting it into an animal, to get to know that the poison is aconitin. I may point out in that connection that there are other questions associated with poisoning of very great importance; for example, the best antidotal remedy for acute arsenical poisoning, assuming a person wilfully or accidentally poisoned by arsenic, was really brought into practical operation by experiments on animals. We had never known about this except theoretically until it was distinctly found to be valuable from researches upon animals. In the early thirties of last century in theory it was believed that the use of a certain preparation of iron would be beneficial. Between 1834 and 1840 certain experiments were made on the dog and on the horse, which showed that this hydrated ferric-oxide, freshly made, would be distinctly antidotal. In 1861 Dr. Watt published in the "Ohio Medical and Surgical Journal" for March of that year certain experiments which he performed on five dogs, in which he gave them poisonous doses of arsenic, and at varying intervals thereafter this freshly-prepared iron oxide, and showed the beneficial effect which resulted therefrom.

12867. He gave it as an antidote?—He gave it as an antidote, and his object was to show that this substance was an efficient antidote if used after the poisoning within a given period.

12868. You say that he found that it was a satisfactory antidote. Did the dogs recover?—I will just tell you the facts. He administered arsenic alone to five dogs in different doses from three grains upwards, and all of those dogs died from the effects of the poison within eight hours after the administration. To a second series of twelve dogs he administered varying doses of arsenic, all of them lethal doses, but afterwards administered to them the antidote, in some instances not until the poisonous symptoms had appeared, but all the animals recovered; and it is upon evidence of that kind that we now in all our books and all our teaching recommend this as an antidote to arsenic.

12869. And is it used for arsenical poisoning of human beings?—Yes, it is the antidote used to-day.

12870. Does it have the same effect as it had upon the dogs, of curing them?—Yes, there have been many cases recorded of recoveries from arsenic poisoning where it has been administered.

12871. Do you attribute that discovery entirely to those experiments?—Surely. Before that it could only be theory.

12872. What gave rise to the theory?—The theory is that if you take a solution of arsenious acid you can precipitate that as the insoluble salt of iron with an iron preparation. You can do that in a test tube—everybody knew that—but the question came to be how could it be practically applied to the human stomach? Could you utilise in any way the human stomach as you would use the test tube? And it was with the object of discovering that practically that these and many other experiments were carried through.

12873. Was this dose that was given as an antidote to human beings the same liquid that was put in to operate upon the arsenic in the tube?—The same preparation precisely, only in the human stomach the quantity of iron oxide must be considerably increased in order to get the same effect.

12874. Then this was a case in which from chemical tests you had discovered the probability of the remedy being effective?—Yes, through the physiological medium of application to individual animals.

12875. And it was to make sure of the results in the human stomach that you tested it on animals?—Yes, and in order to know how it could be made sure—by what doses.

12876. (*Sir Mackenzie Chalmers.*) What I understand is that this remedy was first tried in a test tube, then tried on a dog, and then, having answered those two tests, it was used successfully with human beings?—Yes; the experiments on dogs determined the amount of the antidote to be used to the poison—the method of use.

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12877. (*Chairman.*) What I wanted to get, and what I think you have given me, is that the scientific fact that this was an antidote was discovered in the tube by chemistry?—Yes, exactly.

12878. But it was not safe or certain as a remedy to human beings until you had tested it upon animals?—Surely not, because we cannot use the human stomach as a test tube—the two things are different.

12879. Are there any other special cases of valuable testing of poisons to which you wish to call attention?—I desire to draw attention to one more, and it is a rather interesting one. It is the case of a very fatally poisonous arsenical gas called arseniuretted hydrogen. A few years ago I had two cases of this occurring in a very curious way. A man engaged in chemical works in the manufacture of bleaching powder in a Welldon's retort suddenly became overpowered, and had to be taken out. He died within three days. The man who followed him also was overcome, but he recovered. The process of making bleaching powder by a Welldon's retort is this: Hydrochloric acid is mixed with a certain proportion of manganese dioxide; the manganese dioxide acts upon hydrochloride acid, liberates the chlorine gas, which saturates the lime and forms the bleaching powder. The retort is made to work continuously, night and day, for from two to three weeks. At the end of that time a workman has to proceed in from the top by means of a rope ladder after the retort has been steamed out with live steam to dig out and remove what is called by the workmen the mud, the débris, which lies at the bottom of the still. I was called in to consult with other doctors regarding this man before his death, and after seeing the man I went to the chemical works and got the details of the process just as I have described them. I came to the conclusion that the man died from this gas. The case required to be reported to the Home Office. It was reported, and the man having died, a *post-mortem* examination of his body was made, and Sir Thomas Stevenson, who chemically examined the remains, found arsenic therein. The interesting thing about that case is that this arsenical gas did not exist in the retort when the men went into it. As the implements for the removal of this mud they took an iron shovel and a zinc galvanised iron pail, and they themselves manufactured the gas which caused their death. They manufactured on a large scale what we know as the Marsh's test for arsenic, after Mr. Marsh, who invented the test in 1836. They generated the gas which killed them. Since these cases I have investigated the literature of the world, and I have found a large number; I have collected over 100 cases of deaths from this cause in a large variety of occupations, and perhaps your Lordships will be aware that in the State Ballooning Department of France several deaths have already occurred from this gas being present in the impure hydrogen by which the balloons are inflated. All the evidence that we have regarding the physiological effect of this gas is from experiments made on the Continent. I am not aware of a single experiment having been made in this country on that subject.

12880. What is the sort of experiment on an animal that you would suggest, if it had to be made?—The experiments on the Continent have been by subjecting dogs to varying percentage proportions of this gas mixed with air, and studying the symptoms that follow. My attention will be directed, not to the result of the gas, but to the prevention or cure of it when it happens. Nothing has been done in that way so far as I know. There have been various suggestions.

12881. You would have first to ascertain what in all probability would be the antidote, I suppose?—Yes, for this reason. This gas acts differently from arsenic itself; it breaks up the corpuscles in the blood; it acts directly upon the blood, and thus an antidote is more difficult to be arrived at.

12882. But when you have discovered the antidote you will have to test it upon animals?—Yes, we must use animals. I would not dare use anything else than an animal for this.

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12883. Is there any other fact you would like to bring before us as regards poisoning and experiments upon animals?—May I just finish by adding in a group answer that our knowledge of antidotes with respect to a large variety of mineral and vegetable poisons has been derived from experiments upon animals of different kinds. I have a whole list here of those of which I have data, and to which I can give references.

12884. Perhaps you will furnish them to the Commission?—These are some of them: Mercuric chloride, examined by Sir Benjamin Brodie on rabbits and cats, and the reference is "Philosophical Transactions," Vol. CII., page 222 (1812). Tartar Emetic: Magendie (*Mémoire, sur l'émétique*), experimented on dogs with and without the gullet being tied. (Orfila-*Toxicologie Générale*, Vol. I., page 469.) Lead Poisoning: Orfila, Christison and others examined the effects by its action on animals. (Christison on Poisons, 4th Edition, 1845, p. 548 et seq.) Prussic Acid: Magendie experimented on dogs and birds (*Annales de Chimie*, Vol. VI., page 347; Christison on Cats, Op. cit., page 757.) Hemlock: Orfila on Animals. Christison in 1835 found it a deadly poison for dog, cat, rabbit, mouse, frog, fly and flea; and Geiger, for kitten, pigeon, swallow, glowworm and earthworm. Laburnum: (Ross on Dogs. Christison on Rabbits, op. cit.). Strychnia: Marshall Hall's test with frogs. (Wormley on Poisons, p. 584.) Digitalis and Digitalin, by Tardieu, on frogs in the Pommerais case (Author's Manual, page 493).

12885. You said, I think, that a certificate which was in use at the Home Office now was one that you would be satisfied with?—Yes. I only discovered it after writing my *précis*. That would be quite satisfactory to me.

12886. An alteration from the ordinary certificate to that is the alteration that you would recommend?—For certain heads of departments. I would like the Commission to distinguish, if possible, between the man who is the head of a department supervising the whole department, and a junior working under him as assistant or a research student. The head of the department is the responsible person. I have a department, and there will be men working under me in different directions.

12887. The head of the department holding a licence?—Surely. The head of the department holding a licence ought to have more liberty with regard to the range of work, always subject, of course, to the provisions for inspection and other things, than an assistant or research student should have, and a little more elasticity should be shown in this regard in the certificates by the Home Office.

12888. (*Sir William Collins.*) In regard to the interesting evidence which you have given us with reference to the antidote for arsenic by hydrated oxide of iron, did I correctly understand you to say that the use of that dates from 1861?—No, what I did say was that theoretically the iron was known as an antidote since the early thirties; indeed, between 1834 and 1840 the first experiments on dogs, I think, were made. The reference I have here is to experiments made on dogs between 1834 and 1840.

12889. Did not Bunsen point out the antidotal effect of hydrated peroxide of iron in the case of arsenic?—I do not know the date when Bunsen did it, but he only did it as a chemist would naturally, knowing the reaction between the iron salt and the solution of arsenic forming an insoluble arseniate of iron.

12890. But you stated that ever since the 1861 experiments this antidote had been recommended?—Yes.

12891. I suggest to you that it was recommended before 1861?—I should not like to contradict you, but if you take "Christison on Poisons" I do not think you will find Bunsen's name mentioned in connection with it. I am quoting from Christison just now.

12892. And I think you will. When was "Christison on Poisons" published?—The edition that I have is 1845, the 4th edition.

12893. Do you say that in that you will not find reference to Bunsen and Orfila and Douglas Maclagan making experiments with regard to the antidotal influence of hydrated oxide of iron on arsenic?—I certainly agree with you as to Orfila and Douglas Maclagan, but I cannot remember seeing Bunsen mentioned. Of course, to err is human, and I will not contest the point with you, but my recollection is that Bunsen's name is not mentioned; I may be wrong.*

12894. Should I be wrong in thinking that in 1845 or 1846 in Christison's work hydrated peroxide of iron was recommended as the antidote for arsenic?—Yes, it was recommended then.

12895. You state in your proof, I understand, that it was since 1861?—No, I think not.

12896. I have it here "Ever since that time," that is in 1861, "this antidote has been recommended to be used for human beings in such cases"?—Yes.

12897. I ask whether it was not recommended to be used in such cases prior to 1861?—I have no doubt that it was, and what makes me say that all the more freely is that in Christison's work the results of the experiments were so varied that Christison set himself to find out wherein the inconsistency lay.

12898. Was not the inconsistency largely a question of the quantity of hydrated peroxide used?—Very largely.

12899. Had not the fact that the inconsistency was due largely to the amount of hydrated ferric oxide used been clearly explained by Douglas Maclagan some time in the forties, showing that it was the amount of the dose that gave the varying results?—Yes. Christian made it quite clear, I think, at that time that it was merely a question of the sufficiency or insufficiency dependent on the quantity of the antidote in relation to the poison.

12900. That was known in the forties?—Yes, in 1845. Prior to that many of these experiments were made on dogs to which I referred.

12901. The experiments on dogs, I gathered that you referred to, were by Watt in 1861?—I mentioned other experiments on dogs between 1834 and 1840.

12902. I understood you to give us the date 1861 as the time from which the use of this ferric oxide had been recommended in the case of arsenic poisoning?—Yes, I know that since 1861 in every book on toxicology you will find the remedy given.

12903. But you are not so clear that it was not recommended before?—In some books it was, and in some books it was not.

12904. And in Christison you think it was?—Undoubtedly—I have no doubt regarding Christison.

12905. (*Chairman.*) Christison recommended it to be used by a physician when he came in contact with a case?—Yes, but I would like it to be understood without any doubt that Christison's opinion came after experiments on dogs conducted by M. Soubeiran, M. Miquel, and M. Boulay, and those researches were published in "Annales d'Hygiène Publique," volume XIV., p. 134.

12906. (*Sir William Collins.*) You did not think that Christison referred to Bunsen?—My memory does not serve me that Bunsen's name is mentioned, and I may say that I read over Christison on Saturday night.

12907. You also said that you had searched the literature of the world?—With regard to asseniuretted hydrogen.

12908. And it is a more limited research that you made in regard to arsenic?—I may say that I have here my book on the subject—"Toxicology and Public Health." I have a large number of references in it, and I have read very largely the literature on the subject.

12909. You have yourself conducted researches in forensic medicine?—Yes, many.

12910. But you have never had occasion to use vivisection?—I should not say that. There have been occasions when I should like to have had the advantage of vivisection.

12911. Have you had occasion to use vivisection yourself?—I have never used it.

12912. Then in such researches as have enabled you to enrich the subject of forensic medicine it has not been the result of vivisection by yourself?—No.

12913. You lecture, of course, in Glasgow on forensic medicine?—I have been a teacher of the subject for 25 years.

* *The witness subsequently wrote that he was wrong, having misquoted from his notes, and that Cristison does mention the name of Bunsen associated with the antidote in* 1834.

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12914. Do you illustrate your lectures on forensic medicine by vivisection. I suppose you do not?—No, I have no licence, and I have had no accommodation for animals in my department in the University till the new buildings were opened in April of this year.

12915. In your opinion is it necessary to illustrate your lectures on forensic medicine by vivisection?—Surely not. I do not see any necessity for repeating experiments before a class with regard to poisoning, but I think it is essential that we should have facilities for legal work and work of that kind.

12916. You are aware that under Section 12 of the Act special provision is made for that?—Yes, I understand that that is so.

12917. (*Sir John McFadyean.*) With regard to this Certificate A, is it not a fact that the ordinary form of Certificate A bears that the research may be limited either as to the number of experiments or as to the time or as to both in the opinion of those who certify, and that considerable latitude is always allowed by the Home Office in regard to that?—The rubric on the margin of Certificate A, which I have before me, says that it may be given for such time or for such series as the person signing may think expedient.

12918. That is precisely what I wanted to bring out?—But may I add that the rubric further says, if the certificate is unlimited or limited by time only, the Secretary of State usually imposes a limit on the number of experiments to be performed; and I submit to the Commission that in Certificate A 2 there is no limit to the number of experiments put down.

12919. You think there should not be any such limitation?—May I put it in this way, that there are individual cases in which it is of the utmost importance, from a medico-legal and public health point of view, that the number of experiments should not be limited?

12920. That it should be large perhaps—would not that do? If you put down 500, for instance, do you think there would be any likelihood that the Home Office would override your opinion that that number was necessary?—It seems to me that there would be no advantage in doing that, because each investigator has to return the number of experiments he has made at the end of each year, and in any case the Home Office gets all the information with this advantage, that the operator is not limited as to numbers.

12921. (*Sir Mackenzie Chalmers.*) The existing certificate covers everything; it depends upon the form in which it is granted?—I do not wish to push this, but I never knew until recently that there were any other certificates than A, B, C, D, E and F; but I understand that there is a certificate called A 2.

12922. (*Dr. Gaskell.*) Would you read it?—This is a copy of the certificate which I saw. It is called Certificate A 2. The "2" has been put in in typewriting. It is headed "For Public Institutions." The number of experiments in that certificate is blank, and the time limit is blank.

12923. (*Sir Mackenzie Chalmers.*) To whom was it granted?—I prefer not to say unless the Commission desire it.

12924. (*Mr. Ram.*) Was it from the Home Office?—Yes.

12925. (*Sir Mackenzie Chalmers.*) Not granted by the Home Office; the Home Office do not grant certificates; they are approved by them?—It was not given to Sir Thomas Stevenson, I may say.

12926. (*Sir John MacFadyean.*) Is it possible that the figure "2" is merely to distinguish that particular Certificate A from a previous Certificate A given to the same holder and held at the same time?—That I cannot say.

12927. I suggest that it is. However you are satisfied with that form?—May I read the kind of range of work that is permitted under this certificate?

12928. Certainly?—"Hypodermic intra-venous and intra-peritoneal injection of living cultures of bacteria, bacterial poisons, chemical substances, animal and vegetable extracts, blood and other medicinal poisonous and infective substances. These injections may be repeated at intervals and in various combinations. Immunisation experiments with any of these substances may be performed. Inunction into the skin and administration by the mouth of bacteria animal extracts and vegetable substances. From time to time during the experiments small quantities of blood, peritoneal fluid or other exudation may be removed for examination." That is the range of work covered by that certificate.

12929. (*Sir Mackenzie Chalmers.*) Is that the original certificate given, or is it the certificate as confirmed by the Home Office?—It is a copy sent from the Home Office with the stamp of the Home Office on it.

12930. (*Sir John McFadyean.*) That is precisely the form of certificate you would approve of to be given to heads of departments?—This is the kind of certificate I would like to have, and will apply for, because it seems to me that it is the only kind of certificate that would cover the range of work I am asked officially to do.

12931. You mean that otherwise some case might turn up that you would be really unable to investigate owing to the circumstances not having been precisely set forth in your certificate?—Yes. Supposing, for example, I put in a certificate thinking that I would have a certain number of experiments to perform, and named my animals and omitted to mention a rat, I should not with that limiting certificate be able to do an experiment on a rat without a new certificate.

12932. I wanted to ask you another question with regard to the necessity for animal experimentation in supposed cases of meat poisoning. As you mentioned, Gaertner's bacillus is frequently the offending microbe in these cases. In your opinion is it often or generally quite necessary to resort to experimentation on animals in order to prove that the case is really one of infection with Gaertner's bacillus?—I should not think it needful in every case where Gaertner's bacillus was found in the offending meat where it could be proved.

12933. That is just the point I wanted to raise. Can you tell the Commission whether Gaertner's bacillus can be recognised for a certainty from its morphological characters and the appearance with which it grows in cultures?—Gaertner's bacillus can be differentiated from the other two groups in the Coli series by cultural tests; but there is a series of organisms in the Gaertner group, the enteritidis group, that we cannot differentiate culturally.

12934. So that the answer to my question would be "No"; that in order to be sure that you have got the Gaertner's bacillus you must prove its pathogenic power?—Precisely. I have a long list here of food epidemics caused by micro-organisms to which I could easily add.

12935. I would like to repeat the same question with regard to sausage poisoning, that is to say, the effects produced by the bacillus botulinus. Should I be correct in saying that the bacillus botulinus really cannot be with certainty recognised without animal experimentation?—I quite agree with you in saying that the bacillus botulinus cannot be recognised without animal experimentation.

12936. It operates simply by the poisons which it generates in the meat?—Yes, the toxin does.

12937. So that you cannot prove that a particular parcel of meat is really the cause of illness in this way except by showing that it is capable of producing toxin effects when it is injected into animals?—That is the kind of evidence that I would be inclined to offer if I were asked to give evidence at all with regard to the toxic effect of meat. May I add with regard to Gaertner's bacillus that it is an open question yet whether the toxins are destroyable by boiling temperature or not. The latest investigations show that they are not, and that you might easily get the toxins of this bacillus in meat, there being no organism at all, and still fatal results and a serious outbreak arising.

12938. (*Mr. Ram.*) Although boiled?—Although boiled.

12939. (*Sir John McFadyean.*) Do you see any way of settling whether the poison can be destroyed by heat without animal experimentation?—I think we might easily discover some laboratory method, but it is another thing in practice with regard to public health, because the people take the meat or the soup not knowing the thing is there. If they knew it they would not take it.

12940. Can you suggest some way in which it might be proved that the toxin has been destroyed, without having resort to animal experimentation?—I cannot at the moment suggest any method in the case of meat by which you could destroy its toxin.

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12941. It is not as to how you would destroy it, but as to how you would know it was destroyed?—The only way is by experimentation on animals.

12942. Is anything known whatever about its toxin apart from its effects?—Nothing, except that it is very poisonous, and there is no doubt about it.

12943. With regard to some experiments to which I referred earlier carried out by Dr. Stuart McDonald with regard to cerebro-spinal meningitis, I rather understood you to say that the results of his experiments on monkeys were of little value as showing the way in which the disease is contracted in human beings?—No, as showing the way in which the disease was spread, was my language.

12944. That is the same thing?—I think not. When you are thinking of spread, you are thinking of how the disease may be communicated from place A to place B. Physiologically between the anthropoid ape and the human being, so far as taking the disease goes, there is no difference.

12945. Would you admit that these experiments were of great value as bearing upon the question whether man is infected by ingestion?—Yes, certainly. I was not animadverting on that. I was regarding the spread solely.

12946. Coming to the question of what really led to the introduction of hydrated oxide of iron as the antidote to arsenic, although there were chemical grounds for believing that it would act as an antidote, do you think that the custom of administering it as the antidote in human beings would ever have become universal if we had not been provided with experimental evidence such as you have referred to?—I think it is hardly likely that any man would have ventured to apply this antidote for the first time to a human being before he had seen how the thing would act on animals.

12947. So that you feel confident that these experiments had much to do with making the administration of this particular substance practically universal in cases of arsenical poisoning?—I have no doubt of it at all.

12948. (*Sir Mackenzie Chalmers.*) About how many cases of suspected poisoning do you have in a year in Scotland?—We have not very many.

12949. Half a dozen or a dozen, would you say?—You see we never know until we investigate them whether they are cases of poisoning or not; in many instances we have to make an investigation in the first place before arriving at a conclusion.

12950. Have you ever known of cases where that investigation or conclusion was impeded or delayed from want of a certificate?—No, I do not put that forward.

12951. You are aware that in any case of difficulty, without going to the Home Office, a judge of the Court of Session has power to order any necessary investigation?—That is quite so; I am aware of that.

12952. I take it that your point is this, is it not, that you think that the head of every important laboratory ought to have the wide form of Certificate "A," instead of the restricted form confining him to a specific number of animals and a specific time?—My answer is "Yes," for this reason, that if you take, for example, food poisoning, in which there may be questions of culpability arising, before you can get a certificate from the Home Office, even supposing you follow your suspected meat, your evidence is gone.

12953. You mean that the investigations must be made at once, or they are futile?—They must be made at once if they are to be of any value.

12954. Are there not plenty of people in Scotland holding the necessary certificates to cover any case that has actually arisen?—But it would be no use to me if I had not got it, and I was asked to conduct the investigation; and it is because of the variety of problems one has to deal with as head of the Department that one would like such a certificate.

12955. (*Chairman.*) You said the head of a department and the head of a laboratory; do you mean the head of a laboratory in a public department?—Yes, I mean that. I should say the head of a department, because in my Department of Forensic Medicine and Public Health I have several laboratories under my care.

12956. (*Sir Mackenzie Chalmers.*) You are not aware really whether there has been any difficulty in the head of a department, properly certified, obtaining the necessary certificates?—I do not think so. I think that the Home Office is exceedingly generous. The only difficulty that I have is this, and I would like to put it here, because I may have to salve my conscience and get the Commission, as a Royal Commission can do, to give me an indemnity if I have been wrong. I want to say what I have done. There is great difficulty felt by many of us in being able to determine what really is an experiment under the Act, and there is great difficulty also in defining what is pain under the Act. According to Section 2 a person shall not perform on a living animal any experiment calculated to give pain, except subject to the restrictions imposed by the Act. I want to ask the attention of the Commission for a moment to feeding experiments. It seems to me a ridiculous thing for a man who proposes to feed a cat on cow's milk to have to go to the Home Office for a certificate. I have interpreted the Act differently, and I have acted accordingly, as I shall show immediately. The effect of that on the public mind is this. If that practice is to be followed the total number of experiments at the end of a year is so increased that the public sensitive to this thing think that vivisection is enormously on the increase. I would like to refer in corroboration of what I have said to Questions 205, 206, and 207, which indicate that even in Mr. Byrne's opinion certificates are asked for which, in the opinion of the Home Office, although they would not offer any opinion of that kind, seem to be unnecessary with regard to feeding. Now, three years ago I was asked to deal with the following problem. A series of cows were alleged to have been poisoned with draff, that is, distillers spent grains, on account of minute proportions of arsenic present in the draff and found on analysis. The amount of arsenic found per lb. of draff was between 1-500th and 1-600th of a grain. The people who had to deal with that action had had no opportunity of seeing the animals during the continuance of this so-called poisoning. The animals were dead; they had no opportunity of seeing their bodies at a *post-mortem* examination. I was consulted by the people and asked—What are we to do? I said—It is no good our going to court and simply swearing to a negative, that this amount of arsenic would do cows no harm. The only evidence of any value for the guidance of the court was to give arsenic to cows over a lengthened period, because we knew that the arsenic, to be so given, would do no harm. Accordingly the Principal of a Veterinary College, a Fellow of the Royal Veterinary College, and an examiner of that college, a colleague in the University, and myself agreed that we should feed a series of cows on arsenic doses much larger than these over a period of three weeks. We did not feel that we were under the Act. We knew that the arsenic would do the cows no harm, but would do them good. We selected a bull and two cows, each with sucking calves. We gave each adult animal a quarter of a grain of arsenic three times a day for the first week; we gave them half a grain of arsenic three times a day for the second week, and we gave them a grain of arsenic three times a day for the third week. Thus in three weeks we gave these three adult animals 36¾ grains of arsenic in addition, of course, to the draff as their food, which was blamed as being the cause of the mischief. We weighed the animals at the commencement of the investigation, and their temperature was noted during the currency of the investigation night and morning, and at the end of the experiment every animal had increased in weight. This evidence was given to the court, and the court gave its judgment on that ground; and I hold personally that we were without the Act making that investigation.

12957. (*Dr. Wilson.*) Arsenic is often given to horses?—Yes. Then I may take another case. I am many times consulted with regard to rivers pollution cases in Scotland, and, as the legal members of the Commission will know, Part III. of the Rivers Pollution Act deals with what is called noxious poisoning or polluting liquid. The County Council of Lanarkshire asked me to advise them with regard to an alleged occurrence of this kind, and I made an analysis of the river water above and below the point of effluence of this liquid, but it is impossible to convey to the mind of a court of law whether a thing is noxious or poisonous unless you get some evidence to show that it is noxious or poisonous. I could not ask the court to go and look at the plants in the river, and I did not expect that the court would be able to understand fully the chemical analysis, consequently I did the third best thing. I made an investigation on fish; I carried through an investigation on fish in the different effluents, and I published the results in the Philosophical Society's Transactions in detail.

12958. (*Sir John McFadyean.*) Were the fish transported from healthy water and put into that water?—Yes, they were; they were got from a hatchery, as a matter of fact. The gold fish were bought at a fishmonger's, and the minnows I got caught in the river. I used gold fish and minnows, one-year-old trout, and trout of two years old.

12959. (*Sir Mackenzie Chalmers.*) Did they all increase in weight and happiness?—No.

12960. (*Mr. Ram.*) Did they all die?—Some of them died, certainly.

12961. (*Sir John McFadyean.*) On what ground do you conclude that this was not an experiment?—I thought that nobody would contend that fish, being cold-blooded animals, could at all be ranked with regard to pain on the same level as warm-blooded animals.

12962. Under the Act, do you mean?—No, it is a question of interpreting the word "pain." A definition of "pain" is what I want to get at. Nobody will define "pain" for us. The Home Office very properly decline to do so. The individual certifier to whom you apply has to do it for himself. I have never been able to look upon fish, from my knowledge of fish as an angler and in other ways, as on the same platform at all as warm-blooded vertebrate animals.

12963. (*Chairman.*) You will not expect us to offer an opinion as to whether this came within the Act or not?—No. I want to be quite frank, and I say that I am mentioning these cases in order to get the protection which the Commission, being a Royal Commission, can give me if I have gone outside the Act.

12964. (*Mr. Ram.*) As I gather, all these experiments of which you have told us as to cerebro-spinal meningitis and as to these noxious gases, all those which you decided to perform or did perform you could perform under the Act as it stands to-day?—Yes, they can all be performed under the Act as it stands to-day. My point is that these questions have relation to asking the Commission not to limit the powers which they presently give us, but in the case of heads of departments to somewhat increase them.

12965. I am coming to that. This evidence of yours, so far as I have alluded to it now, is in order to induce the Commission not to recommend narrowing the powers conferred by the Act to-day?—Precisely, that is the object of the evidence.

12966. The only suggestion that you make as to an alteration of the law is to extend, at any rate to heads of departments, such matters as are dealt with in what you call Certificate A 2?—Yes, that is, of course, where one has to deal with a variety of problems rapidly and very often for the guidance of Courts of Law.

12967. Whether that can be done under the Act as it stands to-day or not. If it cannot be done under the Act you are content if such matters as are described in that Certificate A 2 were given to heads of departments?—Yes, so far as I can see, I think the Act would then be quite excellent.

12968. (*Dr. Gaskell.*) I only just want to ask one further question with regard to that point, as to which I am not quite clear; that is, do you limit the word department to any special department in each place, in each University or town, or would you include in your term heads of departments the heads of all those places that are licensed?—My intention in using the word department was to convey this, and I can illustrate it by a concrete case. In every university there are departments in which experimentation is usually carried on; the Pathology Department, for example; the Forensic Medicine and Public Health Department, and the Pharmacology Department may be taken as types.

12969. And the Physiological Department?—I beg your pardon—the Physiological Department. There are four. There may be under each of these departments several laboratories. You may have assistants, you probably will have them in each of them; you may have research students in some of them. What I desire the Commission to understand by my position is that the head of the department, that is, the professor in charge, should have the fullest facilities, that the assistants should have limited facilities such as now exist under the Home Office, and in the same way research students would have perhaps, to a still more limited degree. The professor at the head of the department ought to be looked upon as the responsible person, who would naturally in his returns let the Home Office know exactly what he was doing.

12970. That is what I wanted to get at. The head of the department, whom you would entrust with these further powers, might be the head of any one of those various departments, the head of the Physiological Department or the Pharmacological Department, or what not?—Surely.

12971. It is not for the particular Forensic Medicine Department that you ask it?—No. I may say that now the heads of departments are responsible for the conduct of the departments. They would be practically responsible, if, for instance, they had an assistant or a research student in them, because the animal house is under their control and under their charge.

12972. (*Sir Mackenzie Chalmers.*) You would make the head of a department responsible for any misconduct that went on in his laboratory by his assistants?—Surely, so far as he could control it.

12973. (*Mr. Tomkinson.*) In the investigation of poisoning, such as in Lamson's case, which you have to make and in which you say the discoverable amount of poison is so minute as to be hardly traceable when discovered, and used for experiments upon animals, I suppose you would be content with, you would necessarily have to use a very small animal, like a rat?—Yes. May I say that in the cases I have mentioned the difficulty does not commence at the experimentation on the animal? Before you can do that you have to isolate the substance which is to be injected into the animal from the whole mass of these organs, which is a very laborious business. You require to take the internal organs very largely, and get the poison extracted from those first, before you can use it upon animals.

12974. Where does the experimentation previously upon living animals help you to discover that very small and difficult amount to be discovered?—Assuming, for example, that I carry out a series of experiments on that poison, aconitin, which was used in the Lamson case, supposing I use it upon rats and mice I can see exactly the effect produced by the result. If I get in a rat or a mouse from something extracted from a human body which is dead, symptoms corresponding to those same symptoms in the animal, then it is fairly strong confirmatory evidence that I am dealing with the same poison.

12975. You judge by the symptoms?—Yes.

12976. But if the symptoms developed by the animal by a very minute portion of the poison discovered, tally exactly with the symptoms under which the human being died, that is naturally good evidence, but it does not meet your point which you put to me at first when you said that that was not the difficult point. A poison once discovered it is easy enough to test it upon a small animal?—I agree, but the trouble in these cases is getting the poison from the human body to test it upon the animal.

12977. But how are you helped to find that very minute portion?—You are not helped at all, so far as the animal is concerned, in the preliminary stages. You are only helped by the animal at the closing stages of the inquiry, namely, the detection of your poison, which is the important point.

12978. (*Sir John McFadyean.*) I think the question was, where are you to get the poison, which is not necessarily isolated. I suggest that you use as much of the corpse as you can?—I have just been explaining that. You use so much of the internal organs of the body to isolate the poison.

12979. (*Mr. Tomkinson.*) But are you helped to discover that poison by previous experiments on animals?—Surely; you could not make comparisons unless you had made previous experiments on animals with the same poison, and, as I said, assuming that you use aconitin upon rats and mice, knowing that it is aconitin, and you get the development of certain clear symptoms, then, if you get something extracted from a dead body which, when injected into a mouse or a rat, produces the same symptoms you have got strong confirmatory evidence of the fact that the same poison has been used.

12980. (*Dr. Wilson.*) You attach the greatest importance to animal experimentation, both in public health and in criminal cases?—Yes, in certain departments of each of those.

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12981. And you have been engaged in that sort of research for the last 25 years?—Yes, I have been engaged in researches, without experimentation on animals, for the last 25 years.

12982. And without, of course, as you say, having had any licence?—Yes.

12983. Should I be right then in inferring that you have been able to carry out all those researches and investigations, and I know that they are very difficult investigations and researches, without the aid of animal experimentation?—No, I have not been able to carry them all out.

12984. Could you name instances to the contrary? —Food poisoning cases, for example, would have been done either in the Corporation's Bacteriological Institution, which is licensed, or when one has had an investigation to be carried out, one has had to apply to somebody who had a licence and assume it as his own experiment.

12985. But have there been any cases of illness in which you have had to enlist this additional assistance as it were?—No, I have had very few cases. Fortunately, it is only the expert poisoner who would use the alkaloids for the purpose. An ignorant man has not a knowledge of them. He may use strychnia, as happened in a recent case, but then we can detect that by chemical means; we do not require experimentation on animals except with small quantities, and then we use a frog—Marshall Hall's well-known test.

12986. Has it not been proved by recent research that even sound food, during the process of healthy digestion is broken up in part, or in whole, into these poisons that you talk of—ptomaines and alkaloids?—You mean in the living stomach?

12987. Yes, in the living stomach; that that is the first process of healthy digestion and with wholesome food?—Before I could answer that I would require to know exactly what you mean by ptomaines, because the word ptomaine itself implies that it can only be formed in a dead body.

12988. Yes, I know; but as you know, ptomaines are formed, at least it is contended that they are, both ptomaines and leucomaines, by the decomposition of healthy food when it is attacked by micro-organisms. The food is dead, of course, when it gets into the stomach?—Yes.

12989. Then would you not expect on analysis to find a certain amount, at all events, of these deadly alkaloids and poisons in all food that has been kept for some time, whether flesh or fowl?—I do not think that the present stage of science would enable me to give a definite answer to that question. Brieger and others have isolated alkaloids from dead tissues which were poisonous to animals, and there can be no doubt that there is a difficulty sometimes in determining in the examination of a dead body, whether the poisonous principle that is found has been due to the substance swallowed or to the natural result of decomposition of the body. That is the difficulty, and it is in order to enable science to deal with such difficulties that experiments are carried on.

12990. Then if it is possible to find these ptomaines in animal food that has been kept for some time, you would expect to find them in still greater volume in, for example, venison or game, which is always somewhat high, and yet is edible?—I think that all those foods which are eaten high have their special risks.

12991. Because they contain ptomaines?—Yes.

12992. Then with regard to all these food outbreaks, such as from eating sausages, and so on, the poisoning called botulism, is it not possible to ascertain with certainty that all these outbreaks have been caused by the food?—No.

12993. Or by the condition of the food?—No, if you take, for example, Gaertner's bacillus, of which we have been speaking, you may get meat containing this organism which neither by smell nor appearance nor taste would give any indication of the presence of the organism. It is not a putrefactive organism in that sense; it does not cause a nasty smell, it does not make the meat look bad, consequently it may be taken quite innocently or purveyed quite innocently in the belief that it is good food.

12994. Do you say that Gaertner's bacillus is not a putrefactive organism?—Not in the bacteriological sense, as we speak of putrefactive organisms.

12995. I will put the question in this way then. We will suppose that you have isolated these ptomaines, some from the corpse, some from food about which I have been speaking, either game or meat that has been hung for some time, could you then distinguish between the ptomaines that have been obtained from the ordinary healthy butcher's meat or venison and the ptomaines which have been obtained from the decomposition of the dead body?—Do I understand you to mean healthy butcher's meat?

12996. Meat that has been kept for some time?—I would not call it healthy meat if it has been kept till it is high.

12997. I will say venison then, which can be eaten without doing any appreciable injury?—Venison is usually kept until it is fairly high before it is eaten.

12998. Will you assume then that ptomaines can be obtained from ordinary butcher's meat that has been hung and cannot be condemned as unwholesome, and can also be obtained from game or venison?—In the case of all flesh foods, no matter of what kind, if they are kept until they are high, it means simply that they are undergoing putrefaction, and there are risks attending their consumption because of the presence of these toxin or ptomaine substances.

12999. But you could not differentiate by experiments on animals between ptomaines derived, say, from butcher's meat that has been hung, and yet could not be pronounced to be unwholesome, and ptomaines derived from a dead body?—There is this source of differentiation with some of them. It would be correct, I think, to say that many of the toxins formed in the way we have been discussing would be destroyed by a temperature of boiling point, but that some others of them would not be so destroyed, and among these particularly that of Gaertner's bacillus. That is about the only way in which I can determine the difference between them up till now so far as science would enable me to distinguish them.

13000. Is not Gaertner's bacillus—that is to say the bacillus enteritidis—found almost anywhere where there is decaying vegetable or animal matter?—Not by any means always.

13001. But generally?—No, I would not say even generally; it is exceptional; that is the reason why meat poisoning outbreaks are so uncommon, considering the great use of meat in this country as compared with other countries. We do not eat our meat raw in this country; they do largely on the Continent, and if this Gaertner's bacillus be present in meat they are liable to have outbreaks on the Continent, as the evidence shows.

13002. Do you attach the poisoning of food then to the presence of the bacillus?—All the outbreaks which I have been speaking of were believed to be due to this Gaertner's bacillus or one of its allies in the same group.

13003. You have referred to the cerebro-spinal meningitis disease and to the passive carriers of the meningo-coccus. Has it not been proved lately, by Dr. Gordon, one of the experts of the Local Government Board, that very little reliance can be placed upon the examinations which have been hitherto made with regard to the presence of the bacillus in the excretions from the mouth?—The latest work that has been done on that is this: it is quite likely that the witness may be here, but I can anticipate his coming, if he does come, by stating the fact that within the present year an examination has been made individually of 370 contacts, that is to say, persons coming in contact with cases of cerebro-spinal meningitis in Glasgow and the results are as follows:—Of the 370 contacts 22 are to be deducted, for one reason, and in all, including these, 62, leaving for consideration 308 cases, occurring in 74 families. The meningo-coccus was found in 81 of these, or 26.3 per cent., that is to say that in 81 people out of 308 who came in contact with cerebro-spinal meningitis, but who themselves were not affected by it, in their upper air passages the meningo-coccus is proved by bacteriological examination.

13004. But has it not been demonstrated by Dr. Gordon, because I have just been reading his Report to the Local Government Board, that this meningo-coccus has been confused in these passive cases to which you are referring with the ordinary micro-coccus catarrhalis?—No, I tell you that this investigation was intended to bring out the difference between the micro-coccus catarrhalis and the meningo-coccus; there is a characteristic difference between the micro-coccus catarrhalis, which makes it different at once from the meningo-coccus. The difference is this: if you take a particular medium, that is to say, a material containing neutral red, the meningo-coccus produces an acid reaction; the micro-coccus catarrhalis does not, and that is a very marked distinction. These investigations were based upon that difference, and other differences obtained culturally.

13005. But do not you think that the difficulties attending such research are so enormous that errors are liable to creep in?—Precisely, and it is because we want to eliminate these errors that we hope the Commission will not do anything to prevent our being able to do so, but will give us more facilities.

13006. You think experimentation on animals the final court of appeal on all these points?—Yes, in this respect, that I think we can hardly use human bodies. I think the medical profession and scientific men have not spared themselves in investigations, but there are certain others that we cannot ask them to perform upon themselves.

13007. I will put it in this way. You have been talking about scientific poisoning. Supposing in the Lamson case a needle had been used instead of the poison being given by the mouth, do you think the poison could have been discovered by any process?—It would have made no difference. In either case the aconitin would have been absorbed into the circulation, and have to be recovered from the organs. The mouth administration had nothing to do with it.

13008. Do you attach as much importance to the result of experiments by the mouth as Stevenson did on applying the extract to his lips?—I think that aconitin has such a peculiarly characteristic numbing effect upon the mucous membrane of the lips and the tongue, that nothing else could be mistaken for it.

13009. That of itself was conclusive—the mere testing on the mouse was at the very outside only a slightly confirmatory test?—But may I put it in this way: before Dr. Stevenson could apply this test to his lip or his tongue he had to isolate this extract from the organs of that dead man; there were weeks of labour in that alone.

13010. But I mean in testing the extract?—The testing was the easiest part of it.

13011. It was not necessary really to test it on the mouse; was that of any value?—It was of this value that the infinitesimal amount injected into the mouse could produce the symptoms of poisoning by aconitin. A touch on his tongue could not produce them, but it could poison the mouse, and the mouse showed the toxic symptoms, and thus corroborated the test he originally carried out on his own tongue.

13012. But it would not have been considered a conclusive test of itself. By injecting aconitin into a mouse you could not say without any other experiments that the substance was aconitin?—I am afraid I do not follow your question or your reasoning. If I take the substance I extract from a plant in my garden called monkshood, and inject that principle into a mouse, and the mouse shows evidence of poisoning, surely there is no difficulty as to the cause and effect.

13013. But are the symptoms always the same?—Yes, the symptoms generally are the same.

13014. Have not experimenters differed greatly as regards the results that they have obtained by various testings?—Solely because different plants of monkshood contain different proportions of aconitin; that is the only thing. Every plant does not contain the same percentage proportion of toxic material; that is the only difference.

13015. I think you admitted that you do not perform experiments on animals to illustrate your lectures?—No, and I never should. I do not think in my work I should do it. I am speaking personally. I am not saying that other men may not require to do it. I am only saying that so far as I am concerned I should not use animals to illustrate my lectures.

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13016. But in illustrating the action of poisons do you think it is necessary, in order to teach the students?—No, I do not think I should do it personally.

13017. Or to illustrate the physiological action of medicines?—I have nothing to do with the physiological action. I am beyond the physiological stage, the toxic stage.

13018. Then, of course, I need not ask you whether you support what is called seropathy right through—serum-therapy?—I am quite convinced that nothing has made such a vast alteration in the death rate from diphtheria as the serum treatment of diphtheria. I do not think that any medicinal treatment could have been devised which would ever have effected what this serum treatment has done for diphtheria.

13019. You think it can be regarded as a sort of sheet anchor now in the treatment of the disease?—I have not the remotest doubt of it.

13020. What about serum therapy in cerebro-spinal meningitis?—That has not been so uniformly successful; the anti-meningo-coccic serum has not been so successful.

13021. Has it been successful at all?—In some cases there has been a cure, but I do not know whether you can say it is cause and effect quite so positively as in the case of the serum anti-toxin in diphtheria.

13022. But the anti-diphtheritic serum is the only serum that has been attended with what you might call success?—If you are dealing with the preventive material used in the prevention of anthrax then that has proved an enormous benefit to the world.

13023. I am speaking of sera?—I claim that about anti-toxin.

13024. (*Dr. Gaskell.*) May I ask one question with respect to what you said just now. Do not you think it is advisable for students to see, for instance, the action of strychnia. When you say that you would not show students any experiments, would you not show them experiments in which the animal was completely under anæsthesia?—I must qualify my last answer by the fact that if the animal was under chloroform then the experiment would be of no use.

13025. Not necessarily, might it not be with certain alkaloids?—Chloroform would mask the action of alkaloids you see. The only treatment for strychnia poisoning is chloroform.

13026. Naturally. I was not speaking of strychnia then; I was speaking generally. As regards the action of digitalin, for instance, or many other poisons, would you not show experiments?—You could not show an experiment on digitalin with an animal under chloroform; it would obviate the effect. The only experiment in a criminal case where digitalin was suspected of having been the poison was a case occurring in France; the animals used there were frogs without any anæsthetic at all. That was Tardieu's case. That is the only case in which an experiment with digitalin could be conducted in order to show true results.

13027. What I want to get clear about is this, that the experiments showing the action of various alkaloids and so on you would rather leave to the Pharmacological Laboratory?—That is so.

13028. That is what I understand. It has not come into your department as teacher of forensic medicine? No, because the doses that I give are toxic—fatal.

13029. (*Dr. Wilson.*) I think I gather from what you have just said that anæsthetics interfere very considerably with the results which you would otherwise obtain from the administration of many of these drugs and poisons?—Poisons not drugs. If I want to see the true natural action of a poison I do not want to administer another substance which may act poisonously at the same time and confuse the results.

THIRTIETH DAY.

Tuesday, 29th October 1907.

The Right Hon. The Viscount SELBY (*Chairman*).

Sir W. S. CHURCH, Bart., K.C.B., M.D.
Sir W. J. COLLINS, M.P., M.D., F.R.C.S.
Sir J. MCFADYEAN, M.B.
Sir M. D. CHALMERS, K.C.B., C.S.I.
Mr. A. J. RAM, K.C.
Mr. W. H. GASKELL, M.D., F.R.S.
Captain C. BIGHAM, C.M.G. (*Secretary*).

Mr. STEPHEN FRANCIS SMITH, M.R.C.S., called in; and Examined.

Mr. S. F. Smith, M.R.C.S.

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13030. (*Chairman.*) You are a member of the Royal College of Surgeons?—Yes.

13031. And you are Surgeon to the Anti-Vivisection Hospital?—Yes.

13032. And author of two works on vivisection, "Scientific Research" and "Fruitless Experiments"?—Yes. I may mention also that I am a member of the International Medical Anti-Vivisection Association.

13033. I do not quite understand what the Anti-Vivisection Hospital is. I do not understand its title. Does it mean that no operation is performed there upon animals—that there is no laboratory or anything of that kind; or does it mean more than that?—Primarily it is a hospital; secondarily it has been founded by anti-vivisectors, who took that opportunity to make a kind of public protest against vivisection by the name which they gave to the hospital.

13034. That is to say, it is supported by anti-vivisectors. That is practically what it means?—Yes.

13035. In the treatment of patients you use the usual remedies, I suppose, used in other hospitals?—We do not bar anything that will do the patient any good.

13036. You do your best to cure your patient with whatever remedy is most suitable?—Yes.

13037. That, I suppose, applies both to surgery and to medicine?—Yes.

13038. You have formed an opinion as to the amount of sensibility of animals to pain as compared with man, I think?—I should like to mention first the evidence from special senses in the lower animals. We find that the sense of smell is stronger in many of the lower animals than in man; it is well known to be the case in dogs; and from travellers' accounts of hunting game the scent is stronger in them. I have read in many travellers' accounts of travels in South Africa that their oxen would smell water at a long distance, and very much to the surprise of the travellers. As regards sight, it is well known that vultures can see at an enormous distance, and although that may depend partly on the structure of the eye, yet without a highly developed sensitiveness in the retina, and in the brain centre to which the visual impulses are conducted, that structure of the eye would be comparatively useless. As regards hearing, I have found in my own dog that it has a very remarkable power of hearing, far greater than that of any human being. What I wish to point out is, and I am going to show presently, that there is a widespread belief that sensibility to pain depends upon intelligence; but here I simply wish to indicate that if that theory were correct it would prove that the lower animals could not smell, hear, and see as they do. Next I wish to say something about the teaching of biology and evolution. Wherever a sense or structure is necessary to the preservation of the animal, that sense or structure will be highly developed. Pain has a function. The greater the sensibility to pain, the more care will the animal take to avoid danger and preserve life. Pain is a more important factor to the lower animals than to man. Man, inferior in the special senses which warn him of danger, inferior in strength, speed, and natural weapons to many of the lower animals, makes up for these deficiencies by intelligence. Similarly, his intelligence guards him to some extent against danger, and there is less need for acute sense of pain. We have also the teaching of psychology. There is a popular belief that pain varies with, and depends on, intelligence. This view is contradicted by the evidence of the special senses I have mentioned, and lands us in the absurdity of having to believe that a man can smell more keenly than a dog. It is contradicted also by psychology, which tells us that the higher thoughts and feelings are due to the compounding and recompounding of the simple sensations, while the popular view, on the contrary, is that the simple comes from the complex. I wish to point out that the only right principles on which we can form a conclusion as to pain which the lower animals feel is by taking into consideration the surroundings of the animal; that is, if pain is necessary to the survival of the animal we may conclude by all analogy of evolution that the sense of pain will be highly developed.

13039. I should just like to follow what you mean by the evidence of the special senses. There are some savage races which have a much acuter sense of smell and sight than some civilised races, are there not?—Probably, yes.

13040. Do you think that those savages have necessarily a more acute sense of pain?—Not necessarily. But in the case of a savage there may be something in the surroundings of the savage which make it necessary that the sense of sight should be highly developed, and possibly, though I do not know, about the sense of smell. But it is said that Arabs have long sight, and it may be necessary, or was necessary in the ancestor of the Arab, that he should have long sight in order to see an enemy at a long distance; I would simply point out that his sight was modified to his conditions of life; but I was specially comparing these evidences of special senses with the belief that sensation is proportioned to the intelligence.

13041. Would you say that of different races of men? Would you say that the sensation of the negro in West Africa is more acute than that of a civilised man or as acute?—It might be very difficult to judge directly; but if pain has been necessary to the preservation of the negro I should say that it would be as well developed in him as in civilised man.

13042. You would not say that such a negro in his wilds was equal in intelligence to a cultivated white man in England?—No, I should think not.

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13043. But you think his sense of pain would be the same?—I should think so.

13044. (*Sir Mackenzie Chalmers.*) Greatly increased, ought it not to be?—It ought to be greater.

13045. (*Chairman.*) You think it is greater?—I should think probably it is greater. But I should like to mention my own experience as regards different classes of people. In my occupation it has been my business to inflict a good deal of small pain without anæsthetics on patients, and I have never been able to observe any difference between my hospital patients and private patients. I spoke to a dentist—a dental surgeon to a hospital—and asked him his opinion, and he said he thought there was no difference—that the only difference he could see was that hospital patients gave greater manifestations of pain.

13046. I was interrupting you rather. Have you said all that you have to say on that question?—I was going on to say that it is obvious that if this untenable belief is held by physiologists it is a danger to animals under experiment. I therefore give evidence that such belief is held. Some years ago I received some literature from the Secretary of the Association for the Advancement of Medicine by Research. In it was a pamphlet by an anonymous writer, maintaining that the lower animals are less susceptible to pain by reason of their less intelligence. I was for some time at the Pasteur Institute in Paris. I observed laparotomy (opening of the abdomen) performed on rabbits without anæsthetics. I spoke to an American medical man who had been for some years in the Pasteur Institute, and asked him why an anæsthetic was not given, and he told me it was because the rabbit did not mind the operation. He was not making a joke; that was his genuine opinion. I give the opinions of some English physiologists, Professor Huxley and Dr. Anthony, who, at the last Royal Commission, maintained at Questions 2547 to 2560, that animals do not feel pain as we do, and that their struggles are mere automatic or reflex movements. Similar opinions at the same Commission were given by Dr. Pavy and Dr. Pye-Smith.

13047. Do I correctly understand you to say that Professor Huxley said that all struggles by animals, when painful operations were being inflicted upon them, were merely reflex movements?—I think it was left somewhat indefinite. I do not think he said all.

13048. I see he was asked: "So that an operation which is constantly performed on sows in order to get them to fatten a little faster is an operation decidedly more painful than that which you say is carried on for scientific purposes (that is spaying)," and the answer is, "Yes." That is Dr. Anthony giving evidence and Professor Huxley asking the questions?—Yes, he was asking leading questions.

13049. I will not read through the whole of the questions and answers that you refer to, but perhaps some other member of the Commission will have an opportunity of examining you upon them. That is what you say is the effect of his evidence?—Yes. I was going on to say that similar opinions at the same Commission were given by Dr. Pavy, Dr. Pye-Smith, Dr. Burden Sanderson, Dr. Crichton Browne, Mr. Schäfer, Mr. (now Lord) Lister, Professor Humphrey, Dr. McDonnell, Dr. Cleland, Dr. Sibson, Mr. George Henry Lewes, and, in the *Nineteenth Century* of December, 1881, by Sir James Paget.

13050. And all these gentlemen, you say, were of opinion that animals feel less?—Yes.

13051. (*Sir Mackenzie Chalmers.*) Do they give reasons?—No, I think not.*

13052. (*Chairman.*) I suppose they were asked questions about it, were they not?—I do not think they were. Dr. Cleland said that it was doubtful if frogs feel at all, and would therefore no more think of anæsthetising a frog for an experiment than a worm for fishing. (That is at Questions 4615 and 4616.) It was admitted by Dr. Sibson (Question 4752) that he had no ground for believing curare an anæsthetic, but had employed it as such in painful experiments.

13053. As to curare, I think everybody who has been before us has admitted that curare is not an anæsthetic?—That is not the point that I am referring to at present.

13054. It is what you were reading?—Yes, but it has reference to what I am going to say in a moment. There were similar admissions from Dr. Rutherford (Questions 2918 to 2935). This is all on the point of their belief. I am now going on to some fresh matter. Here is an experiment described by Dr. Rutherford at Questions 2918 to 2934, with his opinion and the opinion of Dr. Sibson: "A dog was starved for 18 hours; it was paralysed by curare through a cut in the throat, and artificial respiration was kept up. The abdomen was cut open and a tube inserted into the bile duct. The animal was kept in this condition eight hours, substances being frequently injected into the bowels. No anæsthetic was given because, in Dr. Rutherford's words, the "result of the experiment might be interfered with." He expresses his opinion of this operation that the pain was trivial.

13055. What was the date of it?—I do not know that any date was given. I have quoted the question in the last report. This opinion was endorsed by Dr. Sibson at Question 4760, because the animal was kept quiet by curare. Dr. Sibson's words are so remarkable that I give them: "There would be no struggling, the incisions would be made with great ease, the animal's mind would be withdrawn from what I would call the domain of attention, and when there is no attention there is no sensation, no sensitiveness, no pain." There are many more similar opinions in the Report of the last Royal Commission. At the present Commission the Inspector for Scotland (Question 750), expressed the opinion that pain varied as the intelligence, and when in Question 741 he was confronted with the evidence of the special senses, he made the curious and ambiguous statement that it is not every kind of dog that is good at smelling. He concluded that this evidence of special senses showed less susceptibility to pain. I give two more proofs of this wide-spread belief. In Mr. Keetley's "Index of Surgery," pages 180 and 181, second edition, is the operation for hare-lip in children. I quote, "Chloroform unnecessary. Secure his limbs by rolling him up tightly but firmly in a towel. If desired anæsthetic vapour may be pumped through a catheter." Then comes a description of the operation for which chloroform is unnecessary: "Begin by separating with the knife the two sides of the lip from the jaw subjacent. . . . Then pare the edges of the cleft. Remove enough. . . . In double hare-lip the intermaxillary nodule is pared"——

13056. I do not quite see how showing that certain operations were performed without anæsthetics, and were justified as being performed without anæsthetics, as I understand, before 1876, is material here, because we have now the Act of 1876, and what we have to inquire into is as to the desirability of amending it. Nobody has suggested to us that it should be recommended to dispense with anæsthetics?—That is not the point that I am trying to bring out at all. I am on one point only. That one point is that there is a wide-spread belief that pain varies as intelligence varies. That is the only point that at present I am trying to bring out.

13057. I think you have done that already by telling us the evidence of some eight or ten eminent medical men who all pronounce that opinion, and I think that opinion, no doubt, does prevail?—Then I will not pursue that subject further.

13058. That experiment is not on animals, it is on a child?—I know; but it has the same inference.

13059. (*Sir Mackenzie Chalmers.*) The inference being that the child feels as much as a grown-up being, although less intelligent?—Yes, but that this surgeon, at all events, did not believe it.

13060. Was he speaking solely with regard to children, or did he think the operation for hare-lip on anybody could be done without anæsthetics?—There is nothing in the book to reply to that question.

13061. (*Chairman.*) I really do not think you need labour that question about what the general opinion is, because, as I say, you have given already the evidence of a great many medical men thirty years ago,

* *Some of them gave reasons.—S. F. S.*

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and I think that is the general view now. I am not speaking of that as a scientific matter myself, but as a general opinion?—Then I will pass over it. I wish to point out, however, that there are certain reasons why this belief is stronger in physiologists than it is in other people. This belief in physiologists is strengthened by two circumstances: First, there is the blunting of the sensibilities. I wish to give my own experience about that. The first time I saw a brutal experiment on an unanæsthetised animal I wished to leave the room—I was sickened by it. The next time I was less affected, and with every experiment I was still less affected, until eventually I was able to look on at the most terrible things without my emotions being moved in any way, and I had left only my intelligence to tell me that the animal was being tortured. And I submit that what occurred in my own case probably occurs to everybody who witnesses an experiment. Now English vivisectors go abroad. These experiments to which I am referring are continental experiments. That they go abroad I know from my own personal experience. Professor Starling told me that he spent some months at the Pasteur Institute; Professor Heger, of Brussels, told me that Sir Michael Foster spent some months there; and I have heard one or two other Englishmen say that they have vivisected abroad. But besides the blunting of the feelings from often witnessing experiments, there is another circumstance which I think deceives the experimenter. Animals which do not often use their vocal chords frequently do not give the manifestations of pain when under severe suffering. Of course, everybody knows that a horse, which does not use its vocal chord often, will endure severe flogging without making a sound, and similar things occur with rabbits. There is one other circumstance, too, which I think deceives the experimenters, and that is that I believe when an animal is under very severe torture it may be to a certain extent paralysed. Mantegazza, in his work on pain, described how he kept animals quiet when he was subjecting them to torture. He said there were two ways; one was by nailing them down by their feet, so that he seriously increased their pain with movement. Another means was by making the pain so severe that the animal was paralysed.

13062. When was this book written?—It was written a long time ago.

13063. Fifty years ago?—No, less than that. I could not tell you the exact date.

13064. What country did the gentleman who wrote it belong to?—Italy. The only point I am trying to bring out is that physiologists may be deceived. In experiments on rabbits, for instance, the severity of the pain may possibly paralyse the rabbit and it may not give the manifestations of pain, and therefore the experimenter may be deceived by that. That is the only point I am trying to bring out. That is all I have to say on that point.

13065. Then the next point I think you want to speak about is the evidence of pain inflicted by English experimenters?—Yes. As to direct evidence, we have nothing but what experimenters disclose. From an analysis of every experiment published in the "Journal of Physiology" from 1896 to 1900, inclusive, I have found that 71 per cent. were cutting experiments.

13066. What was the gross number of experiments in those five years?—I could not tell you. It was not very great. I have simply mentioned that I referred to every experiment in the "Journal of Physiology" for five years. This percentage refers to the nature of the experiments only, for in many cases these were repeated on numerous animals, the precise number of which is not stated. Of these cutting experiments, the animals were not killed at the end of the experiment in 31 per cent.

13067. Is that 31 per cent. of the whole or of the 71?—Of the 71 per cent.

13068. 31 per cent. of the 71?—Yes. The period for which they were kept alive afterwards varied from days to months. In the great majority of these cases I do not think there is much suffering, judging by the analogy of patients recovering from operations, but I think there is some. I am well qualified to state what may be felt after an operation from a personal experience of last year. After an operation for varicose veins one of the wounds, about an inch long, inflamed; there were some pent-up discharges, and the pain I suffered from them was very violent, I might call it almost unbearable. I was obliged to drug myself heavily with morphia. I also wish to point out that it has been said that these operations are done with precautions as to surgical cleanliness, but in an animal there may be very great difficulties in doing it. We have first to assume that the experimenter will always take the trouble to shave away the fur from the animal—that he would not be tempted to omit that. Also an animal will remove its dressings; it tears them off or rubs them off. It is very doubtful whether the wound could be kept surgically clean for that reason. There is another disclosure in the "Journal of Physiology," that in the cutting experiments curare was used in 10.3 per cent. That refers to those in the "Journal of Physiology" for the five years that I have mentioned.

13069. Can you say what particular class of experiment it was in which the curare was used?—I cannot. It is some years since I made this analysis.

13070. Can you say who it was?—I cannot. All I have left is my notes.

13071. (*Dr. Gaskell.*) Instead of saying 10.3 per cent. could you say the actual number of cases in which curare was used?—I cannot, but one can easily find it out by going to the "Journal of Physiology" for those years.

13072. (*Mr. Ram.*) Do you mean used without anæsthetics?—No.

13073. In addition to anæsthetics?—In addition to anæsthetics.

13074. (*Sir Mackenzie Chalmers.*) How many hundred cases altogether would be included. Would it be something like 200 or 300?—No, there would be a less number of experiments made. I cannot tell you the number of animals experimented on, but I do not think there were 100 in the whole five years.

13075. (*Dr. Gaskell.*) Have you a list of the names of the experimenters you are referring to?—No.

13076. (*Sir Mackenzie Chalmers.*) So that these percentages are calculated on a single 100 or less?—They are calculated on as many experiments as are mentioned in the "Journal of Physiology" for the five years.

13077. (*Mr. Ram.*) It may be less than 100 in the whole time?—Yes, I think so.

13078. (*Chairman.*) Have you any observation to make about the use of curare with anæsthetics?—Yes. I think I may pass over the physiological evidence that curare is not an anæsthetic; it is admitted.

13079. Yes?—But that is denied on the strength of one experiment which is in Mr. Paget's book of a servant girl who was poisoned by an arrow in her master's hall and became unconscious and did not feel pain. I think I might refer to that, because it is mentioned in Mr. Paget's book, and it is stated as evidence that curare is an anæsthetic. What I wish to point out regarding that case is, firstly, that there was no positive evidence that the poison was curare; secondly, there is no evidence that artificial respiration was either prompt or efficient. If it was not prompt or if it was not efficient, the asphyxia would account for the unconsciousness. The only authentic account of experiment on man is given by Liouville in the "Bulletin Général Therapeutique," 1865, page 404. An overdose had been given and artificial respiration had to be kept up until the patient recovered. Liouville adds: "The patient then related all he had felt, the preservation of his intellect, the annihilation of all power of movement, of which he gave a clear account, witnessing all that went on around him without being able to take any part in it, the fears expressed by some assistants present being by no means reassuring." Claude Bernard believed that curare greatly intensified the susceptibility to pain, and in dramatic language has stated that under it he inflicted the most atrocious suffering that the mind of man can conceive. That he was correct there is every reason to believe. The afferent impulse has no escape along the motor channels. Herbert Spencer has in

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several places emphasised the fact that an impulse is lessened by escape through surrounding channels.

13080. (*Dr. Gaskell.*) Have you got the quotation of Claude Bernard, where he says that it intensifies pain?—I have not. It is such a well-known quotation and has been so often made that I did not think it would be questioned.

13081. It is the well-known quotation that you are referring to?—Yes.

(*Sir John McFadyean.*) Would it not be easier to find the references if it is so well-known?

(*Sir William Collins.*) At page 282 of Mr. Paget's book I think a reference is given.

13082. (*Chairman.*) That is hardly a quotation, and very likely does not give the whole of what Claude Bernard said. Mr. Paget wrote, "Claude Bernard believed that it did not in any way affect the sensory nerves, and he described in theatrical terms the animal as being unable to stir, but suffering horrible torture." That is an extract from the "Edinburgh Review," for July, 1899. It does not profess to give the exact words beyond that. Perhaps you will go on with your evidence?—I assert that every time curare is given to an animal there is danger of most terrible suffering being inflicted. I think it is certain that in the majority of cases, possibly in every case, the most appalling torture is perpetrated. My reasons are as follows. In giving anæsthetics to human beings, great care is necessary to give enough to produce unconsciousness and to avoid giving more than is required, so endangering life. There are certain indications as a guide. After a certain time absence of the corneal reflex shows that the anæsthetic may be stopped. Reappearance of the corneal reflex shows that more may be given. A slight movement of the patient (which, however, rarely happens with a skilled anæsthetist) shows that more must be given at once. With these guides in human patients, anæsthesia is safe and efficient, although the quantity of anæsthetic required by one patient may differ considerably from what may be required by another similar patient. In a curarised animal these guides are totally absent. Moreover, there are no means of knowing the idiosyncrasy of the animal as in human patients. My experience is that one dog requires more anæsthetic than another of the same size. Further, my experience is that to give just the right amount to a dog, that is, to give it enough for anæsthesia, without killing it, is more difficult than in human beings. My belief is, therefore, that if ever a curarised dog is properly anæsthetised it is by a lucky chance. I do not think there is the consolation of believing that in these curare cases with incomplete anæsthesia (I am presuming there is incomplete anæsthesia) the ether or chloroform given lessens the pain, as would be the case with morphia, I believe, that is to say, that when such anæsthetics as ether, chloroform, and gas are given, the dividing line between complete anæsthesia and complete consciousness to pain is a very sharp one. I have only two proofs to give of this. One is an operation on myself. I was given gas by an experienced anæsthetist. A hole was bored in my jaw, an operation of a few seconds. I felt an inability to move, and pain which appeared to me almost as bad as any pain could be. The second case was that of an intelligent man whom I anæsthetised. I had given him a considerable amount of A.C.E. (alcohol, chloroform, and ether). I believed his corneal reflex was absent. I told the surgeon he might begin. An incision was made. The patient struggled, and the surgeon had to wait. I questioned the patient afterwards, and he gave as his opinion that he could not have felt the pain more if he had had no anæsthetic at all.

13083. (*Sir Mackenzie Chalmers.*) What operation was that?—It was an operation for the removal of suppurating glands.

13084. (*Chairman.*) But in the case of a skilled anæsthetist, has he not sufficient guide, although the subject may be curarised, for seeing whether he is still under the influence of the anæsthetic?—The indications are all absent; there is no indication.

13085. That is what I wanted to know. Are they all absent?—They are all absent.

13086. There is nothing by which he can tell whether pain is felt?—Absolutely nothing.

13087. Is that the view of all anæsthetists?—Anæsthetists have nothing to do with curarised animals.

13088. But under the Act the anæsthetist is obliged to keep the animal under the anæsthetic to prevent pain; then it is his business to see that the animal is kept free from pain. The operators tell us that when curare is administered it is not with the view of enabling them to be careless about the anæsthetic, but with the view of obtaining absolute stillness and absence of reflex movements. Supposing that that is their object, namely, to keep the animal under anæsthetics while at the same time it is rendered absolutely still by curare, can they not discover whether the animal is fully under anæsthetics?—I have just pointed out that the only guides which they have which are present in an animal without curare are absent in the case of a curarised animal.

13089. Could the anæsthetist not tell by the eye or breathing, or anything else?—No.

13090. (*Sir John McFadyean.*) Are all these statements based on your own personal observation of attempts to anæsthetise animals that are curarised; have you ever seen this thing done?—Yes.

13091. (*Chairman.*) I am speaking with some doubt as to my memory, because it is some long time ago, but I think we have had skilled anæsthetists before us who have not come to the same opinion as yourself on that matter, and that is the reason why I asked you the question?—It seems to me obvious, because all you have got is the reflex of the cornea and the movement; that is all you have got in a human patient to guide you; in a human patient you have absolutely nothing else.

13092. However, that is clearly your opinion?—Yes.

13093. (*Sir William Collins.*) Does anyone maintain that the reflexes can be used as a guide in the case of a curarised animal?—No, nobody would maintain such a thing; it would be absurd.

13094. (*Chairman.*) I am not pledging myself as to what previous witnesses have said. I was rather under the impression that they had said that you could distinguish?—I am quite certain that no witness could have said that the corneal reflex is present.

13095. No; I used a very wide expression—whether there was anything that would give them the information?—Something was said about blood pressure, I believe, by one witness, but that is a point on which you want a great deal of corroboration of evidence.

13096. Or as to the quantity of anæsthetic that is being administered; would that be a guide?—I have already pointed out that the amount of anæsthetic that is required by one animal differs from the amount that is required by another animal of the same size. That has been my experience. If you only give enough to keep the animal alive you are in great danger of not having anæsthesia, and if you give sufficient to be more than certain that the animal is anæsthetised you will be in great danger of killing the animal.

13097. But that risk has to be run?—That risk has to be run; but it would depend upon the physiologists to say that they kill a great many animals when they are under curare.

13098. Is there anything more you wish to say upon that subject?—I think that is all I have to say about it.

13099. Then you wish to say something about the usefulness or not of vivisection?—I wish to state, first, that the reason why I am selecting certain claims is that when this book, "Experiments on Animals," was published by the Secretary of the Association for the Advancement of Medicine by Research, a number of medical men, including myself, were requested to inquire into the accuracy of the statements made therein. The work was split up, and what I am giving you now is what fell to my share. Firstly, on drugs. I am giving only a small part of the evidence which exists. I give certain facts first, and the inference to be drawn I shall place at the end. I may state that I am giving now only those drugs which begin with the letter A. I shall not go further than the letter A for the sake of brevity. I begin with alkalis. Referring to the experiments on the lower animals, Dr. Ringer says ("Therapeutics," page 127, fifth edition) that the results are contrary to experience of the action on the human body, and he indicates further that the animal experiments are in them-

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selves contradictory. As regards acetate of ammonia, Stillé and Maisch ("National Dispensatory," page 836) point out that, given to healthy men, there are no decided symptoms; but that in rabbits there are fatal tetanic spasms and dissolution of the gastric mucous membrane. Amyl nitrite was discovered by the French chemist Balard in 1844. Later, Professor Guthrie, when distilling it, found it producing flushing of the face. The natural deduction is that the blood pressure would be lowered if the small arteries are dilated. Still later, animal experiments demonstrated that the blood pressure is lowered, but this was only corroboration of clinical observation. Apomorphia: The clinical effect of this drug on the heart is flatly contradicted by the animal experiments of Siebert and Moerz ("Bartholow's Materia Medica," page 459), and Wood (page 437) points out further discrepancy between human and physiological investigation. I pass over as of less importance, though interesting, the various contradictions between physiologists themselves as to the action of this drug. Arsenic: Drs. Ringer and Murrell ("Journal of Physiology," I., page 217) obtained on frogs different results from those of Dr. Sklarck, of Berlin. They believed the discrepancy to depend on the time of year. I prefer to leave it to a physiologist to answer the problem. If arsenic has a certain effect on frogs in May, what effect would it have on man in March? The absurdity of these experiments is as obvious as the unconscious humour of the experimenters. Atropine (obtained from belladonna): Its well-known action of dilating the pupil in man is absent in birds (Stillé). Birds and herbivorous animals eat belladonna with impunity. "This is one of the many examples," remarks Stillé and Maisch (page 276), "which show the danger of concluding from the lower animals to man in regard to the uses of medicines." I pass over the disputes among physiologists as to the effects of this drug upon animals. I observed this morning that I have left out the drug aconite, which begins with "A," about which similar observations could be made; but for the sake of brevity I state briefly that similar observations can be made concerning the following:—Benzoic acid, bronchitis, caffein, Calebar bean, camphor, carbonic acid gas, chloral, chrysophanic acid, citric acid, coculus indicus.

13100. When you say similar observations, what do you mean precisely—observations that they have not the same effect on man as on animals?—Yes, and that there have been diverse views among physiologists themselves as to the effects of these drugs.

13101. You say that you cannot argue from experiments on animals as to the effect on man in the case of these drugs?—No. Colocynth, conium, sulphate of copper, mercury, croton chloral hydrate, croton oil, coriaria, elaterium, muscarine, gamboge, glycerine, gelseminum, ground ivy (which is glechoma), guarana, hamamelis, hyoscyamis, ipecacuahana, jaborandi, lead, milk (when injected into veins), kalmia latifolia, musk, nitrate of silver, nitropentane, opium, phosphoric acid, podophyllin, rhus toxicodendron, prussic acid, quinine, ergot, sanguinaria, sarsaparilla, senega, soda, sow-bread (cyclamen), cantharides.

13102. Take prussic acid, for instance, is not that fatal both to animals and man?—Yes. I have not said that all the properties of drugs differ. I have only said that there are differences.

13103. What is the difference in the operation on man and animals in the case of prussic acid?—I have not got my notes here at present. I have only given you the particulars as to certain drugs. I simply mention that there is evidence regarding all the remaining drugs for the sake of brevity. If the Commission wished to listen to it all I should be pleased to have spoken for an hour on all these drugs.

13104. We do not want to hear about them all. I would suggest that you might have taken half a dozen of the greatest cases; but prussic acid is a poison that is so commonly known, and is known to be used so often for killing animals, that I thought I would ask you about it, as it caught my ear?—A drug may have common properties; for example, it may kill both; but it may also have some properties which are different. Prussic acid is not used as a medicine in man for killing; it is used for other purposes. You may have a drug which may have some properties in common and yet will have properties which differ.

13105. But you are not at the present moment, without your notes, able to say what the particular difference is in the operation of prussic acid on man and on animals?—No, I am not. Then there are squill, stramonium, strychnine, tartar emetic, thein, coriaria sarmentosa, tobacco, trimethylamine, veratria, and solanum dulcamara. An account of one drug not mentioned—namely, digitalis—is so important that it should be given. I do so because it is so often stated that the action of this drug was discovered by experiments on animals.

13106. Some effects of it. I do not think that anybody has come to say that its use as a drug was not known independently of animal experimentation; but I think what has been contended is that it was discovered to have properties and effects on particular parts of the body which were not understood without experiments on animals?—Then, in that case, if the Commission is of that opinion, I will pass over that. The evidence is overwhelming that whether with old drugs or new, any conclusion from experiments on animals are absolutely untrustworthy, and would be dangerous if relied on. Mr. Berdoe, in his brochure on drugs, puts this clearly. He says: "Let us imagine that a quantity of a new drug, called opium, is being examined for the first time by a special committee. Let us assume that nobody knows anything about its properties except some vague travellers' tales. The physiologists proceed to investigate its action by a long series of experiments on animals. They give it to frogs, and they find that small doses throw them into tetanic spasms. They give 20 grains to a pigeon, which is none the worse for it; 30 grains are given to a rabbit with no effect. They are beginning to think the travellers' tales exaggerations, especially as they discover that ducks and chickens, like the pigeons and rabbits, are never the worse for its administration. They resolve now to try it on a human patient, and proceeding with extreme caution, as they think, they decide not to venture at first beyond the dose they gave to the pigeon—namely, 20 grains. The patient is a powerful navvy. He is promptly killed. If physiological medicine were of any value, surely the method followed by these investigators was right and cautious. Yet how fatal their method when reduced to practice."

13107. Are those physiological facts, that opium, for example, does not operate on pigeons and rabbits and frogs?—Yes. I wish to compare these facts with the statement of Sir Lauder Brunton, at Questions 6929 and 6930, before the present Commission, that animal experiments are absolutely necessary to learn the effects on man, and to learn, in Sir Lauder Brunton's own language, "a safe dose." I would draw attention also to his statements at Questions 6893 to 6897. They are that Gamgee's animal experiments led to the observation that the pulse tension in man is lowered by nitrite of amyl, and that this observation pointed to the treatment of angina pectoris by nitrite of amyl. This may be, but it was previously known that nitrite of amyl caused flushing. The deduction that the pulse tension would be found lessened is so glaringly obvious that if it was really overlooked great want of commonsense was shown. Gamgee's experiments should have been unnecessary. That is all I have to say about drugs.

13108. Then I think you desire to make some observations upon the claims in favour of the value of vivisection, or rather, I should say, of experiments on animals, because some are not cutting experiments, made by Mr. Stephen Paget in his work "Experiments on Animals"?—Yes; the action of drugs, of course, is part of that.

13109. And I think you take first the case of inoculation in typhoid fever?—In what I am going to say now I am only stating what I have published. Dr. Washbourn, Physician to Guy's Hospital, writes ("British Medical Journal," April 20th, 1901): "With regard to the value of inoculation, I am satisfied, from clinical observation, that it does not modify the disease. Mild, severe and fatal cases occur among the inoculated and non-inoculated, and, as far as one can judge, with the same frequency. Whether inoculation diminishes the incidence of enteric fever can only be determined by extensive statistics. From my own personal experience I should not think that the incidence is diminished." Dr. Melville, late civil surgeon to the Natal Field Force, writes ("British

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Medical Journal," April 20th, 1901): "Inoculation.—Of my 295 cases, 30 were inoculated, the remaining 265 being unprotected in this manner. A few statistics comparing the results obtained in the two classes of cases are not devoid of interest. From the following table it will be observed that the uninoculated cases compare very favourably with the inoculated. The complications were more numerous, the duration of the fever longer, and the death rate higher in the inoculated." The table is as follows:—

		Complications.			Deaths.	
—	Number of Cases.	Number of Cases.	Percentage.	Duration of Fever Days.	Number of Deaths.	Percentage.
Inoculated - - -	30	12	40·00	29·57	2	6·67
Uninoculated - - -	265	55	20·75	21·38	5	1·89
Total - -	295	67	22·71	22·21	7	2·38

That table shows that the percentage of deaths in inoculated cases was 6.67, and in the uninoculated, 1.89.

13110. (*Sir William Church.*) These figures which you are quoting are the experiences of those two gentlemen in South Africa?—Yes.

13111. They are not in any way a table of figures compiled to show the effect of inoculation or non-inoculation scientifically; they are merely a report of just such cases as happened to come before them during the South African War?—That is all. I have another table here which sets forth the complications met with in the uninoculated cases compared with those met with in the inoculated.

—	Relapse.	Malaria.	Pneumonia.	Hæmorrhage.	Thrombosis.	Cerebral.	Peritonitis.	Erysipelas.	Total.
Inoculated - -	5	2	2	1	1	1	–	–	12
Uninoculated - -	18	3	9	16	4	1	3	1	55
Total - -	23	5	11	17	5	2	3	1	67

13112. (*Chairman.*) In Mr. Stephen Paget's book, in that part relating to typhoid fever, there are a good many tables of experience, where the inoculated have suffered less than the uninoculated, are there not?—Yes; all that I have said would show that there is a doubt about the matter.

13113. You have cited some in which the uninoculated came off better than the inoculated?—Yes.

13114. And he cites a good many others as well in which the reverse is the case?—Then, as I say, all that I have said simply shows that there is a doubt about the matter. I do not claim anything more than that. I next come to malaria. It is implied, though not definitely stated, by Mr. Paget that experiments on birds were necessary and essential in proving that mosquitoes convey malaria. Let us see how far he is justified in this. The following history is even admitted in Mr. Paget's book. It had long been suspected that mosquitoes caused the disease. The question was obscured by the circumstance that the common variety does not convey the poison, and that the kind responsible, the anopheles, does not buzz nor cause much irritation by its bite. In 1880, Laveran discovered the specific organism in the red blood corpuscles. In 1894 Manson suggested that this organism was removed from the blood by mosquitoes, and by a special variety. In 1895, Ross made observations on the organism found in mosquitoes after feeding them on malarial blood. The mosquito theory had now strong ground for credence. It remained to be proved that persons protected from mosquitoes in a malarial district would not get the disease, and that men bitten by the anopheles in a non-malarial district would get the disease. This was carried out and proved by a commission from the Tropical School of Medicine in 1900. Now, where was the necessity for experiments on birds. Could not a person of average intelligence have thought of the last experiment mentioned? It is true that Ross in 1898 showed that another variety of mosquito could convey an analogous disease to birds, and he also showed that this variety could not convey the malaria parasite to man. His experiment then was valueless, because it did not touch on malaria in man. That it was inconclusive was clearly the opinion of the Tropical School of Medicine, as evidenced by the fact that the final and convincing experiments on man were undertaken to settle the matter.

13115. What experiments on man are there that you refer to that were finally taken up?—One experiment was that they took a man who had had malaria previously, and therefore was very susceptible to it; they put him in a malarious district in Italy, where they kept him protected from mosquitoes, and they found that he did not get malaria. And the reverse experiment was that of I believe it was Dr. Manson's son, who allowed himself to be bitten in England, where there is no malaria, by malarial mosquitoes brought from a malarial district, and he contracted the disease.

13116. I understand that you do not doubt that mosquitoes do convey the disease?—I do not.

13117. But you say that experiments on animals were unnecessary to prove it?—They had nothing to

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do with the discovery, as is inferred, and as has been stated.

13118. (*Sir John McFadyean.*) Could you say whether that is Major Ronald Ross's own opinion as to the usefulness or uselessness of the researches which he had previously himself carried out on malaria in birds?—No, I have no information on the point.

13119. Would it affect your opinion if you knew that the discoverer himself did think so?—It would not affect my opinion unless he showed me the evidence. His mere opinion would not affect me.

13120. (*Chairman.*) Is that all you wish to say about malaria?—That is all.

13121. Then what is the next subject?—Suppuration and blood poisoning, I think, is the next. Mr. Paget writes: "The study of the micrococci has not only proved and made perfect the present methods of surgery; it has also discovered a serum for the cases of micrococcic infection. Take, for example, the disease called ulcerative endocarditis, the growth of micrococci on the valves of the heart, whence they are carried by the blood into different parts of the body. Several cases have lately been reported where recovery from this most hopeless disease has followed the use of the anti-streptococci serum. It has given excellent results in cases of puerperal fever and in cases of dissection wounds. Already, though it is a new thing, it has saved many lives, and is steadily gaining ground in practice." The truth of this statement can be estimated by comparing it with the following, which appeared in a leading article of the "Lancet" (November 18th, 1899): A valuable communication has been published ("American Journal of Obstetrics," September, 1899) by a committee appointed by the American Gynæcological Society to report upon and to investigate the value of the serum in the treatment of puerperal infection. A summary of the cases hitherto reported is given, showing that the results in 352 cases had been collected with a mortality of 20.74 per cent., and after a careful study of the literature of the subject the members of the committee formulate certain conclusions, amongst which they state that clinical observation has shown that the results obtained by employment of the serum leave a great deal to be desired, and apparently indicate that it has little, if any, effect upon the general course of streptococcic puerperal infection. The conclusions arrived at after careful consideration of the personal experiences of those forming the committee are even more emphatic, and include the following: "Experimental work has cast grave doubts upon the efficiency of anti-streptococcic serum in clinical work by showing that a serum which is obtained from a given streptococcus may protect an animal from that organism, but may be absolutely inefficient against another streptococcus, and that the number of sera which may be prepared is limited only by the number of varieties of streptococci which may exist." Again, they say: "The personal experience of your committee has shown that the mortality of streptococcus endometritis, if not interfered with, is something less than 5 per cent., and that such cases tend to recover if nature's work is not undone by too energetic local treatment." And, finally, the report ends with the words: "We find nothing in the clinical or experimental literature, or in our own experience, to indicate that its employment will materially improve the general results in the treatment of streptococcus puerperal infection." At the meeting of the British Medical Association held at Portsmouth this year, Dr. Herbert Spencer opened a discussion on the treatment of fever following delivery, with special reference to serum-therapy, in which the general opinion was expressed that although the whole question of serum treatment was too much in its infancy to justify any definite answer, the results hitherto obtained did not exhibit any material advantages over the methods generally adopted. In the "Lancet" of November 11th (pp. 1296, 1299), we published the reports of meetings of the Obstetrical Society of London, and of the Harveian Society of London, at which the subject was again discussed. At the first named Society, Dr. Herman read a paper on "Two cases in which life appeared to have been saved by anti-streptococci serum." The first was one in which a severe operation was followed by great prostration, from which the patient recovered, but the rally was followed by renewed prostration, such as in the speaker's former experience had ended in death. With anti-streptococcoc serum the symptoms quickly improved, and the patient got well. The second case was a puerperal one. Dr. Herman thought, however, that judgments as to the therapeutic value of the serum was difficult, because sufficient knowledge of the effects of the streptococcus was not yet forthcoming. The succeeding speakers all spoke in a doubtful tone as regards the therapeutic value of the serum. At the meeting of the Harveian Society, Dr. J. W. Washbourn stated that as the results of his experience, the serum was of extreme value in certain cases, but that it was valueless in other cases apparently identical. That is to say, there is a great deal of opinion that serum treatment in suppuration is useless.

13122. Did he mean by that, that in some cases the serum supplied had an effect and in others the serum had no effect, or that in some cases the same serum would have a different effect on different patients?—I do not know that that question is answered. I do not know that the report gives that information.

13123. In all these experiments on serum, so far as I understand, a great deal depends upon the purity and strength of the serum?—There is so much doubt about the whole matter that it must be a matter of opinion.

13124. (*Sir John McFadyean.*) Dr. Washbourn is dead, is he not?—I do not know. I am quoting now.

13125. (*Dr. Gaskell.*) Where is this statement in Mr. Paget's book; I cannot find it?—I think my edition of Mr. Paget's book is the first edition.

13126. It does not occur in the edition that I have; I cannot find it?—I have not seen any other edition but the first, I think.

13127. He does not seem to lay that same stress on it in the later edition which he did in what you quoted; at least, I cannot find it here?—I have seen nothing but the first edition of Mr. Paget's book. I did not know that it was altered.

13128. (*Chairman.*) I think you have read us what Dr. Washbourn said?—Yes.

13129. And you have read also what Dr. Herman said?—Yes.

13130. Then, I think, the next case you deal with is myxœdema?—Yes. It has been found that after removal of the thyroid gland—sometimes performed for hypertrophy—myxœdema follows. Now, since this disease does not come on when the patient possesses a thyroid gland, either healthy or enlarged, it is neither difficult nor brilliant to conclude that this gland produces something which prevents myxœdema. If we lost our watch and found we could not tell the time, it would not require much brain power to see that this was due to the absence of the watch. To buy another watch might occur to an unscientific person, without experiments on animals. Similarly, it is simple to see that to administer to a patient something from the loss of which he is suffering would probably give relief. And, as a fact, it is found that patients with myxœdema are benefited by taking thyroid gland from the lower animals, swallowed in tabloids. What is there wonderful in this? It is only common-sense applied. Mr. Stephen Paget tacitly implies that there is something difficult in this simple reasoning, and that it was necessary to ascertain, by removing the thyroid gland from monkeys, whether excision of a healthy gland would cause myxœdema. It might have been interesting; but why necessary? With a healthy gland—no myxœdema. With a hypertrophied gland—no myxœdema. With no gland at all—myxœdema. One who could not see the plain inference from this must be a person of diminished intelligence. If there had been any evidence that in goitre the function of the gland is destroyed by the hypertrophy, then something, though not much, might be said for the experiment on monkeys. But there was no such evidence. Goitres are only hypertrophy of one of the normal structures of the gland—the fibrous tissue, the vascular tissue, etc. There was no reason at all for assuming the function to be entirely lost. Hypertrophied muscle, or bone, or heart, does not cease to act. On the contrary, the fact that goitres usually cause no symptoms, but pressure, and that severe symptoms only come on when the gland is removed proves that the function is not lost. This is so plain that he who runs may read.

13131. Do I rightly understand your view to be that the cause of myxœdema was thoroughly and completely understood before any experiments were made on animals?—Yes. That is all I have to say about myxœdema.

13132. Then we come to the anti-tetanus serum?—Mr. Paget remarks of anti-tetanus serum: "It is only ten years since Kitasato first obtained pure cultures of Nicolaier's bacillus; yet here is a good record already of lives saved or protected. Dr. Whitla, in his "Dictionary of Treatment," says: "Notwithstanding all the reported successes, there is little evidence of the value of injections of the anti-tetanic serum. Great numbers of cures are recorded in those cases where the period of incubation was prolonged much beyond the average noticed in severe cases, and it must be remembered that these are the class of cases where a spontaneous cure is the most likely to occur, and many reported successes would doubtless have been safe without the serum." Messrs. Rose and Carless state in their "Manual of Surgery": "At present the results of this treatment have proved disappointing, since few cases of acute tetanus have been cured by it, and the effect even in the more chronic cases is not at all certain." The "Lancet," May 20th, 1903, remarks: "Anti-tetanic serum has hitherto not fulfilled the expectations which were hoped from its use. . . . It is very uncertain."

13133. Do you express any opinion as to whether serum has been useful in any cases?—All I have been able to find out is that it is not of use. Are you speaking of serum generally or of anti-tetanus serum?

13134. I am speaking of the anti-tetanus serum?—I have no evidence except this evidence which I have already given.

13135. There have been a good many experiments, have there not, with it?—Yes, there must have been a great many.

13136. Then as to diphtheria, we have heard a good deal of evidence about the success of the anti-toxin in diphtheria?—May I take hydrophobia first?

13137. If you please?—There are certain minor considerations. Those are, that Pasteur has published misleading accounts of his experiments, that he equivocated and was compelled to admit it, that some features of the treatment are empirical guesswork and without foundation. I pass these over to come to the crucial point—the statistical evidence. This evidence is of two kinds. First, the gross mortality in France of French people from hydrophobia before and after Pasteur's treatment. Second, the case mortality. Please note that all the figures refer to French people. At the Pasteur Institute the figures of the foreigners were kept distinct and apart. My authorities for the following statements are the annals of the Pasteur Institute, Dr. Dolan's book on "Hydrophobia," "Etudes sur la Rage" by Dr. Luteaud, editor of the "Journal de Médecine," and Dr. Auguste Marie's work on "La Rage." I take the gross mortality first. There are three sets of figures: those of the Paris hospitals, those of the Department of the Seine, those of all France. Dr. Luteaud has shown that at the Paris hospitals the mortality before and after 1885 was the same. His figures give the names of the patients at each hospital for each year.

13138. (*Sir John McFadyean.*) Might I have a point made perfectly plain? What does this gross mortality mean—mortality among untreated persons?—The mortality among persons treated by Pasteur and not treated by him; but they refer to French people only, not foreigners.

13139. That is not the question. Are the bulk of them treated or untreated?—Presently I shall show you that about half were treated.

13140. You cannot discriminate between those who were treated and those who were not?—Are you referring now to what proportion these people were?

13141. I am referring to what you call the gross mortality?—By the gross mortality I mean the total number of deaths from hydrophobia in France of French people in a year.

13242. But what I meant was, that there did not seem to be any purpose in giving us the mortality of untreated people in judging of the effect of treatment, and I wondered whether you could leave out of account those not treated?—That would be no use. All I am trying to say now is that the total number of deaths did not go down after the Pasteurian treatment began

(*Chairman.*) What we would particularly like to know is, where the rabies anti-toxin has been tried, in how many cases it has been successful.

(*Sir William Collins.*) I also think it is important to know what was the gross mortality distributed among those treated and those not treated.

13143. (*Sir Mackenzie Chalmers.*) What was the gross mortality?—I am coming to that.

13144. In that last statement that you read of Dr. Luteaud, does he state whether they were treated or untreated cases?—Some treated and some untreated.

13145. (*Sir John McFadyean.*) What is the date of this? You say after 1885?—Yes.

13146. What is the date of the report you are quoting now?—I think it is for the following year.

13147. 1886?—Yes.

13148. (*Sir William Collins.*) Can you give the total deaths from hydrophobia in France during a series of years, either per million of the population or otherwise?—I am coming to that in a moment. Professor Michel Peter, another eminent Parisian clinician, showed in the "Journal de Médecine de Paris," July 5th, 1890, that if the figures for the Department of the Seine be compared for four years before and after 1885, they are 38 in one case and 37 in the other. That is to say, there were 38 for four years before 1885 and there were 37 afterwards.

13149. (*Chairman.*) Is 1885 fixed by you as the date when Pasteur began his treatment?—Yes.

13150. (*Sir Mackenzie Chalmers.*) How many years afterwards does this include?—Four years before and four years after.

13151. (*Sir William Collins.*) Is that a rate or total number?—It is simply the total mortality. There is nothing about case mortality at present.

13152. It is the total number of deaths?—Yes, the total number of deaths in the Department of the Seine of French people for four years before and four years after the introduction of Pasteur's treatment.

13153. (*Sir John McFadyean.*) Have you any statistics along with that as to the incidence of rabies among dogs in those years?—Yes, I am coming to that. If, however, the figures are taken for five years before and after 1885 the figures are 41 to 59. That is to say, that whereas in the five years previous 41 died, in the five subsequent years 59 died.

13154. (*Sir William Collins.*) Does that include the figures for Paris?—I do not know. It is simply given as the figures for the Department of the Seine.

(*Witness.*) Thirdly, the gross mortality for all France. The average annual mortality for a great many years, as given by the French Government, was 30. Dr. Auguste Marie gives also 30. Tardieu, in 1863, gave 25 as the average annual mortality. That these figures are correct is supported by the statistics of other countries, which show nearly the same mortality in proportion to population. The figures shown by the Dutch Government showed an average of less than 2, Belgium had an average of 2.6, Sweden, 4.2. In England, from 1864 to 1883 the average was 35. All these figures refer to pre-Pasteurian years. Pasteur's treatment began in July, 1885. In 1886 the deaths were of French people, 36, an increase above average, although Pasteur was treating immense numbers of patients. According to Dr. Dolan and Dr. Luteaud, the gross mortality continued higher than 30 in succeeding years. From the gross mortality there is an absence of any evidence that Pasteur has ever cured a patient of hydrophobia, and no such evidence has ever been claimed. If anything at all is proved, it is that the treatment killed. There is next the case mortality. I am coming now to the question that was asked about the number of dog bites, that is to say, the number of deaths in each 100 people bitten by dogs mad, or supposed to be mad. Pasteur's case mortality was about 1 per cent. Compared with the case mortality of any text book or any authority, this is an immense improvement. But the question necessary to face is this: Is even a single text book or authority right? There are two answers to the question. 1. There is strong presumptive evidence that every one is wrong. 2. There is direct and positive proof that every one is wrong, and that the real mortality from hydrophobia, without treatment, is the same as Pasteur obtained. I take the presump-

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tive evidence first, that is to say, the presumptive evidence that all the text books and authorities are wrong about the case mortality of hydrophobia. Suppose that the age of some one named John Smith were in question. Suppose one authority gave his age as 5 and another authority as 75, and that every other authority gave a different age, not more than one could be right. If there were 20 authorities, 19 must be wrong. And if 19 are mistaken it is probable that the twentieth would be mistaken also. Here are some authorities on the percentage mortality of hydrophobia: Brouardel gives 31 per cent. below the age of 20, and 62 per cent. above it. Tardieu, Bouley and the Comité d'Hygiène in France give a mortality of 30 per cent. for cauterised wounds and 80 per cent. for non-cauterised wounds. Bouley gives another estimate of 47 per cent. Faber (Wurtemburg), gives 20 per cent. Werner states 22 per cent.; Poland ("Holme's System of Surgery"), says it is 25 per cent.; Gamgee ("Reynold's System of Medicine"), says it varies from 5 to 50 per cent.; Gowers ("Quain's Dictionary of Medicine"), gives a mortality of from 50 to 75 per cent. Pasteur himself believed (or at least said he believed), it was 20 per cent. Mr. Paget, with unconscious humour, remarks, "What was the risk of a bite from a rabid animal in the days before 1885? It is a matter of guess work." The reasons for all this diversity of opinion are easy to see. There is the great rarity of the disease. Perhaps not one medical man in a hundred ever sees a case. This factor alone would prevent reliable statistics. There was the custom of killing the dog to prevent its biting more people. There is the fact that there are about half-a-dozen diseases simulating rabies. It is easy to see what has happened. A dog bites a number of people. Nobody dies. Natural conclusions, the dog was not mad. The figures are excluded from statistics. The text book figures are therefore worthless from their mutual contradiction, and from the fallacies which must have come in. I come now to the positive proof.

13155. (*Chairman.*) You are proposing to deal with the figures in the text books?—Yes. I shall show directly that the real mortality in the untreated people is about 1 per cent. Further, I shall show that even a belief in the lowest figure ever given—that is, in the text books—namely, 5 per cent., is a hopeless absurdity. The figures which condemn Pasteur's pretensions come from the Pasteur Institute. In 1886, the first whole year of Pasteur's treatment, 36 French people died of hydrophobia; 19 were treated by Pasteur, 17 were not. It is with these 17 who stayed away we are now concerned.

13156. Are these figures taken from some French statistics?—They are French statistics taken from Dr. Luteaud's work on "Etudes sur la Rage," and Dr. Dolan's book on Hydrophobia. The average total mortality in France was 30, as we have seen. I am going to give certain figures with certain deductions, and these are somewhat difficult to follow. I consider them very important, and I will give them slowly. Seventeen is rather more than the half of 30. We may therefore presume that rather more than half the dog-bitten people stayed away from the Pasteur Institute. Let us assume that at least half stayed away, which is conceding Pasteur a slight advantage; that is to say, apparently, by these figures more than half stayed away; but we will give Pasteur the advantage and concede that one-half stayed away. If half stayed away the other half went.

13157. (*Sir William Church.*) This is only supposition. You do not give us any figures?—I do give the figures.

13158. (*Chairman.*) What you told us was that 36 was the number who died in that year?—Thirty-six French people died in the year 1886.

13159. Then you must take your 19 who died as against the 36, and not against the 30?—The assumption is that Pasteur's treatment modified in some way the number of deaths. I am leaving that for a moment. I am taking 17 who died without going. I am only trying to show at present how many dog-bitten people stayed away from the Pasteur Institute. Do you follow?

13160. I cannot see, if you deal with the figures of 1886, how you can diminish those who actually stayed away to 30. You say that 19 were treated and 17 were not. You must deal with those figures?—The 17 people, then, who died did not go to the Pasteur Institute. Is that admitted?

13161. That I understand to be what you say?—Therefore, if 17 did not go, if 30 was the average, the presumption is that more than half the people untreated by Pasteur, bitten by dogs, mad or supposed to be mad, did not go.

13162. That I do not follow?—I think it is perfectly clear. It is a question of whether you understand me?

13163. No, it is my understanding that is lacking?—It is no good my going on unless that point is clear.

13164. (*Sir John McFadyean.*) Do you mean that a mortality of 30 would mean probably somewhere about 100 or 200 people bitten annually?—I am not talking about that.

13165. Then I am another who is unable to follow you?—Then let me begin again.

13166. (*Chairman.*) I think we understand, so far as you have gone, what you have said, but I was saying that you introduced the figure 30 as the average after having introduced the actual number of deaths and the actual number treated?—I have given a considerable number before that—30 was the average.

13167. I know that. Now, you introduce that 30 which does not seem to me to have anything to do with the question as to a year in which there were undoubtedly 36?—It seems to me it has very much to do with it. Seventeen French people in 1886 died in France who had not been to the Pasteur Institute. Supposing 34 people were the average, we might assume, then, that half the dog-bitten people stayed away. Is that clear?

13168. To my mind it is not of the slightest importance whether half or one more or one less than half stayed away. Say about half?—About half. Is that clear?

13169. You cannot either prove or disprove the usefulness of antitoxins by such distinctions as that?—These are the best figures we have got.

13170. Yes, I understand that?—Is it admitted that about half the dog-bitten people in France stayed away?

13171. (*Sir John McFadyean.*) That is what I want you to explain—how you know the number of people who stayed away from the number who died of hydrophobia without treatment?—This surely shows us.

13172. I tried to help you out a minute ago by suggesting that you might have estimated the number of people bitten annually from the number of people who died from hydrophobia, but you did not take it?—No, not at present.

13173. Then I do not know yet how you arrive at it?—I have not shown you yet. But what I want you to admit at present is that if 17 people in 1886 died of hydrophobia without going to Pasteur, it is probable that at least half the dog-bitten people stayed away.

13174. But we cannot see that?—Because the average is 30.

13175. (*Chairman.*) I think you had better present your argument without asking us to admit it?—Then I will state it, and I think at your leisure you will admit it. Thirty was the annual average; 17 is rather more than the half of 30. We may therefore presume that rather more than half the dog-bitten people stayed away from the Pasteur Institute. Let us assume that at least half stayed away, which is conceding Pasteur a slight advantage. If half stayed away the other half went. I think that will be admitted.

13176. (*Mr. Ram.*) The other half may be supposed to have gone, you mean?—Assuming that half stayed away, you must admit that the other half went. We know the number who went. On arriving they were counted; they were, in round numbers, 1,500.* I take these figures in another aspect. If half the dog-bitten people were 1,500, all the dog-bitten people were 3,000.

* *1,500 is a round number only. The figures in 1887 were 1,531 and in 1888, 1,404. My recollection is that in 1886 the figure was a little less.—S. F. S.*

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13177. (*Chairman.*) When you say that half went, the other half of the 36 or the 30 went; but as to the other 1,460, I do not see why you say that they went to the Pasteur Institute?—They were counted.

13178. (*Mr. Ram.*) You know the number that went to Pasteur?—Yes.

13179. What indication does that give you of the number that did not go?—That can be arrived at by observing the number of deaths of untreated people, and comparing them with the previous average annual deaths.

13180. (*Chairman.*) Besides, the 30 are the number of people who died in France?—Yes, annually.

13181. But how many of these 1,500 people were foreigners who came to be treated by Pasteur?—The figures are kept distinct in the Annals of the Pasteur Institute.

13182. They are beyond the 1,500?—Yes, with foreigners there were about 2,000. I disregarded foreigners altogether. If half the dog-bitten people were 1,500, all the dog-bitten people were 3,000. Now, for the first time in history we have some reliable figures on a large scale as to the number of dog-bitten people in a country in a year. Now, compare the annual mortality in France—viz., 30—with the number of people—viz., 3,000—bitten by dogs mad and supposed to be mad; 30 to 3,000 gives 1 per cent. This is the real mortality of hydrophobia in dog-bitten people. If we accept any other percentage figure of the text books we are reduced to absurdity. If we accept even 10 per cent., then 10 per cent. of 3,000 is 300. To believe that 300 would have died in a year would be to assume that from some cause the mortality suddenly jumped from 30 to 300. If we accept the lowest figure ever given, 5 per cent. mortality, we are in inextricable difficulties with a gross mortality of 150. The pretensions of Pasteur's treatment rest upon statistics and by statistics they are condemned.

13183. All the 36 who died undoubtedly had hydrophobia?—Yes.

13184. Why do you reason from that that the 1,500 who went to Pasteur must undoubtedly represent half of the people who were bitten?—Bitten by dogs mad or supposed to be mad.

13185. I do not quite follow why it should be so?—It seemed to me so clear that it makes it appear to me as if you had not understood me.

13186. I desired to bring it to your mind that I did not understand you?—I think the figures undoubtedly are that in the first year, 1886, 17 people who did not go to Pasteur died of hydrophobia—that is a certainty. It is a certainty that the average annual mortality in France was 30.

13187. (*Sir Mackenzie Chalmers.*) In that year or in other years?—Extending over a number of years. I said that was an average. Then, as 17 is very nearly the half of 30, we may assume that half the people stayed away from the Pasteur Institute.

(*Mr. Ram.*) I cannot follow that; that is where my difficulty comes in.

13188. (*Sir Mackenzie Chalmers.*) There is a *non sequitor* there?—It is so absolutely clear—there is no *non sequitor*. Let us assume that in each year 30 people always died, never more and never less. Let us assume that in 1886 15 people died without going to Pasteur, surely that would show that half the dog-bitten people did not go to Pasteur.

13189. (*Mr. Ram.*) Why are we to assume that?—To make it clear.

13190. (*Chairman.*) It is not a question of assuming a thing to make it clear. The point is to make it clear that you have a right to assume it?—It seems to me already so clear.

13191. (*Sir William Collins.*) Are you suggesting that the ratio of cases treated and untreated must be regarded as the same as the ratio of deaths of treated and untreated?—No, I am not assuming that. That does occur, but I am not assuming it.

13192. (*Sir John McFadyean.*) Are you forming any estimate, then, as to the number of people annually bitten in France from the fact that 30 was the average mortality?—Not directly, but indirectly.

13193. Indirectly, how?—Indirectly, in this way: 30 people died annually. In the year 1886, 17 people, about half of the 30, died without being treated at the Pasteur Institute. Surely it follows—it is no *non sequitor*—that half the people stayed away from the Pasteur Institute. It seems to me to be absolutely clear.

13194. (*Mr. Ram.*) Do you know how many went to Pasteur in that year?—Yes.

13195. How many?—Something over 1,500 in the year 1886.*

13196. Then do you mean that because 19 went——?—No, 19 did not go.

13197. How many went?—1,500.

13198. And how many died altogether?—In the whole of France 36.

13199. Of those you say that 17 were not treated at all?—Yes.

13200. Is your deduction that the rest of the number between 17 and 36 did go to Pasteur and did die?—Yes.

13201. That is your deduction?—No, it is not a deduction, it is a fact given by Pasteur.

13202. (*Sir Mackenzie Chalmers.*) He got 19 deaths out of 1,500 cases?—He got 19 deaths out of 1,500 cases.

13203. You want us to assume that there were another 1,500 cases with a similar number of deaths that were not treated?—With 17 deaths that were not treated.

13204. (*Sir John McFadyean.*) Might I ask you a question with regard to what you call the average mortality, which you said was 30? Can you tell us whether it fluctuated very much?—Yes.

13205. What were the extremes?—I believe there were in one year as many as 47. I think that was the extreme year.†

13206. Perhaps you could tell us if you have the statistics handy?—I have them all here.

13207. (*Mr. Ram.*) What you want to put comes to this, does it not? That out of the total number of 1,500 of those that went to Pasteur he got no better percentage of deaths, rather worse, than out of the number of 1,500 who did not go to him, and who died?—No, not quite like that. I have not worked it out; that is not the point I have been trying to make.

13208. (*Chairman.*) You have been giving these figures with the object of showing that the number of deaths in the natural course of events without Pasteur interfering is 17?—Yes, that is my object.

13209. It does not, to my mind, prove it. I see what you mean by it, but I am not at all satisfied that the same ratio exists between those who go there and die and those who go there and survive?—I would leave out of question at first all those thousands who went to Pasteur for the time being altogether, and I will take the 17 who died without going to Pasteur, and as 17 is about half the average, we may assume, certainly, that half stayed away.

13210. I think you have put your argument. I quite see what your argument is; but all I can say is that, from some deficiency in my mind very likely, it does not convey to me proof that 1 per cent. of the people died?—It seems to me that the proof is absolute so far as any proof of that kind goes; that these are the best figures we have on a large scale of the real mortality of hydrophobia.

13211. When you say that it is absolute, that is where I differ from you. It may be something to help you in forming a rough judgment, that is all I can say?—I think Sir John McFadyean asked for the figures.

13212. (*Sir John McFadyean.*) But do not let me interrupt your argument, it can stand over quite well; it was merely because it seemed to affect the argument whether they fluctuated?—That is all I have to say. My point was to prove from these figures that the real mortality from hydrophobia is 1 per cent., and that Pasteur, in comparing his mortality with the figures in the text books, was comparing his mortality with a fiction—with a figment. I may say that when

* *Less than 1,500.—S. F. S.*
† *The highest was 66 in 1864.—S. F. S.*

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Pasteur's treatment was first introduced there was a great deal of scepticism among Paris physicians as to the value of it, and both his figures and the circumstances of the case were subjected to very close examination. I give what Dr. Luteaud says about the evidence.

13213. (*Sir Mackenzie Chalmers.*) What is the date of that?—Dr. Luteaud's book was published in 1891. This is merely a summary of what is contained there; it is briefer than in the book. Hydrophobia shows itself under two forms—the convulsive form or the paralytic form. All authorities agree that the latter variety is extremely rare. In the year 1886 twenty-two of Pasteur's patients died of hydrophobia. I said 19 a moment ago. There was a dispute, that is why I say 22 here. Seven of these, practically one-third, died of the paralytic form. This is significant, because the virus which Pasteur injected on his patients, the "fixed virus" of the laboratory, invariably produces paralytic rabies in the rabbit. All authorities are likewise agreed that a common symptom at the onset of the malady is pricking at the place of the bite. Now, in many of Pasteur's fatal cases this sign appeared not at the bite, but at the point of inoculation. This occurred in the case of Bernard Sodini, who was treated on October 21st, 1886, and died November 24th. Such was the case also with Léopold Née, of Arras, who was treated November 17th, 1886, and died December 17th. Many more cases might be quoted. But all this is not absolutely conclusive, it may be objected. It is only presumptive. True. But what shall we say of the following? In July, 1887, Arthur Stoboi was bitten by a dog at Lubline, in Russia. On the chance that the dog might be mad he was sent to the Pasteur Institute at Warsaw, where he was treated by Dr. Boniville till August 11th. He died of hydrophobia on November 11th. The disease was preceded by pains at the seat of inoculation. The dog remained healthy, and lived for years afterwards. The same test—the survival of the dog—proved that in the cases of Née, of Arras, De Moens, of Antwerp, and of others, Pasteur was the cause of death and not the dog.

13214. *Chairman.*) Is that all you propose to say about rabies?—Yes.

13215. Have you had any experience of treating rabies yourself?—No.

13216. What you have given us is from the books and opinions and statistics of others?—Yes.

13217. Do you think there is any antecedent improbability in an anti-toxin being discovered that would cure rabies?—It seems to me that that question is one of the widest questions that it is possible to put. It seems to open up everything.

13218. (*Mr. Ram.*) And what is the answer to it?—The question is too wide.

13219. (*Chairman.*) You mean, in fact, that you have never considered the subject sufficiently to say whether you do or do not see an antecedent improbability in treating rabies successfully by an anti-toxin?—The question seems so wide that I am not quite sure whether it has some less wide meaning than it appears to me to have.

13220. I assure you that I put the question in perfect good faith?—I have no doubt of that, but it seems so wide.

13221. You are not prepared to give an answer to it?—I am not.

13222. Very well. Supposing that in 1886, 24 out of Pasteur's 36 patients had died instead of 19 that would alter your figure altogether. It would not be at all an unlikely thing, would it, that out of 36 patients there might be a considerable variety between those who were cured and those who were not cured?—Do you say a considerable variety?

13223. A considerable variance?—A variance in what way?

13224. That in one year two-thirds of Pasteur's patients might die and in another year only half?—No two-thirds never died; only 1 per cent. died.

13225. But I mean that two-thirds of those who did die might be Pasteur's patients, and at another time only half?—Perhaps some variation of that kind might have occurred.

13226. Then your average of 1 per cent. would vary enormously in that case, would it not?—Not enormously, but it would vary to some extent.*

13227. A good many hundreds you would knock off or put on?—It is a question of mental arithmetic which is rather difficult for me.

13228. You speak of it as being such an absolute certainty that because they were 17 out of 30, or 19 out of 36, or whatever it was, of Pasteur's patients who died, there must exist exactly the same proportion always, and that that proves positively that 1 per cent. is the true average?—But it is just as likely that the figures might have fluctuated one way as the other.

13229. You do not know; you cannot take a single year and say it is just as likely the same thing might happen every year. However, I will not argue it further. I point out just why your reasoning does not seem to me satisfactory as to the 1 per cent. You passed over something that you wished to refer to, diphtheria?—The value of the anti-toxin for diphtheria has been disputed by a host of writers, especially continental and American. England is the exception; there has been little scepticism in England compared with what there has been in other countries. The objections to belief in anti-toxin fall into two classes; first, the theory underlying the treatment is not proved (we may call this speculative objections); second, the mortality statistics are a barrier (we may call this positive objections). Taking the speculative objections first, a patient attacked by such diseases as scarlatina, small pox or diphtheria, usually recovers. In some way Nature throws off the disease. So far we are merely stating facts. But when we try to explain how the "throwing off" is effected, we embark on speculation. We may assume, first, that the cells of which the body is composed become resistant to the disease; second, that the body manufactures an anti-toxin (anti-poison), which neutralises the toxin (poison) of the disease. These theories have two points in common—(a) They furnish us with a comfortable and seductive working hypothesis; (b) they may be true, but they lack proof. It is with the second theory that we are concerned, for on it is based the serum treatment of diphtheria. This therapeutic measure was adopted on the following grounds: In 1883 Klebs discovered a bacillus believed to be specific to diphtheria. (By "specific" is meant that it causes the disease.) From further research by Löffler it has been called the Klebs-Löffler bacillus. The reasons for believing it to be the cause of diphtheria are the following: (1) It is found in a large percentage of diphtheria cases; (2) Injected into the lower animals it kills; (3) It causes on mucous membranes an appearance resembling diphtheria. Whether this evidence is convincing we must subsequently consider. The anti-toxin treatment arose as follows: It is stated as a well-established fact that if an animal be inured to small doses of the bacterium it becomes immune to large doses. It is stated also that if the blood serum from an immunised animal be injected into a fresh animal, the latter is protected against an otherwise fatal dose of bacilli. Assuming that the Klebs-Löffler bacillus is the cause of diphtheria, we might expect the blood serum of immunised animals to protect man to some extent against the disease. The weak points in this speculation are the following: (1) There are reasons for believing that the Klebs-Löffler bacillus is not the cause of the disease. If this is true, the treatment cannot correspond with the theory. (2) Even with belief in the specificity of the bacillus, it would be necessary to prove that the results observed in animals occur also in man. The reasons for disbelieving that the Klebs-Löffler bacillus is the cause of diphtheria are as follows: Koch made the following demands before admitting the specificity of a micro-organism: (1) It must always be found in the disease it is supposed to produce; (2) It must not be found in healthy persons or in other diseases; (3) A pure cultivation injected into an animal must cause the disease and be found again in that animal. As the third test can be disposed of briefly, we may refer to it first. The most that can be said for the view that the bacillus causes diphtheria in animals is that (a) It kills; (b) It causes a white

* *Such a supposition would show a natural mortality of 1½ per cent., a slight variation only.—S. F. S.*

appearance on mucous membranes. But these two circumstances are not convincing. Many poisons kill. Many agents, including chemicals, render mucous membranes white. Hencke, after a series of experiments, arrived at the following declaration, in which not even a hope is expressed that the identity of human diphtheria and the artificial disease in animals is established. He says: "Diphtheria of guinea-pigs has, as is well known, not the least resemblance to human diphtheria, as the most enthusiastic adherents of the Klebs-Löffler bacillus have to admit." By Koch's third test, therefore, the claim of the bacillus to specificity breaks down. Here is a convenient place to notice an illogical position, in which the bacteriologists have placed themselves. They deny the value of naked eye appearances for diagnosis of diphtheria in man. They rely on naked eye appearances for diagnosis of diphtheria in animals. They beg the whole question. Koch's first test is that the bacterium must always be found in the disease. Löffler himself regarded the coincidence of clinical and bacteriological diagnosis as doubtful. This has been affirmed by others. He himself failed to find the bacillus in 25 per cent. of diphtheria cases. Hennig believed the bacterium to be absent in more than 25 per cent. The first test, therefore, fails. Bacteriologists have attempted to avoid this objection by denying that any cases are diphtheria unless the bacillus is found. This, again, begs the whole question. We have the position humorously described by Hanseman: "The Klebs- Löffler bacillus is to be found in all cases of diphtheria, provided we select only those cases in which it is present." Koch's second test is that the bacillus must not be found in health and other diseases. Ritter has found the bacterium (fully virulent to animals) in the mouths of healthy children (127 cases), of nurses of diphtheria patients, and in their noses and throats up to four weeks after disappearance of the disease. It is often found even months afterwards. Uthoff and Fränkel found it in the conjunctiva. Meyer noticed that of some children who were exposed to the same source of infection, one caught a harmless sore throat, another a malignant diphtheria, but both with the bacillus equally virulent to animals. The presence of the bacterium is common in harmless sore throats. I ask special attention to this. The importance will appear later. In catarrhal inflammation of the conjunctiva, the bacillus fully virulent to animals is often found. It is strange, therefore, that diphtheria of the conjunctiva (already very susceptible through inflammation) is so rare. Baginsky, Meyer, and others state the bacillus (highly virulent to animals) to be common in the mucous membrane of the nose, associated with rhinitis, a harmless disease. It is remarkable that in so many cases of rhinitis, with virulent diphtheria bacilli, so few contract diphtheria. Todd and Washbourn observed among convalescents from scarlatina, an endemic rhinitis (that is, inflammation of the mucous membrane of the nose) without the formation of a membrane, without swelling of the glands, without fever, etc., etc. In all cases (51) they found the Klebs-Löffler bacillus, which was fatal to guinea-pigs. They note a series of cases with the true Klebs-Löffler bacillus occurring in children strongly susceptible (through rhinitis) as well as susceptible in general, because they had had scarlatina. Among these was not found a single case in which diphtheria occurred. Soerensen points out that diphtheria, following measles and scarlatina, is very fatal. Yet he found the perfectly fatal (to animals) Klebs-Löffler bacillus in 326 patients convalescent from scarlatina. In 85 per cent. there was not a symptom of diphtheria. In 15 per cent. there was a slight membrane. There was not a single case of croup, and not a single case of death. Stoos gives observations about many nasal catarrhs from the Berne hospital, and from different parts of Switzerland, where no diphtheria has occurred for years. The Klebs-Löffler bacillus was found almost without exception. No diphtheria, yet a very susceptible mucous membrane. Kanthack says: "Mention must be made of the numerous cases of fibrinous rhinitis in which diphtheria bacilli have been found." "Further, bacilli, resembling diphtheria bacilli, are found with the greatest frequency in many ulcerations of the skin, gangrene, stomatitis, cancrum oris, noma." He continues: "Todd's observation is a further contribution gradually gaining ground that the diphtheria bacillus is found in many lesions which are not diphtheria." On each of Koch's three tests the evidence against the specificity of the Klebs-Löffler bacillus is crushing, and there are a great many disbelievers. Kassowitz states: "It is now generally admitted that the Löffler bacillus is not the exciting cause of diphtheria" (Medical Press and Circular, June 15th, 1898).

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13230. (*Sir John McFadyean.*) Is that an editorial article?—I do not remember; this is from notes made some time ago. It could be ascertained by looking up the Medical Press and Circular of 1898.

13231. I only wanted to know what importance to attach to the statement?—The statement is: "It is now generally admitted that the Löffler bacillus is not the exciting cause of diphtheria. That is the opinion of Kassowitz. I turn now to the positive objections. The antitoxin treatment depends on the assumption that the Klebs-Löffler bacillus is the cause of the disease. We have also seen that by all the recognised tests this assumption has been proved false. There remains the practical question: What have been the results? These results must be looked at in the form of statistics. These statistics may be examined in two ways. We may ascertain the total deaths from diphtheria in a country for each year. This is called the absolute mortality. We may also take the figures of patients in hospitals, and note the deaths in each hundred cases. This is called the case mortality. Before going further, we must look at several most important sources of fallacy which might vitiate any conclusion. They are the following:—(1) The fact that statistics have been made with selected cases. (2) The fact that the severity of diphtheria varies with time and place to an immense degree. When we have scrutinised these points we shall see that they are formidable enough to deceive even an impartial inquirer, and to enable a partisan who disregards them to prove either that antitoxin has saved or has killed an enormous number of diphtheria patients. Then statistics have been made from selected cases. We may get favourable but misleading results for a remedy by two methods of selecting cases. (*a*) We may eliminate severe instances. Roux, in the first statistics of antitoxin ever made, suppressed 7 per cent. of mortality by this means. Dr. Lennox Browne remarks in relation to this source of fallacy: "When this error is rectified the improvement due to antitoxin, as practised in the Metropolitan Infectious Fever hospitals for 1895 and 1896, is entirely dissipated." (*b*) We may include in our statistics an increased number of mild cases. Lennox Browne remarks on this: "The Klebs-Löffler bacillus being now recognised as an irrefragable test of diphtheria—the clinical criteria being often treated as subordinate—the numbers of cases of the disease are much greater than in former times." Tirard, in his book, "Diphtheria and Antitoxin," says: "Few diseases give rise to such frequent mistakes." He says also: "Some cases in which the bacillus is found to be present would otherwise give rise to no anxiety; while, on the other hand, in some cases of undoubted diphtheria, clinically, the bacillus is sought in vain."

13232. When you say that Dr. Lennox Browne says this and Tirard says that, are these your observations which you are making as the result of your own reading, or are you quoting it all from one article? It is difficult to follow what the evidence is?—I began to quote only here. I said Dr. Lennox Browne remarks in relation to this gross fallacy——

13233. Are you now reading from one of your own notes?—This is something I have already published.

13234. You are not going to read the whole of it?—I want to read the whole of it.

13235. It is quite unusual to read a long statement of many pages?—But I would like to point out that this question of antitoxin is one which has been so prominent in the controversy of vivisection—I have seen a good deal of vivisection controversy, and this point is always being brought up, and, so far as I can see, none of the witnesses who have been here before the Commission have any grasp whatever of the fallacies underlying these statistics.

13236. It is quite impossible to take in all these things mixed up with statistics and figures, and to follow them and to carry anything in one's mind. It can be printed as part of your evidence?—That is quite sufficient for my purpose. I ask this because of the very great importance of the question.

13237. We have been sitting here for many days, and we have heard, I think, almost all these points that

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you have been taking, and, therefore, when you say that it is of vital importance that you should read these thirty pages, you are speaking of it as a person who comes freshly into this room?—I have read Dr. Martin's evidence on diphtheria, and his evidence is simply honeycombed with fallacies, and I have been extremely anxious to point these fallacies out.

13238. (*Sir John McFadyean.*) Has this been written since Dr. Martin's evidence?—No, long before.

13239. Would it not be much more useful and more convincing to this Commission if you would show us the fallacies that are so abundant in Dr. Martin's statistics?—Yes, I will.

13240. (*Chairman.*) When you began reading, I did not understand what you were busy upon. It is very unusual to read whole pamphlets?—I ought, perhaps, to have stated before that I was going to read a lot, and to have told the Commission the reason why I wished to do so, as this is a point in the controversy which is so much more important than practically all other points.

13241. Several witnesses have handed in to us pamphlets which they have written which they did not expect to be printed in the evidence before the Commission. Others have handed in long statements with figures, but not amounting to thirty pages, which have been incorporated in the evidence of the witnesses. This is a very strong demand upon us?—May I, then, say that I have here about fourteen pages of what I consider the most important evidence, which I have not read?

13242. You would prefer reading them to throwing them aside and dealing with Dr. Martin's evidence separately? Because we cannot have it both ways. You say that you want this specially read, as being an answer to Dr. Martin?—Yes.

13243. Then you would prefer that it should be printed?—Yes, dealing with the positive objections.

13244. Then it shall be done?—I ask special attention to this point, for it relates to a fallacy of the greatest dimensions. We have already seen that the Klebs-Löffler bacillus is found not only in perfectly healthy people, but also in numerous lesions which are not diphtheria, and among these, simple sore throats. We must remember that the believers in antitoxin are all believers in the bacteriological diagnosis. Consequently the statistics of serum treatment are made on selected cases—selected by the inclusion of trifling sore throats that by the clinical diagnosis would be excluded. I bring further evidence on this point. The antitoxin treatment was first tried at Paris in February, 1894, and was in general use in 1895. The following figures show how the serum treatment and the bacteriological diagnosis at once swelled the numbers of cases diagnosed as diphtheria. Coakley gives these statistics:

BOSTON.

Year.	Notified Cases.
1891 - - - -	831
1892 - - - -	1,353
1893 - - - -	1,465
Serum years. 1894 - - - -	3,019
Serum years. 1895 - - - -	4,059

NEW YORK.

Year.	Notified Cases.
1891 - - - -	4,874
1892 - - - -	4,654
1893 - - - -	6,463
Serum years. 1894 - - - -	9,155
Serum years. 1895 - - - -	9,925

BROOKLYN.

Year.	Notified Cases.
1891 - - - -	1,850
1892 - - - -	1,829
1893 - - - -	1,672
Serum years. 1894 - - -	3,812
Serum years. 1895 - - - -	4,277

According to the "Medical News," 1896, No. 25, there occurred in the Boston City Hospital the following statistics:—

Year.	Admissions.	Deaths.
1893 - . -	419	203
Serum years. 1894 - - -	598	266
Serum years. 1895 - -	1,566	207

We see here a great increase in numbers diagnosed as diphtheria, while the absolute mortality is not appreciably changed.

From the Hospital of St. Ladislaus we get the following figures:—

Year.	Admissions.
Without Serum. 1893 - - - -	213
Without Serum. 1894 - - - -	224
With Serum. 1895, 1896 - - -	1,046

We see here the diagnosed cases doubled. According to Kassowitz, there were treated by Baginski in 1894.

In the first quarter, without serum 86

In the second quarter, with serum 239

Gottstein gives the following table:—

Year.	Admissions to the Hospital.
1892 - - - - - -	453
1893 - - - - - -	454
1894 - - - - - -	712

In Basle, from 1885 to 1894, the average annual notifications were 245. In 1895 and 1896 we find the number increased to 740. We need not pursue this part of the subject further. The evidence is overwhelming that the bacteriological diagnosis and the serum treatment have brought an enormous number of simple sore throats into the figures of diphtheria. To compare such statistics with statistics from which mild inflammations of the throat have been excluded lands us at once in a fallacy of the greatest dimensions. Now this fallacy the champions of serum ask us to ignore, as they ignore it themselves. I here finish with the first source of erroneous conclusions, and come to the second. The severity of diphtheria varies widely in time and place. If we counted the shipwrecks after a moderate gale, and again after a violent hurricane, we should find a difference in the figures. If we tried to explain the difference without taking into account the difference in the two storms, it is obvious that we should be leaving out a somewhat important factor. Now, one epidemic of diphtheria differs in severity from another, as much as one storm differs from another. I proceed to prove this. Dr. Feilchenfelder gives the following figures of diphtheria mortality in Charlottenburg:—

Year.	Deaths.
1887 - - - - - - -	56
1888 - - - - - - -	11
1889 - - - - - - -	18
1890 - - - - - - -	32
1891 - - - - - - -	37
1892 - - - - - - -	79
1893 - - - - - - -	129
1894 - - - - - - -	47
1895 - - - - - - -	45
1896 - - - - - - -	24
1897 - - - - - - -	53
1898 - - - - - - -	64

Hellström says that at Stockholm in 1894 the mortality of 33 per cent. in the first quarter of the year sank to 6 per cent. without change of treatment. In the South-Eastern Hospital of London, the mortality in July was 37 per cent. In the following August it fell to 18 per cent. without change of treatment (MacCombie). Aub has pointed out that at Munich the mortality during six years oscillated between 6 and 14 per cent. According to Hahn, the mortality of Friedrichshain in some years differed by 18 per cent. from the mortality of other years (with no change of treatment). Buning gives the diagram in Fig. 1 for the twelve largest parishes in Holland. The reader will scarcely ask for more evidence that the virulence of diphtheria at one period of time varies widely from its virulence at another.

and time were the same. In one place no patient died. In the other nearly all died. If the treatment had been different in each case a firm conviction in favour of one would have been established.

Rump, in Hamburg, gives the following table:—

The New Hospital.		The Old Hospital.	
Year.	Mortality.	Year.	Mortality.
1889 - -	28 per cent.	1889 - -	49 per cent.
1891 - -	13 ,,	1891 - -	52 ,,

We see to what a wide extent results will be affected by the selection of cases and the disturbing factors of time and place. Keeping these points in mind, we see how they throw light on the following remarks of Tirard: "Few diseases have been treated by such numerous remedies, and for every remedy a large percentage of successes has been claimed. It has been found that the drugs recommended were first greatly vaunted; that then they gradually fell into disuse. Perhaps they were completely forgotten, and a few years later they were introduced as novelties with the same extravagant praise and a similar list of successful cases." That is to say, when a new treatment was tried at a time when the mortality from natural causes was falling, the treatment got the credit. When the mortality again rose, the treatment was abandoned. The champions of various drugs were sincere in their belief, but they were deceived by ignoring the natural disturbing factors. I shall bring evidence that the believers in serum have been deceived in exactly the same way, and, moreover, have been led astray by one additional source of fallacy. Prior to the use of antitoxin, the advocates of different drugs were only misled by two factors—the disturbing elements of time and place. They could not be accused of introducing a new standard of diagnosis to flatter their figures. The antitoxin believers, on the contrary, based their statistics on the inclusion of an enormous number of mild cases brought in by the new bacteriological diagnosis. This we have already seen. There is another point of contrast. Other remedies, based on more favourable figures than those of serum, have never received a sudden world-wide attention. Roux, at

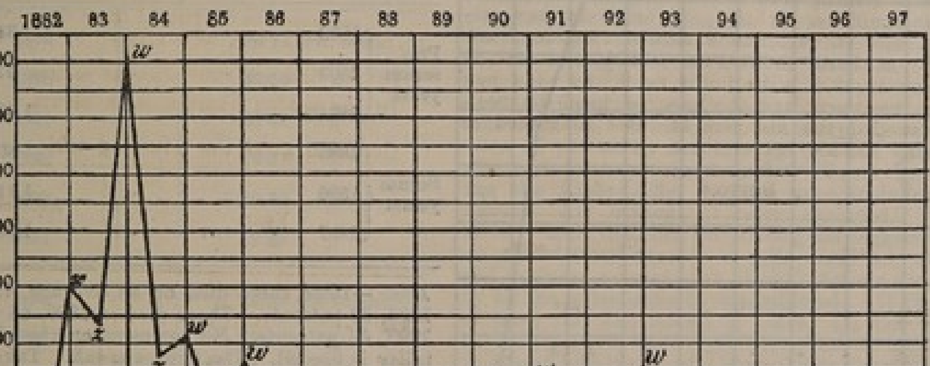

FIG. 1.

The causes of this variation are unknown. It is obvious that if we build up any argument on the assumption that the mortality of diphtheria would remain stationary, unless influenced by treatment, we are at once entangled in a huge fallacy. Yet this is exactly what the champions of serum ask us to do. And they ask it in face of all evidence to the contrary. Time is not the only disturbing factor. Place also has a mysterious influence. In 1888 Baginsky observed an epidemic in two villages side by side. Treatment

the Hospital for Sick Children, Paris, made statistics on 300 cases from February to July, 1894. (I may note, *en passant*, that they were vitiated by every one of the sources of fallacy we have considered.) His report at once received a huge advertisement resounding throughout every civilized land. The treatment was soon adopted in most countries in 1894, and was in full swing in 1895. The explanation of why Roux's figures received such sudden fame whilst the better ones of others were neglected is obvious. Pasteur's

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treatment of hydrophobia was generally believed in. The treatment of diphtheria by Roux (of the Pasteur Institute), was based on somewhat similar lines. The prestige of Pasteur threw a glamour over the new antitoxin remedy. Before looking at serum statistics there is a point to be noticed. The new remedy was introduced at the most favourable moment—when a decrease in mortality was well on its way. Besides a more rapid up and down variation in the virulence of diphtheria, there is a slower movement extending over years. This is clearly exemplified in Figure 1. That the severity of diphtheria had been declining previous to 1894 there is abundant evidence. Göttstein says of Berlin that the virulence increased from 1876 to 1883, then remained nearly stationary till 1886, and after this declined more rapidly, especially in the years 1890, 1892 and 1893. Bernheim says of this: "It is clearly shown that for eleven years we have been on the descending curve of a severe epidemic, and that, apart from the decline of absolute mortality, the severity of the cases has diminished, and this has caused a decrease of the case mortality." This view is endorsed by Soerensen, Heubner, Bötticher, and others. From 1885 to 1890 the total deaths in the Netherlands were 9,656. From 1890 to 1895 the figures were 7,808, showing a spontaneous decrease. *Just previous* to the advent of anti-toxin there was a special fall observed. According to Buning this occurred:—In Paris at the beginning of 1893. In Holland at the end of 1893. In German towns at the end of 1893. In Vienna at the end of 1893. In Stockholm it occurred in 1894, but without the use of serum. Looking at the figures of the Metropolitan Asylums Board, we see a continuous fall from 1889 (see Table below). Buning gives the following diagram of the mortality at Paris (Fig. 2):—

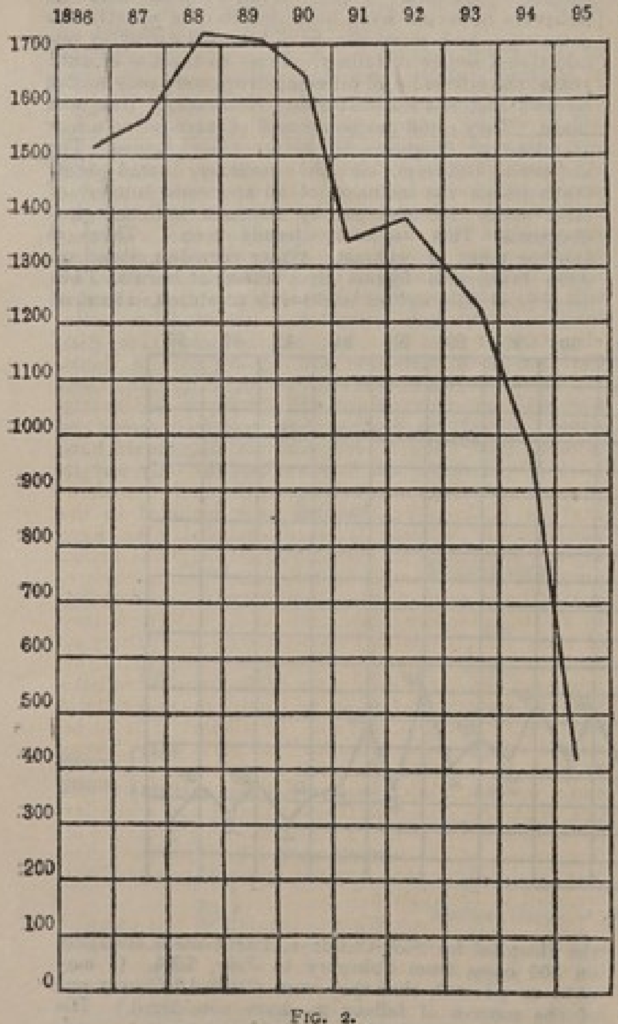

FIG. 2.

Looking at Fig. 2, we see that the spring of 1894 was a most favourable moment for assistance (from natural causes) in getting good figures. We have now considered the factors which, if disregarded, will vitiate any conclusions. We see the conditions necessary before a true comparison of different treatments can be obtained. They are:—(1) The place must be the same. (2) The time must be the same. (3) The statistics must not be aided by selecting the cases. We have seen that this has been done, sometimes by exclusion of severe instances, and, in all cases where serum statistics have been made, by the inclusion of an increased number of mild examples of diphtheria. (Roux's first figures were improved by exclusion of severe and inclusion of mild cases.) The only statistics that have been made under the proper precautions against error are those of Soerensen, of Copenhagen. They are as follows:—Of 51 cases treated with serum there died 17. Of 46 cases treated without serum there died 15; mortality in each case, 33 per cent. These are the only comparative figures existing on which the slightest reliance can be placed. To show the absurdity of making any comparison without regarding the sources of fallacy indicated above, I shall prove:—Firstly: That serum kills an enormous number of patients. Secondly: That serum saves an enormous number of patients. Taking the figures against antitoxin.—I shall examine first the absolute mortality—that is, the total annual deaths. It should be noted that absolute mortality involves one fallacy less than case mortality—viz., the variation due to place. The other two vitiating circumstances, however, operate in full force. *England and Wales.*—The figures of the Registrar-General show that for each million of the population the average annual deaths from diphtheria were, from 1881 to 1893 inclusive, 188. These were pre-serum years. From 1894 to 1900 the average was 269. These were serum years. *Trieste.*—Germonig gives the following figures of the mortality at the hospital:—

Years.		Deaths.
Pre-serum years.	1892 - - - - -	59
	1893 - - - - -	87
	1895 - - - - -	297

In 1895 almost every case was treated with antitoxin. *New York, Brooklyn, and Boston.*—Coakley has shown that the absolute mortality in these places after serum treatment was as high as the worst years since 1832.

ST. PETERSBURG.

Year.		Deaths.
Pre-serum years	1892 - - - - -	333
	1893 - - - - -	378
	1894 - - - - -	1,027
Serum years.	1895 - - - - -	807
	1896 - - - - -	1,118
	1897 - - - - -	1,905

Basle.—Here there died on an average, from 1885 to 1894, 29 patients. There died in 1895, 65 patients; in 1896, 49 patients. No comment on the absolute mortality is needed. The figures speak. Taking the Case Mortality.—For the sake of comparison we take the mortality obtained by Roux at the Hospital for Sick Children, Paris, when serum was first used. This was 26·0 per cent. Against this we give the following results obtained without serum. Neumeyer treated 1,000 diphtheria patients in 15 years with only six deaths. Mortality, 0·6 per cent. Lueddeckens used cyanide of mercury, with the results shown by the following table:—

Year.	Number Treated.	Deaths.
1893 - - - - -	18	0
1894 - - - - -	14	0
1895 - - - - -	32	1
1896 - - - - -	17	0
Total - - -	81	1

Mortality, 1·23 per cent.

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Neumann, of Potsdam, gives the following figures, obtained without serum, and compares them with serum therapeutics:—

Year.	City Infirmary.	St. Joseph's.	His own Practice.
1894 - -	35·5 per cent.	36·9 per cent.	·0 per cent.
1895 - -	9·0 ,,	14·8 ,,	3·3 ,,
1896 - -	14·3 ,,	2·3 ,,	·0 ,,
1897 - -	12·0 ,,	7·3 ,,	·0 ,,
1898 - -	8·4 ,,	11·1 ,,	8·0 ,,
1894–98 -	Average, 15·4	Average, 13·6	Average, 1·6

Göttstein and Schleich quote the statistics of Feer, who in 4,000 cases from 1875 to 1891, had a mortality of 12·7 per cent. Aub has shown that at Munich the mortality for six years oscillated between 6 and 14 per cent., without serum. Hulol and Goubeau, employing perchloride of mercury, report a mortality of 4·7 per cent. Kastorsky, using an alcoholic solution of menthol, treated 37 successive cases without a death. This treatment yielded the same good results to Trutovsky. What shall we say of these figures? What shall we say of the enormously increased totality of deaths since the introduction of serum? What shall we say of the low mortality shown in many cases without serum? If we take up the utterly indefensible position of the antitoxin champions in ignoring the sources of fallacy, we must conclude that serum kills an enormous number of patients. The believers are hoist on their own petard. Having proved that antitoxin kills, we can just as easily prove that it saves. We do this in the same manner—by refusing to consider the factors which falsify our conclusions. Taking now the figures in favour of serum, I have mentioned previously that medical opinion in England has been moulded by one set of statistics. They are those of the Metropolitan Asylums Board. I give them:—

	Year.	Mortality.
Pre-serum Years.	1889 - - -	40·74 per cent.
	1890 - -	33·55 ,,
	1891 - - -	30·61 ,,
	1892 - -	29·51 ,,
	1893 - - -	30,42 ,,
	1894 - - -	29·29 ,,
Serum Years.	1895 - - -	22·85 per cent.
	1896 - - -	21·20 ,,
	1897 - - -	17·79 ,,
	1898 - -	15·37 ,,
	1899 - -	13·95 ,,
	1900 - - -	12·01 ,,

We observe here a continuous decline of mortality, with two falls more abrupt than the others, one in 1890 of 7·19 per cent. and the other of 6·44 per cent. in 1895. These figures have been triumphantly heralded as proof of the value of antitoxin. Let us ask the serum believers for an explanation of the decline, and note the reply. I put the matter in form of question and answer. *Question:* What was the cause of the continuous fall from 1889 to 1895? *Answer:* Natural causes—decreasing virulence or increased number of mild cases sent to hospitals. *Question:* What was the cause of the sudden decline of 7·19 per cent. in 1890? *Answer:* As before. *Question:* What was the cause of the continuous fall from 1894 to 1900? *Answer:* Serum. *Question:* What was the cause of the fall of 6·44 per cent. in 1895? *Answer:* Serum. *Question:* What evidence have you that the decline after 1894 was not due to the same causes as the decline in previous years? *Answer:* We offer you the testimony of our inner consciousness. In relation to the much-quoted figures of the Metropolitan Asylums Board for the years 1889 to 1900, let us look at the figures of the Metropolitan Asylums Board for 1903. I quote from the "Hospital" of August 20th, 1904: "It is to be observed that out of 530 cases of diphtheria admitted to the North-Western Hospital 172 were treated without antitoxin, and they all recovered, while of the remaining 358 treated with antitoxin 42 died, and of the entire 5,422 cases treated in the Board's hospitals 4,139 were treated with antitoxin, of whom 10·18 per cent. died, whereas of the 583 cases treated without antitoxin only 1·88 per cent. ended fatally." More figures can be given in favour of serum. In several places a fall in case mortality was noticed after the introduction of antitoxin. The most quoted is that observed at the Hospital for Sick Children, Paris. There the average mortality for four years had been 51·7 per cent. In the first five months of the use of antitoxin the figures fell to 26 per cent. During the same period at the Trousseau Hospital, Paris, the mortality was 63·2 per cent. without antitoxin.

Hospital for Sick Children.		Hospital Trousseau.
Time.	Mortality.	Mortality.
1890–1893 - -	51·7 per cent.	—
Five months of 1894	26 per cent.	63·2 per cent.

At first sight this looks very convincing in favour of serum. But when we have looked at all the factors we shall see that the comparison is worth nothing. To compare the mortality of one period of time with that of another we have abundantly proved to be useless. I have already quoted examples where two places side by side showed differences of mortality greater than in this instance, without difference of treatment. These criticisms are far from being all. At the Hospital for Sick Children the statistics were under the care of Roux, the bacteriologist of the Pasteur Institute. The cases were selected. Roux admits that he suppressed twenty cases followed by death before treatment could be administered. If included, his mortality would have been 35 per cent., and this was not the only way in which his statistics were helped. All sore throats showing the Klebs-Löffler bacillus were included in the figures. All without it were excluded. We have already seen that this bacteriological diagnosis brings an adventitious number of mild cases into statistics, and excludes severer ones. Roux's figures are vitiated by all the fallacies we have considered. (1) The fallacy of time. (2) The fallacy of place. (3) The fallacy of excluding severe cases. (4) The fallacy of including an increased number of mild cases. The Commission have now the evidence before it, and must judge for itself whether antitoxin is of value, or whether it is to be classed with those remedies for diphtheria which "were first greatly vaunted, then fell into disuse . . . . and a few years later were introduced again as novelties, with the same extravagant praise and a similar list of successful cases." I conclude with an example of this praise: "The terror inspired by diphtheria has been banished, as that inspired by small-pox has been banished by vaccination."

13245. (*Mr. Ram.*) What is the date of this pamphlet?—The date is 1904, but it has been somewhat brought up to date recently. It has been recopied from the volume "Fruitless Experiments," published in 1904, with some few additions to it.

13246. (*Sir Mackenzie Chalmers.*) The first edition was in 1904?—Yes.

13247. It has been republished with corrections in 1905 or 1906?—I think in 1906. I should like though to just point out one fallacy at least, which is so constantly occurring in these statistics of diphtheria.

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13248. (*Chairman.*) Is this contained in the passage you have just put in?—Yes, but it applies to Dr. Martin's figures, and the fallacy of Dr. Martin's figures could only be seen after very careful examination of my statement, and might be overlooked.

13249. Will you just refer us to Dr. Martin's statement, and deal with that separately, because we cannot have it both ways?—It is on page 215 of the Third Report of this Commission. Firstly, taking the figures of the Metropolitan Asylums Board, he has put these figures previous to the introduction of the antitoxin in a misleading manner. He has lumped the years 1888 to 1893, and, as you will see, I have given the figures for each year instead of lumping them. By lumping them he leaves out of consideration the fact that there had been from year to year a decrease—that the mortality was already on the decline every year, which might be inferred but certainly is not apparent from the way in which Dr. Martin has given the figures.

(*Mr. Ram.*) There was not a universal decrease; there was a rise again in the year 1893.

13250. (*Chairman.*) But I think with some slight variations it steadily declined?—Yes, that is so.

13251. (*Sir Mackenzie Chalmers.*) Then, as soon as you get to the serum years it goes down with a jump?—I point out that there was just as big a jump from 1889 to 1890 as there was in the first year of the serum. But I wish to point out one fallacy for this reason, that it is a fallacy occurring over and over again, and always coming up, which nobody seems to have recognised, which fallacy is in a lot of the figures which Dr. Martin has given here. If you take the mortality in 1895 it is 22.8 per cent. If you take it in 1896 it is 21 per cent. That is in the figures of the Metropolitan Asylums Board. Everybody assumes,and we are always asked to take for granted, that this reduction of practically 2 per cent. has been due to the antitoxin, but everybody seems to overlook that you are comparing one antitoxin year with another antitoxin year, and if you keep on like that your error becomes greater and greater as you go along.

13252. I do not follow. Which is the first error?—The first error is that 1895 was an antitoxin year, and that 1896 was also an antitoxin year, and that by comparing an antitoxin year, 1896, with an antitoxin year, 1895, it is assumed that the improvement in the figures was due to the antitoxin. For the life of me I cannot see why it should be. You are comparing it with another antitoxin year.

13253. (*Mr. Ram.*) But you find a rise afterwards in 1901?—That shows that there must have been some other cause.

13254. They are both antitoxin years?——

13254A. (*Chairman.*) No doubt the figures do not absolutely prove one thing or the other, but the suggestion on the other side is that if you find antitoxin being taken up and used more and more in hospitals you find a greater decrease?—I do not think that has ever been shown. I do not think that it is because a larger percentage of patients have been treated by antitoxin. I do not think that has ever been alleged.

13255. I cannot say speaking from recollection. I am only pointing out that there may be an explanation on both sides; on the one that this is a natural decrease going on which would have gone on without the serum, and on the other that the serum, having been introduced for the first time in 1895, the probability is that it was not introduced so fully in the first year as it became afterwards?—I think it was pretty fully introduced in the year 1895.

13256. On the other hand these gentlemen will say that the decrease was not a natural one. I am not deciding which way it was; I am only pointing out that we have these figures before us and we have had these explanations on both sides before us. Is that your evidence upon the diphtheria antitoxin?—Yes, it is.

13257. Is there anything you wish to add?—I am sorry that the Commission do not wish to hear me fully on the question.

13258. You really must not say that. No witness has been invited to read a whole pamphlet to us?—Quite so. I quite admit that it is very long and it may be exceptional.

13259. We cannot hear it both ways. We cannot have the whole pamphlet printed and at the same time hear the substance of it given orally. We gave you the choice?—If I make certain criticisms now on Dr. Martin's evidence before I have pointed out certain general fallacies, I do not see how the Commission will understand it.

(*Sir John McFadyean.*) Might I suggest to the witness that he might keep his mind easy about Dr. Martin's statistics, because some of us are pretty certain to ask for his criticisms upon some of those statistics which do not seem to be touched, and he may have abundant opportunity to disclose the particular fallacies contained in Dr. Martin's statistics.

(*Witness.*) It is that opportunity that I want.

13260. (*Chairman.*) That you will have in questions put to you?—Then I shall be quite satisfied.

13261. (*Sir William Church.*) I think in your *précis* you have referred to this book that you have published, "Scientific Research"?—Yes, I am the author of it.

13262. It was, I suppose, written for the purpose of informing the public of what went on in physiological and pathological laboratories?—Yes.

13263. What is the meaning of this: "A View from Within"—that is on the cover and also on the title-page inside?—It means a view from a person who has been inside a laboratory, a view from inside the laboratories by a person who has been inside a laboratory.

13264. I presume that is what is meant; a person who is conversant with all the work that goes on in laboratories?—Yes.

13265. But then from your preface in this book it does not appear that you were very conversant with all the work that goes on in laboratories. Have you ever worked in a physiological laboratory?—Yes, I have.

13266. Where?—At Strasburg and Brussels.

13267. You were a month in Brussels. You said in your evidence, I think, you were a month in Brussels?—Perhaps I did; I was about a month in Brussels or more. I was several months in Brussels, as a matter of fact.

13268. I thought you said also that you worked a month at the Pasteur Institute?—Yes.

13269. And at Strasburg?—I was about three months at Strasburg.

13270. When working in these institutes were you regularly working in the laboratory at any special point?—At that time I had some idea of going up for the fellowship, and I was working on the muscles of dead frogs. I did no experiments on living animals myself. I did the common experiments on dead frogs.

13271. Have you ever worked in any English laboratory?—No, I have not.

13272. Are you conversant with English laboratories?—No, I am not.

13273. Then should not you have put on this book "A View from Within of the Strasburg and Brussels Laboratories," so that the public should have known what laboratories you had a knowledge of?—I presume that if the public see the outside of a book they will likewise read the inside of the book. I did not see any necessity to put it on the outside of the book.

13274. But it is in the inside also, on the title page?—I presume that, if people read the book, they would get a fair idea of what the book is from reading it. I did not attempt to put a full description of the book on the outside.

13275. I think you were a Licentiate of the Society of Apothecaries first of all?—Yes.

13276. About 1884?—About 1884.

13277. You were then in practice between 1884 and 1900?—Yes, I was, to 1899.

13278. Then you retired from practice?—Yes.

13279. And during those 16 years you were not working in either a foreign or an English laboratory?—I was in general practice from 1884 to 1899.

13280. Still, you had no practical experience then of what was going on in laboratories, either foreign or English?—No.

13281. And, in fact, the whole of your experience has been a month at the Pasteur Institute, a month at Brussels, and a month at Strasburg?—I was about three months at Strasburg.

13282. But there you were not working in laboratories; you were only just attending?—No, I was working in the laboratory.

13283. At some special subject?—On dead frogs.

13284. (*Mr. Ram.*) Did you ever experiment on any living animal?—I never operated on a living animal in my life.

13285. (*Dr. Gaskell.*) Was that in Goltz's laboratory?—Yes.

13286. (*Sir William Church.*) And on that experience of yours you think that you are entitled to say you were in a position to draw back the curtain disclosing the machine?—Yes, I think so.

13287. I am referring to what you say in the preface: "A celebrated lawyer addressed a jury at length to prove that the wheels of a certain machine could not possibly be more than a certain size. His opponent was expected to argue at length in reply. Instead of doing so he merely drew aside a curtain disclosing the machine and remarked—'Gentlemen, there are the wheels'"?—Yes.

13288. Considering that this was for the English public, that is rather a strong thing to say by a person who has never worked in English laboratories, is it not?—It does not refer to English laboratories. A book speaks for itself. The preface does not speak for the whole book. The book itself must speak for itself.

13289. I am only alluding to that passage in the preface?—The book speaks for itself. I did not intend that a person should read the preface only without the book and form a wrong judgment from the preface.

13290. Then turning to the book, that being the case, I should like to ask you this: You were working only on frogs abroad. On page 47 of your book you definitely make these statements. You say, "Curare is used daily throughout England." Do you think that you were in a position to say that with your experience?—Yes. I did not say that it was from my personal experience.

13291. As you told us to-day in your evidence, in five years you have only been able to find about 100 instances of its use?——

(*Sir John McFadyean.*) No, about 10 per cent.

(*Witness.*) The reason is this: I have taken, as I have mentioned already, the "Journal of Physiology" for five years and I found that the proportion of cases of physiological experiments in which curare is used, and inasmuch as from that it appears that in cutting experiments it is used in 10 per cent., if it was used in 10 per cent. of these purely physiological experiments, it was not certainly an exaggeration to say that it was in daily use. That would practically amount to 7 per cent. of the physiological experiments in England for a year, and take 7 per cent. of those I think you would have more than one a day under curare.

13292. (*Sir Mackenzie Chalmers.*) That is the inference which you draw from the fact that in 10 per cent. of the cases reported in the "Physiological Journal" curare was used?—Yes.

13293. (*Sir John McFadyean.*) Along with anæsthetics?—Yes.

13294. (*Dr. Gaskell.*) Ten per cent. of the cutting cases?—Yes, 10 per cent. of 71 per cent.

(*Sir Mackenzie Chalmers.*) I do not think that follows myself.

13295. (*Sir William Church.*) Of course this book is for the public, and purports to be written by a person daily working in pathological and physiological laboratories, and as an authority. With regard to the use of curare, on page 32, you give us your opinion, the result, of course, of observation of your own—"That Bernard was right in believing,"—curare—"to greatly intensify pain"?—Yes.

13296. But have you ever worked on the subject?—Worked on what subject?

13297. On the subject of trying to find out whether curare greatly intensifies pain?—No, I have not tried to find that out.

13298. Therefore you are merely repeating what other people have said?—Yes.

13299. You give it, as you say, "That Bernard was right in believing it to greatly intensify pain is clear also"?—I do not know what the context is there. The word "also" refers to some context which you have not given me. If you give me the context I shall see the reason for it perhaps.

13300. The whole context is this: "We see clearly that the opinion of Bernard and all physiologists is correct—that curare is not an anæsthetic. That Bernard was right in believing it to greatly intensify pain is clear also." That is your statement on page 32, Chapter IV. Therefore you had no reason of your own for that definite statement?—Yes, I certainly had.

13301. It is only repeating what you thought was the opinion of others?—I am repeating the opinion of Claude Bernard; but I do not know whether I put in the context or not the reason for believing that it intensifies pain. "We see clearly that the opinion of Bernard and all physiologists is correct—that curare is not an anæsthetic. That Bernard was right in believing it to greatly intensify pain is clear also." Then what follows goes on this way: "For the motor tracts being shut off, the whole of the stimulus goes to the sensory centre." That is the reason for the statement.

13302. But that is not from any work of your own?—No, it is not. I do not profess it to be. I never said that it was.

13303. Then you go on to mention, if you will turn to page 35, that you saw an experiment at University College, London, on October 19th, 1900, by Professors Starling and Bayliss. This is also with regard to what you say about curare. You say: "When I first saw the animal (a small dog), he was motionless under curare. A hole had been cut in the windpipe, and the bellows kept up artificial respiration. At each stroke of the bellows a little ether was pumped in with the air—a much feebler blast than that used at Strasburg." How did you arrive at that knowledge?—What knowledge do you mean?

13304. That "a little ether was pumped in with the air—a much feebler blast than that used at Strasburg"?—By the sense of sight, and the sense of smell

13305. Nothing further than you thought that because you did not smell more ether?—No. The sense of smell was as to the nature of the anæsthetic.

13306. And the amount of the anæsthetic?—And the amount of the anæsthetic by the bellows and the bubbling. By watching the apparatus.

13307. You judged it then without either inquiring from Professor Starling or Professor Bayliss of the amount, or perhaps even seeing signs of Woulff's bottle or anything?—The bottle was in sight. At Strasburg they had the waterworks. There was a very powerful blast of air sent through the ether, and it was simply a matter of eyesight and observation that at University College the blast was much feebler.

13308. (*Sir Mackenzie Chalmers.*) Was it a lecturer's experiment or a scientific experiment?—After the lecture certain senior students were called in to see the experiment.

13309. (*Sir William Church.*) You really had no knowledge at all?—It was not a question of knowledge. It was a question of the eye.

13310. (*Chairman.*) I understand you to say that you measured it by your eyesight and your memory of what had taken place at Strasburg?—Yes.

13311. In neither case did you ascertain what the precise quantity or strength was?—No, not exactly. It was very easy to see. If I saw some person pouring water out of a glass and another out of a pail I should know that more water was poured out of the pail than out of the glass without measuring in either case.

13312. Do you wish to compare those two cases?—No, it is an exaggerated comparison.

13313. (*Mr. Ram.*) It is your suggestion that the animal was not sufficiently anæsthetised. Is that what you mean?—The suggestion is that it was very probably insufficiently anæsthetised.

13314. (*Sir Mackenzie Chalmers.*) Was the animal killed at the end of the experiment?—We were turned out of the room after being there about half an hour. I saw nothing more of the animal.

13315. (*Mr. Ram.*) How do you mean that you were turned out?—Well, it was time to go.

13316. (*Sir William Church.*) Here you say definitely (and this is for the information of the general public that this book is written) that morphia is not an

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anæsthetic. Surely as a surgeon you do not mean to tell me that you cannot render an animal unconscious with morphia?—When I wrote this book I made that statement on certain authorities. One was the authority of Dr. Mitchell Bruce in his work on "Materia Medica," and Sir Lauder Brunton, who classify morphia among the narcotics.

13317. That is quite a different point?—I was giving you my reason for making the statement. Another reason was the statement by Professor Rutherford before the last Royal Commission, quoting the opinion of Claude Bernard that in animal experiments morphia was not an anæsthetic. Those were my reasons for making that statement in this book. Since then I have had reason to believe that morphia given in a lethal dose may cause unconsciousness.*

13318. But you would not tell the Commission, as some witnesses have, that you cannot render an animal unconscious with morphia?—Since I have written this book I have come to the conclusion that morphia may be used as an anæsthetic in a lethal dose.

13319. There is one other question that I should like to ask you with regard to this book. You say on page 36: "It is clear that in curarised animals anæsthesia is a practical impossibility." This is a book for the general public?—Yes.

13320. What really did you mean by that?—I will repeat what I said a little while ago were the reasons, which I have already given you; but as you ask for it again I will give it again; that is to say that when curare is given the guides which exist in anæsthetising a human patient are absent.

13321. But surely has not the person who is giving an anæsthetic to a curarised animal a very much better guide by knowing the quantity of anæsthetic he is administering?—No, the quantity of the anæsthetic is an unsafe guide, because my observations have been——

13322. But you have never made any observations? —I have seen experiments.

13323. That is a different thing from making experiments yourself?—That is just as good. Looking on at an anæsthetised animal is just as good to me for observing as if I gave the anæsthetic myself.

13324. Unless you have really made inquiries you do not know what amount of anæsthetic the animal is receiving?—Inasmuch as I have seen animals anæsthetised and observed the effects of a fixed quantity of anæsthetics upon these animals, I was in a very good position to judge.

13325. I will not ask any further about that. I should just like to ask you one or two questions on your evidence which you have given to-day. Was anything known as the cause of myxœdema before experiments were made on animals?—Yes, it was known from clinical experience that after the removal of the thyroid, symptoms of myxœdema arose.

13326. Even granting that, does that tell us the cause?—It may not tell us everything, but it gives us a very good idea that something was elaborated in the thyroid which in healthy people prevents myxœdema. That is the inference.

13327. You say it is so simple that one who runs could read?—Yes.

13328. But simple as it was it had never been discovered?—What had never been discovered? You say "it." What do you mean by "it"?

13329. I mean that the importance of the thyroid gland in our economy had never been discovered before experiments on animals were made?—Excuse me. It had been discovered by these operations on the thyroid in which the thyroid was removed and myxœdema supervened. So that this much was discovered by those clinical observations that something was elaborated in the thyroid which prevented myxœdema. If nobody ever thought of that I can only say that everybody was very stupid; it seems to me so simple.

13330. Nothing was discovered. It was observed that in certain cases this condition did follow the removal of the thyroid, but it was not known at all why it did follow?—Excuse me, I think it was known from a reasonable inference, because it was the absence of the thyroid.

13331. (*Sir John MacFadyean.*) Was the healthy thyroid ever removed in any of these experiments?—I do not think so.

13332. How can you infer that it was the absence of the thyroid. It might have been an antecedent disease of the thyroid?—For this reason: that although there had been some hypertrophy of the thyroid or some of the tissues, part of the thyroid, at all events, there was no myxœdema, but when the thyroid was removed there was myxœdema.

13333. That only proved that hypertrophy would not cause myxœdema, but I do not see that excising a diseased gland could prove that the absence of normal thyroid would cause myxœdema?—It seems to me a very simple deduction, that if with thyroid of any kind, whether diseased or not, you have got first no myxœdema, and then, if you remove the thyroid and myxœdema supervenes something has gone. It seems to me a simple deduction.

13334. (*Sir William Church.*) Even that would not have given us the knowledge of the remedy how to treat it. It was the finding that removing healthy thyroids in animals produced symptoms similar to myxœdema in man that led us to think that it might be applied artificially by feeding with thyroids?—You evidently do not agree with me that it was a simple deduction from the removal of a hypertrophied thyroid that it was simply the absence of the thyroid which was the cause of myxœdema. I say that was a simple deduction; you say it was not. The only comment I have made upon that, is that if that was the case with those people at that time, they did not show much intelligence.

13335. It was a deduction that was not made, anyhow, until after experimentation on animals?—That may be, that is quite possible; but if the deduction was not made I maintain that it showed gross stupidity.

13336. I should like to ask you one or two questions on diphtheria. Do you believe that there is such a specific disease as diphtheria?—Yes, certainly.

13337. I do not gather from what you were reading about the different conditions whether you did or did not. You do think that there is a specific disease called diphtheria?—Well, I do not know whether there may not be some varieties. That is a very difficult question. I do not think that anybody could answer that question whether there may not be some varieties.

13338. But still, there is such a disease?—There is such a disease as diphtheria.

13339. Which can be communicated?—From one person to another.

13340. Which can be communicated to animals?—I think the evidence is not sufficient to show that the disease which is communicated to animals is really the same disease.

13341. You are not clear that what is communicated to animals is diphtheria?—No, I am not.

13342. But that also is a specific disease which kills the animal?—Undoubtedly.

13343. And that can be communicated to animals?—Yes.

13344. Do you know how antitoxin is standardized?—No, I do not.

13345. And yet you give an opinion about the value of the antitoxin?—Yes.

13346. Then you are not aware of how the antitoxin is standardized?—No, I do not know how it is standardized.

13347. And how you have to find out what quantity of the antitoxin protects an animal from this communicable disease. You were not aware of that?—I was aware, generally, that there was something of the kind, but the details of these animal experiments I am not acquainted with.

13348. But you were aware that there was something of the kind?—I was aware that there were experiments.

13349. How do you explain to yourself that an animal that is poisoned with toxin, if it receives the antitoxin in sufficient quantity takes no harm?—I am unable to give any explanation of it.

13350. You do not deny that it is the case?—No, I do not deny it.

13351. But you give no explanation?—No.

13352. Have you had any experience of diphtheria yourself?—Oh, yes, a certain amount as a general practitioner.

* *See Question 4133 of Evidence before Royal Commission on Vivisection, 1875.—S. F. S.*

13353. Before or after the introduction of the antitoxin?—It would be chiefly before.

13354. You have had none since?—No.

13355. Have you had much experience of diphtheria?—Well, after having been in general practice about 15 years I suppose I must have seen a good many cases. I could not say how many.

13356. Did you often have to do tracheotomy?—I never had to do it once.

13357. Then you were fortunate in not seeing very severe cases?—Yes.

13358. Could you mention to the Commission the name of any one who has had a large experience of diphtheria, before and after the introduction of the antitoxin, who thinks it is useless?—You mean any individual?

13359. Any individual who has had individual knowledge and large experience both before and after the introduction of the antitoxin, who thinks that the antitoxin is useless?—I would mention Soerensen of Copenhagen, who made some very interesting experiments at Copenhagen. He recognised all the fallacies which exist underlying these diphtheria statistics: the fallacy of time; the fact that the virulence varies from time to time; the fact that the virulence varies in two places at the same time; and also he recognised the factor that there has been a selection of cases. Soerensen guarded against these as far as possible. He treated certain patients in the same hospital at the same time, and he guarded against a selection of cases, and he got about the same percentage of mortality in each case.

13360. He was in charge of the diphtheria hospital?—I presume he was physician to the hospital.

(*Sir John McFadyean.*) Could you give us the number of cases and the date of his report?

13361. (*Sir William Church.*) I was unacquainted with his observations. I do not know his name. Where has he published it?—I have taken that apparently from "Die Diphtheriekrankeit" by Buning of Haarlem.

13362. (*Sir John McFadyean.*) What is the date?—I cannot give the date at the present moment, but it is about 1900, somewhere about there, approximately.

13363. Cannot you give us the number of cases on which Soerensen's conclusions were based?—Yes, I think I can. I am just trying to find it.*

13364. (*Sir William Church.*) You have not given the date in the Bibliography at the end of your pamphlet, so perhaps you have not got it?—I have the book at home somewhere.

13365. It does not matter in the least.

13366. (*Sir John McFadyean.*) In your evidence you offered some opinions with regard to the difficulty of keeping wounds inflicted on animals surgically clean. Is that opinion justified by your own experience?—It is only as regards my own dog.

13367. It will probably satisfy me if you will say that it is founded on observations made on your dog?—Yes.

13368. You have no extensive experience?—I have no extensive experience on that point.

13369. Would you abandon your present opinion if you were told on good authority that operations of most serious kinds on the lower animals are performed in thousands, including opening the abdomen, in veterinary practice, and that the wounds heal by first intention just as in human beings?—I think it is possible.

13370. Does not that dispose of your contention that in inoculating animals for physiological experiments it is difficult or impossible to ensure the wounds healing by first intention?—You said "veterinary practice" just now. I was speaking more particularly of dogs in physiological experiments. If a dog can get at the dressing or tear it off with his paws it will do so.

13371. You do not suggest that a dog behaves differently to a physiologist and to a veterinary surgeon assuming that the wound is the same?—No, I do not, of course.

13372. Will you take it from me that in a veterinary practice dogs frequently have extensive recent wounds inflicted upon them left uncovered, and that they display no tendency whatever to tear them open?—If a veterinary surgeon told me that from his experience I should accept it.

13373. You find nothing incredible in that?—No.

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13374. It is simply in agreement with the fact that aseptic wounds of human beings are comparatively painless?—Yes.

13375. You also offered us some opinions with regard to the difficulty of anæsthetising dogs. Is this your own dog again that has been anæsthetised?—No.

13376. Will you tell me how many dogs you have anæsthetised or seen anæsthetised?—About seven or eight.

13377. I notice that you said in a paragraph of your *précis* "*The uselessness of vivisection.*" Am I to understand that you contend that absolutely nothing useful for human or veterinary practice has ever been learned by vivisection?—I have not gone over the whole ground.

13378. Why did you head it "uselessness"?—The "uselessness" referred to what is contained in the rest of the *précis*.

13379. It is to be restricted to the special diseases that you deal with in your *précis*?—Yes, which I have examined.

13380. You do not offer any opinion with regard to other alleged instances of benefit to men and animals derived from vivisection?—No, because I have not examined them.

13381. We have had evidence given to us here, I may say, that a great deal of useful knowledge for the treatment of farm animals has been got from vivisection. Are you disposed to deny that?—I am disposed to regard it with very grave suspicion from my examination which I have made on these subjects. When I have seen the confident statements that have been made and have examined the experiments and found them so different from the confident statements, I gravely suspect the statements

13382. I would like to go in a little detail into any one of the cases that were mentioned, if you will tell me what are the grounds for your suspicion that the facts are misrepresented, or the conclusions not well founded. Take any animal disease that you like as to which you say you have gone into the evidence. Or perhaps I will suggest one or two to you; but I would rather you would name the animal disease yourself?—Are you speaking of animal diseases.

13383. Yes, certainly?—I beg your pardon; I misunderstood you. I thought you were referring to human diseases.

13384. No, to animal diseases?—I know nothing about animal diseases.

13385. You are not disposed to deny that it may be a fact that knowledge of great value for the treatment of animal diseases has been obtained by experiments upon animals?—I would rather not express an opinion upon that because I know absolutely nothing about it.

13386. But you do hold a very strong opinion that drug experiments on animals intended to throw light on the action of the drugs on men are useless. You have expressed that view?—Yes.

13387. You have cited some instances in which particular agents, poisons or medicines, have a different action on men and on some of the lower animals?—Yes.

13388. Do you want this Commission to believe that that is the rule or the exception?—I wish the Commission to believe that there are a very large number of drugs in which this does occur, and supposing it could be shown that there are 10 times as many cases in which the drug agrees as in which it disagrees, I say if in one-tenth it did not agree it would be very dangerous to argue from animals to man.

13389. But will you answer my question, please. Do these cases that you cited to us constitute the rule or the exception. My point is this. You contend that in a general way it is useless to make drug experiments on animals intended to throw light in advance on the action of these same drugs on man, and you cited to us some admitted instances in which the drug acts differently on the lower animals from what it does on man. I ask you whether you wish the Commission to believe that these cases represent the rule or constitute the exception?—I could not tell you that. It is quite possible that it may be in a great minority of cases.

13390. If you had answered otherwise I intended to ask you how you would explain the extraordinary fact that the veterinary pharmacopœa is practically the same as the human except with regard to doses. Are you aware of that?—No, I was not aware of that fact.

* *See Answer to Question 13844, p. 15. S.F.S.*

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13391. You would not for a moment deny that?—If you say so I would not.

13392. I am not giving evidence, but I do say so?—Then I do not deny it.

13393. Coming to these specific cases in which you allege that unfounded claims have been made as to the benefit derived from vivisection, would you kindly turn to pages 223 and 224 of the Report of the evidence already given to us. You have got, I think, the Third Report there?—I have the page here.

13394. I want to direct your attention to this, because you quoted to us to-day some statistics on, I think, a very limited scale, and already not at all recent, from a book published by Dr. Melville. I want you to notice that the statistics given on page 224, if you look at that in the first place, relate to a large number of persons uninoculated and inoculated, there being among the uninoculated 10,000 and among the inoculated 1,700, and that the percentage of incidence of cases among the uninoculated was 14.14, and among the inoculated 2.05. Would you advise the Commission to disbelieve these statistics or to give no weight to them when they are asked to form an opinion as to the value of inoculation against typhoid fever?—The figures, as they stand, are certainly strongly in favour of it, but judging by the analogy of statistics in general, I should not like to say, off hand, that there might not be some vitiating factor.

13395. But that is not the question. Would you suggest to us what the vitiating factor is?—No, I am not able to do that.

13396. Would you give the same answer with regard to those figures on the preceding page, where there is an equally great difference with regard to the deaths among the inoculated and the uninoculated?—On the preceding page there are three.

13397. At the bottom of the page, I mean. These were officers, non-commissioned officers and men in the same regiment at the same place. There were 317 inoculated and two cases, and 164 not inoculated and 14 cases?—Two cases of what?

13398. Typhoid?—I see.

13399. Assuming that you find that these figures are actually reliable, would you admit that this method of inoculation appears to be of value?—Yes, I am bound to admit that—that it does appear to be of value.

13400. Now, would you turn next, please, to page 214. I want to ask you a question about rabies. Would I be right in saying that your case—as against the contention that the Pasteurian treatment of rabies is successful—is founded on the contention that Pasteur's statistics as to the mortality among untreated bitten persons are entirely fallacious?—Yes.

13401. You did not quote to us, so far as I could see, any statistics at all, but you entered into somewhat elaborate reasoning, from which you inferred what was the number of bitten persons in France in a year?—Yes.

13402. And you said that these were the largest figures available?—I think so.

13403. Will you please look at page 214?—Yes.

13404. We had this evidence presented to us relating, as you will see, to 3,127 tabulated cases of persons who were bitten by dogs reported to be rabid, and showing that the average mortality in these 3,000 odd cases was 16 per cent.?—Yes.

13405. Will you point out to the Commission where there is any probable fallacy in these figures?—Yes, I will point it out. The probable fallacy is that owing to the habit of killing the dog for fear it should bite other people, it was not then ascertained whether the dog was mad or not. Now, assuming that a dog has bitten four or five people and nobody contracts hydrophobia, the probable conclusion would be that the dog did not have hydrophobia, and those cases would be excluded from the statistics.

13406. But why? I cannot follow. Why should they be excluded? They are bitten persons?—Yes.

13407. Why should they be excluded?—Owing to the comparative rareness, and the fact that these cases are scattered about, you would have selected cases there where there was a very strong belief that the dog was mad, or some reason of that kind.

13408. I do not follow?——

13409. (*Mr. Ram.*) Why should they be excluded?—To take a specific instance, supposing that a dog runs round and bites half-a-dozen people. Somebody suspects "That dog is probably mad. Let as kill him before he bites somebody else." The dog is killed. There is a doubt about it whether the dog is mad or not. Nobody gets hydrophobia. The conclusion is that it was not a mad dog. These figures would naturally be excluded from the statistics; they would not find their way in.

13410. (*Chairman.*) But means would be taken to ascertain whether any person had got hydrophobia, and a report would be made long before that?—But so far as I know there is no evidence to show that these were made on every dog-bitten person.

13411. They profess to be?——

13412. (*Sir John McFadyean.*) I do not see how that vitiates these particular statistics. It might be that they left out a considerable number of bitten persons, but the fact would remain that there, among 3,000 persons bitten by dogs, some of which would probably be rabid (but we will assume that they were all rabid), 16 per cent. died?—Yes.

13413. Where is the fallacy in that in assuming somewhere about 16 per cent. as being the average. Because it is not a small number; it is 3,000 cases?—Taken from a great many places all over the place.

13414. But still 3,000 cases. The fact of the locality does not make it objectionable to include them in the statistics?—Not at all. But on the other hand there is a strong probability here that these were selected cases of bites.

13415. How would you select a case? Assuming that they are dogs actually rabid, in what sense could they be selected cases?—That you would get all those cases selected where the patient had hydrophobia. You get a dog that runs round and bites somebody. One of the persons gets hydrophobia. That is now certain. Those bites would be included?

13416. Yes?—If you got another case where nothing happened you would have those excluded from your statistics.

13417. But you do not discriminate between the probable danger of the bite in those two cases so far as one could see. These, after all, you will admit, will probably represent 3,000 mad dogs or 3,000 persons bitten by mad dogs?—Dogs mad and supposed to be mad.

13418. I have not been able to follow your reason why we should adopt your estimate of less than 1 per cent. when you have no precise figures to offer us at all?—Are you referring now in connection with the text book statistics?

13419. I mean this: Do these statistics on page 214 appear to indicate that out of 3,000 persons bitten by mad dogs, selected or not selected, 16 per cent. may die from rabies if they are not treated?—16 per cent., yes.

13420. They do appear to show that?—Yes.

13421. I am quite content with that?—At the same time I hope it will be recorded that I have pointed out the great fallacy in that connection.

13422. I hope that everything you said will be reported. I have no reason to suppose that it will not. Will you turn next, please, to page 218. I want to call your attention to some statistics which have been put before us with regard to the efficacy of the anti-diphtheritic serum. You entertain no confidence whatever in this anti-diphtheritic serum?—None at all.

13423. You actually believe that it is a bogus claim that the diphtheria bacillus has anything to do with it?—It is not on the theory; it is on the statistics.

13424. That is your belief?—Yes.

13425. Would you explain to the Commission the extraordinarily different effect which this serum appears to have according to the period after the onset of the disease at which it is administered?—Yes.

31426. You will observe that when it is administered on the first day the mortality among persons receiving the serum appears to be, as a rule, very slight?—Quite so.

13427. But it gradually rises with the lapse of time. Will you explain the reason why?—In the first place, as regards these statistics, there is no uniformity, you notice. There is a general increase in the mortality the later the antitoxin is given. But there is no uniformity in them, which, for one thing, indicates that there is some other factor.

13428. But might the other factor be one which on *a priori* grounds would be possible, that the serum is not always absolutely of the same strength, and we know is not always given in the same dose?—There is no reason that that should not be suggested. But I have not given my reason why these figures vary so. I merely indicate that there is some other factor besides serum, from their want of uniformity.

13429. Might I interpolate a remark there and point out that the varying factor may be in the human system?—Certainly, that is possible. That is one factor.

13430. That does not affect the value of the serum?—No. But now I had intended to point out to the Commission (but the Commission preferred it to be printed without my doing so) that there is a great fallacy in these statistics from the inclusion of selected cases; and I have here a very large amount of evidence to show that since the introduction, immediately on the introduction, of the antitoxin, owing to bacteriological diagnosis, a large number of cases of mild sore throats were included as diphtheria. I think that is fairly well admitted, even by believers in the antitoxin. Now in the early stage of diphtheria there is great doubt about the diagnosis. Then in the first few days there are many mild sore throats which resemble diphtheria.

13431. Yes, I quite follow that; that among the patients treated on the first day there would be a proportion of them actually not cases of diphtheria?—Yes.

13432. But observe in some of the instances in order to make this explanation acceptable you would have to assume that none of them were cases of diphtheria. There is no mortality at all in some of the statistics among those that get the serum on the first day?—In a large number of cases?

13433. Yes, a considerable number of cases?—How large a number with no deaths on the first day? Where have you got a case of a very large number?

13434. Perhaps I was wrong in saying that there was a large number, because as a matter of fact I do not think in the particular table on page 218, which shows no mortality on the first day the figures are given, unless it relates to those above. But at any rate in the table above that the number you will see was large in some instances; there were 1,189 cases in one series, and 2,428 in another series; and in that large series there were only 2.2 on the first day?—I give two reasons for explanation. Firstly, at the earlier stage of the disease the greater is the probability of error in diagnosis, and the greater the error in diagnosis the better are the statistics. If a child has a case of sore throat, apparently on the second day it is suspected of being diphtheria, and it is given the antitoxin. The child gets well very quickly, and the result is put down to the marvellous effect of the antitoxin. But assuming that some of these cases are hospital cases you have to look for the reason—what was the reason for delay in giving the antitoxin. There must have been some reason for the delay in giving the antitoxin. The delay may have been that some doctors before giving the antitoxin would wait for the symptoms to be more marked, so as to make the diagnosis sure, while other doctors would say, "I will be on the safe side and give the antitoxin at once"; so you have great fallacies coming in for that reason. If they were hospital patients we may assume that a certain amount of delay occurred before the patient was sent to hospital; therefore you get the worst cases sent in.

13435. And you suggest to the Commission that these two sources of fallacy would account for such a difference as 2.3 mortality on the first day, and 34.1 on the sixth day?—Yes, I suggest that, but I think the Commission will be more likely to believe it after they have read what I have printed in this pamphlet.

13436. In speaking about diphtheria you quoted somebody as having said a few years ago that it was generally admitted that the Klebs-Löffler bacillus is not the cause of diphtheria. Do you want the Commission to believe that among bacteriologists and physicians that is the opinion at the present day?—I think probably not in England.*

13437. In Germany?—I do not know about Germany particularly.†

13438. France. Would I be wrong in saying that among hospital physicians and bacteriologists throughout Europe and the United States of America it is the almost universally accepted opinion that the Klebs-Löffler bacillus is the cause of diphtheria?—I could not say. I think the facts speak for themselves. I do not know the opinions of these men.

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13439. You said there was something humorous about the suggestion that only those cases of disease in which the Klebs-Löffler bacillus was found should be called diphtheria?—I put it in this way: that the bacterium must always be found in the disease; that is Koch's first test. Löffler himself regarded the coincidence of clinical and bacteriological diagnosis as doubtful. This has been affirmed by others. He himself failed to find the bacillus in 25 per cent. of diphtheria cases. Hennig believed the bacterium to be absent in more than 25 per cent.

13440. I do not want to have all that over again, but what is there in the least humorous in insisting that only those cases shall now be called diphtheria in which the Klebs-Löffler bacillus is found?—I think perhaps you misunderstood me. What I said was this. We have the position humorously described by Hanseman: "The Klebs-Löffler bacillus is to be found in all cases of diphtheria, provided we select only those cases in which it is present." There is some humour in that.

13441. Nobody has ever made any such contention. Does this line of criticism apply to the present-day definition of tuberculosis, that it is the disease in which the bacillus of tuberculosis is found? Do you see anything wrong in that definition that tuberculosis is the disease in which the bacillus of tuberculosis is found?—In other words, where all the clinical symptoms exist the bacillus is found.

13442. No, on the contrary, we now know that there are a good many other disease conditions with lesions and symptoms simulating tuberculosis, but we agree to exclude them because Koch's bacillus is not found. Is it not perfectly logical in the same way to agree to exclude all cases which appear at first sight to be diphtheria, but in which the Klebs-Löffler bacillus is not found. Where is the humour in that?—The humour is in this: that bacteriologists have attempted to avoid, that is to say, deny, that these well-marked clinical cases of diphtheria are diphtheria because the bacillus is not found.

13443. In the same way that at present one contends that diseases following very closely and simulating tuberculosis are not tuberculosis if Koch's bacillus, the bacillus of tuberculosis, is not found in them. Is not that a precisely analogous case?—You are rather getting me out of my depth there, because I do not know much about tuberculosis.

13444. Surely not. I thought as you came here to criticise bacteriological work you would at least not lose your depth when I mention an elementary fact in bacteriology?—I am not criticising all bacteriology.

13445. Let us take that for granted?—I do not know anything about tuberculosis.

13446. What bacteriological disease do you know anything about. Do you believe in bacteria as the cause of disease at all?—I think one must believe it certainly, of course, in such cases as anthrax and so forth.

13447. That is good enough. We define cases of anthrax as those in which the anthrax bacillus is found, and it does not matter how like a case may be to anthrax clinically for it not to be anthrax unless the bacillus is found?—I may point out that it is a very sudden jump to suddenly exclude all these cases of clinical diphtheria because the bacillus of diphtheria is not found.

13448. (*Mr. Ram.*) Do you object to experiments upon animals if no pain results from the experiment. I am supposing the case of an experiment the object of which is to elucidate some fact which is for the benefit of humanity—an experiment which causes no pain to the animal?—I should like to say that if I thought all experiments are and would be of such a character that pain would be trivial (meaning by trivial what I call trivial and not what experimenters call trivial), I should cease to be an anti-vivisection agitator.

13449. All the evidence which you gave this morning, I think, of any examples you have seen affected only those animals which were allowed to recover?—You say I gave examples?

13450. You gave examples of animals which had

* *Lennox Browne, "Throat Diseases," 5th Edition, p. 546, says that a large number of practitioners have always denied the specificity of the bacillus, and that the number of them increases.—S.F.S.*

† *Buning in "Diphtheriekrankeit" quotes Lubarsch, Martins, Hennig, Hueppe, Baumgarten, Zupnik, and others. S.F.S.*

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suffering inflicted upon them?—Did I? I do not remember.

13451. You gave some examples this morning in the books that you referred to. This book, for instance, which Sir William Church referred to, is full of experiments on animals which are painful?—Yes.

13452. Are you aware that under the Act with regard to cutting experiments the animals have to die under the anæsthetic except in the case of a special certificate being granted to admit of their recovery?—I am aware that there is a law to that effect—that such a law exists.

13453. Do you suggest that there are infringements of that law going on every day in England?—Yes, I do.

13454. Can you give any one instance which has come to your knowledge of an infringement of the law in England?—No, I cannot.

13455. Then on what do you justify your belief that there are many infringements of the law?—My belief is this. I do not believe that the present restrictive law has made any practical modification of vivisection at the present day although it may have modified what vivisectors have disclosed. There are two reasons for this disbelief. In the first place the inspection has been a farce. I consider that any statement to the contrary is an insult to common sense and intelligence. Secondly, obedience to a law merely because it is a law does not exist except in a small percentage of the people. Proof of that exists in the fact that nearly all people will smuggle when they can do so with safety.

13456. Do you put the case of smuggling when a man can do so with safety on precisely the same footing as that of a man of intelligence and education who is bound by law not to inflict pain upon animals or not to do so in an operation without a licence, and so forth?—I have already expressed the opinion that physiologists are mistaken about the pain which they inflict; that they are biassed in their opinion; they think the animals do not feel as they do; and that there is special reason for believing that physiologists hold an exaggerated belief of this insusceptibility of the lower animals to pain.

13457. (*Chairman.*) But would you say that the fact that a certain considerable proportion of the poor and the wretched will steal, and use the knife is a sufficient argument for you to satisfy your mind that a considerable proportion of the physiologists who are under this law break it habitually?—No, I said nothing about stealing and using the knife.

13458. You said the same sort of thing when you said that people would smuggle?—No, excuse me, there is a difference. Where a law exists concerning which the conscience is not exercised, people will not observe that law when they can break the law with impunity.

13459. (*Mr. Ram.*) Then your objections, as I understand them, are twofold, and they go to the root of all your evidence. First that pain is inflicted?—Yes.

13460. If there were no pain there would be no objection?—True.

13461. Secondly that that infliction of pain you believe goes on broadcast because the physiologist has a seared sense of sensibility?—Has blunted sensibilities.

13462. That will do. Whatever their sensibilities were the mischief could only occur in the case of animals not allowed to recover?—Or which were improperly anæsthetised during the operation.

13463. (*Chairman.*) And you say that, although these operations are never carried on in absolute solitude; there are always assistants, and in many cases spectators, and although there is no case that you know of, that you have any evidence about, yet you say it is done every day?—Yes.

13464. (*Mr. Ram.*) Have you read the evidence of the different people who have come here and said that they have operated?—I have not read all.

13465. Have you read some?—Yes.

13466. Do you observe that time after time they say that they have never witnessed any cruelty at all, or any insensibility to the sufferings of animals?—They may have done so.

13467. Do you believe their evidence?—I should disbelieve their judgment as to what pain was. Persons say there was no pain, but I have already given you examples.

13468. If it comes to be a question as many have said, that they have never known an operation performed on any animal that was inadequately anæsthetised, that is a question of fact. Do you disbelieve the assertion because the man is a vivisector?—I should say that it was not necessarily evidence, because I have already shown in evidence that there are people who believe that these movements of the animal are automatic, reflex, and that under curare they cannot tell whether the animal feels or not. I mean to say that they cannot tell whether the anæsthesia is complete.

13469. Do you deny that there are reflex actions under anæsthesia?—No, I think that does occur to a trifling extent.

13470. Then in the absence of the use of curare the animal may be fully anæsthetised and yet may make reflex motions?—I think it is very rare indeed, and I think reflex motions are very slight when they are purely reflex.

13471. (*Chairman.*) But are not these very gentlemen who perform these operations the most competent persons in England to observe reflex actions and to understand them. As to their capability you do not deny that?—I have known competent persons hold such extraordinary ideas about it.

13472. That is not answering my question. I say are not these gentlemen the most competent perhaps that can be found in England, so far as knowledge goes—the physiologists?—Yes.

13473. (*Mr. Ram.*) In regard to your own hospital do you use any antitoxins at all?—No.

13474. Any sera?—No.

13475. Have you any cases of diphtheria in your hospital?—I could not tell you about that. I have never had any cases.

13476. How long have you been connected with the hospital?—Four or five years.

13477. How long has the hospital gone on altogether?—I think I was appointed surgeon very soon after the hospital was opened.

13478. Four or five years ago?—Yes.

13479. Do you mean to say that you cannot say aye or no whether you have had any case of diphtheria or not?—No, because they would not come under my notice.

13480. What is the size of your staff?—Two surgeons, three physicians and a house surgeon.

13481. But holding the views that you do about the inutility of the diphtheritic antitoxin would it not be a matter of great interest to you to see a case treated?—With antitoxin?

13482. Either with or without?—No, I should not judge from one case for a moment.

13483. Could you find out for me whether there have been any cases of diphtheria, and what the rate of mortality has been in your hospital?—I think if they have been diagnosed at all they have not been taken in. We would not take them in if it could be helped.

13484. Why?—We have only one isolation ward. I do not think it is always furnished, and it is in a very inconvenient place.

13485. If the general view with regard to the diphtheritic antitoxin is correct it is lucky for the patients perhaps that you do not take them in?—Yes, if the view is correct that is so.

13486. With regard to your book, in this book which has been already referred to (Sir William Church has asked you about the preface) do you anywhere draw a distinction between the work done in England and the work done on the Continent?—I am afraid I could not answer that without reading the book again.

13487. Do you suggest that vivisection is carried on to-day in England just as it is carried on, or as it was carried on when you were in Strasburg?—No, because there is probably a difference in the nature of the experiments here and there to some extent. Strasburg is rather a special place for particular kinds of experiments.

13488. But you have been dealing in this book with all kinds of cruelties practised at Strasburg?—Not all kinds—some kinds.

13489. Some kinds, if you please, of cruelties practised on animals?—Yes.

13490. Were they operations of a cutting nature on animals not anæsthetised at all?—Yes.

13491. Do you suggest that that takes place in England to-day?—I do not suggest that all those experiments I mentioned there occur in England.

13492. Do you suggest that any of them do?—Yes, I suggest that it is probable, judging from human nature that the vivisector does not always take care to see that the animal is properly anæsthetised, believing as he does that the animal feels little pain.

13493. That is what you said just now. Have you any single fact upon which you can base that belief that you can bring before the Commission?—No, not a single fact.

13494. I want to read you one sentence from page 29 of your book: "My knowledge of what is done on the Continent, and my studies of the 'Journal of Physiology,' which details some of the experiments carried on in this country, prove to me absolutely that vivisection in Great Britain is not different in nature or scope to what it is on the Continent." Is that your opinion to-day?—Yes.

(*Chairman.*) Not if your last answer is correct, certainly.

(*Mr. Ram.*) I want to know whether the witness adheres to the previous or the last answer?

(*Witness.*) Would you mind reading it again?

13495. (*Mr. Ram.*) I was reading your words: "My knowledge of what is done on the Continent, and my studies of the 'Journal of Physiology,' which details some of the experiments carried on in this country, prove to me absolutely that vivisection in Great Britain is not different in nature or scope to what it is on the Continent"?—That refers largely to the nature of the experiments; that is what I had in my mind.

13496. I am asking you whether this represents your opinion of the facts to-day?—My opinion, and I am giving it simply as my opinion (I have already told you that I have no first-hand information to go upon), is that experiments of the present day do not differ from what they were at the time of the last Commission; that the present law has made no difference, and I do not believe that out of respect for the law, merely as the law and nothing more, experiments have altered.

13497. (*Chairman.*) Without having a single fact to go upon, speaking merely from what you call your knowledge of human nature, you mean to say that your opinion is that this Act has had no influence whatever upon the practice of vivisectors?—Yes.

13498. (*Mr. Ram.*) One or two questions about curare. You say, "An anæsthetised animal is immobilised by the anæsthetic. Curare in this case would be unnecessary. To speak of immobilising an anæsthetised animal would be equivalent to speaking of killing a dead animal." Is that your view now?—Yes.

13499. Have you seen the evidence given before this Commission of many cases in which, after the animal has been absolutely anæsthetised, curare has been given in order to prevent the possibility of any reflex action?—I think that the comment I put there is a fair one.

13500. You wish to stick to that, do you?—Yes.

13501. You say "it is clear that in curarised animals anæsthesia is a practical impossibility." Do you believe that?—Yes, certainly.

13502. You mean if the animal is given a lethal dose of anæsthesia?—No. If you say a lethal dose of anæsthesia, I do not say that.

13503. Then what do you mean by these words which I have just read to you "It is clear that in curarised animals anæsthesia is a practical impossibility." What does that mean?—That I am assuming, as in many of these cases, the experiment goes on for a certain length of time under chloroform and ether, and I am assuming that the experimenter does not wish the animal to die quickly, as it sometimes does when an overdose is given, and assuming that the experimenter will not give more anæsthetic than what he thinks is enough to keep the animal alive until the end of the experiment; and that inasmuch as under curare there are not the same guides which there are in giving anæsthetics on a human being, therefore it is highly probable that the animal will be conscious.

13504. Does that mean that anæsthesia is a practical impossibility in curarised animals?—Except by a lucky chance.

13505. Then you go on to speak of an animal which you saw operated on by Professors Starling and Bayliss in 1900 (I think this is an old friend), "When I first saw the animal (a small dog), he was motionless under curare." Then you go on to say what was done. Do you suggest that that dog had no anæsthetic given to it at all?—No, the account states that ether was given through a pump.

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13506. Do you suggest that the dog was not anæsthetised?—I suggest that it is very highly improbable that the dog was properly anæsthetised.

13507. Then you go on to say, "This experiment is a fair example of what is going on daily throughout England"?—Yes.

13508. Do you believe that to-day?—Yes, certainly.

13509. Can you give me any fact on which you base that?—I have already shown you, I think, why it is so.

13510. I do not want deduction. I want you, if you can, to give me any case that has come to your knowledge on which you base that assertion?—I take it it is a question of statistics.

13511. Can you give me any case?—No, that is the only experiment I have seen on a living animal in England.

13512. Then why do you say it is a "fair example of what is going on daily throughout England," that being the only one that you have seen?—You have asked me the question and I think I have a right to answer it.

13513. I am trying to get you to answer it?—It is a fair example, because, firstly, I have seen, personally, cases in which curare is used, and, secondly, there are any amount of similar experiments to be found in the "Journal of Physiology."

13514. Is that the reason why you say that it is a fair example of what is going on daily?—Yes.

13515. You say on page 38, "Before the discovery of ether and chloroform, morphia was used in surgical operations. But it only diminished, it did not remove the torture." Do you still think that about morphia?—I have answered that question already.

13516. It is because you have answered it that I want to ask you a question upon it. You say now, I understand, that you have ascertained that there can be such a dose of morphia given as will produce complete anæsthesia?—Yes.

13517. Therefore you were mistaken in what you thought when you wrote this book?—I would rather say that the authorities upon which I made the statement were mistaken.

13518. At all events, you have now come to the superior knowledge that that is not correct?—Yes.

13519. Do you still believe that you are correct in saying, as you say here, that curare is used daily throughout England?—Yes.

13520. Do you believe that it is used daily throughout England on animals which are not absolutely anæsthetised?—Yes.

13521. Have you any facts on which you can base yourself for that?—Only you put in the word "daily" there.

13522. No, I have not; I am reading your words?—That is another statement, is it?

13523. Yes, it is another statement?—I have pointed out that the evidence of the "Journal of Physiology" is as to the proportion of physiological experiments in which curare is used, and I have already given you the reasons why, in my opinion, it is almost an impossibility, when you use curare, to have the animal properly anæsthetised.

13524. Do you know anybody who holds that opinion besides yourself?—I think all my colleagues do so.

13525. At the hospital?—Yes, and who belong to the Society.

13526. Eliminating those gentlemen, can you give me the name of any professional gentleman of eminence in England who holds that view?—I have not had an opportunity. My acquaintance is so limited that I could not give you any profitable information on that point.

13527. You give, in this pamphlet, certain statistics on page 67, called "Figures in favour of serum." You bring them down to 1900 only?—Yes.

13528. You have brought out another edition of this pamphlet, or brought it up to date, in which you deal with figures as late as 1904, in fact, later than that,

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because you quote the "Lancet" of 1905 on page 86?—Yes.

13529. Why was it that in those later editions, or in this bringing up to date, you did not bring these figures down to a later date than 1900?—Because I have not had the source to take them from.

13530. You have now got it before you in the evidence of Dr. Martin?—This pamphlet was printed before.

13531. But it was brought up to date later?—I have not said that I brought everything up to date. I said that some additions were made afterwards.

13532. I ask why you did not bring these figures up to date?—Because, firstly, I do not think it would have been of any interest whatever.

13533. Are you aware that if they had been brought up to date it would have shown a decrease in mortality as the use of serum was continued?—We will assume that it would have done so considerably; but I do not see the use of it.

13534. Will you answer my question. Are you aware that if you had brought your figures down from 1900 to 1905 (as you might have done), in this book, it would have shown a decreasing mortality since the use of serum became more common?—No, I am not aware of that fact.

13535. One question with regard to what you began your evidence to-day with. You said that animals probably are more sensitive to pain than other creatures because they have a keener sense of smell and hearing?—I did not say for that reason alone. I said for various reasons.

13536. You quoted the keen sense of smell?—I said that that was one part of the evidence.

13537. I see in your book that you quote an article about animals and their capacity for pain. You quote a case of a horse whose leg was broken, and you say the horse walked about on the broken bone and grazed and so forth, carrying the fractured limb dangling. You remember that case?—Yes, that is quoted.

13538. In your opinion does that show that the horse, at any rate, did not suffer very much?—I should want a good deal more evidence before believing that. It does not harmonise with all the evidence I have seen of pain in animals. Besides, even that is unauthentic.

THIRTY-FIRST DAY.

Wednesday, 30th October 1907.

PRESENT:

The Right Hon. the Viscount SELBY (*Chairman*).

Sir W. S. CHURCH, Bart., K.C.B., M.D.
Sir W. J. COLLINS, M.P., M.D., F.R.C.S.
Sir M. D. CHALMERS, K.C.B., C.S.I.
Mr. A. J. RAM, K.C.
Mr. W. H. GASKELL, M.D., F.R.S.
Mr. J. TOMKINSON, M.P.
Mr. G. WILSON, LL.D., M.D.

Captain C. BIGHAM, C.M.G., *Secretary*.

Mr. FRANCIS GOTCH, M.A., D.SC., F.R.S., M.R.C.S., called in; and Examined.

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13539. (*Chairman.*) You are a Doctor of Science, a Fellow of the Royal Society, and a Member of the Royal College of Surgeons, I think?—Yes.

13540. You are Waynflete Professor of Physiology in the University of Oxford?—Yes.

13541. And you come here at the request of the University?—I come here at the request of the Faculties of Medicine and Science of the University of Oxford.

13542. You are a Fellow of Magdalen College?—Yes.

13543. You yourself hold a licence, I presume, under the Act?—Yes.

13544. Have you had large experience in experimenting yourself?—I have had experience in experimenting extending now over 20 years.

13545. In research of all kinds?—In research and such experiments as I considered necessary for teaching purposes.

13546. There was a correspondence which was shown to us relating to an alleged illtreatment of a dog. Of course, I shall not omit to ask you about it, but I would rather first take you through your *précis* before I come to the correspondence?—Very good.

13547. You desire, I believe, to speak in the first place of the importance of extending physiological knowledge?—Physiology and pathology are so intimately associated, that advance in the one leads to a corresponding advance in the other; hence the undue restriction of either branch retards the progress of the other one. Medical science is based upon the existing condition of knowledge in both these subjects, since it deals with tissue processes normal and abnormal, and these are the direct subject matter of physiology and pathology. The more extensive the knowledge of these processes becomes, the more helpful will these sciences be for the prevention and mitigation of suffering in regard to both human being and domestic animals. My object in bringing forward this particular point is to enforce the fact that physiology and pathology are practically two branches of the same science, and that you cannot interfere with the one without interfering with the progress of the other; that although there may be no obvious utilitarian results from some physiological work, you cannot judge physiology on that basis.

13548. You mean that it may throw light on questions of pathology?—It may throw light on questions of pathology. I contend that you can no more separate them, for instance, than you can separate the different parts of a tree, the fruit of which is the obviously useful thing, but the physiological stem cannot be unduly interfered with without doing away with the fruit which comes from pathology. These two sciences, physiology and pathology, are under an enormous debt to physics, chemistry, and biology; this debt they partially repay by the fact that physiological and pathological discoveries and methods have in their turn helped the advance of these other sciences. The discovery of electricity, and the formulation of the law of the conservation of energy are notable instances of this. Natural science as a whole is retarded in its progress if the free development of any one of her branches is unduly restricted.

13549. (*Mr. Ram.*) Do you quote the discovery of electricity as an assistance given by physiology and pathology to chemistry and biology?—I quote it as an instance. Certain phenomena observed in animals and animal tissues led to the discovery of electricity. I may perhaps refer also to the conservation of energy. It is a very remarkable fact that this law was practically discovered by Dr. Robert Mayer in consequence of his observations on the blood of patients in Java; from these he formulated the great law which has practically determined the advance of modern physics and chemistry.

13550. (*Sir William Collins.*) Had not Joule something to do with it?—Mayer was the first formulator. I can only refer to the well-known essay on this subject by Tyndall, in which he states the history of the events quite succinctly, and puts the whole credit to Mayer.

13551. My question was, had not Joule something to do with it?—Joule certainly elaborated the question in regard to heat and its relation to various other forms of force.

13552. I thought you said that Tyndall gave the whole credit to Mayer?—The whole credit of the first formulation of the law.

13553. (*Chairman.*) Was Joule an experimenter on animals?—No.

13554. And was Mayer?—Mayer was a medical man. I could name very many other points, but I do not know whether I should name them now. One of the peculiarities I should say of physiological science is the necessity for accurately measuring time, since the events occur so quickly. The accurate measurement of time started by physiologists has been employed by engineers and has been used for measuring the muzzle velocity of projectiles. The recording arrangements employed by physiologists, first brought forward by Helmhotz and Ludwig, have been employed by engineers for obtaining graphic records of the time and working of various parts of machines. This is only an illustration of the way in which one science helps another.

13555. You mean that physiologists find it a necessity to have some very accurate method of measurement?—Yes.

13556. Who discovered the methods of so doing?—They were chiefly invented by the physiologists Helmhotz and Ludwig. Besides those branches of knowledge generally designated under the term natural science, there are others which are of great importance for the social community. Such are the study of the effect of environment on men and animals, and the study of heredity; in addition there are such scientific branches as psychology and education, this last comprising the appropriate training of both mind and body. The extension of physiological knowledge is of the utmost importance for that of the above subjects. I do not think I need dwell on that.

13557. Not unless you think it necessary. Then your next point is the methods of extending physiological knowledge?—All methods resolve themselves into two—observations on uncontrolled phenomena and observations on controlled phenomena; the control is the determining factor in what is termed experiment. Owing to the extensive scope of physiology, which comprises chemical, physical, and anatomical points of view, many of the observations of the experimental type do not necessitate the investigation of the actual living processes, at any rate, in the first instance. Others are simplified by extensive introductory study upon the isolated tissues of technically dead animals, cold-blooded or warm-blooded.

13558. What do you mean by technically dead?—The animal as such is dead, but the muscular and peripheral tissues are not dead.

13559. (*Sir Mackenzie Chalmers.*) Is the conscious life dead? Yes, the conscious life is dead, the animal being killed, and the particular organ or tissue removed from the body. But in all these cases a point is reached sooner or later when it becomes necessary to study the changes in the whole animal—that is, in one still possessing its co-ordinating mechanism. I explain co-ordinating mechanism later. I do not think I will pause to explain it now.

13560. (*Chairman.*) As you please.—This necessity must obviously exist from the first as regards the study of the co-ordinating mechanisms themselves. The co-ordinating mechanisms bring about the further co-operation or the antagonistic restraint of the activity of any one set of organs through the presence of changes in another set. Such a co-ordinating mechanism is the transfer of chemical substances by means of the circulation, and the more subtle transfer effected by means of the nervous system. For their efficient study and continued investigation experiments upon higher living animals are necessary, since in these only do the particular co-ordinating mechanisms attain a sufficient degree of development. My object in bringing this forward is to show that the scope of physiology and experiments in physiology is very large, and does not necessarily involve any experiments on living animals until you reach a certain point; but that as regards two essential things—namely, the blood as a co-ordinating mechanism and the nervous system as a co-ordinating mechanism—the very fact of co-ordination, meaning the simultaneous value of all parts, does involve experiments on the whole animal.

13561. Then your next point is the co-ordinating mechanisms of the higher animals?—In regard to the first of those, our knowledge of the circulation of the blood, of the condition by which it is modified, and of the part which is played by the character of the conveyed blood, is all due to experiments made upon living animals, and in particular upon mammals. This knowledge is still in many respects incomplete, but has been greatly advanced during the last half-century, the advance being initiated by the introduction by Ludwig of accurate recording methods. As regards the character of the conveyed blood as a determining factor in this co-ordination, there has been during the last 20 years a remarkable development through the discovery of a number of internal secretions. This discovery is entirely due to experiments on higher mammals and by demonstrating an extensive co-ordinating mechanism, which was previously unsuspected, forms a new starting-point, whose value is of the same order as that of the earlier discovery of the reflex activities of the nervous system.

13562. When you speak of the introduction of accurate recording methods, do you mean those methods that we have heard described in the evidence—I am afraid I am not competent to describe them accurately, but when you are testing the pressure on the heart, and so forth?—Yes, the methods of recording the blood pressure, and recording the effects upon the blood pressure of various circumstances. The co-ordinating mechanism, which is termed the central nervous system, is so elaborate in higher animals that its investigation has to be conducted along many different lines. The extension of our knowledge as to the structure and processes which constitute the two main aspects of neurology has a very special importance, inasmuch as psychology, and with it scientific education (I mean an education based upon scientific principles), must be more and more based upon neurology. Much has been learnt by dissection of dead animals and by microscopic improved technique, but such fundamental principles as the distribution of functions within the different parts of the whole nervous mass, the paths by which these parts intercommunicate, and those by which they are connected with other non-nervous body structures, are all the fruit of experiments carried out on higher mammals during the last quarter of a century.

13563. I have not been asking you questions myself, because there are gentlemen here more competent than I am to ask questions on many of these points, and if they desire to do so they will do so afterwards. You now desire to speak of the necessity of a free choice of mammals for physiological research?—This choice is necessary even for the purpose of acquiring a knowledge of the gross and minute structure of tissues and organs, for which animals are painlessly killed and the dead animal then utilised. The frog and rabbit are only suitable for more general tissues, muscle, nerve, and such connective tissues as bone, cartilage, etc.; the tissues of carnivorous animals (dog, cat) must supplement the above, particularly in the case of the digestive and excretory organs; finally, the monkey's tissues must supplement the foregoing, especially in connection with the nervous system. Similar freedom of choice is demanded for the purpose of acquiring a knowledge of the process which, during life, are displayed by these structures, but in this case the study may be carried on during the stage of complete anæsthesia, thus absolutely eliminating all pain. It is, however, occasionally necessary to keep the animal alive after some particular experimental procedure has been terminated, and this is especially the case in connection with the extension of our knowledge as to the working of the co-ordinating mechanisms previously referred to. A most striking example of the great increase in knowledge brought about by experiments of this kind is that of the structure and functions of the central nervous system, as displayed by the degenerative changes and functional losses following definite nervous ablations. Here the introduction of aseptic conditions eliminates subsequent pain.

13564. With regard to the necessity of a free choice of animals, have you experimented on all these animals—the frog, the rabbit, the monkey, the dog, and the cat?—Yes, some time or other.

13565. Can you give us an instance of what you speak of as the necessity of making some experiments upon the monkey, as opposed to the dog or the cat, for example?—The question of the localisation of different functions in the cerebral hemispheres is carried out in the monkey with far greater detail than in any such animals as the dog and rabbit.

13566. If you want to discover what particular part of the brain governs the action of a particular limb or muscle or nerve?—Yes, I should certainly not accept as convincing evidence for a higher mammal any experiment except one which was on a higher mammal—that is to say, the monkey, from that point of view.

13567. Would an experiment on a dog suffice for it?—No, because the distribution of these functions in a dog is different from that in a monkey, inasmuch as the cerebral hemisphere of the dog is framed on a

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different plan. It is framed on what is termed the longitudinal plan of fissures. The transverse plan of fissures, which allows for a large nervous mass without undue protrusion forwards, is the highest type; it begins in the monkey, and goes on, of course, to man.

13568. (*Sir Mackenzie Chalmers.*) Is there an ascending scale among monkeys?—Yes.

13569. (*Chairman.*) Are experiments upon the brains of dogs or cats ever useful?—Certainly. They give useful information from the point of view of determining the action of the higher cerebral centres upon what may be termed the lower centres. The lower centres in the dog and cat, I should say, have far more effective value than in the monkey and man, for the very reason that the cerebral hemispheres themselves have not replaced them to the same extent since they are less developed.

13570. By the lower centres you mean the nerve centres?—I mean the nerve centre, including those which are at the base of the brain in what are known as the basal ganglia, the corpora striata, and the optic thalami.

13571. Then your next subject is the value of physiological experiments for physiological teaching?—Teachers of physiology hold positions of almost exceptional responsibility. In all cases they deal with a subject which forms an essential part of every medical man's intellectual equipment. In many cases, as in universities, the subject has to be also treated as a branch of natural science, and, besides medical students, there are others who approach it chiefly from this point of view. Some of these scientific students enter the teaching profession, and may have, even at present, to conduct elementary courses of physiological instruction based upon sound principles, whilst it is probable that in the future the value of enforcing the physiological principles underlying national hygiene and the physiological improvement of the young will be more and more recognised by all branches of the social community. The demand for elementary physiological knowledge will thus increase. I should like to point out that I myself have always had some students of this type, and that in some local universities these students form a considerable part. They are students who are taking up physiology with the object later on of becoming teachers.

13572. Teachers of physiology, do you mean?—Not necessarily of physiology.

13573. Some of them are teachers of science?—Teachers of science.

13574. Treating physiology as one branch of it?—Yes.

13575. But desiring to learn all?—Yes. I do not know whether the Commission is aware of the very extensive work that is done in the North in this way. Professor Sherrington has a large number of students of this type, for instance.

13576. I do not think we have had any evidence upon that point. I think we have some gentlemen coming from the northern universities. Are you referring to Manchester and Liverpool?—Yes.

13577. I think we shall have some gentlemen from there?—In all scientific teaching it is now recognised that the great desideratum is to help the student in drawing his own conclusions; this is the only real virtue of personal teaching, apart from mere book-work. This help can be afforded by personal intercourse, by lectures, and finally by enabling students to observe for themselves certain phenomena, instructing them how to control these phenomena and ascertaining that they do draw from these the legitimate conclusions. In order to accentuate this last and most convincing (practical) educational method, it is essential that the teacher should himself go through such practical work with or before his students. It is for this reason that every lecturer thus feels the necessity of illustrating his lectures by experiment; he does this not merely to display a phenomenon but to show the conditions of its control, to draw the logical conclusions himself, and make the student realise the intellectual steps by which knowledge of scientific principles has been attained. There may, however, be phenomena which, though of fundamental importance from this point of view, are difficult to control and observe; and others which for other reasons it is undesirable, even impossible, for students themselves to control and observe. The teacher has then thrust upon him an additional responsibility, since he has to determine how far he should display these phenomena, not merely for drawing conclusions, but for giving students a first-hand knowledge of their occurrence. In physiology, as in all science, teaching methods have during the last thirty years undergone a very notable change in this direction. Practical work by students in microscopic study, in physiological chemistry, and in ascertaining such features of the muscular and nervous processes as can be readily observed on the tissues of the pithed and of the dead, decerebrated frog, now forms a large part of the student's work. The extent of this work varies in different teaching institutions, but the general character is everywhere the same. There are, however, certain fundamental aspects of physiology which these practical courses, however extensive, do not display, and, as already indicated, among these aspects are those connected with the co-ordinating mechanisms. The preservation of the circulation and of the chief co-ordinated reflex activities of the animal necessitates the animal being more or less intact. But the seat of consciousness may be absolutely paralysed by anæsthetics, or destroyed by decerebration without invalidating this preservation. On fully anæsthetised animals phenomena of this character can be displayed, and the questions for the physiological teacher to settle appear to me to be four.

13578. I gather from what you said that you attach great importance to demonstration experiments on living animals accompanying lectures?—Yes.

13579. And you have fully explained your reasons for that here?—Yes.

13580. Then you come now to matters of more detail. I see you tabulate four questions that you think are involved in this question of the value of demonstrations?—Yes. I would rather put this as my personal view. I do not know that others would agree to this.

13581. We understand that this is your evidence and your opinion?—The questions for the physiological teacher to settle appear to me to be the following: (1) Is it necessary for efficient teaching of the subject to display some of these phenomena, on mammals so fully anæsthetised that no pain is possible? (2) Is it certain that such an animal can be effectually and completely anæsthetised? (3) What particular groups of phenomena should be chosen out of the whole extensive series as the most essential for students to observe? (4) What circumstances should determine the choice of animal and of anæsthetic in such demonstrations?

13582. Then you proceed to give the answers?—In answer to the first question I, personally, have no doubt of this necessity; whilst as regards the second I am convinced by practical experience that complete insensibility can be easily produced in any mammal.

13583. You have, of course, told us that you have had an experience of twenty years. You have had a very large experience I suppose of anæsthetising?—I have anæsthetised mammals of different sorts in large numbers, though not in such a large number of individual cases as many of my colleagues have.

13584. And you are very familiar with all the different anæsthetics or mixtures of anæsthetics that are used?—Yes.

13585. And you have formed your own conclusions by experience as to which are the best for particular operations?—Yes.

13586. I ask you this because we have had some gentlemen before us who have doubted whether complete anæsthesia can in some cases be obtained, and have very much doubted whether it is obtained in most cases, or in many cases, so I should like to ask you particularly upon that, whether your experience leads you to speak confidently on this question as to whether complete anæsthesia can always be obtained in experiments?—I should say, so far as my belief goes, that it is a certainty that it can be obtained.

13587. (*Mr. Tomkinson.*) For an unlimited time almost, at least for a great length of time?—For a very great length of time.

13588. What do you mean by a very great length of time?—I have been under a severe operation myself which lasted two and a half hours. I mean it is a question of hours.

13589. (*Chairman.*) And in your case, of course, it was part of the surgeon's duty to see that you did not get a lethal dose?—Yes.

13590. Which is not a thing it is necessary to avoid (in fact, it is almost desirable to produce it) in the end in many operations upon animals?—Yes.

13591. I suppose the necessity of avoiding a lethal dose increases the difficulty?—I should think it does. I should think the most difficult animal to maintain in a prolonged condition of anæsthesia would be man.

13592. For that reason?—Yes.

13593. We have had a good deal of evidence and opinion as to whether you can be sure that you have produced complete anæsthesia in cases where curare is given—curare not given as an anæsthetic but for the purpose of keeping the animal still. There have been some strong opinions expressed before us that there are no means by which you can be sure that you have produced anæsthesia. Have you met with that difficulty?—I have had very little experience of curare.

13594. Have you used it?—I have assisted in its being used, but I have never used it myself on mammals. I should, however, have thought that it was easy to see whether there were any of those violent and inco-ordinated reflexes which we take to be the ordinary indications of pain, even under curare, because ordinary voluntary muscles whilst with the dose of curare used are put out of court, the involuntary muscles remain. And with pain you get inco-ordinated activities of these involuntary muscles, which very readily displays itself—which displays itself for instance in extreme alterations of blood pressure in consequence of the varying contraction of the muscles and the arterials.

13595. What is the object of using curare if you cannot prevent involuntary movement?—The object is to prevent the gross movements of the voluntary muscles and to leave the involuntary muscles free to be affected by the central nervous system. That is the object of using curare.

13596. You mean that the object is not to produce perfect stillness?—Only to produce perfect stillness of such muscles as move the limbs voluntarily, but not of the involuntary muscles which move the intestines, for instance, and other things.

13597. It would prevent the animal from blinking its eyes, I suppose?—Yes, that would be a voluntary muscular movement. But you would still have the indication of uncontrollable and inco-ordinated reflexes through the altered activity of the involuntary muscles which are unaffected by the dose used.

13598. Do you consider, then, that there would be something visible to the eye of the operator?—Yes.

13599. Something to tell him whether or not the animal was feeling pain?—I should say so in the alterations of blood pressure.

13600. You say that you have never yourself operated under such circumstances?—In earlier days I have assisted at operations under those circumstances, as a demonstrator I mean.

13601. What you are saying is rather a matter, as I understand, of what you think is the scientific probability than of your own personal experience and observation?—Yes.

13602. (*Mr. Ram.*) Does curare prevent the utterance of cries?—Yes, those being voluntary movements.

13603. (*Dr. Gaskell.*) It is a rise of the blood pressure is it not?—Yes.

13604. Owing to the contraction of the involuntary muscles and the blood vessels which would not be abolished by curare?—Yes.

13605. But it would be diminished if the animal was absolutely anæsthetised?—Yes.

13606. (*Chairman.*) And supposing you had taken the pains which, of course, ought to be taken in these cases—that is to say, extreme pains—to ascertain that you got the proper anæsthetising dose administered to the animal, and you took note of the time at which it was administered, would that convey any certainty to you?—I should say that it would. But you have, I should have thought, the additional certainty that in all experiments of this type, the experiment would be vitiated if there were these sudden and uncontrollable rises of blood pressure which would be produced under curare if the animal had a consciousness of pain. In experiments of this nature they are not produced.

13607. (*Sir Mackenzie Chalmers.*) For what class of experiments, then, is curare used?—Blood pressure—for certain blood-pressure experiments.

13608. Outside blood pressure it is unnecessary to use curare?—I think so. I do not know of any other.

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13609. (*Chairman.*) Supposing you were experimenting on a limb which would very likely move in case of pain, like a leg or an arm, you would take direct physical means to keep it still you mean, or would you trust to the anæsthetic alone?—You cannot trust to the anæsthetic alone because there are reflexes from the spinal cord. Moreover, it is necessary to fix any animal, whether alive or dead, in order to make a proper dissection.

13610. I mean that when you do not give curare to animals to prevent these involuntary movements you rely on physical means taken to fix their attitude?—Yes.

13611. Then what is your answer to your third question?—The answer to the third question requires more detailed treatment. In making his selection the teacher must, I think, be influenced by a number of considerations. First, the character of the physiological principles which underlie the displayed phenomena. They should be fundamental, and such as cannot be convincingly set forth by experiments on the pithed frog or on any isolated organ of the dead animal. Such I consider to be the character of such demonstrations as the following:—The blood pressure, the working of the heart, the intrathoracic pressure and its changes during respiration, the respiratory movements, the action of such substances as raise and lower the blood pressure, the reflex control exercised by the nervous system over the circulation and the respiration; the nervous control of secretion and the digestive organs, and, finally, the localisation of reflex motor centres in special parts of the central nervous system. A further consideration is whether all these can be so displayed that a considerable group of students can observe and appreciate the significance of the phenomena. This depends very largely upon the possession of appropriate apparatus and of skill in its use. Associated with this is the circumstance that physiology offers extensive fields for work, so that one teacher is better acquainted with one mode of physiological technique than with another; thus he naturally selects those phenomena whose display involves the technique in which he is most skilled.

13612. I think at this point I ought to ask you a question about the conduct of students at lectures where there are demonstrations. Some witnesses have spoken of students habitually and frequently making a jest of the operations on the animals, and have accused them of demonstrations of hard-heartedness or cruelty as if they treated the whole matter as a jest. Have you seen anything of that sort at your lectures?—I cannot say that I have ever seen it. On the contrary, I am always impressed by the circumstance that this has been the most serious part, and is regarded as the most serious part, of practical physiological teaching.

13613. How many students would pass through your hands in the course of a year now at Oxford?—I have this year altogether about 70 working in my laboratory. I have not demonstrated to more than 15 or 20.

13614. It is not desirable to have a greater number at a demonstration?—No.

13615. They cannot follow it?—No, I think not. But teachers differ about that.

13616. (*Sir Mackenzie Chalmers.*) Are they all advanced students?—No, of two years—about 35 of each year.

13617. Do you demonstrate before all students, or only before advanced students?—I demonstrate some of these things before the first-year students and others before the second-year students.

13618. All students at Oxford now see actual demonstrations?—They all can see demonstrations.

13619. On mammals?—On mammals.

13620. On anæsthetised mammals?—Yes; the choice of the animal I have not yet referred to.

13621. (*Chairman.*) When you say that there are something like 15 students at a demonstration, there would be more at a lecture?—Yes, the lectures are attended by a variable group of between 30 and 50 first and second year students.

13622. Do you describe beforehand what the demonstration is to be?—Yes.

13623. At the lecture?—At an informal lecture or at the actual lecture.

13624. Do only those who are making a special study

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of physiology for medical purposes go?—No, I make no distinction.

13625. (*Mr. Tomkinson.*) Would that apply to any undergraduate who was reading for honours or for a degree in natural science?—Only those reading for a degree in physiology. At Oxford the schools are separate.

13626. I thought that you said in reply to the last question that they all went without reference to a particular line?—But all those who come to my laboratory are taking this one subject of physiology.

13627. (*Sir Mackenzie Chalmers.*) You have 70 physiological students?—Yes. Natural science is a very large subject, with about 10 different ways of taking honours in it. Only the physiological students come to me, and most of them are medical students. I do not think out of my number there are more than four or five who are not.

13628. (*Sir William Collins.*) Would all the students attending the physiological course within the two years of their course see all the experiments that you demonstrate, whether they are medical students or others?—They can see them, they all can. They have the choice of seeing them; I do not say that they do.

13629. (*Chairman.*) But in groups of 15, I understand?—Yes.

13630. (*Sir Mackenzie Chalmers.*) That is as many as can be comfortably accommodated?—Some experiments I show to a larger group in the lecture room, say 30, where there is no doubt that the essential thing can be seen without any difficulty.

13631. (*Sir William Church.*) But your course in physiology and your lectures on physiology are open to any student in the University, whether he intends to take a degree in arts or intends to take a degree in natural science, are they not?—I do not think that is the case. The laboratory is by special rule placed under my supervision.

13632. You can exclude?—I can if I wish.

13633. Do you exclude a man from your courses of lectures who does not pledge himself to go in for science?—If an Oxford student came to me and said he wished to attend either lectures or courses of laboratory instruction, I should inquire what his reason was. Nor do I allow anybody to attend either lectures or courses of practical instruction unless he has seen me and given his reason for doing so.

13634. Then would you exclude a man who wished to become acquainted with physiology, but yet who probably wished to take his degree in arts rather than in science?—No, I should not do so provided he satisfied me that he had a serious intention.

13635. (*Sir Mackenzie Chalmers.*) A serious intention of studying your subject?—Yes.

13636. Not coming merely for curiosity?—Yes.

13637. When he attends your lectures has he not to enter his name and pay a fee?—There is no fee for attending professional lectures in the University.*

13638. (*Chairman.*) I presume as a matter of course that you would have the right, and would probably exercise it, of excluding any student in the lecture room who did not behave himself properly?—Yes, I have that right. I may add that I do get students of a different type altogether—gentlemen who ask to come specially to the last course of lectures that I give on the central nervous system. These are students of philosophy and psychology.

13639. (*Sir William Collins.*) I was going to ask you this question: you have suggested that all education should be based on neurology, have you not?—I have. I should like to add that in a special annex to my department which is now being built, special rooms are reserved for this particular subject, psychophysics, which is taken up, not only by philosophers, but by those who are qualifying for the education scheme of the University.

13640. (*Chairman.*) Are you now prepared to give the answer to your fourth question. What circumstances should determine the choice of animals and anæsthetic in demonstration?—The answer to the fourth question depends very largely on the selection made—that is, what experiments you select out of that group; but may be influenced by other more special considerations. The size of the animal is an important element in some, such as blood pressure, nervous control of secretion, etc.; the character of the animal in others, such as the localisation of cerebral motor centres. A considerable number can be shown upon large rabbits, and whenever this is possible it is, in my opinion, to be preferred. As regards the choice of an anæsthetic, I hold that the demonstration of the anæsthetising influence of chloroform and ether, singly or mixed, itself presents phenomena of the greatest practical importance to medical students. Apart from this, I am inclined to use, in my own demonstrations, which are made upon rabbits, chloral-hydrate, or urethane, which introduced some time previously in the alimentary canal in sufficient quantity causes a profound and increasingly developed anæsthesia; the animal's condition gradually passes beyond anæsthesia into painless death.

13641. I should like to ask you this question: we have had some evidence given which rather suggested that the interval between giving a lethal dose and painless death was too short for any use in operation. How is that?—That is not my experience in chloral-hydrate.

13642. How long will the animal live supposing you wish it to live under anæsthetics?—An hour and a half or two hours.

13643. Without giving it a fresh dose?—Without giving it a fresh dose.

13644. With a dose that is lethal when given and secures complete anæsthesia the animal may live an hour and a half?—Yes, or two hours.

13645. What animal are you speaking of?—A rabbit.

13646. Would a dog or cat live as long?—I have not had much experience in chloral-hydrate and in urethane. I may say I have had no experience upon dogs at all. I have had rather limited experience upon the cat.

13647. I wanted to see whether it was the case or not that a lethal dose operated almost immediately?—No, it is a prolonged stage.

13648. (*Mr. Tomkinson.*) Under those two substances?—Yes, being absorbed slowly from the alimentary canal.

13649. (*Mr. Ram.*) In your last answer you said you had had little or no experience in dogs, and on cats it is limited as to the use of urethane?—Yes.

13650. You have had a large experience of other anæsthetics?—I have had some experience, not so large as those of some of my colleagues.

13651. You have had some?—Yes.

13652. (*Sir Mackenzie Chalmers.*) Is this hour and a half with or without artificial respiration?—Without artificial respiration.

13653. It is the mere action of the poison that takes an hour and a half?—It depends upon the amount of dose, but you can give it so that it would take, I should say, two hours.

13654. (*Mr. Ram.*) And yet with perfect anæsthesia?—Yes.

13655. (*Sir Mackenzie Chalmers.*) Perfect anæsthesia throughout?—Yes, perfect anæsthesia throughout, and gradually deepening.

13656. Gradually deepening into death?—Gradually deepening into death.

13657. (*Chairman.*) There is no doubt of the perfect anæsthesia?—I have no doubt; there are no evidences of these irregular activities.

13658. (*Mr. Tomkinson.*) Would a rabbit be fastened down as rule?—When it is in this stage finally, do you mean?

13659. Yes?—You can fasten it or not, as you like. It lies perfectly flaccid. If I produced a rabbit here under chloral hydrate it would lie so.

13660. It would not kick or struggle?—No.

13661. Therefore there is no necessity to strap it down?—There is no necessity to strap it down, except for the purpose of making the necessary dissection.

* *The Professor of Physiology is authorised by a special statute to charge statutary fees on a given scale for Laboratory and is further authorised to charge a small University fee for students who commence attendance on lectures. This fee is to be applied towards meeting the expenses of the Department and do not go to the Professor; the charge is made at the beginning of the academic year and gives the Professor additional control over the admittance of students.—F. G.*

The necessity would practically be the same as in a dead animal.

13662. (*Sir Mackenzie Chalmers.*) To keep it in position, and to prevent your moving it with your own hand in the experiment?—Yes.

13663. (*Chairman.*) To keep the rabbit, for example, on its back?—Yes; it will not lie on this condition of absolute profound anæsthesia its back unless it is supported. The fact that in consequence of an ultimately lethal dose can be produced with such certainty in the rabbit, opens a further question. The demonstration method is not an ideal one as regards the students' acquisition of physiological principles. Viewed from this last standpoint, such acquisition would be greatly aided if it were possible for senior students under skilled and authorised supervision, and guarded by careful regulations, to themselves carry out on such profoundly and lethally anæsthetised animals some of the experimental procedures previously alluded to. The responsible character of medical, and particularly of surgical practice, renders it eminently desirable that this extension of existing possibilities should at least be seriously considered.

13664. Supposing you were consulted on the subject, what advice would you give as to the regulations that should limit the power of senior students to operate? Would you confine it to those who had taken a degree, and not begun to practise, or to those in the last year?—I should confine it to those who were in their last year in the university.

13665. But would you exercise any control, by examination or otherwise, to see whether they were qualified persons?—The control that I should exercise would be the control of a responsible person present.

13666. But in recommending them or in permitting them, if you had the power of permission, would you do it on the personal recommendation of the teacher?—I think so. I do not understand how one can make any other adequate selection. It is putting an extra responsibility, I confess, upon the teacher, but his burden already is a very heavy one, if he is a physiological teacher, and I do not think it is much extra burden.

13667. You would rely upon his observation and knowledge of the particular student?—Yes.

13668. I want now to ask you about this correspondence which took place. You are aware that Mr. Coleridge, in his evidence, called attention to a correspondence which he had with some undergraduate of Keble College; it is at Question 11455, and I will not read the letter, because you know it, and the Commission will remember it, but it was practically stating that that undergraduate had heard the yells of a dog at the University Museum, that he had satisfied himself that they proceeded from a dog under actual operation, and he spoke in strong language about the cruelty and the torture, "devilish practices," and so forth. That was signed by several other undergraduates, and then there was read a letter, I think, from yourself, denying that such a thing had taken place. Then Mr. Coleridge read a letter from Mr. Sharpe, the undergraduate in question, in which he in very strong language denies the truth of what you state, language probably which he did not, when he wrote it, intend to be read out to the Commission or anybody else, but thought he was writing to Mr. Coleridge only. However, it was read, and there the matter was left. Some questions were asked as to what Mr. Coleridge knew, and of course he said that he knew nothing about it personally, but that these gentlemen had given him this information, and he had no doubt that they were speaking the truth. I understand that since then this gentleman, Mr. Sharpe, has stated that he has inquired into the question, and has withdrawn the charge, and apologized for having made it?—Yes; and I should like in that connection to read two letters which I have been asked to read, in order that they may appear in the minutes. The first letter is a reprint of a published letter which appeared in the "Oxford Times" and the "Oxford Chronicle" of October 19th, 1907, from Dr. T. H. Warren, President of Magdalen College and Vice-Chancellor of the University of Oxford:—"Vivisection at the Oxford Museum. To the editor of the 'Oxford Times.'—Sir,—In your last issue you published an article with the above heading, taken from the 'Morning Leader,' which consisted largely of certain extracts taken from the evidence given by the Hon. Mr. Stephen Coleridge, before the Royal Commission on Vivisection. Mr. Coleridge quoted letters received by him from certain undergraduate members of Keble College, stating that it was within their knowledge that vivisection was being practised upon dogs in the Museum, and, further, that Dr. Francis Gotch, Professor of Physiology, when he stated that no vivisection on dogs had been carried on in the laboratory during the present year was not speaking the truth. I think it is just, alike to Professor Gotch and to the undergraduate members of Keble College who wrote or supported the letters, to say that I have had placed in my hands a complete apology by those undergraduates to Professor Gotch, a recognition by them of the correctness of Professor Gotch's statement, an explicit and unqualified withdrawal of the letters and statements sent and made by them to Mr. Coleridge, and an expression of their willingness that the fact of their apology and withdrawal should be published. I should be obliged, therefore, if you would now give the same publicity to the apology and withdrawal that you gave to the original statement.—I am, yours faithfully, T. Herbert Warren, Vice-Chancellor." The second letter is from the Rev. Walter Lock, D.D., Warden of Keble College, October 18th.—"Dear Professor Gotch,—I am very glad to find that you are satisfied with the apology which Mr. Sharpe and his friends have made to you, and with the action that the Vice-Chancellor is taking to make the apology public. And now that the matter is at an end, I should like to express my regret, and that of my colleagues, that you and the Museum authorities should have had this trouble, and that the relations between our college and the Museum, which have always been close and friendly, should have had even a momentary clouding. Mr. Sharpe and his friends are all excellent well-meaning fellows, who were actuated by a humane feeling, and honestly believed that the evidence which they were giving was true. But none of them is a science student or acquainted with the details of laboratory work, and I am afraid that they were not competent to estimate the real value of their evidence, the worst portion of which was based on a complete misunderstanding. They made some attempt to make inquiries, but the attempt was obviously inadequate, and I think that Mr. Coleridge treated them a little badly in accepting and making public the second letter without further investigations on his own part. As soon as further inquiries were made, and explanations given to them, they at once saw how their mistakes had arisen, and how impossible it was to substantiate their charges, and they have wholly withdrawn them. May I also thank you for the way in which your own patience and forbearance have contributed to reduce this matter to its true proportions, and to lead a friendly settlement of it. Pray make any use of this letter that you may think fit.—I am, yours sincerely, W. Lock."

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13669. Have you Mr. Sharpe's letters that are referred to there?—They are in the hands of the Vice-Chancellor. I would rather not produce even a copy of them.

13670. I was going to say that it is entirely in your hands whether you wish them read or whether you wish simply to say that you are satisfied with his apology?—I am satisfied, but I should like to inform the Commission as to what the complete misunderstanding was.

13671. (*Mr. Tomkinson.*) Was one of those letters just read from Mr. Sharpe himself?—No.

13672. (*Chairman.*) Will you say how it arose?—The misunderstanding was one which may seem incredible, yet at the same time I know it does happen. These gentlemen imagined that dissection and vivisection were the same thing, and the evidence of the gentleman who was in for anatomy resolves itself into their having overheard a conversation between two students describing the work that they were doing for an examination in zoology involving the dissection of the rabbit; these students were not in my laboratory.

13673. (*Sir Mackenzie Chalmers.*) A dead rabbit?—Yes. That is their misunderstanding. The second point is that they heard dogs bark, and jumped to the conclusion, therefore, that it had some relation to the dogs. The third point is that they made inquiries of what they term an official. They made these inquiries by going in one Sunday morning, when they

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saw a man going round with the keys of the museum, and they asked him whether animals were cut up in that place, to which he said, "Yes." Those are the three facts upon which the whole statement is based.

13674. (*Chairman.*) Is your laboratory in the museum?—My laboratory is at the back of the museum.

13675. And there is a dissecting room also, is there, in the museum for purposes of zoology?—Yes, at the back of the museum, for human anatomy, and also a zoological laboratory where animals are dissected.

13676. (*Sir Mackenzie Chalmers.*) Dead animals?—Yes.

13677. (*Chairman.*) When Mr. Sharpe wrote his letter, of course he was not writing about the dissection of a dead animal; he was writing about what he believed to be the vivisection of a dog?—Yes.

13678. Can you say (I only ask it because Mr. Tomkinson appears to have some further questions about it), was there any dog vivisected at that time?—No, there was no dog vivisected.

13679. (*Mr. Tomkinson.*) There is one question I want to ask you. It seems rather strange that these young men, all of them, say that they are perfectly acquainted with dogs, and that it was ridiculous to say that they could have mistaken the case of a dog in agony for the mere yapping of puppies. Can you explain it to us? Was there any dog persistently howling there day after day?—The puppies made a great noise. I am bound to say that there was no concealment in the matter at all. These puppies played in the yard, and the students themselves going through the museum used to play with them, and certainly they made a noise, a very considerable noise.

13680. (*Sir Mackenzie Chalmers.*) Whose puppies were they?—They were mine.

13681. (*Mr. Tomkinson.*) They were not there for physiological purposes—for experiments?—Not for experiments; but I will explain in a moment what was done with them. The puppies themselves were in the yard, which lies 160 yards from Keble College, with two buildings in between. But the yard opens on the parks, through which anybody may walk. There is no concealment of the animals in the physiological department; and any of these gentlemen could have walked through a public way into this yard, where the puppies were barking, and have seen them. But they did not.

13682. (*Chairman.*) I did not ask you any of these questions that you are being asked now, because I understood that this gentleman had stated that the whole thing was absolutely unfounded?—He has.

13683. And apologised?—Exactly; that is the state of affairs.

13684. You thought that was enough?—Yes.

13685. (*Sir William Collins.*) I think you were going to add what the puppies were there for?—Yes, I thought the Commission would like to know, and I am glad to add it, because I wish to introduce the subject later of the supply of animals. The puppies were kept in order that some of them might be killed in the lethal chamber later and the tissues used for the histological class of the present term. They were so killed. They were not all wanted, and two of them were given away, at the request of two of the students who used to play with them.

13686. (*Mr. Ram.*) With regard to the apology, did it contain a complete withdrawal of the assertions they made?—Yes.

13687. You observe, with regard to what you have just told us as to their mistaking dissection for vivisection, that in one of their letters they say: "A man in Keble, who is in for anatomy, told Mr. Heald (one of those who signed my paper) that they did cut up rabbits, and that lately they had cut up a small dog, and this had been yelling for a week." You observe that apparently the information which they received, and upon which they based their assertion, was not merely that a dog was cut up, but that the dog cut up, whether alive or dead, had been yelling for a week. Was any of that withdrawn?—The whole thing was withdrawn. I can only say that it does not say whether the dog was yelling before or after. The puppies yapped for a great deal more than a week.

(*Chairman.*) If there is any doubt about the fulness of the apology, I think the apology had better be read. I think it would be better, because here there are several suggestions that the charge might not have been withdrawn. Nothing is clearer than what the opinion of the Vice-Chancellor and the Warden is.

(*Mr. Tomkinson.*) I have a letter direct from Mr. Sharpe, which I have authority to read. I wrote a fortnight ago to ask him whether he had anything further to say. He wrote to me on the 20th, and I again wrote to him, asking him whether he was prepared to have his letter read to the Commission; and his last letter says: "By all means read my letter at the Commission. I quite realise the publicity it will receive, and I am perfectly prepared to take the consequences. I have heard indirectly from Professor Gotch, through the Warden, and it is his intention to make my full apology as public as possible. I do not in the least mind that, as, through false information, I made unjust charges against him with reference to the points I mentioned to you. I have therefore no wish to make a simpler withdrawal." I asked him whether he would like this letter to be read, or to make a simpler or more formal withdrawal. Now he authorises me to read this: "Dear Sir,—In reply to your letter of the 19th inst., I am sorry to say I have no more evidence to give. I have found that on closer investigation the facts which were told me as true—viz., that a dog and rabbits had been cut up, and had been yelling—were entirely false. I felt therefore compelled to withdraw these statements, and to apologise to Professor Gotch for my language concerning him when I heard he denied this. I offered to see Professor Gotch when I knew this, but he refused to see me. I am sorry he did so, because I think he might have given me further proofs if I was wrong in *all* my charges. I cannot possibly withdraw my statements in which I said I frequently heard the cries of dogs or animals of some kind in pain, nor that when I asked an official if vivisection was carried on he replied in the affirmative. I did not ask him, it is true, whether it was done with or without anæsthetics. But at any rate I have not yet had a sufficient explanation of the cries I heard, although, as a gentleman, I am willing to believe Professor Gotch's statement that 'no vivisection experiments have been carried on in the past year.' I shall be very glad to answer any more questions you might like to put to me, and might I ask if I may have a copy of the report of the last sitting of the Royal Commission."

(*Sir William Collins.*) Could we not have put in the actual letter sent to the Vice-Chancellor by these students, as that has been read?

13688. (*Chairman to the Witness.*) I do not know whether, after that letter has been read, you think it desirable to read their letter?—I will read the letters if they are desired; at the same time, they are in the hands of the Vice-Chancellor.

13688a. You have a copy of them, have you not?—I have. The first letter was from Mr. Geoffrey Heald. Mr. Geoffrey Heald is the person quoted by Mr. Sharpe. You will see that he is quoted at the bottom of the second letter.

13689. (*Sir Mackenzie Chalmers.*) Is he a student of anatomy?—No, none of them were students of anatomy or science students at all. Most of them, the only ones I know of, are theological students. "Dear Sir,—I have just seen a statement from the book relating to evidence given before the Royal Commission, and I find therein a statement made as follows: 'Dr. Gotch is a liar.' I entirely disassociate myself from such a discourteous remark, and hereby beg to assure you that I never lent nor gave authority for my name to be joined to those who have made such an imputation. I did hear from a student in the museum that a dog had been vivisected, and I concluded that that was the noise that I heard. But since you have informed me to the contrary, then I accept your statement that no vivisection has been performed during the past year.—Yours truly, Geoffrey Heald." I am prepared to state what the student was. I have his evidence. I am prepared to state what the student said. It was a student who was discussing the question of his examination in zoology.

13690. That is what you told us; that it was dissection and not vivisection?—Yes.

13691. On that letter you made inquiries about it?—Yes, on that letter I made inquiries. Mr. Sharpe wrote to me first: "Dear Sir,—I wish to write and apologise to you for the rude way in which I have spoken of you recently, and which I am sorry to say has been published, a thing I never dreamed of. In my hastiness last term I said many things which I have since regretted. I hope you will accept my sincere apology, and regret that this should have happened." I did not accept that. Then he wrote again: "Dear Sir,—I have seen the Warden, and find from him that you do not regard the apology in my letter of yesterday as adequate. As this is so, I should like to state quite explicitly now (1) I am quite prepared to withdraw my letter of June, and to express my regret for having written it. I find that these points—viz., a dog and rabbits having been cut up alive—were quite untrue, but they were told me as facts. I was misinformed, and I cannot substantiate the charges which I and those who supported me honestly then believed to be true at the time. (2) I heartily apologise for having spoken as I did of yourself in my second letter, and withdraw my statement about you. It never occurred to me for one moment that that second letter would ever become public property. I knew that it might go before the Royal Commission, but did not know their evidence would be published. I will do what I can to cancel the publicity given to it by the Press without my consent by writing to Mr. Coleridge withdrawing my charge. I leave you free to make the withdrawal public in any other way that may seem fit to you.—I am, Sir, yours truly, Harold S. Sharpe. We, the undersigned, beg to corroborate this.—Arthur H. M. Peate, Cyril A. Bonser, Alfred C. C. Harvey Evans, Basil W. Truman."

13692. Whether he did communicate it to Mr. Coleridge you do not know beyond that letter—I do not know.

(*Sir Mackenzie Chalmers.*) At any rate, we have had no communication from Mr. Coleridge on the subject.

(*Witness.*) I may say that I have had no communication with Mr. Coleridge except that first letter on this subject at all from first to last.

13693. (*Chairman.*) That clears up the matter entirely, and makes it probable that what you say you learnt from some student was correct, that it was a mistake made as regards vivisection, and that what really was spoken of was dissection?—I have explicit information that that is the source of it.

13694. (*Dr. Wilson.*) There was no vivisection going on?—Not on dogs. I have another letter on the subject.

13695. (*Chairman.*) We have had so many letters that perhaps you had better read it?—This is an inquiry made by the Warden of Keble and Dr. Jackson, the Radcliffe Librarian, on behalf of the museum.

13696. I do not know whether it is worth while having this letter. It seems to me we are getting into too many letters?—I do not wish to bring it forward. It is an absolutely clear case, and can be vouched for by all the students, of course, not only in my department, but in other parts of the museum.

13697. (*Sir Mackenzie Chalmers.*) We have it on the Notes that no experiments were made on dogs at that time?—No vivisection experiments.

13698. When was the last time that a dog was operated on under anæsthetics—how long before, do you know?—I should think four years ago.*

13699. No dog had been operated on either with anæsthetics or without for four years in your laboratory?—No, not operated on. I cannot say definitely, but I do not think there has been a vivisection experiment.

13700. (*Chairman.*) Not for some time before that? —No, not for the last year and a half.

(*Mr. Ram.*) We have an absolute withdrawal of the charges that were brought before us. Need we go further?

(*Witness.*) I should like to say one thing more. The undergraduates were all freshmen in their first year, who knew nothing about science, and I consider that you cannot view them quite in the same light as you would view other people. But I cannot in the same light, I am bound to say, acquit Mr. Stephen Coleridge. He received this second letter; he never communicated it to me; he took no pains to find out whether this was or was not a true statement, and he interpreted it to mean cruelty without any inquiry.

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13701. (*Mr. Ram.*) Although he had received your denial?—Although he had received my denial. Moreover, I want to point out again that the second letter which came before him was written in the sort of language which ought on its own showing to have awakened his suspicions. I note, for instance, that Mr. Coleridge himself, commenting in the "Times" on a statement of Lord Justice Moulton, given before this Commission, uses this very excellent language—this is quoted from his letter to the "Times": "Violence of language is only indicative of shallowness of thought and inaccuracy of statement." This is from one of Mr. Coleridge's own letters. It appears to me therefore that Mr. Coleridge, on his own showing, in consequence of the violence of the language of the second letter, might have suspected inaccuracy of statement. I have nothing more to say.

(*Chairman.*) I think that closes that particular incident.

13702. (*Sir William Church.*) What would be your definition for the Commission of the term "pithed," when you speak of a pithed frog? We have had varying evidence given to us upon the point?—I always use the term for the destruction of the whole central nervous system.

13703. The spinal cord as well as the brain?—Yes, I use it in that way.

13704. Therefore in this *précis* "pithed" and "decerebrated" are not the same?—No.

13705. Do you think that there are any reasons for believing that a decerebrated animal is capable of feeling pain?—No, I think there is no evidence for it.

13706. I suppose it may be taken as an accepted fact from human anatomy, that when an injury takes place to the spinal cord, so long as it is not sufficiently high to implicate the breathing apparatus, the body is insensible to pain—the lower part of the body, and all below the seat of injury?—Yes, certainly.

13707. Supposing there is a sufficient amount of pressure on the spinal cord of a man, you might say that you can have an amount of mere pressure upon the spinal cord which entirely prevents a man having any sensation below the seat of injury?—I think it is a certain fact that if the spinal cord is cut off (functionally or structurally) from the higher cerebral centres, below that point there is no sensation of pain.

13708. And do you know anything which would lead you to think that in a frog whose cerebral centres were removed the sensation of pain would remain?—No, I know of nothing. I do not wish to put before the Commission the wide question of the evolution of consciousness and pain, but all scientific evidence appears to show that this is a late acquisition, and that pain is a specific form of consciousness having a specific value like such definite forms as visual consciousness or tactile consciousness, and is related to the presence of definite structures, those structures being the cerebral hemispheres.

13709. It has been definitely put before us by a witness, if not more than one, that the view is held by some physiologists that sensation of pain remains even after destruction of the cerebral hemispheres in the lower animals—I mean in cold-blooded animals?—What physiologists, may I ask?

13710. That I cannot tell you. The evidence before us has been: a person has come here and stated that it is a moot question of physiology?—The physiology of the central nervous system, I should say, is the growth of the last thirty years. I do not know a single physiologist of the last thirty years who would make such a statement.

13711. There is one more question that I should like to ask you, and that is this: You think the head of a laboratory, or of a department which might embrace more than one laboratory, should be entirely responsible for the work which goes on there?—Yes.

13712. And therefore that the decision should be trusted to his judgment as to the standing of the men who are working in that laboratory?—Yes.

13713. That, of course, should they in any way either transgress the existing law, or even do anything which in his judgment he thought was not a fit experiment to be made, it ought to be in his power to be able to, in the first instance, represent the fact that they had gone

* *The witness subsequently wrote that after careful inquiry, he could not find any evidence of any licence in Oxford for performing vivisection on a dog with or without anæsthetics during the last five years.*

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outside their licence, if they had a licence from the Home Office, and in the second case to refuse that they should work any longer in his laboratory?—Certainly, that is my view. I should like to make some suggestions later as to the Act.

13714. (*Sir William Collins.*) I was going to take you upon that point myself. I do not think you told us how long you have held a licence?—I am afraid I cannot recollect. I had one before I went to Liverpool. Twenty years, I should say.

13715. And I do not think you told us what certificates, if any, you hold?—I hold only Certificate C at present.

13716. What does that enable you to do?—To give demonstrations.

13717. Have you ever held a certificate enabling you to dispense with the use of anæsthetics?—Completely, do you mean?

13718. Certificate A?—No.

13719. Can you tell us how many animals a year you use for vivisection purposes at Oxford?—I personally only give a few demonstrations. I do not use more than half a dozen.

13720. Could you give us any idea of the number of animals you have used for vivisection during the twenty years for which you have held a licence?—At one period I did a large number of vivisection experiments, so that I should think I must altogether have performed experiments upon 150 mammals.

13721. Not more than 150?—No.

13722. And you have enumerated some animals that you have employed. Was that a complete list?—Monkey, cat, dog, rabbit, mice, guinea pig, rat, frog, and fish.

13723. You have used your licence both for purposes of research as well as for purposes of teaching, I understand?—Yes.

13724. Have you encountered any restriction in obtaining a licence or certificate?—Never. I am bound to say that although I have not done so, a recommendation made by me was not accepted.

13725. In your capacity as a lecturer on physiology? —A recommendation for a certificate signed by me in my capacity as Professor of Physiology in the University.

13726. That is what I intended to say. I thought it was as a lecturer or Professor of Physiology in the University that you are yourself entitled to sign an application for a certificate?—Yes.

13727. Would you tell us what that case was?—It was a case which I considered to be a very important piece of work on the feeding of animals, and the effects produced by deprivation of food in a dog, in carnivorous animal.

13728. (*Sir Mackenzie Chalmers.*) A starving experiment?—Yes.

13729. (*Sir William Collins.*) Is that a case in which a licence was refused?—No, a certificate.

13730. Was that a case, then, in which, although the certificate had been signed, the Home Office intervened to prevent its coming into effect?—Yes.

13731. (*Dr. Gaskell.*) How many days starvation was it asked for?—Either six or seven days. I forget the particulars at this moment. I do not know that it was very definitely stated.

13732. (*Sir William Collins.*) In your *précis* you speak of the importance of extending physiological knowledge, and of the value of physiology in matters of psychology and education, this last, as you say, comprising the appropriate training of both mind and body, and you say that the extension of physiological knowledge is of the utmost importance for the above subjects. I gathered in the course of your examination that you intimated that in your opinion the science of education should be more and more placed upon a neurological basis?—Yes.

13733. Should I be right in saying that you think the study of neurology is desirable for the training of teachers?—Yes, that is my point.

13734. That teachers should undergo a course of instruction in physiology and specially in neurology?—I think they ought to know the elements.

13735. And that they should see these demonstrations to which you have alluded?—That is not involved, is it?

13736. I am anxious to elicit your opinion?—I should have no objection to their seeing demonstrations if they were serious students.

13737. You laid importance on what is commonly known as the heuristic method of study?—Yes.

13738. Should that apply to these cases of teachers investigating neurology?—I think it does, but I think they would generally go through what we may term the first university course, and would not therefore probably occupy the position of senior students.

13739. Should they not become acquainted with the functions of the central nervous system?—They would get their general acquaintance in the first part of this course, which is divided over two academic years.

13740. But is it possible to understand the training of mind and body without understanding the working of the central nervous system?—Some of the main principles of the working of the central nervous system can be grasped in the first year's course.

13741. Does that mean without any demonstration?—No, nor should I advise anybody to do it without doing experiments himself; for instance, in the tissues of the frog. They ought, at any rate, once to see some experiments involving higher reflex actions.

13742. But I want to understand. You place importance upon training in psychology and neurology for the training of teachers. Would you think it necessary for them to undergo a course of demonstration exhibiting the functions of the central nervous system?—I think it is desirable that they should see some typical instance of this reflex co-ordinating mechanism. The other parts of the neurology do not necessarily involve vivisection experiments, not the experiments themselves. They involve the results of vivisection experiments. Even practical work can be done, and different parts examined microscopically without the students themselves having to do a vivisection experiment.

13743. Did I correctly understand you to say that you think it is desirable that such students should have, at any rate, one demonstration upon the co-ordinating mechanism of the central nervous system? —They should see co-ordinating reflexes.

13744. Should that be on the higher mammals, as I understand you to suggest?—I think it should. I suggest the rabbit.

13745. Would not the monkey be preferable from that point of view?—That is an advanced part of the subject. I do not find that the monkey is an available animal, owing to the expense, except for a few advanced students.

13746. I understood you to say that you would only accept convincing evidence for conclusions respecting the higher animals from experiments performed upon higher animals?—I was alluding then to the brain.

13747. I was alluding to the brain?—But the central nervous system is not merely the brain.

13748. I have not heard anyone suggest it in this room?—But I said just now that I think these people ought really to see an example on an animal of this nervous co-ordinating mechanism. That includes the brain, the medulla, and the spinal cord, of course. They must see a control reflex. That is my view; it is only my own view, of course.

13749. Would that include a demonstration showing the localisation of cerebral functions?—No, I should not necessarily show that to students of this type.

13750. Why?—I think it is rather an advanced experiment.

13751. Superfluous for those studying psychology and methods of teaching?—No, I do not think it is superfluous, but you must recollect that the last monkey I had for histological purposes cost £2.

13752. It is a matter of economy?—Largely a matter of economical cost, and many considerations come in in connection with what one would demonstrate, it being implied, of course, that the animal is under an anæsthetic, and killed under the anæsthetics in every case.

13753. I gathered that both for your research work and for your teaching you have never had occasion to perform experiments on living animals without resort to anæsthetics?—No, I have not.

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13754. Do you think it is ever necessary to do so?—I think it is absolutely necessary to do so. And that allows me to bring forward the first point I have in connection with the working of the Act.

13755. If you please?—I draw attention to the fact that in the working of the Act there is practically only one certificate, Certificate A, for all experiments which are done in which the anæsthetic is dispensed with. I know it is realised, I may be wrong, that inoculation experiments coming under this certificate are now treated in a separate class, not from the point of view of the certificate, but in the returns that are made I think they are put separately.

13756. (*Sir Mackenzie Chalmers.*) Separately from feeding experiments?—Separately from experiments dispensing with anæsthetics. At any rate, whether that is so or not, I want to draw attention to the fact that there are a very large number of most important experiments which have to be done on animals without anæsthetics, for which I suggest that there ought to be some separate category. These are the experiments of feeding and experiments such as Dr. Haldane is carrying on at this moment on change of environment, alteration in the atmospheric environment.

13757. (*Sir William Collins.*) Experiments not involving a cutting operation?—They do not involve a cutting operation. As it goes out to the public the public is liable to consider that all experiments coming under this certificate involve cutting operations, whereas I think it is the rarest thing. I cannot remember myself a case of an experiment having ever been done without an anæsthetic which has involved a cutting operation.

13758. (*Sir Mackenzie Chalmers.*) Certificate A would not be granted if the experiment involved a cutting operation—anything more than the prick of a hypodermic needle?—Exactly. But that is not sufficiently realised by the public, who imagine that all the experiments which are done of this class under this certificate are what they call cutting operations. I draw attention in the next place to the fact that I consider two of the most important sets of experiments that have to be done under this certificate are the experiments as to food and the experiments as to altered environment. Experiments as to food are being largely done now in America, and I need not point out their importance. The matter has formed the basis of discussion in the British Association Physiological Section at the yearly meeting. The whole question of the minimum diet, whether of the Army, Navy, workhouse, of any diet of any type whatever, is bound up in it. The question of the alterations which occur in the absence of food is not only a very important one from the point of view of ordinary starvation, but involves some principles of great importance for medicine. In fever you have starvation—semi-starvation, as well as a temperature, and we are not yet certain, in consequence of not having experiments sufficiently numerous on this particular head, whether the changes which are produced are produced by the temperature or are produced by want of assimilation of the food. This is a very important point.

13759. (*Sir William Collins.*) What you speak of as feeding experiments would include the starving experiments?—Yes, including the starving experiments.

13760. And would the alteration of environment include such things as drowning?—I should personally have said that that is an alteration of environment, and that experiments must be made upon it. I am aware that that has been before the Commission.

13761-2. Is there anything you wish to add in regard to modifications of the Act or the certificates?—I should like to make a remark (I do not know whether it is worth making) about Certificate D.

13763. Is that the certificate about repetition of experiments?—Yes.

13764. I think it would be very valuable if you would tell us your views about that?—I would point out, about that, that no experiment is a repetition; that every experiment is a display of a phenomenon and its interpretation by the mind of the person who experiments, and this interpretation must be one thing in one generation and another thing in another generation; and it is impossible, therefore, for one to repeat an experiment so far as the whole result is concerned.

13765-6. You mean that the personal equation of the investigators must be brought in?—I mean that scientific knowledge is a group of things which are framed by the minds of men.

13767. For instance, I think that the argument which you have been developing has specially been mentioned in connection with the experiments by Sir Charles Bell on the spinal cord. It has been suggested that it is unnecessary to repeat those experiments with a view to ascertain the functions of the spinal cord with regard to the motor and sensory fibres. What do you say in regard to that?—I say in regard to that that the introduction of the aseptic method has altered completely our whole knowledge of the way to do experiments so as to limit the structural alteration to the part which appears to be involved; that the experiments which were made before the introduction of the aseptic method ought to be repeated, because the introduction of the new method involves a limitation which could not otherwise be attained.

13768. At one time there was a sharp conflict, was there not, between the evidence produced by Brown-Sequard and that produced by Sir Charles Bell with reference to the function of the spinal cord?—By the Brown-Sequard conflict I presume you refer to the crossing of the sensory paths.

13769. I refer to the suggestion that was made by Brown-Sequard, I think, and supported by G. H. Lewes, that Sir Charles Bell's views were altogether erroneous?—I think that means the crossing of the sensory paths. Brown-Sequard supported a view that the sensory path crossed in the cord.

13770. It was also, I think, in reference to the relative functions of the grey matter and the white matter in the cord?—I am afraid I do not know this particular conflict.

13771. I call attention to the fact that Lewes stated that the doctrine of Sir Charles Bell having been devotedly followed for many years, was shown to be wrong in every particular on the experiments of Brown-Sequard?—What date was that?

13772. 1858?—I have already pointed out that in the last 30 years the whole knowledge of the paths in the spinal cord has had to be entirely modified, due to the circumstance that the introduction of the aseptic method has defined more correctly the obvious lesion. I may point out that the method used is that of ablation—removing the part, and seeing what happens; but you must be certain what part you have removed.

13773. That does not answer the question as to whether there was this conflict or not. But do I rightly understand you to suggest that both must be put aside because they were before the introduction of the aseptic method?—I should think that they both had some elements of right and a considerable amount of wrong, and I should suggest myself that the final details of those who did work upon the central nervous system at a date prior to 50 years ago are more or less fallible.

13774. Would the same description apply to the work at the present time?—Well, we are none of us infallible; but, at the same time, the introduction of that method I have just described does, by its limiting more definitely the lesion, make our knowledge far more precise.

13775. We started from the question of the need or otherwise for the repetition of experiments. It has been suggested to the Commission that in certain areas of research knowledge has been so definitely ascertained that it is unnecessary to repeat by way of further vivisection some of the experiments that have been previously made. I rather gather from you that, in your opinion, no experiment is to be regarded as a repetition of another?—That is my view.

13776. Therefore, this Certificate D is superfluous, and there is really no finality to such research?—That is my view. I am bound to maintain, as a scientific man, that there is no finality in science. I am bound to maintain that attitude.

13777. Do I correctly understand you to suggest, in regard to fallibility, that you thought the earlier researches of Sir Charles Bell and Brown-Sequard stood in a different category from those which are being conducted now?—Yes.

13778. Did I rightly understand you to say that in the course of your 20 years' experience you have not had occasion to use curare on your own responsibility?—I have not used curare. I do not use it for the blood-

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pressure experiments that I show. I have not used it for the research experiments that I have done upon the nervous system.

13779. Did I rightly understand from you that curare, in your opinion, does not paralyse the involuntary muscles?—The involuntary mechanisms.

13780. Is its action in the case of the voluntary muscles upon the muscular fibres or upon the ends of the nerves?—I should say it is upon the ends of the nerves.

13781. And does it not so act upon the termination of the nerves in involuntary movements?—Not with the customary dose. It would with a very large dose, I suppose, but I have no evidence of it.

13782. It is a question of doses, is it?—I have no evidence of its being able to paralyse really involuntary nerve endings, even in very large doses.

13783. Is a change in blood pressure the only mode by which you can objectively determine whether the animal is conscious or suffering pain if under the influence of curare?—I suggest that that is one obvious way; there may be others. I really cannot say. I should have thought the other involuntary mechanisms, in the urinary bladder, for instance, or the intestines, might show similar changes. The reason why I suggest the circulation is because curare is used in circulation experiments, and consequently this particular fact of the condition of the blood pressure is brought prominently before the experimenters.

13784. Is there any other objective sign that you could mention to us which could be relied upon in addition to the variations in blood pressure?—I am afraid that I have not got any evidence of another one, but I should imagine, as I say, that most of the involuntary mechanisms which are largely influenced by pain would give objective signs of this type in the alimentary canal or the urinary bladder, for example.

13785. But you have no evidence of it?—I have no evidence.

13786. (*Dr. Gaskell.*) Would you not modify what you said just now with regard to involuntary mechanisms with respect to the action of curare on the heart? It does abolish that?—The heart is rather in an intermediate position. I was not including that at the moment in what I call the involuntary movements.

13787. (*Sir William Collins.*) Would you say unstriated rather than involuntary?—Yes, I would say unstriated.

13788. That leaves the heart out?—Yes.

13789. Then, in order to ascertain whether any changes were taking place in the blood pressure in a curarised animal, would it be necessary to have a manometer?—Yes.

13790. Without apparatus, then, it would not be possible to determine whether consciousness or pain were present?—No, I do not think so, but I do not really know.

13791. (*Sir Mackenzie Chalmers.*) In your evidence do you speak simply as Waynflete Professor, or do you in any way come as representative of any body in the University?—Three of us were appointed to represent the Faculties of Medicine and of Science—Professor Osler, myself, and Dr. Haldane.

13792. We may take it that your evidence is on behalf of those Faculties?—Yes.

13793. Is Professor Osler not coming to give evidence?—I have only heard this morning that he is not.

13794. Then, may I ask, does your evidence represent also the opinion of Professor Osler?—Yes, I communicated verbally the *précis* of my evidence to Professor Osler.

13795. He is Regius Professor of Medicine at Oxford?—Yes.

13796. And as regards Dr. Haldane, I forget what he is?—Reader in Physiology.

13797. Is he coming or not?—I think that he would rather allow me to represent the views of the three of us.

13798. Then we may take your views as representative of the school at Oxford?—Yes.

13799. I want to ask you now on one further point. You yourself have medical qualifications?—Yes.

13800. Are there many physiologists now, and teachers of physiology, who are not medical men?—There is a growing number, I should say.

13801. Who make physiology, so to speak, their profession?—Yes.

13802. Do you think that there is any difficulty arising out of that? For instance, when a licence is granted by the Home Office to a medical man, you have the guarantee of his profession and his position in a great profession; but what guarantee have you of competence and humanity in granting a licence to a man who says simply "I am a physiologist"?—You have certainly a guarantee when a man is in a responsible position in a great university or in any university in this country, have you not?

13803. I want you to tell us what your opinion is?—I should say that that is a better guarantee really than the guarantee of belonging even to such a great profession as the medical profession—the guarantee of your being a responsible teacher in a university.

13804. Appointed by a great university?—By a university.

13805. Would you go further in granting licences to physiologists who were not medical men, or would you confine the licences to people who hold university appointments?—No, I think I should require additional information, but I should not exclude them if that information was given by people who would themselves accept the responsibility of giving it—people I have mentioned.

13806. The people who sign the certificates?—You might have a further guarantee if you wished—of that type, I mean.

13807. Now, taking your evidence generally, have I understood you rightly that the main advances in physiology have been within the last 25 or 30 years?—During the last 50 years there have been extraordinary advances in physiology.

13808. In your opinion would those advances have been possible without animal experimentation?—I should say that they are absolutely based upon animal experimentation.

13809. And in your opinion further advances must equally be based on further animal experimentation?—Yes.

13810. As regards experiments before students, you have told us the necessity of them from an educational point of view. We have been told by some witnesses that they have a brutalising effect on character. Have you any opinion on that point?—I do not believe it.

13811. We have been told, for instance, that witnessing surgical operations has not a brutalising effect, but that witnessing a physiological operation would have a brutalising effect, for this reason—that the object in a surgical operation is to save life, but the object in a physiological operation is merely to obtain knowledge. Have you any comment to make upon that?—I make the comment that I do not consider that a serious pursuit for the attainment of knowledge is brutalising. That would be the case with these students.

13812. You have had a large number of students through your hands. What has been the effect, in your observation, upon your own students?—I think that witnessing demonstrations, and even doing experiments themselves upon the pithed frog, are always regarded as a very serious part of a student's work.

13813. Do you think it is necessary for students to go any further in the way of experiment than operating upon a pithed frog?—Yes, I do. I have indicated that.

13814. What sort of licence would you suggest that students should have?—I should suggest that they should have a licence for carrying out experiments belonging to a certain category, which I am not prepared at present to frame, upon an animal which is presented to them in a fully anæsthetised state.

13815. You mean that the professor, or whoever is in charge of the laboratory, should be responsible that the animal is anæsthetised and in a state of complete insensibility to pain?—Yes.

13816. That the animal in that condition should be handed over to advanced students to do certain operations?—Yes, and killed.

13817. Who would be responsible that the animal was kept under anæsthesia until death?—I should have thought the students, under the control of this responsible official. I do not think that students ought to be allowed to do the experiments without his being present. That is what I mean. They should be done in the presence of a responsible official.

13818. A responsible official who understands anæsthesia?—Yes.

13819. And who would be responsible that the animal was kept under anæsthesia until death?—Yes, that is my view. I should rather limit this control to university officials holding responsible teaching positions.

13820. You mentioned Professor Sherrington's school. What number of students has he?—I could not tell you at this moment.

13821. Are they medical students or training physiologists?—He has medical students and physiologists, and I know he happens to have a considerable class of those who are taking physiology because they are subsequently going to become teachers. May I be allowed to make one more reference to the Act?

13822. Certainly?—I want to bring to the attention of the Commission the extraordinary fact that the Act applies to such a wide group of animals.

13823. Would you kindly explain what you would suggest?—I think it ought to be limited to warm-blooded animals.

13824. The Act should be limited to warm-blooded animals; that is to say, frogs and fish you think have such small sensation that you think they ought not to be included?—I think the whole evidence on general grounds is that pain is a function of the higher animals and evolved as a potent element for certain specific purposes, for producing or restraining certain activities. But that is only a view. At any rate, I maintain that it is inconsistent to penalise work upon fish and amphibia for purposes of scientific research since procedures as regards these animals are quite unpenalised for other purposes—for instance, the whole fishing industry. Then I have a note with regard to the whole question of the supply of animals. I think that is the weakest part of the present position.

13825. That is not dealt with, of course, by the Act at all?—No.

13826. What is your suggestion?—My suggestion is that animals like cats and dogs might be handed over by responsible people, on certain payment, and the only responsible people that I know of are the police authorities.

13827. You mean stray animals that have to be killed?—Condemned animals.

13828. Stray animals that are, so to speak, under sentence of death?—Yes.

13829. That they should be handed over without payment?—Sold by the police authorities for the purpose. I think that there is not merely a sentimental but a possible practical difficulty in the way in which animals of this kind have to be at present obtained. In my own case I make every inquiry possible, but I take very few animals of this type at Oxford, and even when one attempts to grow them for histological purposes you see the difficulties one is landed in.

13830. But on the other hand what you mean is that about 30,000 dogs a year have to be destroyed in London alone, and that as they have to be destroyed you would rather have one of those animals than breed a puppy for the purpose?—I would certainly. That is my view. I may say, of course, that when I spoke of growing these animals I did not grow them for experimental purposes, I grew them to use them as dead material, and even then I was subjected to criticism.

13831. You suggested an amendment of Certificate A. Certificate A is only granted when no cutting operation is involved?—It is granted for inoculations.

13832. It is granted for inoculations; it is granted wherever there is nothing more severe than the prick of a hypodermic needle. How would it benefit anybody to split those up?—I do not see why all these experiments should be grouped together under this one certificate. They are all very different. This is the one certificate I should have thought in which you could make categories.

13833. What would the practical object be?—That the public would receive more ample information from the publication.

13834. It is limited to the public?—Yes.

13835. You referred to one case when I think about three years ago a certificate was refused by the Home Office, by Mr. Akers-Douglas?—Yes.

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13836. If I remember aright, I think he said that he could not authorise starving experiments that would last longer than three days?—Yes.

13837. In your opinion would that have vitiated the particular experiment you required?—Yes, the experiment had to go on for longer than three days.

13838. Would it have involved very severe suffering?—Different views are held about that. I should maintain that it would not.

13839. Starving experiments, of course, would come under Certificate A?—Yes.

13840. In your proposed new classification such would be feeding experiments?—Yes.

13841. Modifying food?—Yes.

13842. Then of course Certificate A does include certain kinds of experiments with food—for instance, experiments with new drugs?—I should group those in an entirely different category, as pharmacological experiments.

13843. How would you group inoculations of disease by feeding? When once you get into grouping you get into difficulties. You may inoculate disease by feeding?—They are divided at present, I understand, into physiological, pharmacological or pathological.

13844. You think there ought to be three categories?—Yes. I think there ought to be three.

13845. A 1, A 2, and A 3?—Yes. That is a suggestion merely.

13846. (*Mr. Ram.*) In the third part of the *précis* which you have given to us you say that as regards the character of the conveyed blood as a determining factor in the co-ordinating mechanisms of the higher animals there has been during the last 20 years a remarkable development through the discovery of a number of internal secretions. Have those matters been discovered entirely by experiments on animals?—Not entirely, but the most important of them have been discovered entirely by experiments on animals.

13847. Has the discovery of those internal secretions been a direct benefit to humanity?—I do not know to what extent they have been a direct benefit to humanity, but I should hold, of course, that the next sentence is an answer to that—namely, that by demonstrating an extensive co-ordinating mechanism, which was previously unsuspected, the discovery forms a new starting point, for the whole question of co-ordinating mechanisms has been practically brought forward.

13848. That is exactly what I am upon. That is the good you think that has resulted from those experiments?—Yes.

13849. And may that discovery in its application be a direct benefit to mankind in your opinion?—In the future I should have thought it was likely to be as directly useful as the discovery of the nervous system control. I may point out that one of these new mechanisms is the discovery by Professor Starling of secretion, a means of co-ordinating the mechanism of the pancreatic secretion.

13850. With regard to the use of some of the higher animals, in your experience do animals of a higher class of development experience greater pain than those of a lower class; are they more liable, do you think, to pain?—I hold the view, of course, that pain is a late evolution, therefore I am bound to say that I think they do. I think man of course is likely to experience it in the highest degree of all.

13851. But save in so far as they are affected by the use of anæsthetics, cats or dogs might experience a greater degree of pain in an operation than a rabbit or a lower animal than a rabbit?—That would be my belief.

13852. (*Sir Mackenzie Chalmers.*) A witness told us yesterday that he concluded that animals which have very sensitive smell or sight are more sensitive to pain than animals with less sensitive sight or smell. Is there any connection whatever between special senses of sight and smell and the power of feeling pain?—No, I do not think so.

13853. (*Mr. Ram.*) Can you see any connection between those two conditions at all?—No.

13854. (*Sir William Collins.*) Are sight and smell an earlier or a later development?—Earlier than pain; I should have thought; the relation, if any, is between

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tactile sense and consciousness of pain; but even there the relation is not really close.

13855. (*Mr. Ram.*) With regard to students, Sir Mackenzie Chalmers has asked you some of the questions that I wanted to ask you, but I have one or two that I still want to ask. You would only allow students to deal with an animal under an anæsthetic, which anæsthetic had been given and was watched by a responsible person?—That is my view.

13856. And you would not advocate operations, I understand, by a student in any case in which the animal was allowed to recover from the anæsthesia?—No, I do not think I should allow a student to do it as part of the physiological curriculum or for the mere purposes of teaching.

13857. Of education?—Of instruction. At the same time I am bound to say that I represent only my own view, and that Professor Osler does not agree with me in that; but he treats the matter from the point of view of the use of such experiments for subsequent medical and surgical practice, and he thinks that in many cases it would be necessary that the animals should recover.

13858. (*Sir Mackenzie Chalmers.*) He goes further than you?—Yes.

13859. (*Dr. Gaskell.*) He would rather advocate the John Hopkins' method of treating an animal like a patient?—Yes.

13860. (*Mr. Ram.*) At any rate, the action of the student would only, in your opinion, be desirable when the animal was under anæsthesia?—Yes, for physiological purposes.

13861. And even if Professor Osler's view prevailed and a student was allowed to deal with an animal which ultimately came from under the anæsthesia, at the time that the student was operating on the animal, the animal would be under anæsthesia?—Yes. I should like to add that I think what Professor Osler means is that this should not be physiology, but a subsequent course, as it were—an introduction to the practice of surgical treatment. I only deal with it from the point of view of physiology.

13862. Would you have the responsible person who is to superintend the student, and be present all the time the student is operating, to be a licensed person; would you limit it to that?—Yes, I should limit it to a licensed person. I should further limit it to a person holding a responsible position in a university.

13863. There would be very few of those. There would be some two or three in the university, perhaps?—Yes.

13864. In one answer that you made just now as to amendment of the Act you stated that you thought that work done for scientific purposes should not be penalised, as the Act is at present, by its being limited to cold-blooded animals. Do you regard the Act as to any extent penalising scientific work?—Experiments on fish are extremely difficult to carry out.

13865. Before I come to the question of cold-blooded animals, taking the Act as it stands at present, both with regard to cold-blooded animals or warm-blooded animals, do you regard it as penalising scientific work?—Perhaps I used the wrong word when I said "penalising." May I say restricting?

13866. The Act does restrict it, you think?—It does restrict it.

13867. Leaving out the question for the moment of warm-blooded animals and cold-blooded animals, does the Act restrict scientific work to an extent which, in your opinion, damages scientific research at all?—I think it does by its scope, not by its detail. I think this one question is rather an important one—it is too wide in its scope. I suggest that it is rather absurd that you should not be able to make any experiments for scientific purposes under the Act, at any rate, on fish. It is a very difficult thing to anæsthetise a fish.

13868. Then you would say that all animals other than warm-blooded animals should be exempt from the protection of the Act?—Yes.

13869. (*Sir Mackenzie Chalmers.*) They should come under the ordinary law of cruelty to animals?—Yes.

13870. (*Mr. Ram.*) Now with regard to warm-blooded animals, do you regard the Act as restricting scientific research?—No, I do not think it restricts it. I think it has operated well.

13871. A good deal has been said by some people who have been before us as to the desirability of making inspection more effective. Have you ever in your own experience been visited by the Inspector?—Yes, I am visited by the Inspector two or three or four times a year.

13872. Do you find his visit any nuisance to you at all?—Not in the least. I often am not there; that is because I do not know when he is coming.

13873. If it was thought to be otherwise desirable and feasible, should you, so far as you are concerned, have any objection to the Inspectors being multiplied in numbers and their visits being multiplied?—Not at all.

13874. With regard to curare, you have told us the effect of curare, and we have had evidence as to the amount of the use of it. Would you tell me, in the small number of cases, for we are told it is only small, in which curare is used, is the effect of curare so essential to the success of the operation that to forbid totally the use of curare would be a serious impediment to scientific research?—I think there are lines of inquiry in which it would be a very serious impediment. For instance, supposing you wished to ascertain the working of the nervous system, the discharge of nervous impulses from the nervous system without these nervous impulses being able to produce the movement of a muscle, under those circumstances it would be absolutely necessary somehow to block the end of the passage into the muscle.

13875. And the absence of curare would make that experiment impossible?—I do not know how it would be done in any other way.

13876. Would it be possible to limit and define those cases?—I think they could be more or less defined, but I think it would be difficult. It is difficult at any one stage of scientific knowledge to make a definition. That is my difficulty.

13877. Do you see any objection to its being enacted that there should be a special certificate for the use of curare?—No, I see no objection to that at all.

13878. One question as to starving operations. The carnivorous animals, as we know, can go without food for a very long time without dying?—Yes.

13879. Would it be possible in your opinion to allow starving experiments to extend with regard to carnivorous animals for such time as might be useful for science, and yet not so long as to cause any material pain to the animal?—I do not know that there is any material pain (that is my difficulty) after the discomfort of the first few days. I do not think the later stages, so far as all our evidence of starvation goes, even in man, are associated with material pain.

13880. Then if the animal suffered any pain, it would be in the first two or three days?—Yes, the discomfort would be at the start.

13881. (*Sir Mackenzie Chalmers.*) Did those exhibitors, those men who starved for forty days, suffer pain?—I do not know.

(*Dr. Gaskell.*) Then is it necessary to have a licence at all? The Hon. Stephen Coleridge told us that it was not.

13882. (*Mr. Ram.*) Should you feel justified in performing a starvation experiment without a licence?—No.

13883. (*Dr. Gaskell.*) Although you think there is no pain?—I think there is discomfort. I may say that I represent also the view of Dr. Haldane about his own experiments, which are in very much the same category; they are experiments upon alteration of environment, especially in compressed air, and are performed at the Lister Institute. There he has a licence, and on my asking him why, he said, "I cannot say that at the start there is not discomfort."

13884. (*Sir Mackenzie Chalmers.*) He has tried it on himself?—He has tried it on himself, and he found that there is some discomfort.

13885. Some discomfort at first?—Yes.

13886. (*Dr. Gaskell.*) You said just a minute ago that you would have no objection to a multiplication of inspectors. Would you adhere to the Hon. Stephen Coleridge's desire that an inspector should be present every time?—No, I do not think it is possible that he should be present every time, because then you could not perform even a simple demonstration to your class without waiting for his arrival. I understand the question to mean that there would be more inspectors than there are at present, who would pay, I presume, surprise visits.

13887. Not that they should be so much multiplied as to be always present?—No.

13888. (*Mr. Ram.*) I understand that your objection would not be to the presence of the inspector, but to the inconvenience of always waiting for him?—Yes.

13889. You would not mind his being there?—No.

13890. (*Dr. Gaskell.*) Then with respect to urethane, I want to ask you a question. The Hon. Stephen Coleridge in the Bill which he proposes to bring forward, proposes to limit the anæsthetics to respirable gases. Have you much experience of urethane?—I use in my demonstrations either chloral-hydrate or urethane almost entirely.

13891. How many years ago is it since you used it?—The last two years urethane, and chloral hydrate the last four or five years.

13892. Do you consider that that is as efficient an agent for producing anæsthesia as a respirable gas?—Yes, and I prefer it because the dose being one which is derived from the absorption of the substance in the alimentary canal must go on gradually increasing to a certain point. You are not dependent, in short, upon the working of an external apparatus for maintaining your anæsthesia.

13893. With respect to the question of cold-blooded animals, do you know Harrison's experiments on cutting out portions of tadpoles and joining them together again?—I know of them.

13894. I presume that under the present condition of things those experiments would require a licence in England?—Certainly. I suppose they would require Certificate A.

13895. Still an experiment on a crayfish would not require anything of the kind?—No.

13896. In your opinion is not a crayfish a much more sensitive animal than a tadpole?—I think it has got a more complex nervous system, though I much doubt whether it has the particular sort of nervous system which involves a consciousness of pain.

13897. (*Sir William Collins.*) Is the crayfish a later or an earlier evolution?—An earlier evolution I should say.

13898. (*Dr. Gaskell.*) Do you consider that analgesia, the absence of sensation of pain, is not necessarily coincident with anæsthesia?—I consider that it is not necessarily coincident with anæsthesia.

13899. Which would you consider would come first?—Analgesia, by which I mean the absence of pain as in a large number of cases of hypnotism.

13900. (*Chairman.*) What other perceptions would survive when the analgesia came on and not yet anæsthesia? What would the other perceptions be that were left?—I think sight and hearing and touch might still be left as in the case of the hypnotic condition in which it is evident that the person sees and hears and feels and is entirely insensible to pain. There are cases of that description.

13901. (*Dr. Gaskell.*) Quite so. Have you made use of Certificate B at any time—to keep the animal alive after the operation?—Yes.

13902. Has your attention been called to the evidence of the Hon. Stephen Coleridge on this matter?—No.

13903. (*Sir William Collins.*) Do you hold Certificate B now. I thought you told me that Certificate C was the only one you held?—It is the only one I hold now. But I have held Certificate B and done a large number of experiments under it.

13904. (*Dr. Gaskell.*) I want to know what in your opinion is the meaning of the term "initial operation." The reference is at page 202, Q. 11473. The Hon. Stephen Coleridge there said, in answer to a question of mine: "My point is that under certificate B, as now worded, anæsthesia must be applied during the initial operation of opening the animal and fixing the electrodes to different nerves, or whatever it may be." Then I say that "under Certificate B the animal can be kept in that condition on the board, and allowed to recover its consciousness, and the animal can be kept there disembowelled, or whatever it may be, as long as the operator chooses, without any anæsthesia to allay its sufferings"?—That is an entire misconception, I should say, on Mr. Coleridge's part of the meaning of Certificate B. I hold, and everybody who has got Certificate B always holds, that anæsthesia must be applied during the whole period of any cutting operation.

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13905. What is the meaning of that period, the initial operation? Is it until the wound is sewn up, or what is it?—It is until the wound is not only sewn up, but I should say dressed.

13906. Dressed too?—Yes.

13907. That is your meaning of the initial operation?—Yes.

13908. And, so far as you are aware, is that the meaning that all physiologists give to that term?—Every physiologist that I know of gives that interpretation to that term.

13909. The only person, then, that you know of who does not give it that interpretation is the Hon. Stephen Coleridge?—Yes. I may say that it strikes me as a most extraordinary statement. I had not had my attention directed to it.

13910. As we are on Certificate B, there was a suggestion made by Lord Justice Moulton with respect to experiments that may involve pain such as those under Certificate B. That would be one class. That is on page 261. He advises that "certain men of acknowledged capacity in research, who have shown that they are not only capable of serious research, but have already done it, should have general certificates entitling them to perform such experiments, subject only to one thing, and that is that they should report fully every experiment. They are immeasurably better able to judge what is worth doing than any body of men that you could put over them. They are the people who are working at the subject itself, and they are, as a rule, men of such high position and high character in England that there can be no ground for being afraid of trusting them with this power." I should like to know whether you think that is a good suggestion and whether you agree with it?—I think it is a good suggestion and I agree with it, but I do not think we are seriously hampered by the working of the present Act except in regard to its scope.

13911. Do not you think that the difficulty of having to get a new certificate if you want to change the animal you are working on is a bit of a hamper?—It involves writing some letters, but I have never had any difficulty.

13912. Does it not involve loss of time?—Yes, it involves a little loss of time. I think there would be no objection to this any more than an objection to the present system, so far as I can see, and that it would be, on the whole, a more satisfactory method of giving the power.

13913. Then there is one other suggested alteration in the Act that I should like to have your opinion upon, and that is, whether it would not be advisable, certainly in the case of animals operated on under Certificate B, to allow them to live in some other place than the laboratory where they were operated upon, to have an outside farm place in which such animals could be kept. The present Act insists that they should be kept in the same place as has been licensed?—Of course, I think it would be an immense advantage, not only for science, but for the animals themselves.

13914. Would you strongly advocate that the Commission should report in favour of such a change as that?—I should. At the same time I should not wish it to be made necessary that you should transfer the animals.

13915. No, but that they should have power?—That it should not be permitted to put them in better hygienic surroundings seems to me to be wrong.

13916. Only one other question. You know most of the physiologists, or I might say all the physiologists in England practically. Have you any evidence whether or no they are kind-hearted men, who are fond of animals?—I think it is rather a remarkable thing that all physiologists almost are fond of animals, and most of them, so far as dogs are concerned, are particularly attached to dogs and keep them as pets.

13917. And the dogs are particularly attached, naturally, to them?—Yes.

13918. (*Mr. Tomkinson.*) You hold very strongly that any limitation of the freedom of choice of mammals or the higher living animals from experiment would be a distinct blow to the researches of science?—Yes.

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13919. In what order do you place the highest animals? Do you put the monkey first as being nearest to man in structure?—Yes.

13920. And on the whole you think in—whatever you call it—instinct or intellect?—No, I put the monkey as being nearest to man because the peculiarity of man is the development of the nervous system, and in the monkey the nervous system is the most highly developed; but I do not consider the monkey to be as near to man from the point of view of the digestive and excretory organs.

13921. And the dog next, I suppose?—The dog is nearer to man from the point of view of the digestive and excretory organs.

13922. Do you think it possible to draw an exact and a very marked dividing line between the highest, the most developed and most intelligent animal, and the very lowest form of human being known to us?—Yes, I could draw that distinction.

13923. Quite enough, apart from other considerations, to make experiments upon the one unthinkable and upon the other quite permissible?—Yes.

13924. What is the object of these operations for the sake of the investigation of the circulation and blood pressure? May I ask whether in those are included experiments for watching the operation of shock?—I presume that that would be one. But the object, as I understand it, is that the only means by which one organ can influence another is either by sending a message to the nervous system and so reaching it, or by the direct plan of the circulation, which, of course, passes through them all and is common to them all, by which I mean the assemblage of the whole organs. Therefore it becomes an extraordinarily important feature in an animal to extend our knowledge so far as possible of this means of intercommunication—viz., the circulation—and that of course is the object, as I understand it, of experiments upon the heart and blood pressure.

13925. But have not experiments of that kind and with that object been made over and over and over again, and is it necessary that they should be repeated?—Yes, and I have no doubt that they will be made over and over again.

13926. Is there something new to be discovered?—Something new is to be discovered almost in every succeeding year.

13927. Have you read a description of 14 cases of operations upon dogs by Dr. Crile?—No, I have not read them. I know the general character of the work, but I have not read it.

13928. You told us that the preservation of circulation was possible, even in an animal when paralysed by anæsthesia, or with the circulation absolutely destroyed by decerebration—that is, the taking out of the brain?—Yes, of the cerebral hemispheres.

13929. Can this circulation go on after the brain is taken out—that is, in some animals, I suppose?—The cerebral hemispheres—not the whole brain.

13930. Does pithing mean the severing of the spinal column?—I do not use the word pithing in that sense.

13931. I mean a pithed frog?—A pithed frog is one in which the whole central nervous system has been destroyed, including the spinal cord.

13932. Is the head severed from the body?—No, the whole system is destroyed in the usual way.

13933. And you are satisfied then that the frog cannot feel?—There is no central nervous system left at all then.

13934. (*Chairman.*) Is the brain taken out and the spinal cord also?—A hole is rapidly made, and by means of a wire the brain and spinal cord are destroyed with great rapidity.

13935. (*Mr. Tomkinson.*) In the case of the experiment of starvation of a dog, would water be withheld as well as food, or allowed?—In the particular experiment for which the Home Office refused a certificate water was not to be withheld. I should say that the experiment could not go on if water was withheld.

13936. In these cases of fasting by men, whether they were actually *bona fide* or not (there was some question about that, particularly in the case of Succi, whether he did not have some food), unlimited water was allowed?—Yes.

13937. Would that, in your opinion, tend to mitigate what are called the pangs of hunger?—Yes, I think so. There might be an experiment made upon the effect of withholding water, but ordinary starvation experiments must be accompanied with allowing the animal to have large amounts of water.

13938. But you rather seemed to think that the pain of starvation is negative rather than positive?—I think it may be expressed by early discomfort better than any other word.

13939. Then you think that the general opinion of the pangs of hunger and the dreadful pain of starvation is somewhat exaggerated?—I do. I think, of course, that provided you take water or something into the cavity of the stomach you will not get these particular cramps.

13940. I want to ask you one question as to the ground of your belief in the insensibility of animals, whether there is no analogy to be drawn between the sensitiveness and the development of the senses in an animal like the dog, and its application to its feeling. You admit that out of its five senses three are extremely highly developed, its sense of sight, of hearing, and scent. Everyone must admit that who knows dogs?—Yes, certainly.

13941. And as regards the fourth sense, taste, we do not know about that, but taste and smell are very close to one another, therefore we may presume almost that the dog has that fourth sense?—Yes.

13942. Then on what possible grounds are we justified in saying as we do, knowing that three at least of the senses are most highly developed, that the fifth sense, that of sensation, is duller in the dog than in man?—Do you mean by the fifth sense cutaneous sensation?

13943. I mean the power of feeling?—You mean touch.

13944. I mean the power of suffering?—Do you mean the sense of pain or the sense of touch?

13945. I put the two together?—But they are quite distinct. As I have already pointed out, the sense of pain and the sense of touch are quite distinct.

13946. But surely the sense of touch is sensitiveness?—I do not wish to go into the details, but even in a man it can be shown quite plainly that there are certain points of the skin most susceptible to contact; there are quite other points most susceptible to heat, quite other points most susceptible to cold, and quite other points most susceptible to the particular pressure which produces pain.

13947. But is it wrong to say that the five senses are seeing, hearing, smelling, tasting, feeling?—They have been greatly extended.

13948. I am afraid that the man in the street does not think so, and I am the man in the street?—I am afraid that the cutaneous sensations, as they are termed now, really comprise groups as different from one another almost as sight from hearing.

13949. The sense of touch is most highly developed in the ends of the fingers?—Yes.

13950. And surely the ends of the fingers, or the ends of the toes, are the parts of the body most sensitive to pain?—I think there are spots in your arm which could be demonstrated which would give you more pain.

13951. I would rather be vivisected in any other part of my body than in the ends of my fingers or the ends of my toes?—Then may I point out that you can touch hot things with your fingers.

13952. Only for an instant?—Without your being able to bear that temperature in other parts of your skin.

13953. Only because the skin at the end of the fingers may be a little rough or hardened by work when they have not got a covering like a glove.

13954. (*Dr. Wilson.*) I observe that you have not used dogs for two years at Oxford?—Not for vivisection experiments but for histology.

13955. As dead animals merely?—Yes.

13956. You do not believe in the necessity of demonstrating these blood pressure experiments before your class?—Yes, I demonstrate them on large rabbits, and sometimes on a cat.

13957. So that according to you—at all events, according to your experience at Oxford—dogs can be dispensed with altogether in these experiments before students?—Some things cannot be done without dogs, but then I do not do those things. I do not say that

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they are not advisable to be done. For instance, the influence of the nervous system on secretion would be very difficult to do upon any other animal except a dog or a cat—preferably a dog.

13958. You do not consider it necessary for your class?—I do not do it, but I have a difficulty in the supply of animals.

13959. That is the reason, is it?—That is one reason.

13960. You say that no experiments are final?—That is my belief.

13961. The results of each experiment must be translated in a different way by different experimenters, there is that possibility always?—Yes, I should say that.

13962. So that there can be no finality?—I do not think there can be finality.

13963. And, of course, you strongly advocate experimentation on animals purely from the scientific point of view, not as an aid to medicine or surgery, or the relief of pain. You think it is absolutely essential for the acquirement of useful knowledge as a part of science?—Yes, that is, if I may say so, the trunk of the tree of knowledge. I think that physiology is in the trunk, but you cannot get the fruits, of course, if you damage the trunk. Some of these fruits are the medical and surgical advancements which result in the relief of pain.

13964. But supposing that it were even possible to prove to your satisfaction that no further cutting experiments on animals are necessary for the advancement of either medicine or surgery, you would still advocate the performance of these experiments before students?—I cannot really conceive such a condition, but I should advocate the advance of scientific knowledge under any circumstances.

13965. Would you use urethane and chloral hydrate instead of chloroform if you were experimenting with dogs?—I have had no experience of the use of urethane and chloral hydrate with dogs.

13966. Your laboratory, of course, is not very far from houses?—It is in the Parks. It is some way from houses, but still it is very open to the public.

13967. But to allay the sensibilities of the public you would recommend that laboratories for experiments on living animals should be at all events kept away from houses?—I suppose it is necessary, or at any rate desirable.

Mr. M. S. Pembrey, M.D., called in; and Examined.

Mr. M. S. Pembrey, M.D.

13968. (*Chairman.*) You are a Doctor of Medicine and Lecturer on Physiology at Guy's Hospital?—I am.

13969. And a member of the Army Medical Advisory Board?—Yes.

13970. As a lecturer on physiology, I presume that you have had experience in experiments on living animals?—Yes, I have had considerable experience, both from the point of view of research and of teaching. I have taught physiology at Oxford, Charing Cross and Guy's.

13971. I will ask you shortly first of all a question which has been asked of many witnesses. Do you consider that the demonstration of experiments to pupils is or is not a valuable or essential part of education?—I think it is absolutely necessary if they are to obtain a proper knowledge of physiology in its application to medicine. I may take one example—coal-gas poisoning. We can show coal-gas poisoning on a mammal such as a rat or a rabbit. It takes 30 seconds to kill a rat or a rabbit with coal-gas. I can then demonstrate to the men the fact that when an animal is poisoned by coal-gas, as is perfectly well known in man, it dies a painless death—it becomes gradually unconscious, then gives convulsive movements, due to asphyxia, and dies—from lack of oxygen. If when it is dead, one cuts open the animal, one can at once demonstrate the characteristic change in the colour of the blood. Of the application of that to coal-gas poisoning we have a case in hospital at the present time. One points out to the men that the colour is quite characteristic, and the same knowledge is seen in man. And one can also show them on animals the treatment of such a case that it is no good to transfuse, as the chief thing to do is to give oxygen. Further, one can point out to them that an animal, such as a rat or a mouse, can be used as a test for the presence of carbon monoxide in the air of a mine after an explosion, and show them that by sending down an animal such as a rat or a mouse it can be used as an indicator of the presence of a poisonous quantity of the gas. This method is used. That is only one instance. Would you like me to give some further instances?

13972. If you would give us shortly one or two?—I may take another instance from experiments on the nervous system. This is one of the cases which is objected to by the anti-vivisectors. One can show upon a frog that after destruction of the cerebral hemispheres most complex movements can be carried on. The practical application of that one can bring out by showing specimens of brainless monsters. These brainless monsters, as it well known, can breathe, suck, cry and perform complicated movements, but there is no evidence of course that they are conscious.

13973. I do not quite know what a brainless monster is?—I am using the term "brainless monster" in the medical sense, that is a child born without any cerebrum. It ought not strictly to be called a child, because it is not really an individual. Then following from that example one can lead up to all the complicated mechanisms which can still act in a man who is perfectly unconscious. I could give more instances if you like, they are so many, and we use them every day. These of course are experiments which do not come strictly under the Act. Perhaps it would be well if I were to give an experiment which comes strictly under the Act, an experiment in which it is necessary to give an anæsthetic. I think it is necessary to do some experiments upon an animal under anæsthetics in order to demonstrate fundamental points on the circulation and respiration —to show, for example, the influence of opening the pleural cavity. Medical students find it very difficult to understand the mechanism of the pulmonary ventilation unless they see these experiments; they cannot form a definite idea of the contraction of the heart and of the passage of the blood through an artery and vein unless they see these experiments upon a mammal. A frog is not big enough to show the difference in the circulation in an artery and a vein. I could give many instances, but I think it would only take up your time.

13974. These are instances of the kinds of experiments which you think are absolutely necessary?—Yes, I think they are absolutely necessary.

13975. I want now to ask you some questions with reference to a statement made by Miss Lind-af-Hageby in her book, which is referred to at Question 9354 and following questions. The statement was: "We once saw a marmot the spinal cord of which had previously been divided by the vivisector," and then the story continues. You, I believe, were acquainted with that marmot?—It was my marmot.

13976. Was it kept at Guy's Hospital?—Yes.

13977. Was this experiment at Guy's Hospital?—No, it was at the University of London, South Kensington.

13978. And you brought the marmot there on that occasion?—Yes.

13979. I ask you at once, because there seems to have been considerable dispute about it, was the spinal cord of that marmot ever divided?—No, that is absolutely untrue. I have the body of the marmot here, and if you like I can show it. Perhaps I had better do it at the end of my evidence, and then I can wash my hands.

13980. How long ago did it die?—It died in 1903, but I think this marmot ought to be thoroughly well examined.

13981. Has it been preserved?—Yes, it has been preserved.

13982. Then I think you had better show it?—Shall we leave it to the end or shall I bring it out now?

13983. I think you had better do it at once—it is rather a principal point of your evidence?—Of course it may be suggested that this is not the marmot, but my laboratory man is here, and can confirm my statement that this is the marmot. (*Exhibiting the marmot.*)

13984. (*Sir Mackenzie Chalmers.*) You know that it is

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the same marmot?—Yes, but if my word is not good enough my laboratory man can confirm it, because that may be a point of dispute.

13985. (*Chairman.*) No, that is not the point, I think?—This is the same marmot that Professor Thane examined. We did not try to mount the specimen after Professor Thane examined it. I thought of having it macerated so as to obtain its skeleton. I can cut down on to the spinal cord and show you that it has not been divided. It is as Professor Thane examined it, absolutely intact. There has never been any operation on this marmot at all.

13986. (*Sir William Collins.*) Has the spinal cord ever been exposed?—No, there has never been any cutting operation on this marmot at all. I am perfectly willing to cut through the skin and show you that the cord is undivided. I have got my laboratory man to bring a scalpel on purpose.

13987. (*Chairman.*) No, I think the Commission do not need that?—I think, at any rate, it ought to be on record that I offered to do so.

13988. Miss Hageby says that she never saw the marmot, except on that one occasion, and from what she saw on that occasion she spoke?—Well, that is the marmot.

13989. There was some question about whether the marmot had been used for experiments?—That statement is also completely false. No experiment under the Act has been performed upon this marmot by me, nor has any experiment under the Act been performed by me upon any marmot.

13990. When you say no experiment under the Act, what do you mean exactly?—I use the term in that sense simply for this reason, that confusion has arisen because Professor Thane said that no experiment was performed except taking the temperature. Taking an animal's temperature I do not consider to be an experiment, and that is the whole point upon which the apparent conflict about Professor Thane's evidence rests. It is simply the use of the term "experiment." I told Professor Thane that no experiment under the Act had been performed. The taking of an animal's temperature is not an experiment.

13991. Was the temperature taken with a view to ascertain what was the effect of hibernation upon temperature?—Yes. Simply an ordinary thermometer put into its bowel or into its mouth.

13992. Were there any experiments of starvation?—There were certain fasting experiments, and in reference to that I sent down to the Commission some letters which appeared in the "British Medical Journal."*

13993. In the experiments on fasting, was the animal hibernating at the time?—No. I have a copy of a paper here containing an account of them. They were experiments upon the marmot during the deposition of fat. These animals can put on an enormous quantity of fat, as was shown by this paper, and in order to study it on some occasions the animal was kept in a fasting condition, and exception was taken to the one in which it fasted for 52 hours. The confusion is simply due to ignorance that hibernating animals, as is notorious, often in the summer will go into a sleepy condition, and take no food for days, because they have a large storage of fat—they can almost double their weight by putting on fat—so that it does not matter to them in the least if they go without food for a week or more. In the winter, for example, they may go without food for six months.

13994. I do not know that this question of fasting is very material. What I want to know is whether the experiment—testing the effect upon the marmot of fasting 52 hours—was a natural hibernation in the summer time, or was it that while it was lively and willing to feed it was not fed?—All the conditions are stated in the paper. The rectal temperature, the weight, and all the conditions are given in this paper† (*handing in the same*). The animal was then, of course, awake; it was not hibernating. But even when they are awake they often for days take no food. In support of my word I have references to the most recent monographs upon hibernation, and I can give you them to confirm my view.

13995. Perhaps you will just give us the reference?—As a general point, it is well known that in warm climates animals often hibernate in the summer; I will give the reference to that. *Ergebnisse der Physiologie*, third year, second part, pages 220 and 221. That is the most recent work in Germany on hibernation.

13996. The only experiment in the way of feeding, I understand, was leaving the animal without food for 52 hours on one occasion?—Yes.

13997. And something less on another?—Yes; on one occasion 52 hours and on another 42 hours. They are all given in this paper.

13998. Did the animal survive that?—Yes. They would survive, as I said just now, weeks of it in the summer, if that were done. That has been done, but not by myself.

13999. What was the object of it?—The object was to see the contrast between fasting and feeding.

14000. The contrast in what way do you mean—in the appearance of the animal or in the temperature?—In the animal's temperature and respiratory exchange. The experiment was on the gases that it gives out from its lungs and bear upon the question of the deposition of fat.

14001. (*Mr. Tomkinson.*) And the weight?—And the weight.

14002. (*Sir Mackenzie Chalmers.*) So far as you could tell, did the animal suffer or not?—It suffered absolutely no pain. I have two marmots now, and I should like, in support of my evidence, to bring up one of these marmots, or to let you have the two marmots so that someone can keep one, without your knowing which, without food for 52 hours, and then you could not tell the difference. There is an opportunity for you to make the experiment. I will provide the two marmots, and you will not be able to tell the difference unless by delicate weighing.

14003. (*Chairman.*) I think we must be content with your evidence. This is very much a by-point which happens to have cropped up?—If I may say so, I think it is frivolous, because I am willing any day to fast for 52 hours without suffering pain.

14004. You are not a marmot?—But a marmot can do it all the better.

14005. Yes, I know the marmot has the advantage of you?—Yes.

14006. You say that those are the only experiments that were performed upon this particular marmot?—No experiments under the Act were performed upon it. Those were only observations, and they are not under the Act.

14007. (*Sir Mackenzie Chalmers.*) They are not under any certificate or licence?—No.

14008. You did not consider them painful?—I did not consider that they came under the Act as painful, and therefore I did not apply for a certificate. The responsibility is thrown upon me of determining whether experiments come under the Act or not, and I think it would have been frivolous if I had applied for a certificate. I could have applied for a certificate, and have protected myself from bother, but I think it would have been frivolous to do so.

14009. (*Chairman.*) We know now that the spinal cord of this marmot has never been divided; you have produced the marmot. What was the condition of the marmot on that day of which Miss Hageby spoke?—It was in a condition of hibernation, and it was waking up. I can quite easily explain why the mistake arose as to the paraplegia. The condition of the animal was that the hind legs were quite paralysed and drawn out behind the body. Instead of the animal walking like that (*describing*), it would be drawn out thus in a condition of paraplegia. This is mentioned in one of my papers, and there is no excuse for the statement that the animal's spinal cord was cut, because I can give you references to the actual papers, in which I say that no operation had been done, and in which I also give the explanation of the paraplegia. On page 80 of this paper of mine I actually explain the paraplegia (*handing in the same*).‡

14010. (*Sir William Collins.*) When was that paper published?—In 1901, two years before that statement in "The Shambles of Science" was published.

* *May 23rd and 30th, 1903.* † *Journal of Physiology*, 1901, *vol. xxvii.*, *p.* 407.
‡ *Journal of Physiology*, 1901, *vol. xxvii. p.* 80.

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14011. (*Chairman.*) It does not follow that it had been brought to the author's notice?—No; but she refers to these papers in the Blue-book and in the evidence too. Further, I can show by reference to the *Ergebnisse* again that the condition of paralysis of the hind legs is perfectly well known to all people who have worked at the question. I can give the reference to that if the Commission wish it.

14012. I do not think it is necessary?—It is a thing that should be known. I can give the pages—227 and 228 of the *Ergebnisse.*

14013. (*Sir William Collins.*) Are the symptoms just the same as from paralysis after division of the spinal cord?—Yes. A very interesting thing also is that as the animal wakes up this paralysis passes off, and on that very day, at the end of the demonstration, by the time that the marmot got back to Guy's there was no paraplegia at all. It is a condition that comes on each winter.

14014. (*Chairman.*) Did the marmot live for some time after that?—I gave the lecture in 1902. It lived for a year, and you see that marmot now upon which, if the Commission wish to see it, I could demonstrate in one moment that the spinal cord is uncut.

14015. (*Sir Mackenzie Chalmers.*) What did this particular marmot die of a year afterwards?—It died of an abscess in its throat. There was no experiment done. The animal got an abscess in its throat which extended to the eye, and it died. It was kept practically as a pet from 1898 to 1903. During this winter I am willing, if the Commission wish, to demonstrate the paraplegia upon two marmots which I now have at Guy's.

14016. (*Chairman.*) If our functions are extended to examining into the habits of the marmot we will avail ourselves of your offer, but at the present time I do not think it is necessary.

14017. (*Sir William Collins.*) Is that the only marmot that you had at that time?—That is the only marmot I had at that time, and I believe it was the only one that any physiologist had in England.

14018. (*Chairman.*) I think in answering those questions you have answered also some attack that was made upon you in a newspaper to which you wish to refer. I think that is sufficiently answered by what you have told me?—Yes. I would like simply to show the way in which we physiologists are misrepresented. Complaints were made that I was freezing hibernating animals by exposing them to a temperature of 10°. That complaint was made and inquiry was made about it. I was asked to supply details about it. It was an absolutely fallacious statement on the part of the anti-vivisector. It was 10° Centigrade, which is the ordinary temperature of my room in the winter. They thought it was 10° below freezing point upon the Fahrenheit scale.

14019. (*Sir William Collins.*) Has that statement been made in the Blue Book?—No, it has not. This is a copy from a letter to the "British Medical Journal."

14020. (*Chairman.*) I think we are getting rather beyond the question. We are not looking up all the statements which have been made by anti-vivisectors and then proceeding to contradict them. We are inquiring into the working of the Act?—The reason why I put it in was because they did consider these to be painful experiments.

14021. It was somebody writing in the papers that they did not consider this or that, but it is not really material; it is not an attack that has been made specially upon you with reference to any evidence that you have given or anything of the kind. Those I think are the points to which you wished to draw attention, especially as regards the marmot?—There are other points, my Lord. I wish to refer to the answer to Question 7410. That is with reference to a joint book published by me and the late Dr. Phillips: "The Physiological Action of Drugs."

14022-3. This was in MissLind-af-Hageby's evidence?—Yes. I wish to bring to the notice of the Commission that it is carefully pointed out in the preface to that book that the law relating to experiments on animals renders necessary the destruction of the animal's cerebral hemispheres. I say that to me the statement of the witness is absolutely misleading, as I carefully pointed out in the preface what was the law, that these experiments must not be performed without destruction of the cerebral hemispheres, and I contend that, so far as I can understand Miss Hageby's evidence, it maintains that we did the opposite.

14024. The statement there is that you said that (*after referring to the passage*) physiologists looked upon experiments on frogs as legitimate for students to perform?—Yes, and I point out in the preface of this book that on account of the law it is necessary in all these cases that the cerebrum should be destroyed.

14025. In frogs?—In frogs; so that Miss Hageby's evidence absolutely gives an entire misrepresentation of what we say in the preface.

14026. I do not think that is quite so. Surely an experiment on a frog with its brain taken out would come under the Act?—No, it is not under the Act. A frog with its brain removed is not under the Act, and that is carefully stated in the preface.

14027. You do not mean that the Act excepts frogs altogether?—No.

14028. Do you say (the Act does not say so in so many words) that if you perform an experiment on an animal which kills it it is not an experiment?—No; it is not an experiment under the Act, if the brain has been destroyed.

14029. If part of the experiment is killing it?—Then you cannot kill the animal in any possible way without doing an experiment.

14030. You kill the frog by pithing the brain?—By destroying the function of the brain.

14031. (*Sir William Collins.*) Is that pithing that you are speaking of?—I carefully say destruction of the cerebral hemispheres.

14032. Is that pithing?—I say that pithing is the destruction of the total central nervous system. That is the definition which I use of pithing throughout the book.

14033. Destruction of the cerebral hemispheres would not, in your opinion, be pithing?—Not in the sense in which I use the word, and not in the sense in which I teach my students. I carefully draw attention to that—that pithing is used in different senses; by some observers it is used for the complete destruction of the central nervous system, and that is the sense in which I use it.

14034. (*Chairman.*) You complain that Miss Hageby says that you have spoken of frogs as animals upon which students might operate without first stating that you had said that they should only operate after the frogs have been pithed?—Yes, after pithing the cerebrum.

14035. (*Dr. Wilson.*) After decerebration?—Yes; thank you.

14036. That is the difference?—Yes. Then there is a chapter to which I would like to refer in the "Shambles of Science" on "painless experiments." It refers to a lecture of mine, and seeing that it is full of false statements, I should like to have an opportunity of being taken right through them. It begins at page 11 of the book.

14037. (*Sir Mackenzie Chalmers.*) Which edition?—I have the first—I did not know that there was another. If there is a second edition it seems to me that it is a libel on me.

14038. We have "the fourth and revised edition"?—I have only the first one, published by Bell.

14039. (*Chairman.*) Will you refer to the passage?—I should like to be taken right through. First of all it is maintained that I inserted a pair of forceps under the skin behind the skull for a moment. That is absolutely untrue.

14040. You were the lecturer on that occasion, were you?—I was the lecturer at London University. I believe the date is wrong.

14041. That is a little material as to identifying you as the lecturer?—Yes, I raised that in connection with the question whether I could bring an action for libel. I believe the date is wrong by one day. I have not looked that point up in my pocket-book just now, and I could not swear to it.

14042. (*Dr. Wilson.*) It is immaterial?—Yes, it is immaterial here. The experiments were done by me, and Dr. Waller can support my statement. I did not insert a pair of forceps under the skin. I destroyed the cerebrum, and this is also shown later on towards the end of the chapter on page 14.

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14043. (*Chairman.*) It does not seem to me very material whether you put a pair of forceps under the skin behind the skull for a moment. You did destroy the cerebrum?—Yes; but the inference from the statement that I inserted one point of a pair of forceps under the skin behind the skull for a moment is, to any one who knows the subject, that I made a farce of destroying the cerebrum, which I did not. Then the next point is on page 13.

14044. (*Sir Mackenzie Chalmers.*) Will you read the words first to which you object?—This is a very minor point, but it is suggested there that I thought it would be better to cover over the frog after the destruction of its brain, with a cloth. I did nothing of the kind. It is often noticed in lectures that if the animal is not covered over, whether anything has been done to the animal or not, it distracts the attention of the audience, and for that reason it is constantly covered over. When an animal is brought into the theatre, it is absolutely imperative to cover up the animal before anything is done with it, or it distracts the attention of the audience in the same way as a butterfly in church will distract the attention of the congregation. It was not thought that it would "produce disagreeable reflections in some of the audience," as is stated. Then, again, on page 13, it is said that the rabbit was placed in a freezing machine. There was no freezing machine there at all. There was only an ice chest, and a piece of ice was put into that ice chest in order to lower the temperature of the ice chest.

14045. (*Chairman.*) Do you mean by an ice chest a chest with double walls for ice?—Yes. The temperature could never be down to zero, so that the rabbit was never exposed to the cold to which it is constantly exposed in winter. As a matter of fact, the temperature was four or five degrees above freezing point. The reason for that will appear later. Then it is further stated on page 13 that after fifty-five minutes the rabbit was taken out of this freezing machine, and found to be "beyond the stage for observation." Now the actual facts about it are these; and upon this I could bring Dr. Waller and Dr. Alcock, I believe they were both there. Dr. Waller I know was there. I showed before the audience that the animal's temperature had not gone down, that I had waited too long. I took the animal's temperature before the audience, and showed that the temperature was about 37 degrees Centigrade—that it had not gone down one degree.

14046. What is that Fahrenheit?—About 99 degrees—between 98 degrees and 99 degrees.

14047. What is the normal temperature of a rabbit?—About 99 degrees. Then these references must be taken together, because it is maintained on page 14 that the rabbit was "quite conscious but frozen stiff like a piece of wood." That is absolutely false, and it is shown on the face of it. An animal frozen stiff could not have a temperature of 37 degrees; it could not jump. It is absolutely absurd; it is entirely false. The animal's temperature taken before the audience was not one degree below the normal temperature, it could not therefore be even suffering from cold. The statement is absolutely false, and is shown to be false by the statement that the animal tried to get away.

14048. (*Sir Mackenzie Chalmers.*) If I understand you aright, what you meant when you said that the animal was beyond the stage of observation was that the temperature had fallen, and had very nearly gone back to normal?—Yes.

14049. That was all that you meant?—Yes, that was the only point of it, because the reason was to show the audience the importance of paying attention to the question of cold during an operation. It is well known to surgeons that unless the temperature of the air can be kept up during an operation, the patient's temperature goes down from the influence of the anæsthetic, and he is in a worse condition for recovery. I anæsthetised the rabbit. It was, therefore, not under the Act. I exposed it to a temperature of only two or three degrees above freezing point, and constantly in winter it is exposed to 10 degrees of frost under natural conditions, or even inside a laboratory it may be exposed to actual freezing point. I forgot, owing to the lecture, to take its temperature before the anæsthesia passed off. It had gone down slightly. I did not prolong the action of cold; it was beyond the stage of observation, because I did not observe it early enough, and, of course, the observation of the temperature showing that it was 37 degrees Centigrade, showed conclusively to the audience that the animal was not even suffering from cold, and that was the important thing. It is absolutely false to suggest that that animal was even suffering from cold, much less that it was frozen stiff as a board—stiff as a piece of wood.

14050. Miss Hageby states then that you again inserted an instrument into the skull cavity of the frog, moved it round several times, after which you said, "Now the brain is thoroughly destroyed," and she says, "What, then, about the first destroying"?—It is said on the first page that I only put a pair of forceps under the skin. She contradicts herself when she says that I put it into the skull in the first case. Those two points have to be taken together.

14051. She says that you informed them that you had destroyed the brain, and that the frog could not feel pain any longer?—Yes.

14052. (*Chairman.*) What had you done?—I had destroyed the cerebrum first; it was a decerebrate animal in the first case.

14053. And in the second case?—In the second case I did the complete pithing.

14054. You mean that you destroyed the rest of the brain?—Yes. I was perfectly fair at those experiments. If you read page 15, you will see evidence of my fairness, and what I complain of is that outspoken remarks are absolutely turned against me. I said I was not licensed for that laboratory, and therefore that I could not perform vivisection experiments. I said that I did not think the laboratory was licensed for vivisection experiments, and anything, therefore, that I did would be experiments which were not under the Act, and were painless experiments. And to freeze an animal, even partly, without anæsthetics, even to let its temperature go down 10 degrees, apart from any question of freezing it as stiff as a piece of wood, would be a cruel experiment. The suggestion, I maintain, is absolutely monstrous. I think it is contradicted by the statement: "The animal springs back." The only other point is about the same animal, only enforcing the same remark. The animal was not half paralysed by the cold, its eyes were not glazed in the least, and there were no signs of terror in the animal at all.

14055. Had anything been done to the rabbit at all, except putting it into this box?—Only giving it the anæsthetic.

14056. (*Sir Mackenzie Chalmers.*) Was the animal any the worse for it afterwards?—None the worse. Can I bring out a few other points that I have notes of in reference to some of the statements given in evidence before the Commission?

14057. Certainly?—One is in reference to sulphur dioxide experiments in relation to the destruction of rats on plague ships. The reference to that is in your last volume of evidence.

14058. (*Chairman.*) I have not had any notice of any other statements, except those with regard to the marmot. I did not, in fact, know that you were going to speak about these things?—When I was asked to come, I was not told at the time that I should be expected to give a *précis* of all the points I wished to bring before the Commission, but I thought, seeing that the Commissioners did touch upon the point of the sulphur dioxide experiments in connection with the destruction of rats on plague ships, I might state that I did some of those experiments within the last year or two in conjunction with some work done by Dr. Wade, who did the chemical part. It was work done for the Local Government Board. I want to draw the Commission's attention to the different position in which I was placed as a physiologist, compared with an ordinary man. A rat catcher on the Thames could use sulphur dioxide, and could be paid for using sulphur dioxide, and he could not be interfered with for the destruction of rats in that way. It was necessary for me, a physiologist, to get a special licence, which was limited to so many rats, in order that I might do experiments on rats, with the object of finding out for the Local Government Board what was the minimum dose of sulphur dioxide necessary—that is to say, I, as a physiologist, am in a worse position than a rat-catcher. These experiments are painful experiments. I returned them as painful experiments.

14059. How does the poison operate on a rat—at once?—No, it takes some time to produce fatal symptoms.

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Further, one cannot do these experiments with anæsthetics. If one does them with anæsthetics one cannot arrive at the minimum-dose of sulphur dioxide which is necessary to kill the rats on board ship, and therefore one cannot tell the length of time for which the ship should be shut down in order to kill all the rats. The sulphur dioxide acts upon the eyes in the first place, and I have not the least doubt causes pain. The eyes become quite opaque from the irritant action which the sulphur dioxide produces. As I said, I returned these as painful experiments. I could see that the animal felt the irritation by its trying to wash off the offending matter. The sulphur dioxide irritates its trachea and lungs, and causes violent bronchitis, and it is chiefly in that way that death is brought about. These are the painful experiments which I have done.

14060. You were not using this sulphur dioxide on board a ship for the purpose of killing all the rats on the ship?—No, I was using it in my laboratory.

14061. You were making an experiment with a view of ascertaining what was the best method of killing a rat?—And the proper dose.

14062. That was clearly an experiment under the Act?—Yes.

14063. Then you say that you are comparing your position with a rat catcher's position?—Yes; I say that I am worse off than a rat catcher.

14064. The one is killing the animals and the other is experimenting on them; that is the difference?—Yes, but a rat-catcher can kill them by the thousand and I am limited to experimenting on only five or six. I say that the limitations of the Act are against research.

14065. Under the Act there is nothing to prevent a man shooting his pack of hounds, all of them on the same day, but if he hands one of them over to you to experiment upon, that is an experiment?—Yes.

14066. Although you may be going to kill it under anæsthetics?—Yes; but the rat catcher is not killing the rat in the most painless manner possible; it is a painful death.

14067. I think we see your point?—I wanted to mention it in order to show the necessity of painful experiments. I think that painful experiments are necessary.

14068. That, of course, is the whole question. There is no question if they are not necessary?—I mean painful experiments, as against experiments under anæsthetics.

14069. (*Sir Mackenzie Chalmers.*) You maintain that they may be necessary in certain cases?—Yes. Can I give another example?

14070. Certainly?—I have worked on the question of bleeding—on the influence of bleeding and transfusion. These are experiments that I did in Germany, where it was not necessary for me to be licensed. I did them with Dr. Gürber.

14071. (*Chairman.*) With a view to what point are you bringing this forward?—To show the necessity of experiments without anæsthetics. I want to bring out the fact that if one studies the question of bleeding or transfusion, it is impossible to give an anæsthetic. If one gives an anæsthetic, one cannot study the effect of bleeding and transfusion. It is of the utmost importance that we should know what is the effect produced upon a man by the loss of a large quantity of blood. These experiments were directed to see what would be the difference in respiration, and the rabbits were not anæsthetised at all; they were simply tied down, and they did not show any violent struggling or signs of pain. I may say that my laboratory man has brought two rabbits—I did not know whether the Commissioners would like me to show the experiment, but I would like to show one point in connection with this, which I think is overlooked—that is, that if an animal is bound down on its back it often passes into a condition of hypnotism, which I can show you if you will allow me. I could take a perfectly intact rabbit, simply put it down on its back, keep a little pressure on it for a short time, then take away the pressure, and the animal will pass into a condition of hypnotism. I have seen in Germany, when I have done these experiments without anæsthetics, that these animals, when tied down, pass into a condition comparable to hypnotism, and it is known in the case of man that operations can be done under the influence of hypnotism without the patients feeling pain.

14072. Are you suggesting that hypnotism should be used in cases of experiments and no anæsthetics?—In some cases. I want to point out that you could dispense with anæsthetics and hypnotise the animal in this way.

14073. Would there be any advantage in using hypnotism instead of anæsthetics?—Yes, because in the case of respiration the anæsthetic diminishes the respiratory exchange by one-half. Would you like me to do the experiment? There is not the least sign of pain.

(*Chairman.*) Personally, I do not desire it. There are three members of the medical profession present who understand those things better than I do. I would like to know what they think about it.

(*Sir William Collins.*) It is not new to me.

(*Dr. Wilson.*) No, we may accept the statement.

(*Dr. Gaskell.*) Certainly.

(*Witness.*) Animals do not appear to feel pain then even without an anæsthetic, and I can give evidence of that from some of the experiments that I did in Germany.

14074. (*Chairman.*) That was your experience?—That was my experience.

14075. Using hypnotism for the purpose?—Yes. Directly after the bleeding and transfusion the animal was able to run about. It was kept warm for about two hours, because there was a certain amount of shock.

14076. Did it lie still while you operated?—Yes, the animal was tied down as well to keep it in position.

14077. Did it struggle?—It only struggled when it became unconscious from asphyxia. That is another point to which I want to draw attention—that all the evidence in man, and I could do the experiment upon a man to prove it, shows that in the first stage of asphyxia, before struggling begins, there is loss of consciousness. If students experiment upon themselves by breathing in and out of a bag so that there is a lack of oxygen, they will often become quite unconscious, and tumble down before any struggling movement takes place, and one can actually anæsthetise a man simply by producing this stage of asphyxia—from the lack of oxygen. In Java anæsthesia, for example, if one presses upon the two carotids (*describing the action*) to prevent oxygen going to the brain, the man becomes unconscious. Asphyxia is not painful; it is preceded by a condition of anæsthesia from lack of oxygen. A dentist can produce a like effect simply by making a man breathe in and out of a bag in which the oxygen is gradually diminished. Although asphyxia is constantly mentioned as a painful condition, I can demonstrate upon any objector that it is not painful.

14078. What is the precise meaning of asphyxia? We all use the term. What do you say it is precisely?—The precise meaning of asphyxia, I think, is that it is the condition brought about by the lack of oxygen in the blood, and the accumulation of carbon dioxide.

14079. That is the scientific explanation, but what is the condition that is brought about?—The condition of unconsciousness and convulsions; that is to say, in popular language, unconsciousness and convulsive movements.

14080. (*Dr. Wilson.*) Garrotting, then, you would call not a painful experiment?—Not if it be done properly. You can make the experiment upon yourself, if you press upon your two carotids; there is no danger, because directly one becomes unconscious the pressure goes. If you press upon your two carotids so as to produce asphyxia of the brain, you become unconscious; then your arms are paralysed, and you save yourself by relaxing the pressure.

14081. (*Mr. Ram.*) Do you cause the brain to be gorged with blood?—No, the brain becomes in a condition of asphyxia from the combination which I gave just now—from lack of oxygen and accumulation of carbon-dioxide.

14082. (*Dr. Wilson.*) Like charcoal poisoning?—As regards lack of oxygen, yes, but there may not be an accumulation of carbon-dioxide then.

14083. (*Dr. Gaskell.*) Does not the Japanese ju-jitsu, as they call it, put a man to sleep?—I do not know. There is only one other point that I desire to draw

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attention to, and that is this: I think one ought to recognise that pain is not the supreme evil, as one of your witnesses maintained. Pain, I maintain, from the physiological point of view, is a protective mechanism, and the modern idea of trying to abolish all pain is absolutely absurd. It is simply, from this point of view, one of the conditions of every-day life. If we take the distribution of pain we see that pain often protects an animal from danger. There is a fractured limb. The pain brings about uncomfortable sensations, so that that limb is kept in a condition of rest, which is the most satisfactory condition for repair; or there is an injury to the abdominal cavity, when pain brings about reflex alterations of the abdominal viscera, so that the viscera do not move, and therefore puts the patient in the best condition for recovery. In the case of some abdominal wounds it was notorious in the South African War that the men who were left out all night, and left to themselves to suffer this pain, and given no anæsthetics, were the men who recovered afterwards with or without operations, while the men who were treated at once and given brandy or something to relieve the suffering, were the men who died. Pain, then, is a protective mechanism, and I maintain that we are becoming so over civilised that we run a danger in our attempts to abolish pain of doing an enormous amount of harm. Take, for example, the extent to which anæsthetics are now used in cases of midwifery, in ordinary cases of delivery.

14084. (*Chairman.*) Is this an argument to show that there is no occasion for anæsthetics?—This is an argument to show that a common sense view should be taken of this question, and that pain must be admitted. I admit that I have done painful experiments, and I am not ashamed of admitting it. They are absolutely necessary. I want to show that pain is part of the scheme of nature, and that we must recognise its existence.

14085. We know that pain and pleasure and everything else are part of the scheme of nature, but I do not know that we can have the Act amended with a view to acting on that principle. When a dentist pulls a large double tooth out, he causes a great deal of pain, but I do not know that I should call it protective. It does not protect me in any way. So that if you perform an experiment deliberately on an animal and leave out anæsthetics altogether, you perform an experiment on an animal which is a painful operation?—I say that in some cases pain is absolutely protective. I will give you an example.

14086. Supposing you proceed to perform an operation on an animal, it may be protective in one sense; it may be better for the animal—I do not know, I am sure—that it should suffer some pain if it has to have an operation. But you are not protecting it in inflicting pain upon it, you are inflicting pain upon it simply?—But the pain that it actually feels is protective.

14087. I do not think we need go into such fine points as that. Surely it is a very simple proposition that if you take a knife to an animal and cut it open you inflict pain upon it?—Yes.

14088. And if you inflict pain upon it the Act says that you must use anæsthetics?—Yes.

14089. How does your theory affect that? If everything that you say about pain being protective is true, what has it got to do with the case?—In this way. I think the Act is not really what one would expect from the point of view of common sense. If an animal is cut and there is a reflex painful effect produced which would act upon its heart, under ordinary conditions—for example, the animal has been injured by a dog and torn—some of these very mechanisms which we may shut out by an overdose of anæsthetics would be protective to the animal, if the animal was to be kept alive after the operation. Therefore I maintain that it would be wiser to allow these operations often to be done without anæsthetics.

14090. The Act only applies to experiments; it does not apply to a veterinary surgeon using the knife for the purpose of saving an animal's life. This is a question only of the infliction of pain?—I think we ought to be given a licence to cover all experiments. I think that the Act is entirely antagonistic to the advancement of physiology. If we were given a licence for all experiments there would be no more cruelty. There would be a great saving of time and no limitation of work, and there would actually be in the long run a saving of life.

14091. What do you mean by a licence for all experiments?—I mean without any conditions.

14092. With or without anæsthetics?—Yes, without any limitation at all, and without certificates.

14093. That is to say, you are to put yourselves in the condition in this country which I understand physiologists are in in some parts of Germany, where there is no limit?—I think there should be no limit; that is to say, that a recognised physiologist should be given a licence to cover all experiments.

14094. Without anæsthetics?—Without anæsthetics or with anæsthetics and without certificates.

14095. And without limitations as to length?—Without reference to any conditions at all, but that he should, of course, be subject to inspection. I would point out that the difficulty of the present situation is this: I live in the country, and I could actually reckon myself as a farmer, seeing that I have more than three acres, and I do have to give a return to the Board of Agriculture. As a farmer, if I had lambs I could castrate those lambs without any anæsthetics; it would be absurd to give them anæsthetics. I have seen castration done over and over again. At Guy's I cannot castrate one lamb without a special licence, and if I tried to get a licence to castrate a lamb without giving anæsthetics it would probably be refused. My condition, then, is this: that as a private individual, if I liked to tax my conscience, I could make experiments in the country, and I could publish those experiments as a private individual, not as experiments, but simply as observations, which I could not do in my own physiological laboratory.

14096. I think you would find it rather difficult to do it if you published it, without bringing a great deal of odium upon yourself?—If I did it simply from a commercial point of view, I believe that legally I could not be touched. And then, of course, as regards pain, one causes very much more pain in the country, even unconsciously, than one does in a physiological laboratory. The worst pain that I ever caused I am convinced—I could bring evidence in support of it, but I am quite convinced—was in a case where I took away a calf from a cow. The cow was exceedingly fond of the calf; the calf was too big, and I wanted the milk for my children, so I took away the calf and sold it. For two days that cow was in obvious pain; it would not take its food; it went about bellowing, roaring, and moaning, and was so bad that the cottagers complained. There is a case in which it was perfectly obvious to me that the animal was suffering pain for two days. Never in any physiological experiment have I caused so much pain, even under the Act, as one does every day in the country.

14097. The Act deals with physical pain?—I cannot see the difference between physical and mental pain from a physiological point of view.

14098. There are a great many cases which come on the border line in everything, but I think there are a vast number of cases in which you would say that the pain is mental pain, and not physical?—I do not think that physiology can really separate the two.

14099. I am not using any scientific language. I am using the language of every day conversation. In a certain sense, of course, you may trace mental pain to certain physical causes?—I quite see what you mean; it is that mental pain is the worse form of pain.

14100. There is a great difference between what we call a broken heart and a broken leg?—Yes.

14101. You seem to think they are the same thing?—No, I do not admit that, if it is taken down in evidence.

14102. (*Sir William Collins.*) Is the mental pain protective, too?—I believe that it is protective, because it is followed by reaction.

14103. (*Mr. Ram.*) Would the pain caused by an operation for purposes of experiment be protective; is the cutting of an animal's skin protective to the animal?—If you take the condition in which you could see the animal's reacting mechanism it would be protective. What I want to show is that the protective mechanism must be there for meeting the contingencies which will probably arise.

14104. (*Chairman.*) What I put to you is this: If you inflict a pain upon an animal, and you want to do an experiment upon it, you do not do it to protect the animal against anything, the pain that you cause is incidental to your making the experiment?—Taking into account all the conditions which you mention, the reactions are not actually protective, but in ordinary conditions, if an animal were to get such a cut as that, and it were kept alive, the reflex reactions would then be protective.

14105. But the pain that you would inflict would protect no animal?—In that individual case it would not be protective unless the pain was enough to produce syncope.

14106. It would be protective against something that it would not suffer unless you caused it?—But I say that even if I caused it, it might produce syncope, so that the animal would not suffer pain. There is only one other point which I think ought to be recognised. One of the reasons why vivisection is objected to is entirely from the theological point of view, and that comes out in the first chapter in this book, "The Shambles of Science." It is believed that physiologists are rank materialists. I for one absolutely deny it. The chief arguments in favour of vitalism, the modern arguments in favour of neo-vitalism, have come from physiology. The opposition from the clergy to vivisection largely comes from the fact that they believe that all physiologists are materialists. There has always been, so far as I know, a division in physiological thought—there have always been two schools of physiology, the materialistic school and the vitalistic school. The vitalistic school is increasing in force, and it is quite unfair for the writers of "The Shambles of Science" to maintain that physiologists are materialists. My experience as a teacher is that the students often come from the study of physics and chemistry, from their ordinary studies, and from their reading of the ordinary literature of the day, to start physiology with a materialistic bias, and, judging from their papers in our students' society, the study of physiology is the best corrective of these materialistic views. I should say that many of them change their views from the materialistic side to the vitalistic side owing to the study of physiology. I insist again that the best support of the vitalistic position at the present day comes from physiology.

14107. (*Dr. Wilson.*) The vitalistic side is the agnostic side rather?—A vitalist might be an agnostic. It depends upon the sense in which you use the term agnostic, because the vitalist has a definite position. A vitalist of course believes that there are conditions which cannot be explained by the ordinary terms of physics and chemistry, and is not agnostic therefore in that sense.

14108. (*Sir William Collins.*) How long have you held a vivisection licence?—Fifteen years. I was demonstrator first at Oxford in 1892.

14109. What certificates, if any, do you hold?—I have got a good many. I do not remember what they are now. I could look it up.

14110. Do you hold a certificate to permit you to dispense with anæsthetics?—I do not know exactly what certificates I have, because I have not been doing any vivisection experiments this year.

14111. You are, I think, joint lecturer on physiology?—No, I am entire lecturer; there is no joint lecturer.

14112. I took it from the title page of the book you handed in?—My late colleague is dead, and I am now full lecturer.

14113. As lecturer on physiology are you one of the persons authorised to sign certificates?—No, I cannot do so.

14114. Have you anything to do with supporting applications for licences?—No, I have not.

14115. I understood you to say in the course of your evidence that the Vivisection Act was entirely opposed to the advancement of physiology?—Yes.

14116. Is that opinion held by others engaged in physiological research besides yourself?—Yes, I think so. I have already given evidence as to experiments which I could not do in this country which I could do in Germany. The medical members of the Commission will recognise the importance of understanding the effects of bleeding and transfusion.

14117. I think I gathered your point with regard to the experiments in Germany?—It was that we were able to show that after the loss of half the quantity of the red blood corpuscles we could revive an animal and render it perfectly fit.

14118. Without anæsthetics?—Yes.

14119. Did I rightly gather that in your opinion anæsthetics are unnecessary?—Yes, I am convinced that they are often unnecessary, not only in the case of animals, but of men.

14120. For vivisection and for surgical operations?—For vivisection and for surgical operations; and I mentioned the case of midwifery.

14121. Would it be going too far if I were to express your opinion as being that pain is a good thing in itself?—I say that pain is a beneficent and protective mechanism. I have written that in an article which I have recently published, and I am prepared to defend it. It is not a new view at all; it is recognised by surgeons, I believe, that a certain amount of pain is a protective mechanism.

14122. There was a good deal of argument of that kind, was there not, at the time of the introduction of anæsthetics?—Yes, a large amount. When we came to the question of midwifery and the use of anæsthetics, there is not the least doubt that unborn children are injured by the dose of chloroform that is taken in. It may not be a large enough dose to poison the unborn child, but it will interfere with the circulation through the mother's uterus and thereby injure the prospects of the child. The amount of pain that a woman feels in childbirth is protective because the pain will actually inhibit a movement; the pain in parturition will actually produce a condition in the mother which will prevent the too rapid or too slow passage of the child.

14123. I want to be quite clear that I took down your words correctly. I think you said that to abolish pain is absolutely absurd?—To abolish all pain in the condition in which we are at present would be absolutely absurd. The condition would be this: that if we met with an accident we should straight away go from bad to worse without any natural protective mechanism.

14124. Just one word about the marmot. I see that it was implied in one or two of the questions put to Miss Hageby that a post-mortem examination had been made of this marmot. Was that so?—That is Professor Thane's evidence. It depends upon your interpretation of a post-mortem examination. Post mortem means after death.

14125. Did Dr. Thane make any further *post-mortem* examination than that which honourable members have made to-day?—Yes, it was further, for the reason that the animal was much fresher then; it had not long been preserved.

14126. Was there any *sectio cadaveris*?—No, but it is absolutely correct to say that there was a *post-mortem* examination. There was a *post-mortem* examination in the strict sense—that is to say, the body was examined after death and under much more favourable conditions, because it was fresh, but there was no cut made.

14127. You mean that there was a *post-mortem* examination in the same way that we have made a *post-mortem* examination to-day?—Yes, but I do not think that was a thorough *post-mortem* examination to-day. I wanted you to let me cut through the skin and demonstrate to you and make you admit that its spinal cord has not been cut.

14128. I understood you to say that you drew a distinction between a brainless frog and a pithed frog?—Yes.

14129. You say that the law makes it necessary that the cerebral hemispheres should be destroyed?—Yes.

14130. I do not remember it in the Act?—Because the experiment must not be one calculated to cause pain. All the evidence shows that the pain is connected with the cerebrum. I can give examples of that.

14131. I only wanted to know whether there is any definition of pithing laid down in the Act?—So far as I remember I do not know of any.

14132. Would there be a difference in the physiological manifestations visible in the case of a frog which had only been deprived of the cerebral hemispheres compared with one which had only the spinal cord left intact?—Yes.

14133. Should I be right in thinking that in the former case purposive movements would be present as they would not in the latter, to the same amount?—No, not of the same order; of a different order; but they would be present in both cases.

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14134. Should I be right in thinking that the higher order of purposive manifestations would be present in the case where the cerebral hemispheres alone have been destroyed?—Yes.

14135. And you compared the condition of a frog to that of a brainless monster?—Yes.

14136. Do you think that purposive movements even of a higher character do not imply possession of consciousness?—I say not so far as one can judge. Shall I give you the reason? We can only judge of consciousness from ourselves philosophically—that is the first point. Then we must go to other people. If you take the case of a man with a section of the spinal cord (I have made the observation several times) that man with a fractured spinal cord will perform purposive movements—he will draw up the leg if tickled; but if he is asked whether he feels the tickling he says, No. He knows nothing about it unless he sees the movements.

14137. Do you not draw a distinction between reflex actions and purposive movements?—These movements are all purposive?—Further, I have seen the case of a woman, who was pregnant at the time, with a fractured spinal cord in the dorsal region. She was brought into the hospital in a condition of paralysis. The physicians and surgeons in charge of the case were doubtful whether abortion should be produced in the interests of the woman. They consulted Dr. Mott and myself from the physiological point of view, and we recalled at once the physiological experiments done by Goltz in Germany, which showed that pregnant animals with a section of the spinal cord could go quite safely to full term. This woman went to full term, and—this is what I want especially to draw attention to on the question of consciousness—all the complicated movements of pregnancy, all the movements in connection with the alteration of respiration, straining movements, and contraction of the uterus went on, and the woman was absolutely unconscious of the fact that she was in process of delivery; although she knew the condition from previous experience, being the mother of several children, she felt no pain at all. That child was born, and until the mother was shown the child and told she had been in labour she did not know that it had taken place. I say then that the common-sense view to take of the matter is this—we cannot be absolutely certain about the question of consciousness except in our own case. In the next case ? we must take what is told us by other people as regards consciousness. They may or may not tell us the truth. We can only argue from our own consciousness and from what other people who have a fracture of the spinal cord tell us about their condition of consciousness, whether an animal with its spinal cord only has any consciousness. That is a perfectly fair, straightforward statement. That is the only way. I say that I do not deny that there may be consciousness; I believe myself, for example, that philosophically one should believe in the germ of consciousness in the spermatozoon and in the ovum, and there is no reason to suppose that consciousness comes in at any special time during pregnancy. All life, I maintain, must possess the germ of consciousness. I am quite willing to be perfectly straight on that point, but judging by people with a section of the spinal cord—with a fracture of the spinal cord—they do not feel in the paralysed part, and yet they perform the most complicated purposive movements, such as contraction of the bladder, contraction of the uterus, contraction of the rectum, purposive obviously to get rid of the urine from the bladder, to get rid of the full time child from the uterus, and to get rid of fæces from the rectum.

14138. But all involuntary muscular fibre?—No, certainly not. If you tickle the feet, for instance, they are drawn up.

14139. Those that you referred to are involuntary muscular fibre?—I gave you the most complicated because I thought they were the more important. If you tickle one foot it is drawn up. And those that I referred to are not entirely involuntary muscular fibre, because in the case of parturition there is a large amount of accessory pressure brought about by contraction of the abdominal muscles.

14140. I was referring to your quotation of the uterus, the bladder, and the bowel. Those would be involuntary muscular fibre?—Yes.

14141. Can a brainless frog swim?—With the cerebrum alone removed.

14142. Can a man with the spinal cord divided high up?—The only case that I can draw attention to is this, and the case bears on the question of brainless animals. During the construction of the Forth Bridge a workman fell down, and, falling into the Forth, he struck against a great many planks of timber. There was a boat kept there, and the man was picked up and conveyed to the Edinburgh Royal Infirmary. The man was still breathing at the time, and breathed for some time after, and he showed reflex movements. When a *post-mortem* was made after the death of that man it was found that the entire cerebrum was completely destroyed; the skin had simply kept together the fractured skull; the brain was destroyed, but the lower parts, such as the medulla, were intact, and so he had been able to breathe and perform these reflex actions in the same way that a brainless monster will suckle and cry and can do purposive movements and can breathe perfectly well. That man showed no sign of consciousness.

14143. Can a frog with the spinal cord only left swim?—I have not seen one swim.

14144. (*Mr. Ram.*) With regard to pain I want to make your position clear. I understand you to say that pain is very often protective?—Yes, I believe that pain is very often protective.

14145. That means this, does it not—that when a man or an animal suffers a lesion, the pain produced by that lesion is protective?—Yes.

14146. But if there is no lesion, if you take the case of an animal that is perfectly sound and well, do you justify the infliction of unnecessary pain upon that animal?—You want me to assent to the "unnecessary"?

14147. I want you to answer my question?—I consider that it is perfectly right to inflict pain upon animals.

14148. Unnecessarily?—Not unnecessarily—no.

14149. Do you think it is right to inflict pain upon a healthy animal—I mean when there is no question of protecting mechanism—when you can discover all that you want to discover if that animal is under anæsthetics?—What I should say to that is exactly this. I will be perfectly straightforward. I say that you should not inflict pain if you can obtain the knowledge in any other way; but I say that even where there is an operation, the pain there is of a protective nature; it may produce syncope, and therefore less sensation of pain. Further, I say that the introduction of an anæsthetic may produce complications, so that more experiments may be necessary than if the experiment were done on the animal in a condition of hypnotism, which I offered to show to the Commissioners. That I think is one very important point. These animals pass into a condition, so far as one can see, comparable to hypnotism. If you give them anæsthetics you are introducing a complication which you could remove, and therefore without anæsthetics you actually save life and actually diminish the infliction of pain.

14150. (*Dr. Gaskell.*) Do I rightly gather that you would advocate the abolition of the Vivisection Act altogether?—No, I would not do that. I think only people interested and people who are competent should be allowed to make vivisection experiments—that is to say, there should be a certain limitation that these people have the requisite knowledge, and that no ordinary man without the requisite knowledge should be allowed to cut up an animal or to carry on such experimental work. I think that a licence should be given which covers all experiments, those with and those without anæsthetics, painful or otherwise.

14151. Do I rightly understand you to mean that you would diminish the number of people at present holding licences?—No, I would simply leave it for the Home Office to decide by means of special advisers—such, for example, as the Association for the Advancement of Medicine by Research—whether certain applicants for a full licence are competent people for carrying out experiments in physiology or pathology. There would be no limitation of numbers. The limitation would depend only upon the capacity to carry out the work.

14152. You would not consider that some of the points which you have put before us might be solved by introducing the term hypnotism as one of the anæsthetics under the Act?—Yes, I think that ought to be done. Further, I think that hibernation should also be introduced, because it is known, for instance, that

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a hibernating mammal passes into a condition of anæsthesia by natural causes, and I think that all natural conditions of anæsthesia should be introduced in the Act. I am told—I am only speaking from memory—that in Denmark it is lawful to vivisect an animal during the winter when it is in a condition of hibernation, but it is not lawful to vivisect it in summer when it is not in a condition of hibernation. Hibernation is a natural condition of anæsthesia, and we ought to be allowed to avail ourselves of that natural condition of anæsthesia.

14153. (*Chairman.*) If you could satisfy the Home Office that an operation on a hibernating animal did not inflict pain, would you require a licence?—It is left to me to decide what operation is calculated to cause pain, but the difficulty that I find is that I may consider that it does not cause pain, and then the Inspector, who, with all respect, does not know anything about hibernating animals, may say that he does not believe that to be the case.

14154. Exactly. If the Home Office were satisfied that it would not inflict pain upon the animal, you would not require a licence?—No; but the Home Office throws the obligation upon me, and it is very difficult to simply convince them.

14155. Very?—But I am willing to show the Commissioners during this winter hibernating animals and to show them that they do not feel pain from the cut of a knife.

14156. (*Mr. Tomkinson.*) Did I rightly understand you to contend that pain of itself is not only protective but a blessing, and that all anæsthetics are a mistake?—No, I do not go so far as that. I say that pain in general, if one takes a wide view of biological processes, is a protective mechanism, and it is in that sense beneficent.

14157. But does not that extend only to this, that if one puts one's hands by mistake into the fire or into boiling water, thereby violating a law of nature, one is punished for it, and pain is naturally beneficent and protective in order to teach us how to obey the laws of nature and not to subject ourselves to danger in future? Surely it goes no further than that?—Yes, I think it goes further than that. How otherwise do you explain the pain that we feel with a fractured leg and the pain goes on and the pain is still protective after the fracture?

14158. I confess that I cannot see the analogy between that and the infliction of gratuitous pain upon an animal?—I gave you an example of the infliction of gratuitous pain upon an animal without anæsthetics which produces a condition of shock, so that there is less circulation of the blood and therefore the animal's brain is not so active and it does not feel pain so much, and therefore to that animal upon which you inflict pain, that pain is actually protective in the example which I have taken.

14159. Now I want to ask you a personal question?—I am quite willing.

14160. If you had to undergo a very severe operation yourself, say the amputation of the thigh or some very severe skin lesion, do I rightly understand that you would think anæsthetics a mistake?—I should consider the question of what would be the effect of the operation and the effect of the anæsthetic. For a small operation I should have no anæsthetic. I have had my lip sewn up without anæsthetics, and the pain is much less than the trouble caused by the anæsthetic. I have seen a man come into the theatre and sit down in the chair and without moving a muscle have a cancer cut out of the lip and have the lip sewn up and march out. If you give an anæsthetic you forget that one of the effects of an anæsthetic is pain. If you go round the wards after anæsthetics have been given, you see patients vomiting from the effect of ether. Is not vomiting painful? I am certain that in many cases in dentistry also, people suffer considerable pain afterwards from the anæsthetic used. In the case of an animal or man if the pain to be inflicted is of a trival kind an anæsthetic is a greater discomfort than the slight infliction of pain. That is my candid view.

14161. Surely you do not contend that the horrors of the cockpit on board a vessel in action and of field hospitals have not been marvellously mitigated by the blessed discovery of chloroform and other anæsthetics?—But these things also bring disadvantages. There is no doubt that the excessive use of anæsthetics is a modern danger. In proof of that is the death roll from anæsthetics. I have seen cases of people dying—of children dying from an anæsthetic given for simple operations like circumcision. I have circumcised children without anæsthetics. If they cry it does not matter; it is much better that a child should cry and moan than that its life should be lost. That circumcision is not such a serious operation is shown by the fact that the ordinary medical man does circumcision under anæsthetics and the Jewish non-medical man does it without anæsthetics. There is an exact case in point in which the introduction of anæsthetics has done a considerable amount of harm. Take also the case that I gave you of midwifery. The pain that a woman suffers in childbirth is protective. Extend the analogy of the use of anæsthesia and every woman in childbirth ought to be given an anæsthetic. What would be the result? You would make the nation more degenerate, if possible, than it is at present. And the very idea that women require anæsthetics during childbirth, I am convinced, has done a good deal towards bringing about the decrease in the birth rate—that is to say, women will rather take preventive measures or produce abortion, will rather be immoral, than suffer a certain amount of pain. It is really owing to the nonsense talked about destroying the bad effects of a small amount of pain. A small amount of pain is part of the scheme of nature, and should be so recognised.

14162. One question more on another point. You mentioned having performed and seen performed the operation of castration. A good deal has been made of that from certain quarters before this Commission. You have seen it performed on sheep?—In hundreds of cases or more.

14163. And on pigs?—Yes.

14164. It is very momentary?—No; it is a short operation if it is done efficiently, but the man is an ordinary farmer in most cases. I will tell you the way it is done if you like.

14165. The effect upon the animal is apparently very slight?—I will not admit that. The lamb is taken by its four legs.

14166. I do not want a description of it?—But I cannot agree to your view that it is momentary pain. It is not. The lamb is taken by the four legs by one man. Then the farmer—this is exactly the way I have seen it done—seizes the scrotum, slits it with a penknife on one side, seizes the testicle, which is a sensitive part, with his teeth, which is quite a good method, pulls on it, and cuts it off with the knife. The operation is then repeated on the other side, the other testicle is removed and the lamb is put down. You see at once the condition of depression. The lamb, which was jumping and frisking about before the operation, stands with its tail between its legs in a marked condition of depression. That depression—I say nothing against it—is protective; it will keep that lamb quiet and that lamb's scrotum will quickly heal up, I grant you. But there is the depression; it is not only for a moment that it feels pain.

14167. But in the case of bulls, certainly, and of horses, it is a necessary operation, we know, for domestic purposes?—Why?

14168. Because they could not be kept?—Why? I can understand that it is advisable, but I do not see why it is absolutely necessary.

14169. Entire horses could not be used for draught purposes in towns and other places; they would not be amenable to discipline.

14170. (*Dr. Wilson.*) I think you said that you had not performed any vivisection—I mean cutting experiments—this year?—Not so far as I remember.*

14171. But you are a lecturer on physiology?—Yes; but I have done the experiments that I have mentioned with carbon monoxide.

14172. What I want to get at is this, that you did not consider it necessary this year, at all events, to give experimental illustrations of your lectures?—Not experiments under the Act—that is so. So far as I know I have done no experiments under the Act.*

14173. You have done no experiments on dogs?—I can state on that point that I do no experiments on dogs at all.

* *The witness subsequently wrote that after reference to his lecture note-book he found that he had performed three experiments.*

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14174. Yet Guy's Hospital is a large school of medicine?—Yes, I suppose I have about 70 or 80 fresh men every year, and of course those men do physiology for two years, so I may have practical classes of 75 men.

14175. You do not consider it necessary to use a dog for experimental illustrative purposes?—I have not used a dog at Guy's for illustration of experiments at all. I use rabbits, rats, and guinea-pigs. I am very fond of dogs, and if I want to vivisect, and can get a rabbit, rat, or a guinea-pig I do it in preference to vivisecting a dog.

14176. But even those experiments you dispense with. You can teach physiology thoroughly without?—I can teach physiology without experiments on dogs; but there are certain experiments of course which can only be done upon dogs.

14177. But you do not consider them necessary for teaching physiological courses?—In my case I do not use dogs for illustration of lectures.

14178. Then with regard to feeding experiments, do not you think that more knowledge would be gained by experimenting on human beings?—Yes, experiments have been done on fasting men. But the point that I want to bring out is that the reason why one takes hibernating animals is that in their case the deposition of fat is brought up to a point which is found in no other animal.

14179. But men do not hibernate?—No, but the deposition of fat takes place when the animals are not hibernating, so that from the point of view of the condition in men it is really useful to do this. The only other way of experimenting, which has been done, is to use fattened geese.

14180. I threw out that hint, because I was reading the other day that Professor Wiley has instituted or started what his students call a poison squad; that is to say, a certain number of students volunteer for feeding experiments and experiments with drugs?—I think that to safeguard the students those experiments should be preceded by experiments upon animals. Some of the drugs might produce poisonous effects upon the students. I quite agree that those feeding experiments should be done upon man, but in this case the animal may almost double its weight. The process therefore is accentuated, and one can study it better on the marmot, and of course during the deposition of fat the animal is not actually hibernating.

14181. You believe that even severe experiments could be carried out on hibernating animals during hibernation without causing pain?—Yes, when it was in the protective condition of torpor. It has actually been done.

14182. Can you not wake up an animal out of the hibernating sleep?—Yes, if the experiment or the actual performance of the operation took too long, the animal might wake, but the process of waking is a fairly long process, and the experiment should be timed to be over before the animal is fully awake.

14183. But how could you tell?—By previous control experiments. I could give you figures as to the time it takes.

THIRTY-SECOND DAY.

Tuesday, 5th November 1907.

MEMBERS PRESENT:

The Right Hon. the Viscount SELBY (*Chairman*).

Sir W. J. COLLINS, M.P., M.D., F.R.C.S.	Mr. A. J. RAM K.C.
Sir J. MCFADYEAN, M.B.	Mr. W. H. GASKELL, M.D., F.R.S.
Sir M. D. CHALMERS, K.C.B., C.S.I.	Mr. G. WILSON, LL.D., M.D.
	Captain C. BIGHAM, C.M.G. (*Secretary*).

Colonel DAVID BRUCE, C.B., F.R.S., R.A.M.C., called in; and Examined.

Colonel D. Bruce, C.B., F.R.S., R.A.M.C.

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14184. (*Chairman.*) You are a Colonel in the Royal Army Medical Corps?—Yes.

14185. And you are a Fellow of the Royal Society?—Yes.

14186. A member of the Advisory Board, Army Medical Service?—Yes.

14187. And you have acted on the Sleeping Sickness Commission of the Royal Society in 1903?—Yes.

14188. And you were Chairman of the Mediterranean Fever Committee of the Royal Society in 1904-6?—Yes.

14189. For a good many years past, I believe, you have been accustomed to perform experiments on animals in the study of disease?—Yes.

14190. Would you tell us, generally, the occasions and places at which you have been so employed?—After I came into the service, I was stationed at Malta for five years, from 1884 to 1889, during which time I worked at several medical subjects, among them, the principal one, from a medical point of view, being Malta fever. Then in 1889 I went to Netley Army Medical School and remained there five years as an assistant professor of pathology.

14191. And did that involve the study of bacteriology?—Yes; I was bacteriologist there.

14192. And animal experiments?—And animal experiments. Then in 1894 I went to South Africa, and soon afterwards went up to Zululand, where I remained for some two years, working at a disease called nagana, or the tsetse fly disease, which was causing a great deal of destruction of cattle up in Zululand, and giving the Government a good deal of anxiety.

14193. It was not at that time ascertained to be identical with the tsetse fly disease, I think?—No. Then in 1897 I returned to Natal and shortly afterwards began working at a very serious disease of animals called South African horse sickness, and after that the war began and I was put on a commission to study dysentery and enteric fever in soldiers, especially in the field, and there, of course, there was a slight amount of animal experimentation, but very little, as we were up in Pretoria, surrounded by the enemy, without any books and without any animals; the only animal we could get to use would have been the horse, and that is too large, of course, for ordinary subjects.

14194. Then you were not engaged in experiments on animals during the time you were in the field?—No, on account of our being in a beleaguered town without any opportunity. But, in my opinion, you could not work at dysentery or enteric fever with any chance of success without animal experimentation. It was only want of animals that prevented our working in that way at that time. Then I came to England in 1902, and in 1903 I went to Uganda to study sleeping sickness. I remained there some seven months, and on coming back to England in 1904 I became chairman of the Mediterranean Fever Com-

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mission for the study of Malta fever. At the present time I have a licence to perform experiments on animals at the Royal Army Medical College, but at present I have not much opportunity of doing research work on account of other duties.

14195. You have had very large experience, at any rate, spread over a good many years in animal experiments?—Yes.

14196. And you have had an opportunity of forming an opinion as to the value of them in advancing medical knowledge?—I have.

14197. What is your opinion, generally, on the subject?—My opinion is that it is the most valuable method we have of advancing medical knowledge, and without it we could hardly advance a single step safely. These questions are so complicated, and one is so apt to fall into error, that unless each step is checked by animal experimentation one soon goes wrong.

14198. And have you observed during your twenty years' experience advances made in knowledge by means of experiments on animals?—Yes, certainly.

14199. And in the method of dealing with disease as well as the cause of it. I mean with regard to the serum treatment more especially?—Yes, I think I can say that.

14200. Have your experiments been more directed to the cause of disease?—Yes, my work has been nearly always on the causes of disease with a view to their prevention on broad lines rather than on drug or serum lines.

14201. You are going to tell us, I understand, what your experience is with reference to the causes of disease in some special cases of disease that you have dealt with?—If you please. I have worked at many different things, of course, but I have especially worked at three diseases, namely Malta fever, nagana, and sleeping sickness. I think in Malta fever you have a good example of the result of experimental work in elucidating the natural history of the disease and finding out how the infection is carried from one animal to another. The result of this work has been very striking; that is to say, we have had in the garrison at Malta an average of 700 men down with this fever, for, on an average, 120 days each up to last year. This year to the end of August there has not been a single case in the Navy, and only six cases in the Army, a reduction from 700 down to six; and during the two worst months of this year, July and August, there has not been a single case in the Army or in the Navy in Malta. So that there is yearly saved to the State the services of almost a whole regiment of soldiers, and 700 men from remaining in hospital 120 days with a painful disease, a disease which very often shatters the health for years and sometimes for a lifetime.

14202. Will you describe what the nature of the disease is as regards the pain and the suffering of it, and the length of time it lasts disabling a man?—It is a disease which is characterised by its extremely long duration; that is to say, the men remain, on an average, some 91 days in hospital in Malta, and as most of them are invalided to England and spend another two months or so in hospital here, you can put down the average duration of this disease at the extraordi narily long time of four months, or 120 days. Of course, it lasts much longer in some cases.

14203. You mean before they can be discharged from hospital?—Yes, before they can be discharged from hospital. Many are invalided from the service altogether, and a certain percentage of them die. Some cases of this fever last as long as three years, rendering the man absolutely incapable of work of any kind during this time; and two years is by no means an uncommon duration. In fact, it is a disease of very long duration; it is characterised by extreme weakness; that is to say, a man is brought down to the very edge of the grave without absolutely dying; he is extremely emaciated; he suffers from severe pains in the joints and nerves, and altogether we look upon it as a more severe disease than even enteric fever. Although the mortality in the case of Malta fever is not nearly so large as in enteric, on account, probably, of the different anatomical regions that the two diseases attack, enteric fever attacking the intestine and ulcerating it, very often giving rise to death by perforation of the intestine, whereas in Malta fever there is no ulceration of the intestine, and therefore that is one reason, no doubt, why the mortality is less.

14204. Is it widespread as well as serious?—Yes.

14205. You speak of it as Malta fever. Is it confined to Malta?—No, it is found all over the world. It was first studied in Malta, hence the name. The Americans found it in the Philippines lately, and it has also been found to be very widely spread in South Africa, in the Orange River Colony, and in Kimberley and other parts of Cape Colony. It is also found in Egypt, and generally round the shores of the Mediterranean, and it has also been found in Arabia, India, China, the Fiji Islands, North and South America, and the West Indies, so that you may say it is a world widespread disease in the sub-tropical and tropical regions of the earth.

14206. And what is the particular micrococcus that you find causes it?—The parasite which causes it has been called the *micrococcus melitensis*. It is a minute bacterium, which, if you magnify it a thousand times, is just visible to the eye, and it is a parasite which can only live in the warm bodies of animals, on account of its requiring a high temperature for multiplication. It multiplies very little, or not at all, at ordinary temperatures, but when kept at the temperature of the blood it multiplies fairly rapidly, so that you may call it a parasite that depends upon its parasitic existence for its own existence. It cannot live outside the body and multiply there. It can only live in a dry condition, retaining sufficient vitality to set up again its activity if it again reaches the blood of a warm-blooded animal.

14207. It is called Malta fever because it was first found there or studied there?—It was called Maltese fever because it was first studied there. It was studied by Army medical doctors who go there on service, and at that time it was thought to be fairly well restricted to Malta, so that naturally, being one of these local diseases, or at least, thought to be local at that time, it received a local name. It has other names, for example, rock fever in Gibraltar.

14208. (*Sir Mackenzie Chalmers.*) Is that the same fever?—Absolutely the same fever.

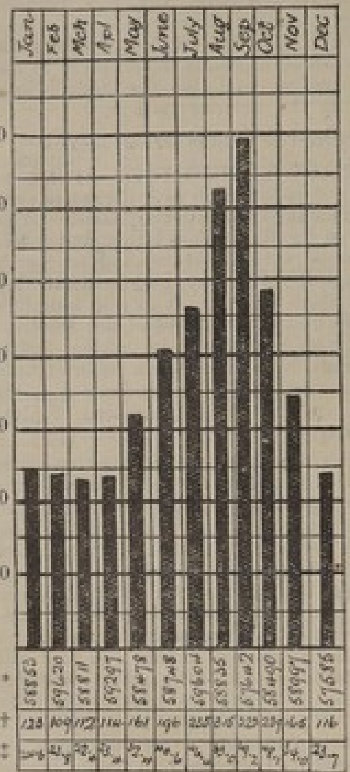
14209. (*Chairman.*) And in these other places it is not always known as Malta fever?—No; in Kimberley it is known as camp fever, and every place gives it its own name. Now, of course, it has been pretty well worked out, and everyone knows of it, and at present it is usually called Mediterranean fever. In the nomenclature of diseases it is known as Mediterranean fever, with the synonym of Malta fever. The change from Malta fever to Mediterranean fever was a little politeness to the Maltese people; but I think it is better to keep to the term Malta fever until they do something to clear their island of it. That is to say, we have suffered a great deal in the past from Malta fever, and therefore I always encourage the use of the term "Malta fever" in order to drive the Maltese to try to remove the stigma from their island. After they have purified their herds, then we will see about renaming this fever. The Italians call it the septicæmia of Bruce; so we might call it by this name, as I have no objection to have my name associated with a disease, after they have purified their herds.

14210. You have a table, I believe, that you can hand in showing the amount of Malta fever in the troops at Malta at different times in the year?—Yes.

MONTHLY PREVALENCE—RATIO PER 1,000 OF STRENGTH EXPRESSED IN TERMS OF AN ANNUAL RATIO.*

CHART I

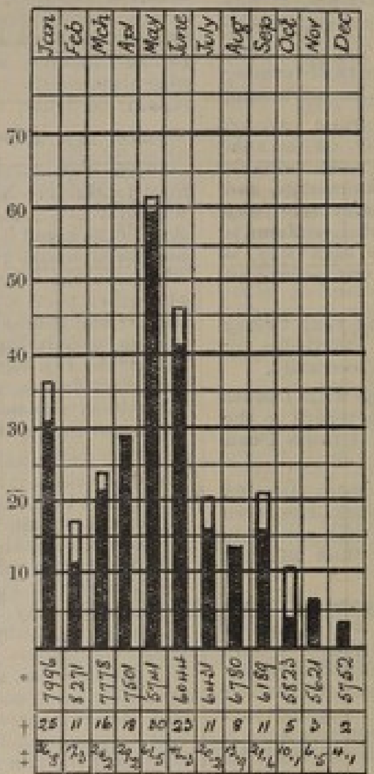
1899 TO 1905.



* Aggregate strength for 7 years.
† Admissions.
‡ Ratio per 1,000 of strength.

CHART II.

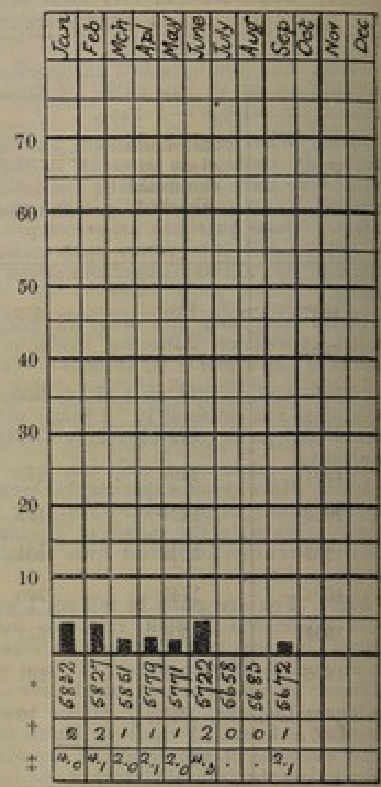
1906.



▯ Readmissions shown thus.
* Average strength.
† Admissions.
‡ Ratio per 1,000.

CHART III.

1907.



* Average strength.
† Admissions.
‡ Ratio per 1,000.

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14211. That is from 1899 to 1905?—Yes, that is the chart made up from those years.

14212. It is very high in August and September?—Yes.

14213. And less just in the months each side of them and less still in the more distant months?—Yes, but at the same time there is not much to be said in regard to seasonal prevalence. When you make a chart of this sort out of a number of years you get a fixed chart, and naturally in the hottest months of the year, when people are lowered in health and when they probably take more of the infective agent in, there would be more cases; but sometimes epidemics arise in February or March, or December in some years, changing the aspect of this chart and showing that seasonal prevalence has very little to do with the disease. This chart shows that on an average there are some 400 cases of this fever among the soldiers in Malta; and at another part of this paper I give the number of cases for the Navy, and it comes to very much the same.

14214. I think you made it 37.6 per cent. per 1,000 men in garrison?—Yes.

14215. There are about 8,000 men in garrison?—Yes.

14216. Then there are three other tables which perhaps you will put in also?—Yes; the first table is the incidence of continued fevers (including Malta fever, simple continued fever, and enteric fever), among the troops in Malta from 1897 to 1906. That is put in to show that a number of cases that are returned under the name of simple continued fever are probably cases of Malta fever, and when Malta fever disappears a number of these simple continued fevers also disappear. Then in regard to Table 2 the incidence of Malta fever among officers, women and children in Malta, you find that the incidence among the officers is about four times as great as among the men. We discovered the reason for this when the cause of the mode of infection of Malta fever was discovered to be due to the milk of goats. Naturally, the officers drank more milk than the men.

14217. You mean that they were in the habit of drinking more milk than the men?—Yes.

14218. Therefore, when you discovered that, that explained the greater incidence among officers?—Yes, we could not understand before that, seeing that the officers live in good quarters and have good food and are not exposed to the same risks—that is to say, they do not go among the lower classes as much as the private does—why they, their wives and families, were much more struck by this fever than the private soldiers; but the moment we found out that this fever was due to the milk of goats, then the habits of the officers and their families in the way of drinking much more raw milk than the soldiers explained it at once.

14219. In Malta, I understand, the milk used is the milk of goats generally, just as it is the milk of cows here?—Yes, there are very few cows in Malta; even if you ask for cows' milk, and the contractor says it is cows' milk, in all probability it is goats' milk. The goat is the animal that supplies milk in Malta.

14220. Then the other table is to the same effect, showing the number of officers, non-commissioned officers and men invalided from Malta?—Yes. In the Army we were not in the habit of invaliding the men as much as in the Navy. In the Navy it was made a rule that if a man had Malta fever the moment he was able to be invalided to England he went. Of late years, for example, in 1905, there were 21 officers and 382 non-commissioned officers and men invalided from Malta, so that the habit of this invaliding was getting more marked in the Army than it had been. It was found that the men remained debilitated for such a long time that it was better to invalid them home to England. That, of course, means a great deal of expense to the State, providing passages to England, and treating the men in hospital in this country for some two months or more.

* *These charts were afterwards substituted by the witness in place of the one referred to during the evidence, in which the figures did not go beyond 1905.*

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The following are the Tables handed in by the Witness.

TABLE I.

INCIDENCE of continued Fevers, including Malta Fever, simple continued Fever and Enteric Fever, among the Troops in Malta, from 1897 to 1907.

Year.	Total admissions for continued Fevers.*	Total Admissions for Malta Fever.	Simple continued Fever.	Enteric Fever.	Malta Fever Ratios per 1,000. Admissions.	Deaths.	Simple continued Fever. Ratios per 1,000 admissions.
1897	1588	279	1275	34	34·7	1·49	158·9
1898	1771	200	1509	62	27·1	1·08	204·2
1899	1423	275	1107	41	37·0	1·21	149·1
1900	1347	158	1158	31	19·4	·98	142·2
1901	1499	253	1205	41	31·1	1·10	148·1
1902	1174	155	981	38	17·7	·68	112·0
1903	1203	404	781	18	45·4	1·01	87·
1904	1749	320	1350	79	35·1	1·32	148 0
1905	1906	643	1199	64	77·5	1·93	144·6
1906	683	161	508	14	24·2	·60	102·6
to September, 1907.	298	10†	283	5	1·7	·2	49·7

* Continued Fevers includes Malta Fever, simple continued Fever and Enteric Fever.

† Including readmissions.

TABLE II.

INCIDENCE of Malta Fever among the Officers, Women and Children in Malta, from 1901 to 1907.

Year.	Officers. Average Strength.	Officers. Admissions.	Women. Average Strength.	Women. Admissions.	Children. Average Strength.	Children. Admissions.
1901	341	25	435	26	813	9
1902	254	26	551	25	1120	18
1903	266	54	609	70	1226	33
1904	245	25	567	70	928	39
1905	217	44	548	48	860	15
1906	208	10	527	20	920	18
to end of September, 1907.	197	1	495	2	900	1

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TABLE III.

NUMBER of Officers, Non-Commissioned Officers and Men invalided from Malta to England for Malta Fever, 1897 to 1907.

Year.	Officers.	Non-Commissioned Officers and Men.
1897	9	110
1898	10	121
1899	8	120
1900	15	43
1901	9	92
1902	21	36
1903	21	133
1904	14	100
1905	21	382
1906	12	165
To end of Sept. 1907	—	1

14221. So much for the severity and distribution of the disease. The most important part, which follows, of course, is the question of animal experiment in the investigation of Malta fever. Will you now come to that?—In regard to the discovery of the germ causing a disease, it is very easy to be led astray, because when people are sick their tissues are very often invaded by other micro-organisms, so that when you examine a man's blood or examine the various excreta you may come across a bacterium, or bacteria, and it is difficult to know that you are dealing with the right one. And it is very important, of course, to know that you are really dealing with the cause of the disease, because, naturally, it would be a waste of time to work up the natural history of another harmless bacterium which had got in by accident; so that after one has found a germ in the body of a man or animal suffering from a disease, it is very useful if you can find one of the lower animals that will take the disease and so prove that the germ you are dealing with is the real cause.

14222. But it is when you have found one of these particular bacilli, wherever you find it, that it is important to test in a lower animal what its effect is?—Yes. It was laid down years ago by Professor Koch, the famous German bacteriologist, that the proof that a micro-organism is the cause of a certain disease must fulfil three conditions—(1) you must find the same micro-organism in every case of the disease, (2) you must be able to cultivate that micro-organism outside the body in such a way as to remove it from all products of the body, and (3) you must be able, by the injection of this artificial growth into healthy animals, to give rise to the same disease. Certainly, one of the most important things in beginning the study of a bacterial disease is to be able to put your finger on one germ and say that absolutely is the cause of the disease. Having settled that you can go on to study its natural history further. In the case of Malta fever another animal besides man is susceptible to the disease under ordinary natural conditions, and that animal is the monkey. So that after having tried the ordinary laboratory animals, such as the rat, guinea-pig, rabbit, and finding no result from them, I tried monkeys. When one injects this micro-organism into the former animals there is no rise of temperature and no appearance of disease, but in trying it on monkeys one found that they took a fever very similar to the fever in man, that they suffered from arthritic pains, and that, so far as you could see, they suffered in the same way as man did; and, lastly, when they died the same micro-organism was found in large numbers in pure culture in their organs. In this way Koch's three postulates were satisfied, and it was proved that the *micrococcus melitensis* is the cause of Malta fever. And it it difficult to know how you could prove this without animal experimentation, because in diseases such as dysentery you often find in the spleen, or the gall bladder, or the liver various forms of bacteria which have nothing whatever to do with dysentery, but have got in there on account of lowered vitality of tissues, and you might be led to think that they were the causes, or one of them was the cause of the disease, and in that way be led into error.

14223. Did you, by this process, discover what you now call the *micrococcus melitensis*?—Yes.

14224. You ascertained that that was the cause?—Yes.

14225. So far as you could ascertain it by those means which you think are satisfactory?—Yes.

14226. When was that?—That was in 1886.

14227. That was, I presume, by a series of experiments?—Yes.

14228. On monkeys?—Yes. The monkey is an expensive animal, and at that time there was no help given to private pathological work in the Army, so that one had to buy these monkeys out of one's own slender pay, and naturally I did not buy many, but I bought a sufficient number to enable me to say that this micro-organism was the cause of the disease.

14229. (*Mr. Ram.*) As long ago as 1886, did you say?—Yes.

14230. (*Chairman.*) The same bacteria I think you say you got from spleen of men who died of the fever in some cases?—Yes, I found this particular micro-organism in every case of death from Malta fever in man; after death, at the *post-mortem*, one removed a small quantity of the spleen and planted it out in various materials suitable for the growth of these things, and on a particular material and at a particular temperature this particular micrococcus grew, and it was from the growths, of course, on the artificial media that the animals were inoculated; that is to say, you find the micro-organism in the animal, you grow it outside their body, away from the animal, and, thirdly, you inject it into a susceptible animal and give rise to the same disease.

14231. Did you infect other animals from the blood of the monkeys that had the fever?—At that time we could only infect monkeys, because the monkey is the only animal sufficiently susceptible to take the disease by ordinary means; by feeding or by injection under the skin.

14232. Did you use experiments for the purpose of ascertaining how you could convey infection into man, or how it was likely to be conveyed?—Yes. Of course, later on, when we had the Commission, all these experiments were done in large numbers; I do not know how many monkeys were used as experimental animals, but there must have been some 150 or 200 monkeys; we had a large number of monkeys, and in using them they were used in many ways.

14233. That was in 1904-6?—Yes. For example, in finding out how the *micrococcus melitensis* leaves the body (which is naturally a very important thing to find out), it is a minute germ, and it might possibly come out in the expired air, in the urine, or it might come out as the bacillus of enteric fever principally does, from the alimentary canal in the dejecta. Then, of course, in examining how the micrococcus leaves the body, naturally you would examine the sweat, the saliva, the tears, and the other secretions. At the time this work began everybody supposed that Malta fever was due to effluvia, that it was due to this minute micro-organism finding its way into the air of wards from the sick or into the air of rooms from drains, sewers, etc., and so causing infection. Now, to put that to proof, you have to drive thousands of cubic feet through an apparatus in order to collect the micro-organisms contained in the air, and as you collect a number of ordinary air bacteria, you could not, by simple examination under the microscope, distinguish between the *micrococcus melitensis* and these others. So that here it is necessary to use an animal. The animal, as it were, picks out the pathogenic organism, which alone has the power of growing in the tissues and so causing the disease. All the other common organisms found in the air are killed off by the tissues and fluids of the animal, leaving the particular pathogenic one alive, and so its presence in the air can be proved.

14234. How would you infect the animals. By allowing them to breathe this air?—The micro-organisms would be taken from the air into a quantity of water, that water would be centrifuged in order to throw the solid matter down, and that solid matter in a small quantity with water would be injected under the skin of a live animal. In this particular case, as the monkey was the only susceptible animal the Commission used monkeys. At one time it was thought, for example, that the grand harbour of Malta was the breeding place of this fever. On account of its being used as a sewer for hundreds of years it was until recently in a disgraceful condition, and it was considered that the sailors got the fever when going back to their ships at night. Now, in order to prove that, large quantities of the harbour water had to be examined. For that purpose a large quantity of water is passed through a Chamberlain filter in order to retain the little particles contained in the water, and then removing these germs from the filter you can experiment as to whether they contain the *micrococcus melitensis* by injecting them under the skin of a monkey. If there are any *micrococcus melitensis* they will probably set up the disease, and the one particular germ will be recovered from the animal afterwards, all the other harmless ones having been killed off by the tissues of the animal.

14235. And you found, I understand, from these experiments that the micrococcus was not conveyed either by the air, by the effluvia, or from the harbour water?—Nor from dust collected in suspicious places where one might expect to find it, contaminated places, nor from the air and dust, for instance, of fever wards, or of rooms where cases of the fever had occurred, and so on.

14236. (*Sir Mackenzie Chalmers.*) All those experiments were negative?—Yes.

14237. (*Chairman.*) And by that exhaustive process you came down to what is the only source?—By that the Commission came down to the fact that it was nothing but the milk of goats which conveyed the infection of Malta fever.

14238. (*Sir Mackenzie Chalmers.*) What put you on to the milk of goats?

14239. (*Chairman.*) First, I suppose, you found that it was something that was swallowed?—Yes, it was discovered by animal experiment that infection could be conveyed by the mouth.

14240. Something in food I meant?—Yes, this discovery came as it were as an accident at the end. In an investigation of this kind you naturally examine all the animals round about, in order to find out if any of them harbour the disease. The goat was looked upon by us as the most refractory and the most insusceptible animal we could imagine; we do not consider that the goat is readily susceptible to human diseases; it does not even take tuberculosis, which is such a common animal disease, so that we look upon the goat as the animal that is about furthest away from taking a human disease; and as only the monkey took this disease, and not the rabbit, guinea pig, rat, or dog, we considered that it was very improbable that a goat would take it. But, as a matter of routine, some goats were taken and inoculated under the skin with the *micrococcus melitensis*, and some were also fed on a small quantity of the culture—that is to say, a little of this *micrococcus melitensis* is taken out of a tube and put on the food, so that the animal eats it. These animals were examined afterwards as to their temperature and general appearance, but nothing happened. Now there is a a more delicate way of knowing whether an animal is affected by an infectious disease, and that is by an examination of the blood of the animal with the micro-organism that causes it. The blood of an animal which has been attacked by a particular micro-organism has the power of drawing all these micro-organisms into clumps. It is a very peculiar thing, but it is true that if you take the blood of an infected goat and mix it with an emulsion of the *micrococcus melitensis* in a few seconds all these micrococci are drawn together into little clumps or lumps. You then say this micro-organism has had a certain influence on this animal. So when on examination of the blood of these goats this clumping was noticed, it was seen that something had occurred; but for almost a whole year nothing more happened, and it was only, I think, in the middle of 1905, or the beginning of 1906, I forget which, that, the other methods proving useless or futile, again the goat was taken into consideration.

14241. Do I correctly understand that the goats which you inoculated got Malta fever, or only that it affected the blood in a certain way?—They did not get the fever in the way that we consider characteristic of fevers. They got the fever, but did not show any external manifestations of that fever.

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14242. They carried about the poison that gave the fever, but did not suffer from it?—Yes, the micro-organism did multiply in their bodies, but it did not give rise to any ill-health, it did not give rise to any fever. By looking at the goat you could not say that that goat was ill—it gave as much milk as a perfectly healthy goat, it was as fat and smooth-looking as a healthy goat, so that it was only by this blood examination that it was suspected that something was occurring.

14243. That is to say, you suspected that the goats had it in them before you put the inoculation in already?—We suspected that the stuff we put in had not died off as ordinary non-pathogenic organisms would when brought into contact with a non-susceptible animal, but that it had gained some ground and was multiplying to some extent, and giving rise to these changes. So that in June 1905 a careful examination was made into this question again, more goats were bought, and before injecting them, or before feeding them, their blood was examined as a matter of routine to see that the blood was all right, and, curiously enough, goats bought at this time gave this same Malta fever reaction; and then the idea struck us that the goats, the ordinary goats of Malta, might be suffering from this fever. So that at once, after having found that they give a reaction, you can examine the blood itself by removing a certain amount of blood from the veins for this micrococcus, and you can also examine the milk, and on examining the blood of these ordinary Maltese goats the *micrococcus melitensis* was found in it, and on going further and examining the milk of these apparently healthy goats a certain percentage of of them were found to contain that *micrococcus melitensis* living in their milk, and that, of course, threw a great flood of light at once on the whole question.

14244. (*Dr. Gaskell.*) Had you previously found out from experimentation that the micrococcus left the body in the milk and the urine?—Yes, in the urine, but not in the milk of human beings. You very seldom find a woman suffering from Malta fever, and at the same time secreting milk; and the monkey, of course, is not as a rule secreting milk in captivity. So that before this discovery was made the *micrococcus melitensis* had not been found in the milk of any animal. After the discovery was made, it was found in the milk of human beings—nursing mothers. But you can easily understand that beforehand one would not think of this.

14245. (*Chairman.*) The important discovery was that it was in the milk?—That was the important discovery, because it at once explained various things in the epidemiology of the disease: that seasonal prevalence had little or nothing to do with it; that whether it was a large town or small village nothing in different methods of sanitation seemed to matter; the curious fact that officers were much more struck by this fever than the privates was also explained; and after more work the Commission came to the conclusion that the only way that a man takes Malta fever in Malta is by the drinking of goats' milk. Perhaps once in a thousand times he may take it in some other way, but you can sweep all that aside; the drinking of goats' milk is the main path of infection, and naturally, of course, it is easy to stop to a great extent our sailors and soldiers drinking goats' milk, so that when the thing was once brought to the attention of the authorities, at once the disease was blotted out of the garrison of Malta, and from what I have said, of course, we look upon that as a very important and useful feat to have accomplished.

14246. Will you tell us what measures were taken then? Your Commission I suppose reported that you had discovered this particular micrococcus to be the cause of the disease?—Yes.

14247. Then were orders given that no goats' milk should be supplied to the troops?—At first there was a certain amount of hesitation, and an attempt was made to sterilise the milk by boiling and that kind of thing; but we soon pointed out that while boiling milk, if it was properly done, of course, would sterlise the milk, yet when you are dealing with the natives of these countries you never can trust that the thing is done properly. There is a small test by which you can tell whether milk has been boiled or not, and by carrying that in your pocket, and by going into the hospitals and demonstrating to the head of the hospital that his milk

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was not boiled when he thought it was, after a month or two of this the conclusion was come to that no goats' milk whatever was to enter any military or naval hospital, or any ship or regiment. The General gave an order that not a drop of goats' milk was to be used, and all the troops were put on condensed milk, and it was then that the great fall in the number of cases took place.

14248. Down to six cases instead of 700?—Yes, six fresh cases.

14249. Have you any doubt that your discovery was correct, and that you found the real cause?—I do not think there is the slightest doubt—it is so marked.

14250. You have spoken about Malta, and you have told us that the same fever exists in other places. Has it been traced to goats in those other places?—In other places they have a very strong suspicion of the goats, and certain goats have been found to be affected; but, of course, the thing has not been investigated as closely as it has been in Malta.

14251. Take, for example, a quite distant place, the Philippine Islands, where you say it exists; have any experiments been made there as to what causes it there?—I do not think so. I do not know that I know the whole literature of the subject in the Philippine Islands; but all I think that they have done at present is to find that men do have it there.

14252. Does this fever exist in places where goats' milk is not used?—Not that I know of. In India in the Punjaub, where it is found pretty frequently, goats' milk is used; and down in the south of Africa they have been bringing evidence forward lately that the more goats are used the greater the prevalence of this fever.

14253. Is it found in places where goats' milk is not used?—I do not think so. I would not say at the present moment that in every country in every part of the world the goat, and the goat alone, is responsible, because it is evidently a disease which affects in a mild way many species of animals; that is to say, that even a cow, if it was exposed to infection, would become infected. So that if you removed all the goats from Malta to-morrow and replaced them by cows, you might find yourself in the same position after some years.

14254. Do the natives go on drinking goats' milk?—Yes.

14255. Do they suffer from fever?—Yes; but, of course, not to the same extent apparently that the soldier suffers. The soldier goes there a susceptible man. The Maltese are always living among it. I imagine that nearly every Maltese has this fever at one time or another; but they drink it with their mother's milk, and they have it when they are young, so that they become, as it were, immune to it. The incidence amongst the Maltese at the present time is put down at about a tenth of the incidence the garrison suffers. But, of course, the diagnosis of the fever away in these country villages by country doctors, and the desire of those people not to offend their patients, and all that kind of thing, makes the notification returns very unreliable.

14256. (*Sir Mackenzie Chalmers.*) Is one attack of this disease protective?—That is not absolutely proved, but in my opinion it is to a great extent protective We very seldom find a soldier coming in suffering from a second attack, and the monkeys experimented with, after having recovered from a first attack, showed no signs of taking a second when infected again.

14257. They were resistant?—Yes.

14258. (*Sir William Collins.*) At what interval?—That, of course, is only an interval of about a year or so. This matter was not pushed very far. I do not think it is at all proved that one attack of enteric fever or Malta fever will make a person immune not nearly as much as scarlet fever, smallpox, or measles; those fevers do not seem to confer such marked immunity, and it is really a difficult question and a question that I would like to have answered—it has a bearing on our system of anti-typhoid inoculation in the Army as to whether one attack of typhoid fever protects sufficiently.

14259. (*Sir Mackenzie Chalmers.*) Or for a specified period?—Or for a specified period.

14260. (*Chairman.*) Supposing that you had been sent to Malta to discover the cause of Malta fever with a positive prohibition against any experiments upon animals, how would you have set to work?—Of course you could have discovered the micrococcus in the dead bodies of men, but then you would have stopped there, I think, because if you wanted, for example, to find out whether goats suffered you could not inoculate a goat; you could not even take a drop of blood from a goat's ear. I think you would have been stopped in the investigation at the outset.

14261. Supposing that you had suspected the goat, which you say you would not have done except after these exhaustive experiments in other directions—supposing you had happened to light upon the idea that it might have been due to goats—what would you have done?—In the first year of the Commission a Local Government Board man, who was recommended by Mr. Power, went out to Malta to work at the fever from the epidemiological standpoint, to examine by the method of statistics the incidence among those who drank water, beer, milk, and all that kind of thing, in order to see whether any light could be thrown upon this fever; but after very hard work for six or seven months, during which time he lost four stone in weight, he came to the conclusion that milk at least was not the cause; so that the epidemiological way of working at the causation of this fever absolutely failed. We sent out others; we sent out one or two men, and we sent out Colonel Davis, who is a man who worked at it in the same way, and he came to the conclusion that milk was not the cause, showing the difficulty of any other method of discovering the cause.

14262. Supposing you had discovered, as you might, from accidental observation in a place, that the people who drank goats' milk got it and that people who did not drink goats' milk did not get it, and that made you suspect the goat as being the cause, and that you had found this bacillus in the milk of the goat, would that have been sufficient?—I quite agree that it would not have been anything like so complete as it was after the experiment, but would you upon that have come to the conclusion that that bacillus caused the disease?—I think so.

14263. If you had found it in the spleen of all men who died of it, and you found it in the goats' milk, and that men who drank goats' milk died of this disease or got it, and that others who did not drink goats' milk did not get it?—Certainly that would be a very strong argument, and one would certainly be able to take practical measures to prevent the disease.

14264. By isolating people from goats' milk and testing it in that way you might find that you were right—you might be corroborated by the result in your conclusion that people who did not drink goats' milk invariably avoided the fever and that those that took it did not?—Yes; but what I would say with regard to that is that you would depend upon something fortuitous, some sort of accident, to make your discovery; whereas when you begin the study of a disease by the experimental method you simply go along, as it were, a well-marked track. You begin at one end and you simply go slowly forward, carefully scenting it out, and you feel sure you must find out the cause of the disease if you only have enough patience and perseverence.

14265. You go along a path which must lead you to the result. In the other case you are walking along a path with a vast number of turnings, and you do not know which is the right one?—In the one case you might sit for 20 years waiting for some fortuitous circumstance to arise which would point out the particular thing which you want to arrive at; but in the other case you begin working just as a man begins working at mending a road or any other piece of work, and you know that within a year, or two or three years, you stand a very good chance of finding out the cause of the disease and its mode of infection, or you find out the reason why you cannot find it out. For example, in cancer you cannot see anything, and you cannot get any further forward even after years of work, because there is nothing to see or cultivate or work with.

14266. (*Sir John McFadyean.*) But that experimental routine method is only certain to be successful in cases of human disease when that disease is communicable to an animal?—Yes.

14267. (*Mr. Ram.*) As I understand, the use that you made of animals, where they were of use to you, was that you wanted to eliminate all other cocci except the particular one that you wanted to find?—That is only one small part.

14268. You said that you tested these animals, and

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that other micrococci might be taken up which become dead in the tissue of the animal?—Yes.

14269. But that only this latest one ultimately survived, and you found it in other animals?—Yes. That is the very beginning to prove that the particular micrococcus is the cause of the disease.

14270. But this particular micrococcus, given to animals, did not help you further than that?—Yes, it did.

14271. How?—How did the goat help us when we took a drop of blood from its ear? The goat was helping us very much, when we found the micrococcus in her blood; and the goat helped us very much when we fed it on the micrococcus, and found that, by again taking a drop of blood from its ear, a certain change had taken place, which showed that this micrococcus was living and multiplying in the interior of that goat.

14272. But with regard to other animals, monkeys and so forth, what you wanted was to enable you to recognise this particular micrococcus?—But we did not know that the micrococcus in milk, for example, was in a condition to give rise to the fever. By taking 30 monkeys and giving each one a drink of milk and finding that 25 or 26 got the fever, we had a very strong argument that the micrococcus was in a virulent form in the milk, and that the same thing would probably occur in man.

14273. After you suspected the milk you tried the milk in animals?—Yes.

14274. (*Dr. Wilson.*) Was the milk fresh when it was given to the monkeys?—Yes, quite fresh.

14275. (*Sir Mackenzie Chalmers.*) You found that the milk was not merely a concomitant, but was the cause?—Yes, we found that it was the drinking of the milk that gave rise to the fever.

14276. (*Sir William Collins.*) You mentioned the case of cancer as a case in point. Am I right in thinking that in that case investigation was not restricted to a fortuitous cause?—Yes.

14277. And that investigation has hitherto been unsuccessful?—Yes.

14278. (*Sir John McFadyean.*) I venture to suggest that there would be no objection on any ground to taking blood from a goat for examination if once a suspicion arose that the goat was actually diseased. That would not constitute an experiment even in this country. I submit that such a thing is not covered by the Act, the goat being suspected, and that it is not an experiment, but an operation conducted with a view to diagnosis; that it is analogous to the injection of mallein and tuberculin, which is often done, not with the intention of curing the animal, but with the express intention of killing it?—As I read the Act I cannot take a drop of blood from a rabbit's ear to examine, but I can knock a rabbit on the head and take all the blood in its body to examine.

14279. That is in the case of healthy unsuspected animals?—But these goats looked healthy animals.

14280. (*Dr. Wilson.*) But you suspected them of the disease?—No, we did not.

14281. (*Sir John McFadyean.*) I quite recognise that you examined their blood, although you had not suspected it?—As a routine examination, just as you take a temperature, and we were very much surprised to find that the blood reacted. I was exceedingly surprised, I remember.

14282. (*Chairman.*) You were speaking about accidental discoveries. There was an accidental experiment, was there not, on board a steamship with a cargo of goats from Malta?—Yes.

14283. Will you tell us what that was?—That took place about the time when we discovered the complicity of the goat. After the matter had been reported, of course, the attention of medical men all over the world was directed to it, and I got a letter from Alexandria saying that a sailor had been taken into the hospital there suffering from Malta fever, and mentioning that in all probability it would be found to be due to drinking goats' milk. That letter led to a complete working up of this ship epidemic. It turned out that the Americans had sent over a man to Malta, the Maltese breed of goats being a very famous breed on account of supplying a large quantity of milk, to purchase milch goats and bring them to Washington, there to set up a dairy for the supply of goats' milk for children; it was on account of the tuberculosis question." That man bought 65 goats, put them on board a steamship called the "St. Nicholas," and took them first of all to Antwerp. During the voyage in the Mediterranean the captain and nearly all of the crew—with the exception, I think, of three or four—drank the milk of these goats, and broadly every man who drank the milk took Malta fever; and even when the goats got to America, in spite of all the experience that they had had, the wife of the man who was in charge of them there also took Malta fever, and the man Thompson who had brought the goats from Malta to America died about that time, and there was a suspicion of Malta fever in his case; so that you could hardly have had a better experiment on the action of milk than in that particular case.

14284. You do not know, I suppose, what was the history of these goats after they landed?—Yes. We, of course, informed the American Embassy here, and they took care to have those goats thoroughly examined after they landed, after they got to their destination; and I forget the percentage of the animals found to be affected by Malta fever, but it was quite a large percentage. Those animals were then slaughtered, and the healthy ones kept under observation. What has happened since I do not know. I have not heard anything since, but I imagine that even if a case of that sort had occurred under ordinary circumstances it would not have attracted any attention.

14285. That was in 1905 whilst you were pursuing your experiments?—That was the result of our discovery as regards the milk; that is to say the discovery of that epidemic on board that ship was due to our discovery of the fact that the *micrococcus melitensis* was found in the milk; because it was only that which made the man in Egypt write to me.

14286. It was only a confirmation of your discovery?—It was only a confirmation of it, and it would not have been discovered if it had not been for our previous discovery. One of these men was treated at Alexandria for Malta fever, which is a common thing there. Another man, the captain, called at Valetta, and asked the local doctor about his fever, and another was treated at the Seamen's Hospital here, and so on; this ship epidemic would probably never have been noticed if it had not been for the work of the Commission.

14287. (*Dr. Wilson.*) But no precautions were taken with regard to shipping those goats?—No, that was done without our knowledge at all.

14288. (*Chairman.*) Have the same precautions been taken with regard to the disease at Gibraltar as were taken at Malta?—I give in my *précis* of evidence a chart by Major Horrocks, which shows that the Maltese goat has almost entirely disappeared from Gibraltar in the last few years. Major Horrocks is the sanitary officer at Gibraltar. He has examined every goat on the Rock, and he has the power of slaughtering those that he finds affected, so that the precautions taken in Gibraltar are very much greater than the precautions taken in Malta.

14289. (*Sir Mackenzie Chalmers.*) And with what result?—There has been no Malta fever there for some years.

14290. Rock fever has died out?—Yes.

14291. (*Chairman.*) However, we may take it now as an accepted fact, may we, that Malta fever does come from the milk of goats?—So far as human proof can go I think it is absolutely proved.

14292. And you have told us the history and how far experimentation on living animals had a part in the discovery?—I think in any other case the discovery would have been accidental or fortuitous, but by having animals to experiment on, if you can find an animal that is susceptible to a particular organism, and if you can find the organism and grow it outside the body, it is only a question of time to discover the natural history of the organism, and find out some method of stopping the disease.

14293. Then I think you have also investigated nagana or the tsetse fly disease, which are both discovered to be the same now?—Yes, they are both the same.

14294. But that was not known at the time?—No.

14295. That, I think, was in 1894?—I went up to Zululand in the end of 1894. The tsetse fly disease before that was looked upon as being a disease caused

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by the poisonous bite of a particular fly, just as a snake bite would poison.

14296. Or as a wasp is said sometimes to kill people in England who are very sensitive?—Yes.

14297. Did you use experimentation on living animals in that investigation?—I used a large number of ordinary native dogs for the purpose, and also horses and mules and donkeys.

14298. Was that for the purpose of discovering a bacillus?—When I went up to Zululand at first, the natives sent me in a certain number of cattle suffering from this nagana. They looked very ill, but one did not know anything about their illness. The only thing to do was to examine them systematically. One of the easiest tissues of the body to get at for examination, of course, is the blood, so that one makes a very careful examination in all sorts of ways of the blood of the animal; and while I was doing that I came across a peculiarly shaped thing in the blood; it was very scarce in the blood of these cattle, but looked as if it was something foreign to the blood. At that time, of course, one was working quite in the dark. In nagana in cattle this particular micro-organism is very scanty; you may examine the blood for several days very carefully without finding one; but in some other species of animals they grow in much greater numbers in the blood, so that when I took as a matter of routine some of the blood of the cattle and put that blood into a dog it was found that the dog rapidly became ill, that its blood was swarming with this peculiar-looking thing, and therefore there was probably some connection proved between this peculiar looking thing and the disease.

14299. The dog is liable to the disease?—Yes, it is an acute disease with dogs; they die in ten days or a fortnight; whereas cattle will live on for a year, and even recover in some few cases. Often they will live for six months or a year or two years with very few of these things in their blood. The discovery that nagana and the tsetse fly disease were the same was a matter of accident. Within a few miles of the hill on which I was living was a fly-belt, and as I had read about this fly disease in books of travel, I was curious to know what the fly disease was. Accordingly, as a matter of curiosity, I sent down one or two horses, some cattle, and several dogs to live in this fly country for a fortnight, and when they were brought back to the top of the hill I examined them and, much to my surprise, I found this same creature living in their blood; and then I began to suspect that nagana and tsetse fly disease were probably the same, and that the disease was really caused by this trypanosome, this parasite.

14300. (*Sir Mackenzie Chalmers*). When the animals came back had they developed any signs of illness?—Yes, dogs develop nagana in a very few days; they had been a fortnight in the country.

14301. (*Chairman.*) You mean that you came to the conclusion not only that this thing which you found in the blood was the cause of the disease, but that the tsetse fly was the cause of its being deposited in the animal in each case?—That, of course, is a matter of experiment.

14302. And then you ascertained that?—Yes.

14303. I thought that nagana was a disease which did not necessarily occur in what is called a fly-belt?—All diseases are apt to burst over their bounds. Once in ten years or so, on account of some question of temperature or dryness of the country, the wild animals accompanied by the flies spread out over the country up the valleys in order to get moister places for grass and water, and therefore every 10, 15 or 20 years the fly disease is apt, as it were, to burst out and spread 50 or 60 miles from the fly country.

14304. It follows the wild animals wherever they go?—These flies live on the wild animals, and follow them wherever they go. If the wild animals cannot get sufficient food and water in the fly country they spread over the surrounding country.

14305. (*Mr. Ram.*) Are the wild animals made ill by the presence of these flies?—No.

14306. (*Chairman.*) Were any experiments made on animals, beyond those of which you have told us, for the purpose of discovering the cause of nagana? Did you make any preparation from them?—No, those organisms will not live outside the body on the ordinary media then in use. It is quite a different thing from a bacterium; it is an animal parasite, and at that time there was no idea of its being cultivated outside the body.

14307. The tsetse fly, I suppose, draws blood from the animal which it attacks?—Yes. In regard to animal experimentation, as I said, first of all there is the small foreign-looking thing found in the blood of cattle in very small numbers; you could not say that it had anything to do with the disease; but when you put a quantity of the blood into dogs and horses you found that this body pressed very much to the front. Then you found out by accident that the animals coming from the tsetse fly country have this trypanosome in their blood. The natives thought that the tsetse fly disease was caused by eating the vegetation in these unhealthy places, especially where there was wild game. So, in order to experiment further, I sent down horses into this fly country for a certain time, muzzled in such a way that they could not eat or drink anything. They were taken down in the morning, brought back at night, and not allowed to drink water or eat a blade of grass. The horses which were sent down in this way came back to the top of the hill and developed the same parasite in their blood.

14308. (*Sir Mackenzie Chalmers.*) They were sent down in the morning, and brought back in the evening?—Yes, and invariably we found this trypanosome in their blood, and in three weeks or so they died of this disease. Then, naturally, one began to think that the tsetse fly had something to do with it, because the flies were biting them when they were down there, and one began experimenting by bringing up these flies in cages, feeding them on a dog suffering from nagana, and then at various times afterwards feeding them on healthy animals, after twelve, twenty-four, forty-eight, and sixty hours, and so on. Thus I discovered that this fly could carry this trypanosome from an infected animal to a healthy animal readily up to forty-eight hours; so that one could see in the case of the animals going down to the fly country, and being bitten, that if they were bitten by flies even forty-eight hours after they had fed on an infected animal, it might give rise to the disease. Then another question arose: Were the flies themselves, as you found them in the fly country, capable of giving rise to this disease without any feeding on infected animals up above? Accordingly, I brought up cages full of these tsetse flies to the top of the hill, and put them straightway on healthy animals, and the healthy animals all got the same disease. Therefore, the fly down in the fly country was infected with this parasite, and could give it to any domestic animal that came near.

14309. (*Chairman.*) A fly, do you mean, that was not a tsetse fly?—No, only the tsetse fly.

14310. I thought you said the common or garden fly. You meant the common tsetse fly?—Yes. I meant the ordinary tsetse fly as found in the bush and not artificially infected. Then one naturally wondered where these things got this little parasite from, and, in order to find that out further experiments were made. The flies feed, of course, upon the wild animals; what more natural then than that they got this parasite from the wild animals? The wild animals are perfectly healthy looking and sleek, and do not appear to be suffering from anything. Accordingly, I went down and shot many wild animals, buffaloes, koodoos, wildebeeste and others, and the moment an animal was killed I sent up a bottle full of its blood to the top of the hill, where there was no nagana at the time, and injected that blood into healthy dogs. A certain number of these dogs took nagana, and I found this same parasite in their blood. In this way I showed that these wild animals had this thing in their blood; and afterwards, to make this more sure, by microscopic examination of the blood of the wild animals, and by patiently looking for a long time microscopically at the blood of these wild animals, I discovered the thing itself in their blood, in small numbers, of course; in large numbers it would affect their health. In small numbers, so far as I know, it does not affect their health. So that there, again, by animal experimentation, it was proved that the wild animals act as a reservoir of this particular parasite, and I do not see how one could have proved it otherwise. Even when one knew that the parasite was in the blood, it took a whole day of examination of very many specimens to find a single parasite with the microscope; and one would not have examined so

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long and so carefully unless one knew that one would find it if one had patience.

14311. It helped to ascertain the cause of the disease?—I think that animal experimentation helped in everything.

14312. Has it helped you at all at present to discover any remedy?—It was in 1895 I sent home a dog suffering from nagana to London, which dog arrived here alive; that is to say, as one dog died another was inoculated, and at last a dog was delivered in London with this living trypanosome in its blood, and these trypanosomes have been spread from London from that one dog to scientific workers all over the world, and there has been an enormous amount of work done in the last ten or twelve years with regard to these trypanosomes. There have been, I should say, thousands of animal experiments with a view to find a method of serum treatment, and quite an extraordinary number of experiments have been made to find a method of medicinal treatment: and, as you know from the daily press, within the last few days there has been discovered a powerful drug, one injection of which, for example, into a guinea-pig which is just dying of this disease, which would not have ten minutes to live (I do not think I am exaggerating), which is at death's door, will drive all the trypanosomes out of its blood, and in a day or two that animal will appear healthy, and animals so treated have remained without trypanosomes in their blood for, I think, two months.

14313. What is the drug?—That is not published yet, so I will not mention the name; but another drug which has been used very much is a drug which is called atoxyl, an arsenical preparation.

14314. (*Sir Mackenzie Chalmers.*) That is a dangerous preparation, is it not—dangerous to patients, sometimes?—M. Nicolle, of the Pasteur Institute, said that he thought it might be dangerous by accumulating in the system, but I do not think there is any proof that with care there is danger to human beings. Koch has been to Uganda, and in the last few months has been using it on many thousands of natives, as far as I am aware, without any fatal results.

14315. (*Chairman.*) I understand that you are not specially engaged in the discovery of remedies at present?—I am on the Royal Society's Committee for the discovery of a drug to cure sleeping sickness.

14316. I presume that in almost every case the discovery of the cause of a disease is the first step towards the discovery of a remedy for that disease?—Yes.

14317. And, to that extent, at any rate, you think you have got on to assured ground with regard to nagana?—In regard to these trypanosomes, certainly. Sleeping sickness is another disease of the same kind.

14318. Do you think that progress is being made towards recovery and remedy by means of experiments on animals?—Yes. I never dreamed that we would be able in time to cure an animal of trypanosomiasis. It is a very difficult question, and one does not want to say anything decided, the matter being still *sub judice*. When I was in Zululand, in 1894 and 1895, I tried arsenic on horses affected by nagana, and I was able to keep those horses alive for a year by giving them arsenic in their food. As a rule, horses die in about three weeks of this acute disease. Of course, during that year I thought I had a method of curing horses of this very fatal disease. But at the end of the year the trypanosomes came back into the blood, and, although one doubled or trebled the dose of arsenic in the animal's food, the trypanosomes paid no longer any attention to the arsenic, but multiplied and killed the animal, and in that condition our knowledge has remained till within the last year or two. Lately a great deal of work has been done, especially in regard to sleeping sickness, and now the idea is that by giving an arsenical preparation first, and killing all those parasites that are susceptible to arsenic, you can kill off the remainder by giving them another poison. The poison used has been, first, atoxyl, and then a preparation of mercury, and no doubt Mr. Plummer has kept animals alive in this way for some 300 days. These rats ought to die in five days. The Liverpool School of Tropical Medicine have got much the same result. It is a question whether we can say that these animals are really cured; but, however sceptical I may be, I imagine that when a rat lives 300 days instead of five, it looks very much like a cure.

14319. (*Sir William Collins.*) These are all chemical agencies to which you refer, not sera?—Purely drugs, just like quinine in malaria. The parasite in sleeping sickness and nagana is an animal parasite closely related to the malarial parasite. It is much more delicate than a bacterium. If you put it on this table it would die in a few minutes; if you put the *micrococcus melitensis* on the table, it would live for months. The one is much more delicate and much more easily got at by drugs. These protozoal parasites are more easily affected by drugs given by the mouth than are bacteria.

14320. (*Dr. Gaskell.*) They are supposed to be mere animal parasites, and the others are supposed to be vegetable parasites?—Yes.

14321. (*Chairman.*) There is something you can tell us about experiment with sleeping sickness. You, I believe, went out to Africa to study the disease?—I went out to Uganda in the beginning of 1903. Up to that time there had been a certain amount of work done on sleeping sickness, especially by the Portuguese, who sent out a Commission to the West Coast of Africa. The result of their work was the belief that sleeping sickness is caused by a certain streptococcus, one of these vegetable parasites belonging to the same class as the micrococcus melitensis. When I arrived in Uganda, the Royal Society's Commission had been at work for about a year, and the conclusion they had also come to was that this disease was caused by this bacterium. I found one member of the Commission still there, and in the course of conversation he told me that on five occasions he had seen a different kind of parasite in the sleeping sickness patients, namely, one of the trypanosomes. Trypanosomes by this time, of course, had become well known, and on the West Coast of Africa they had been found in the blood of man. These parasites had been named Trypanosoma Gambiense, and cases of this disease had been sent home to England. Curiously, it was never dreamt that this trypanosoma fever, as it was called, had any relation to sleeping sickness; so that when this man in Uganda mentioned to me that he had seen on several occasions trypanosomes in sleeping sickness, he said that it was curious that this trypanosoma fever extended into the interior of Africa, and he also, like the others, had no idea that the trypanosoma had any causal connection with sleeping sickness. Now, on account of my old work in Zululand, having worked at a trypanosome disease for two years, I was naturally very interested in this observation of his, and I said to him—If you will wait a few weeks longer (he was going to England at the time) I will examine cases with you on the hypothesis that this trypanosoma may really be the cause of the disease.

14322. You mean that you would examine cases of people with sleeping sickness?—Yes. So he remained for three weeks longer, during which time we examined some 30 cases of sleeping sickness for trypanosomes, and for trypanosomes alone. I simply wanted to find out in how many cases one could find trypanosomes.

14323. Taking blood from them, do you mean?—No, in that case we took the cerebro-spinal fluid.

14324. After death?—No, during life. We gave the natives chloroform, and removed about an ounce of fluid from the vertebral canal, and had it centrifuged in order to throw any little solid bodies down to the bottom. I examined the sediment, and in most of these cases we found the trypanosome. In fact, after this man left, and I went into the thing more carefully, this trypanosome was found in the cerebro-spinal fluid in every case of sleeping sickness examined. Now, the moment one finds a parasite occurring in 100 per cent. of a particular disease, one begins to think it may have something to do with it. Of course, it is no proof that, because you find it in 100 per cent., it is the cause of the disease, but still it is a hopeful beginning. I put aside the idea of sleeping sickness being caused by a bacterium, and went for the trypanosome. After finding it in 100 per cent. in the cerebro-spinal fluid, we went on to examine the blood. Just as in the cattle in nagana the trypanosomes are exceedingly scarce in the blood in sleeping sickness; and it is only by very patient work and examining large quantities of blood that you can find them. In this way we found this same trypanosome in every case in the blood, except on one occasion. So we had now learned the fact that in the blood of sleeping-sickness cases, and in the cerebro-spinal fluid, we could always find this parasite. The next point we investigated was this. If this parasite is an ordinary harmless guest of the people of this country, then it will be found in the natives who live outside the sleeping sickness area, as well as in those

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who live within this area. Just as in nagana you have a fly country, so in sleeping sickness you have a sleeping sickness area, outside which the natives will not suffer from sleeping sickness. So we examined some 200 cases of natives from a region outside the sleeping sickness area, and we could not find this parasite in a single case.

14325. Examining in the blood?—Yes. We examined the blood in the same careful way that we examined the sleeping sickness blood, and in not a single case could we find the parasite. Then it began to look as if this trypanosome had really something to do with the disease. Accordingly we took cerebro-spinal fluid containing these living creatures, and injected this fluid into the ordinary animals we found round about. The monkey, of course, is very common there, and so are dogs and cattle. I had taken out some guinea-pigs and rabbits, and so we injected this parasite into all these animals and awaited results.

14326. Did you consider that the sleeping sickness must be caused by some bacillus or coccus in the cerebro-spinal fluid?—I thought, of course, that it must be caused by some living organism, because when you have a so-called infectious disease attacking large masses of men it is probably caused by a living organism.

14327. It was only in the place where you looked for it? You spoke first of having examined the cerebro-spinal fluid?—The principal symptoms in sleeping sickness are nerve symptoms, and you would naturally make a careful examination of the nervous system.

14328. You had expected to find traces of it there?—Yes. Just as Pasteur, in hydrophobia, went to the brain of the animals to find the virus, so when an animal is suffering from a nervous disease you go to the nervous system.

14329. It was the symptoms that induced you to look there?—Yes, the symptoms are purely nervous symptoms. To return to the injected monkeys. The blood of these monkeys was examined every day, and their temperature was taken very carefully, and they were watched. After five or six days the same trypanosome appeared in their blood, and we found it daily for some time. Sleeping sickness is a very slow, chronic disease, and at the end of three, four, and five months these monkeys began to show the same kind of symptoms that men show; they became very lethargic, and sat all day, as it were, with their heads on their knees, apparently asleep, and in the course of time these monkeys died. Then, on some occasions, we examined the cerebro-spinal fluid of a monkey during life, but it is difficult matter to examine the cerebro-spinal fluid of such a small animal; after death we examined the cerebro-spinal fluid, and we found this trypanosome in it as we had found it in the blood. In man there is a particular change in the brain, first described by Dr. Mott, so that if you cut a section of the brain you can tell by the appearance of it that he has died from sleeping sickness. If these monkeys died of the same disease, you would naturally expect to find this peculiar anatomical change of the brain. This anatomical change of the brain was found first, I believe, by Captain, Harvey, R.A.M.C. And therefore the conclusion has been come to that the disease of which the monkeys died is the same as the disease in man, and that in all probability the trypanosome is the cause.

14330. (*Sir William Collins.*) Is that change always found in the monkeys?—No, not nearly always, because the monkeys die much more quickly than man, and this change is only found in the chronic cases.

14331. (*Chairman.*) Only found in man, do you mean, in the long chronic cases?—It is only found in the monkey in those cases which last a long time. It is a very slow, chronic change which takes place. The monkey dies in three or four months, and man dies it may be in three or four years. The length of time the poison has been acting brings about these chronic changes in the brain; whereas if a man dies in the early stages of the disease, or if a monkey dies after three months, neither of them will show this peculiar anatomical change.

14332. (*Sir Mackenzie Chalmers.*) For instance, if a man infected with sleeping sickness was killed accidentally three or four months afterwards, you would not expect to find this particular anatomical change?—No, I do not think that it would be found after an illness of only three or four months.

14333. (*Sir William Collins.*) Are these microscopical changes that you allude to often absent in the brain of monkeys that are subjected to the disease?—Yes.

14334. (*Sir Mackenzie Chalmers.*) If the disease runs its normal course you usually find this microscopical changes?—Yes, it is a difficult matter, of course, making this particular observation, and in Uganda it was not done very often; but the examination was made for the Commission in London.

14335. (*Sir William Collins.*) It was found by Dr. Mott, was it not?—Yes, and the cases in which he found it were usually chronic cases. We sent him specimens of every brain we examined in Uganda, so that he had a great deal of material to work on during this investigation. We did not cultivate these trypanosomes outside the body, as was done in the case of Malta fever, so that the proof is not as good. You may inject into a monkey cerebro-spinal fluid, which you know to contain a certain living thing, but it may also contain something else, so that the proof is not as complete as in the case of a bacterium which you can grow outside the body, and then inject into a susceptible animal. The growth of the trypanosome outside the body of sleeping sickness has never been accomplished. But when you have done a number of experiments of this kind; when you find the same parasite in every case of the disease; when you find it giving rise to the disease in animals, you come to the conclusion that in all probability you are dealing with the right thing. Then the next point to investigate was how are these natives infected, and, from my old work on nagana, I at once thought that a tsetse fly would be the carrier of the trypanosome. At that time it was not known that there were any tsetse flies in Uganda, but when we went down to the lake shore and began to hunt for them we found a particular species of tsetse fly, different from the nagana one present, and in large numbers. We caught these flies, kept them in cages, fed them on sleeping sickness natives, and then on healthy monkeys, and we found that these tsetse flies could carry sleeping sickness from a sick native to a healthy monkey. And then we made the further experiment. As there were a great number of infected natives on the lake shore, we collected these flies from among these natives, brought them up to the laboratory, and put them straightway on healthy monkeys, and we found that these monkeys took the disease. Then the next thing we did was to investigate the distribution of the disease. The distribution of sleeping sickness in Uganda is very extraordinary. It is confined to a narrow belt running along the margin of the Lake Victoria and its islands. Further, the disease is only found where there are forests. You do not find sleeping sickness where grass plains came down to the shore. You find no sleeping sickness if there is a belt of papyrus reeds growing along the lake shore. In the interior of the country there is no sleeping sickness. It is a very peculiar distribution. Therefore, I said to myself, if this tsetse fly carries it, very probably this fly has also a peculiar distribution. The tsetse fly which carries nagana is spread all over the country. Then we got hold of the native chiefs, and they set their minor chiefs all over Uganda to collect flies, and within a couple of months they sent in about a thousand collections of flies from all over the country. At the same time, we asked them to send in a word as to whether sleeping sickness was found where the flies were found. Then we separated all these biting flies into two categories, namely, this particular tsetse fly and the others, and we took two large maps, and whenever a collection of flies came in from a place containing one or more of these tsetse flies we put a red mark on that spot on the map. We also had another map of the same size, and if a chief reported that sleeping sickness was found at a particular place, we put a red mark on the same place on the second map. This went on for some months, until at last we had about a thousand observations, and then, when you looked at the two maps, you found that the red spots on one map coincided with the red spots on the other map, and the blue discs which we put on the places where no tsetse flies were found coincided exactly with the places where no sleeping sickness was found. Accordingly, we came to the conclusion that the distributions of sleeping sickness and of this particular species of tsetse fly were identical, and that gave us another proof that this trypanosome was the cause of the disease, and that there was only one way by which man could become infected, and that by being exposed to the bites of this particular species of tsetse fly. Since then a good deal of work has been done, but that is now, I think, established beyond doubt.

14336. (*Chairman.*) From what quarter did the tsetse fly get it in order to convey it to man?—I think in Uganda the principal reservoir is man himself.

14337. From man to man?—From man to man.

14338. (*Sir Mackenzie Chalmers.*) Carried by the fly?—Yes.

14339. (*Chairman.*) In Africa, then, the tsetse fly bites mankind?—Yes; I think they all bite man. All the flies we worked with were caught on men. The tsetse fly gives the sleeping sickness trypanosome to man, but it does not give the nagana trypanosome to man.

14340. (*Sir Mackenzie Chalmers.*) The tsetse fly of nagana feeds on man and on animals, but man is not susceptible to the disease. It does not live in man's blood. The trypanosome of nagana is communicated to man, but does not affect him?—That is so; it dies off; it does not live in his blood.

14341. Do you find the trypanosome in the body of the tsetse fly itself?—Of course, if you examine the blood from the stomach of a tsetse fly, naturally if it has fed upon an animal containing trypanosomes, you find them there quite readily, and you find them there for some five days; it takes about five days for the full feed of blood to disappear altogether from the stomach, and as long as there is any remnant of blood in the stomach you may find the trypanosome.

14342. Do you know at all the mechanism by which the tsetse fly communicates the trypanosome from itself into the wound that it makes in man?—No; I believe it is by simple mechanical transference. There may be a more complicated mechanism behind. All that we have been able to discover up to the present time is that in the proboscis, which is a well-marked tube (I have examined thousands of these probosces), up to forty-eight hours after feeding on an infected animal, you can often see living trypanosomes. Whether they wriggled up from the stomach, or whether there was any other mechanism by which they got, as it were, from the blood, or whether they simply remained there after feeding, is not known, and it is rather difficult to find out. But let us suppose there is a living trypanosome in that tube. Now, when the fly thrusts that tube into a new animal, it injects a certain amount of fluid. This is the cause of the inflammation of the part—it is the cause of the greater supply of blood to the part, just like the sting of a gnat. If, then, you have a tube with a living trypanosome in it, and the fly squirts down it a quantity of fluid, in all probability the living trypanosome will be thrown into the blood of the attacked animal, and when it is there it at once begins to multiply.

14343. The trypanosome inoculates you, in fact?—Yes, the tsetse fly inoculates you with the trypanosome.

14344. (*Mr. Ram.*) Is there any way of accounting for the distribution of this fly in Uganda? You spoke of its distribution as being very curious?—It is a very curious distribution, but it is very difficult to arrive at the reasons in nature for the peculiar distribution of animals. You will find on one hillside in England one particular species of butterfly, which you will not find in any other place. I do not know that any person can give the reason for that. Sometimes you may have a theory, but it is difficult to find the true reason. In this particular case they evidently depend upon the presence of clear water. We do not know anything in their natural history which would make them depend upon clear water.

14345. They are always found in the company of clear water?—Yes, if you have a river full of reeds, a marshy river, which is a common kind of river in Uganda, the fly never goes up there; it requires a river which has always lots of water in it; it must have forest and clear water; it will not live in grass alongside water; it will not live in a banana plantation alongside water. It lives where there are tall trees and jungle and clear water. Koch says, or is reported to have said, that they live on crocodiles principally, and that may be the reason for their distribution.

14346. (*Sir Mackenzie Chalmers.*) How far away do they fly?—In Uganda I do not think they are found, as a rule, more than 150 yards away.

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14347. (*Dr. Gaskell.*) The jungle must be close to water?—Yes.

14348. So that if you cleared away all the jungle close to water you would clear away the fly?—Yes.

14349. That is being done at Entebbe, is it not?—Yes, and at ferries, and such-like places.

14350. (*Chairman.*) You have told us that you used experiments on monkeys for the purpose of finding this trypanosome and identifying it as the cause of the disease, and I understand that you also used experiments on monkeys for the purpose of testing whether the tsetse fly was the distributor?—Yes.

14351. Do you consider that those experiments were necessary in order to make the discoveries which you believe you have made?—I think so. It would be very difficult to imagine any way of proving that the tsetse fly carries the disease unless you have an animal to experiment with. You could not do it on man, and after finding the trypanosome in the cerebro-spinal fluid and in the blood of man, you would have stuck—you could not have gone a step further. It was only by using the monkey in place of a man that you could prove that this thing did give rise to a disease, which was similar to sleeping sickness, and also that the flies feeding upon the natives on the lake shore could give the disease to a monkey. Further, it would be difficult to prove that flies fed on a sleeping sickness patient could give rise to the disease in healthy monkeys unless you used animal experimentation. I do not think you could have gone any further than the mere discovery.

14352. You were first set on inquiry about the capacity of the tsetse fly by the recollection of what had happened with the tsetse fly in nagana?—Yes.

14353. And having done that and looking upon it as a suspicious individual, then you got the chiefs to give you this information in regard to the distribution of the flies?—Yes.

14354. That was not a matter of animal experiment?—No.

14355. That seems to have satisfied your mind that the tsetse fly did convey the disease?—Yes.

14356. So that so far you may have used experiments as confirmatory of your theory about the tsetse fly, but you were almost morally certain, before you made those experiments, that the tsetse fly was the distributor?—These animal experiments were made to begin with; the chiefs were called in after the experimental work had been done. Of course, there were no more experiments made on the monkeys than were supposed to be necessary to prove that the trypanosome was the cause of sleeping sickness.

14357. I was mistaken. I thought your experiments to find the distributor of the trypanosome had been subsequent to your giving these instructions to the chiefs?—No.

14358. You gave those instructions in consequence of finding the trypanosome in the tsetse fly?—In consequence of finding that this tsetse fly could convey this trypanosome to healthy animals. The next point was to find out why the distribution of sleeping sickness was so peculiar.

14359. Do you think that in regard to this sleeping sickness as in regard to nagana, the animal experiments were valuable and essential to confirm your discovery?—Yes, I think so.

14360. As to remedies, you have not gone into that in regard to sleeping sickness?—There has, of course, been an enormous amount of work done on sleeping sickness in regard to remedies, but I have not done any.

14361. I understand that you have not made a special study of remedies?—No. Not in sleeping sickness.

14362. I think we shall have another gentleman from Liverpool who has done so.

14363. (*Sir William Collins.*) I should like to ask you a few questions to supplement your very interesting evidence. In the first place, as regards the experiments that you have made in these various researches and experiments, should I be right in thinking that they were very painful experiments?—No, none of these experiments were painful. Most of them were done by a hypodermic needle being inserted into the skin, and a small quantity of fluid injected.

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14364. I rather gathered that you would tell us that your experiments were mostly confined to punctures with the hypodermic needle—drawing blood, feeding experiments, and so forth?—Yes.

14365. Was there anything more painful than that?—I do not think so.

14366. And would the result in the animals in which there were pathogenic results be painful illnesses?—No, I do not think so. In the case of Malta fever most of the monkeys recover after a week or two of fever. A small percentage of them die, and, of course, they must have a disagreeable feeling due to the high temperature, and those who recover may have a chronic disease as in man, lasting for some months. I dare say they may have symptoms of arthritis, slight swelling of the joints, and that kind of thing, just as in man, but I would not say that in the monkey there was what you would call an agonising or even painful disease.

14367. Have any of your experiments necessitated the performance of anything like surgical operations upon animals of any extensive character?—No, I have not been associated with many experiments of that kind. I worked with Wright at one time on the leucocytes of the blood. We made a few experiments on placing a canula in the thoracic duct of dogs and removing a certain amount of fluid for the purpose of examining it; and I have also performed a few operations, such as removing the spleen of dogs, to find out what effect it would have upon the leucocytes of the blood; but those were all done under chloroform, and the dogs were killed after the thoracic fluid was drawn off. I do not think that any experiments, in the performance of which I have been associated, have been what one would call painful experiments.

14368. I understood you to say that as the result of your experiments with Malta fever you claimed that you had been led to a system of prevention of that disease, on what you call broad lines rather than on serum lines?—Yes.

14369. Would you tell me exactly what you mean by that?—If, after a great deal of work, you discovered a serum which would immunise a man against Malta fever, you would require to inject each of the men several times with the serum, and that would be what I call narrow lines. If you give an order that no goat's milk is to be used in the Army or in the Navy, and you stop the disease at one blow, like cutting off the head of a snake, I call those broad lines.

14370. I thought that was what you had in your mind. When you claimed to have saved the garrison and the Navy, it was by the method of stopping certain articles of food, not by methods of vaccinating or inoculating?—That is so.

14371. (*Sir Mackenzie Chalmers.*) Or medicine, or cure?—Yes, that is the kind of way of stopping disease I like to aim at always.

14372. Prevention?—Yes, it is far better.

14373. (*Sir William Collins.*) I think it has been called the stamping out and stamping in method?—Yes; stamping out.

14374. Has an attempt been made to prepare a serum against Malta fever?—Yes, and it has up to the present failed. Eyre, in this country, tried to prepare a serum from the horse, but his conclusion was that the serum was of very little use.

14375. Did it sometimes produce rather alarming symptoms?—I did not see the experiments, and I do not know. You will find his paper in the reports of the Royal Society on Mediterranean fever, but I do not think he gives any alarming results in that paper. I do not think there were any alarming results; they were merely experiments made on guinea-pigs.

14376. On page 120 of Part VI. of the Reports of the Mediterranean Fever Commission I find that he speaks of the disquieting results recorded by Lieutenant Bousfield?—Is that serum or inoculation fluid?

14377. That is in the Report of the Commission?—Was Bousfield working with serum or with inoculation fluid?

14378. It is under the heading of "Sequelæ of Inoculations"?—Those are very different things; the one is the serum of an animal immunised against the disease; the other is simply dead bacilli injected under the skin of a man to bring about the same sort of changes in a minor degree that the man would get by having the disease itself.

14379. My question was rather whether any of the methods, which you spoke of as narrow lines, had resulted in any disquieting symptoms?—I do not think so. The method of immunising by an injection of the dead *micrococcus melitensis* has been tried to some extent; that is to say, Wright has tried it, Bassett Smith, of Haslar Hospital, tried it as a curative agent, and several others; and we tried it in Malta, but up to the present I should say with very little result indeed, either as a curative or as an immunising agent.

14380. You compared the mortality of Malta fever with that of typhoid, and I understood you to say that although Malta fever was relatively rarely fatal, it caused a prolonged amount of suffering and ill-health; but could you give us some proportion as to the case mortality of Malta fever as against that of typhoid?—I do not suppose that the mortality of Malta fever exceeds 2 per cent., as a rule; whereas, of course, in typhoid you may have 10, 15, or 20 per cent.

14381. In the pamphlet which you have handed in you give the total admissions of Malta fever, simple continued fever, and enteric fever among the troops from 1897 to 1905, and you call attention to a drop in the Malta fever admissions from 1905 to 1906 of considerable magnitude; is that not so?—The preventive measures came into being on the 1st of July, 1906, and it was after that that there was a big drop in the number of cases.

14382. Then, during the first half of 1906 were there no preventive measures at work?—No.

14383. Could you complete the figures for 1906, because these are only to the end of October, and if the preventive measures began on the 1st July, and you give the figures only to the end of October, that would only cover a very short period?—I have not got the figures with me. We have them in the office, of course. But I have the figures for 1907. You must remember that when you start preventive measures of this kind naturally there are a number of men who are sickening, as it were, for the disease. The incubation period of Malta fever is probably in some cases long—a month or two months, it is supposed—a man may be carrying the micrococcus without evincing the disease, so that after you begin your preventive measures you expect, of course, a certain number of cases to occur for some time to come. Accordingly, in that Chart 2, in July there are still a number of cases—there are ten cases, which gives an annual ratio of 18.6. In the chart from 1899 to 1905, in July there are 235 cases in the seven years, giving a ratio of 47.8. In July, 1905, there were 77 cases.

14384. I was confining my question to Table I. on page 4. There you see that the total admissions of Malta fever in 1905 were 643?—Yes, that is the same chart as Chart 2, on page 10.

14385. I daresay. Then the figures there given for 1906 to the end of October are 147. I understand from you that the preventive measures began on July 1st, 1906?—Yes.

14386. I was anxious to know whether you could complete the figures to the end of the year 1906, or whether you can say that the drop from 643 in 1905 to 147 in 1906 to the end of October could be attributed wholly or in part to the preventive measures, seeing that they did not begin, I understand, until the 1st of July?—In 1905 the total admissions of continued fever were 1906.

14387. I am speaking of Malta fever?—Of Malta fever there were in 1905, 643, and in 1906, up to the end of October, 147.

14388. But do not you see that there was a great fall from 643 cases in 1905 to 147 down to the end of October, 1906? I understand from you that the preventive measures began on the 1st of July?—Yes.

14389. So that, really, during the first half of the year 1906 those preventive measures could not have been operative?—In the first half of 1906 there may have been preventive measures to some little extent, because the thing was getting known, but it was only about the beginning of July that the order came out that no goats' milk was to be used under any circumstances whatever. It would be tinned milk in the year before that they had been using, or milk that they tried to boil or sterilise.

14390. But in order scientifically to attribute a particular fall of a disease to some particular cause, it is necessary to eliminate other causes which might have been at work, is it not?—Yes. But I think this thing is so clear, and so clearly dependent upon the

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milk, that I shall be obliged if you can show me any fallacy.

14391. Then will you tell me, do you think that the whole drop in admissions of Malta fever from 643 in 1905 to 147 down to the end of October, 1906, is attributable to those preventive measures?—Yes.

14392. Although I understand you to tell me that those preventive measures did not begin until the 1st of July?—Yes.

14393. (*Dr. Gaskell.*) Have you not stated, on page 11 of your pamphlet, that in July, August, and September, 1905, there were 258 cases, whereas in the same months of 1906 the numbers fell to 26?—Yes, and I have said that the preventive measures only came into use about the beginning of July, 1906, and the remarkable diminution is at once seen.

14394. (*Sir William Collins.*) I will not trouble you further on that point. If you would still look at that table on page 4, you will see there is a great drop in admissions of continued fever in the same period from 1905 to the first ten months in 1906 from 1,199 to 462. To what do you attribute the fall in simple continued fever; would that also be due to the preventive measures that you adopted?—Yes. Simple continued fever is one of those vague terms in which is probably included a number of mild cases of Malta fever, and also mild cases of enteric, so that anything which would lessen the cases of Malta fever or lessen the cases of enteric fever would have an effect upon the number of cases returned of simple continued fever.

14395. Does the same answer apply to the next column, where the enteric fever fell from 64 to 9?—To answer your question categorically, I should say no, because enteric fever is a more definite entity than simple continued fever; but it also is a difficult fever to diagnose from Malta fever. When I went out to Malta first, every severe case of Malta fever was called enteric, and all the milder cases were called remittent; so that, doubtless, in spite of the excellent diagnoses made by medical officers of the British Army, cases of Malta fever do get returned as enteric in spite of all the care that is taken, because the symptoms of the two diseases are the same for some time. I daresay that a man with experience at the end of either of the two diseases would give a very accurate guess whether it had been Malta fever or enteric; the longer duration of the one, perhaps, and these arthritic pains and swellings of the joints in a well-marked case, would help to make the diagnosis, but people with less experience are very apt to say: "This must be enteric; the man has a black tongue, high temperature, and a little delirium"; and it is diagnosed as enteric. And it is a difficult matter, as you know, to change the names of diseases in the Army. There is a certain amount of changing names in books and that sort of thing involved, and in many cases I daresay the diagnosis remains, and that might account, to some little extent, for that nine among the enteric fever.

14396. You think that the preventive measures in respect of the prohibition of milk put in force in July, 1906, are responsible for the drop in all these cases of Malta fever, simple continued fever, and enteric fever from 1905 down to the end of the first ten months of 1906?—Why not? Enteric fever is very often carried by milk. If you blot out the chief cause of Malta fever—namely, milk—you will also make a difference in enteric fever, because you must remember that as the Malta water supply is beyond reproach, enteric fever would be carried by flies contaminating milk, and that kind of thing, I should say, frequently. We did not investigate into enteric fever, but if you blot out the whole of Malta fever you will make a difference in simple continued fever and enteric.

14397. You blot it all out?—If you blot out the whole Malta fever.

14398. Do you mean that there is so much error in diagnosis that the whole typhoid will disappear if these measures are put in force with regard to Malta?—No, I do not think so. I do not think that enteric is carried only by goats' milk. I imagine that a fair percentage of enteric fever is caused in Malta by milk, and if you blot out all goats' milk, and only use condensed milk, say from Switzerland, in tins, you will blot out a certain amount of enteric, but you will not blot it all out, because enteric does not depend altogether, I should say, on milk. In fact, we do not know all the routes by which enteric is carried. I should be very happy to work at that most important question, but enteric fever has never been subjected to as searching an investigation as this Malta fever; and at the present time we do not know exactly how enteric fever is carried, especially in different parts of the world. In England you have often water-borne enteric, or you have an epidemic when it is milk-borne. In India you may have an epidemic where you cannot say it is from water or milk, but you must go in for some other theory, such as contamination of food by flies. In enteric fever it is fairly unknown as to how the infection is carried from one case to another.

14399. (*Sir Mackenzie Chalmers.*) There are probably many channels in England?—Yes, probably.

14400. (*Sir William Collins.*) With regard to the special case to which you called our attention, of the "Joshua Nicholson," and the goat's milk on board ship, the full story is told, is it not, in Part VII. of the Reports to which you have already alluded?—Yes, by Staff-Surgeon Clayton, R.N.

14401. Did I rightly understand that all the crew of the "Joshua Nicholson" were affected?—No, there were several of the crew who were not affected, so far as one can make out. It is all written at very great length by Clayton, and I think the general result that is brought out is that the men who drank that milk in any quantity got the fever.

14402. What happened to the goats at Antwerp?—We have everything that is known about the matter in that paper. We know where the goats were kept; they were kept in some kind of enclosure in Antwerp; but we failed to trace any fever cases in Antwerp, and we failed to trace any cases of fever between Antwerp and America.

14403. I just wanted the story completed. I think that is so?—It is all put in that paper, so far as I remember, but that is my impression. We could not follow these cases any further; we could not get hold of the people after the goats left the "Joshua Nicholson."

14404. I find that at Antwerp: "The staff of the quarantine station and many individuals in the neighbourhood are said to have partaken of the milk, both raw and boiled, during the five days the goats were interned here, but no information can be obtained of the subsequent occurrence of cases of illness resembling Mediterranean fever." And, further, "The s.s 'St. Andrew' carried 30 cattlemen and the three goat herds, and Mr. Thompson, in addition to a crew of 30 men. Most of these drank of the milk, but the master of the ship and also his owners state that none of the men suffered from any illness"?—The captain of a sea-going ship is often not very desirous of giving information; it may give him trouble, and that kind of thing. As a matter of fact, any person reading the history of that "St. Nicholas" epidemic cannot come to any conclusion but that it was the milk of the goats that caused it, and because there are hiatuses in the story that does not, so far as I can see, weaken that evidence. If you take 30 monkeys, and give them all one drink of milk containing these micrococci, and the great majority of them take Malta fever, I think that is pretty conclusive. A judgment is formed, as a rule, not from one fact but from several. There are a series of arguments that come and affect your mind, and in the end you have a certain opinion. My opinion is absolutely fixed that the goats' milk gives Malta fever, as I think I have made clear to the Commission.

14405. Am I right in saying that recently, in the "Times" of September 12th, some attempt was made to pluck the laurels from your crown in regard to this discovery?—There was a letter written by a retired officer of our own corps, but he is a well-known gentleman in our corps, and I was the unfortunate means of breaking him about two years ago. I was sent out to Barbadoes, where things were not going on well, and the result of my inquiry was that he was put on the sick list, and sent home, and left the service; so that anything which he would say about me I should say must be received with caution.

14406. Was not the claim put forward by Colonel Fitzpatrick that Captain Hughes was the discoverer?—Yes.

14407. Yes. I only wanted to give you an opportunity of stating it. The claim made on behalf of

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Captain Hughes was that he had priority in regard to the discovery which you have announced to the Commission of the goats' milk as the cause of Mediterranean fever. Is that not so?—Yes.

14408. To what did Captain Hughes attribute the Malta fever? He worked greatly at it and published a work on the subject?—Yes, he published a large work on the subject. He attributed it to effluvia from drains.

14409. I think his words were: "A specific micrococcus emanating during hot weather from a saprophytic existence in soil polluted with the fæces and urine of those suffering from the same disease"?—Yes, that is the danger of a scientific man, instead of using experiments, going in for theory and speculation.

14410. Do you say that Captain Hughes did not make experiments on animals?—He made a few—some one or two experiments, and he did not do much good.

14411. Should I be wrong in saying that Captain Hughes made many experiments on animals?—I do not know that he made many; I think he only made a few.

14412. Have you seen his book?—I knew him very well. He was a pupil of mine at Netley.

14413. He is no longer living?—No. Captain Hughes was a great friend of mine, and I speak with great respect of him; but he had not that particular stamp of mind which we call the scientific mind. He was not able as it were to distinguish very clearly between speculation and fact. As a matter of fact, his book, which is a large book, does not contain a single sentence which advances our knowledge of this particular disease. He was very industrious and a very nice fellow indeed, but he had not that particular thing which the gods give a man to enable him to do scientific work—investigation work—advancement of knowledge work.

14414. Did Dr. Allan McFadyean possess that rare gift?—I do not know his work very well.

14415. Did he not fall a victim to inoculation in the laboratory with typhoid and Malta fever?—He is said to have died of Malta fever on account of experimenting with it in the laboratory.

14416. Was any serum used in his case?—I do not know. He had been working, of course, for years at typhoid inoculation.

14417. At the Lister Institute?—Yes.

14418. In regard to nagana, do I rightly understand that in your opinion the trypanosome is transferred by the tsetse fly from wild animals to domesticated animals?—It must be so, I think.

14419. And that in wild animals it appears to be non-pathogenic?—So far as we know it is non-pathogenic.

14420. But in domesticated animals it is pathogenic?—In certain domesticated animals.

14421. And in man is it pathogenic?—I have never seen a case in man.

14422. That would point, would it not, to the great importance of the influence of soil upon the seed that you implant in it?—By soil you mean the human blood and tissues?

14423. Yes?—If you put it in that way, of course, but you can only put these things boldly. One animal is susceptible to the disease, and another animal is not. The house mouse is susceptible to anthrax, the field mouse is not. The two animals look very much the same; with any chemical analysis or microscopical examination you cannot see any difference between the blood and tissues of the two animals, but one is susceptible and the other is not. It is a part of Nature that is too hidden for us to be able to examine with our limited intelligence.

14424. Then as regards sleeping sickness, I think you mentioned that before the discovery of the trypanosome, a claim had been set up for a streptococcus as the cause?—Yes, by the Portuguese, and by the first Royal Society's Commission.

14425. That was very stoutly supported at one time, was it not?—I never believed in it.

14426. Did not Castellani support it?—I was a Fellow of the Royal Society and a member of the Sleeping Sickness Committee when his paper came home giving this coccus as the cause, and I said to the committee that there was absolutely nothing in the paper which amounted to anything like proof. It wanted animal experimentation. I wondered why he had not been able to give this sleeping sickness, by means of his particular coccus, to some animal. He showed no signs of ever having tried to do so.

14427. Were no experiments performed by the Portuguese Commission?—I think so, but I must say that I have not read the Report of that Commission very thoroughly, because it is written in Portuguese.

14428. And did they point to this streptococcus as the cause?—Yes. Castellani also pointed to it.

14429. And Castellani afterwards changed his mind, did he not?—He changed his mind after I wrote my paper.

14430. No doubt the influence of the divine gift?—In that particular case I should say that it was great luck my having worked for two years at a similar disease. I gave Castellani the paper describing the observations made by us during the three weeks he remained in Uganda after my arrival. I gave him that paper to take with him to London, in order that he should not go back empty-handed. In the interests of scientific history he ought to have associated my name with his, or at least described in the text the part I took, but he did neither.

14431. You point out the importance of differentiating and avoiding fallacies. I am anxious to know whether, besides this streptococcus, there was not at one time another cause asserted very strongly, and held by a great many as the cause of sleeping sickness?—*Filaria perstans* was one.

14432. It was found in a negro in London?—Yes, I think so.

14433. And then found in many others?—I should say, judging from the natives that I had to deal with in Uganda, that if you looked sufficiently long you would find it in 100 per cent., in the same number as you find the trypanosome, showing the difficulty of distinguishing between two parasites. The one is evidently a harmless parasite, living in the blood of man, just as Nagana trypanosomes live in the blood of wild animals. Here you have a parasite, and 100 per cent. of the natives have it in their blood.

14434. Healthy natives?—No, many of these natives had sleeping sickness, and we used to find the *perstans* in them almost as regularly as the trypanosome.

14435. Was it also found in healthy natives, and in the Indians in America?—Yes, and it is found also in natives living outside the sleeping sickness areas, and in certain sleeping sickness areas where there was no *perstans* found, the trypanosome was found.

14436. Do you know Sir Patrick Manson's work on tropical diseases?—Yes.

14437. He says: "In dealing with newly-discovered parasites, our experience with *Filaria perstans* should serve as a warning against precipitancy in drawing conclusions from the mere fact of concurrence." Do you agree with that?—Certainly.

14438. And he says: "Moreover, what we already know about some African trypanosomes should also make us hesitate in definitely committing ourselves to a trypanosome as the cause of sleeping sickness." You do not agree with that?—Well, you see, I look upon Sir Patrick Manson as what we call an armchair naturalist. He lives here in London, and is very clever at reading papers and prophesying what is going to happen, but he has not the practical knowledge; he does not go and work at the thing practically on the spot; and if a man is going to write a text book he cannot expect every word in the text book to be true, or even to be fairly reasonably true. There must be a lot of padding and nonsense in a text book. A text book is the lowest form of literature that I know of.

14439. I take it that you do not agree with Sir Patrick Manson when he says: "Experimental evidence, therefore, so valuable in the settlement of such questions, has in this matter, to say the least of it, been far from conclusive." That is referring to sleeping sickness?—Is that five or six years ago?

14440. It is the year before last?—I was opening a discussion on sleeping sickness some three years ago in Oxford, at the meeting there of the British Medical Association, and I quoted Sir Patrick Manson on sleeping sickness, he having opened a discussion on the same question, I think, the year before. I put down all the conclusions that he had come to, and I said:

"Every conclusion of a year ago is now proved to be false except one, and that is that medicinal treatment has no effect." Unfortunately, that still remains true. All the other conclusions were absolutely wrong. I put down in the same way, under separate headings, some ten or twelve statements, and I asked the British Medical Association to bring these up a few years later, and find out how many of them remained true. I think at the present time all mine remain true.

14441. How long ago was that?—1904, three years ago.

14442. What about the medical treatment? Does not Koch claim that a subcutaneous injection of arsenic is beneficial?—Yes, but Robert Koch is getting an old man, and he is much frequented by interviewers, and it is very difficult to get an interviewer to repeat exactly what you say.

14443. You do not accept the crocodile?—No, I do not, and I do not think Koch himself does. He has never himself said that atoxyl will cure. He said that he had every faith in it, and so he gave the lay mind to understand that he was curing them by the hundred, but I have never believed it, and I do not think that any person who knows the subject has believed it; and I think he has gone back to Berlin very disappointed that the atoxyl treatment has been useless.

14444. You think that attention should be directed rather to destroying the haunts of the tsetse fly than to an onslaught on the crocodile?—That is the broad way. I think myself that in a place like Uganda, especially after the population has been so reduced, if the Government and the chiefs made a big effort, they could take the whole of the natives out of the sleeping sickness areas, and put them in healthy places, and if they could do that, theoretically in forty-eight hours there would be no more sleeping sickness; there would be no *fresh* cases of sleeping sickness in Uganda. The tsetse fly only carries infection for forty-eight hours, and if you could remove the reservoir of the disease, in forty-eight hours every Glossina palpalis in Uganda would be harmless, and if you did not expose affected people to the tsetse fly again, then sleeping sickness would be blotted out by a wave of the magician's wand sort of business.

14445. If you were made plenipotentiary there would be no fear of its spreading in East Africa or down the Congo or the Nile; is that so?—It is mainly a question of money. If you know the distribution of the fly, and you know the natural history of the disease, you can do a great deal. The great thing in all these cases is to know the natural history from beginning to end, and not till then to try to do anything. You require to keep the people away from the sleeping sickness areas until the healthy-looking ones have developed the disease; you require to keep them away in the interior of the country. Those who are affected by sleeping sickness must, of course, never go back to the fly area. If at the end of forty-eight hours you sent back the population to the lake shore you would not be able to distinguish between the healthy and the unhealthy; you would therefore require to keep the sleeping sickness area population away from the tsetse fly for several months.

14446. (*Mr. Ram.*) You must give the fly perfectly healthy food for forty-eight hours?—You must give it either healthy food or none at all. At the end of forty-eight hours it is harmless.

14447. Those whom it had infected might again go on and develop the disease afterwards?—Certainly, after, let us say, six months.

14448. Affording unwholesome food again at the end of the six months?—Yes; you require to keep the place clear for some time, but I think at the present time it is not out of practical politics to keep the fly area of Uganda clear of population for a certain time. The Uganda natives are docile, and their chiefs are intelligent, energetic people, so that I would not look upon it now as outside practical politics to stamp out sleeping sickness in Uganda.

14449. (*Sir Mackenzie Chalmers.*) Does Sir Patrick Manson still hold the opinion that sleeping sickness is not due to a trypanosome, or has he altered his mind in any way, do you know?—I should say he has altered his mind.

14450. At any rate, his opinion at that time was one of scepticism. He thought it might be due to a trypanosome, or it might be due to other causes. Was that it?—It is impossible for me to say.

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14451. You do not know how far he had studied the subject at the time when he gave out these dicta?—I do not know.

14452. May I take it generally that, in your opinion, without experiments on animals you could not have discovered the cause of Malta fever, and that without experiments on animals it would have been a mere chance if you had attributed Malta fever to the milk of the goat. Would you go so far as that, or not?—Yes. As I said before, it would be an accident. Two epidemiologists were sent out to Malta to work at it, from the statistical standpoint, but they both failed. Perhaps, if a third had attacked the question, the third might have succeeded. But I say that the other way is the proper and reasonable way.

14453. It is not only the proper way, but it is the way that you pursued, and it was by means of animal experimentation that you came to these conclusions as to the cause of Malta fever?—Certainly.

14454. I am not sure that we have them, but can you give us the figures at all for 1907. Do you know what the effect of stopping the milk has been in 1907?—Yes, certainly. I put these figures down here, because it is very striking. I saw the Director-General of the Navy a few days ago, and he told me that they had not had a single case in the Navy this year.

14455. What about the soldiers?—Among the soldiers we had two cases in January, two in February, one in March, one in April, one in May, two in June, none in July or August, and only one in September; that is ten cases. These are the official figures, but I have received private information from Malta showing that at least 3 of these are readmissions; deducting these, therefore, there have been 7 cases this year.

14456. Can you give me the total for the same months in the year before?—From January to September, 1906, there were 151 cases.

14457. Then take from January to July, 1905 and 1906—I should like to get the three years. Perhaps you can give it later on?—From January to September, 1905, there were 538 cases among the soldiers alone.

14458. Can you also give us the corresponding months for 1905?—I have not got that here, but will supply it. No change has taken place in the incidence of this fever among the native population in Malta this year; it is not a particular year.

14459. The soldiers and sailors having been deprived of goats' milk, you have this curious fall in the incidence of Malta fever?—Yes.

14460. There have been a few cases amongst the sailors in 1907?—Yes, there have been 10 cases, or, deducting readmissions, 7.

14461. But there have been 7 or 8 cases tabulated as Malta fever?—Yes.

14462. It is always possible, I suppose, that those soldiers in the town may have access to goats' milk?—It is impossible to prevent their getting it, because in those small public-houses soldiers very often have egg-flip, a glass of milk with some brandy in it. It is rather a common thing if they are out late at night.

14463. Apart from stopping the supply of goats' milk to them as soldiers, have they been forbidden in any way to touch milk in Malta?—They have been lectured a great deal on the subject.

14464. Do you yourself attribute the few cases that have happened this year to the probability of milk infection?—Certainly. Some of the cases which have occurred this year are distinctly returned as being due to the drinking of milk.

14465. There are conceivably, I suppose, other ways? You say, for instance, that the micrococcus is excreted with the urine?—Yes.

14466. Is it possible that there could be contamination in that way; for instance, in the case of drying urine, you might get it through the air?—We held that theory for some time rather strongly.

14467. But you have no evidence in support of it?—We could not get any evidence in support of it—not in nature. If you grossly contaminate dust with the *micrococcus melitensis*, and blow it about a room in which a monkey is living, you will give the monkey the disease; but the Commission never could give the disease by blowing dust gathered from natural positions, say, in the neighbourhood of urinals, or from fever wards, or places which might be contaminated. Major Horrocks, R.A.M.C., a member of the Commission, made many experiments on these lines. He

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never got any evidence in favour of the dust theory except under artificial conditions. And naturally if you take billions of micrococci, and mix them with a small quantity of dust, it is like feeding an animal; it gets its tongue all covered and its food all covered; it is like a feeding experiment; so I do not think infection by dust takes place once in 1,000 times. We cannot tell, of course, but there is no proof that dust causes the disease.

14468. So that, so far as you know, the disease is communicated purely by food, and not by inhalation?—Purely by food.

14469. Possibly you inhale the micrococcus, and it gets destroyed in the lungs?—If you inhale sufficient then that would be a feeding experiment, because it sticks in the mucous membrane of the nose and mouth and is swallowed. But no doubt if the micrococci reached the lungs they would be absorbed into the circulation and not destroyed.

14470. But before the disease germinates it has to get to the alimentary canal?—No, it gets from the alimentary canal into the blood, and it grows in the tissues and especially in the spleen. There is no evidence that it grows when it is in the alimentary canal.

14471. Where does it start?—It is such a small thing that it may be absorbed by the mucous membrane of the tonsils. In my opinion, it may be absorbed by any mucous membrane.

14472. It starts into the blood, you say?—The moment it gets into the lymphatic system it passes, probably, from there to the blood, and from there to the different organs, settles down in the spleen, and grows there, and gives rise to a certain poison, and so on.

14473. May we take it generally that if remedies are to be found for those diseases experiments must be made on animals before they are made on man?—Certainly.

14474. For instance, when a toxyl was suggested as a remedy, was it tried first on animals or first on man?—You ought to have Professor Cushny here to answer that. He is a pharmacologist. I do not know the history of drugs in that way.

14475. You have not studied these tropical diseases with reference to medicinal remedies yourself?—I have done a certain amount. I tried, for example, arsenic and other drugs on nagana when I was in Zululand, and I tried without success many drugs on Malta fever. I did not try any medicinal treatment on sleeping sickness, because I was only in Uganda seven months.

*14476. You were investigating merely the cause, and not the treatment?—Yes. The Commission afterwards, of course, did try different remedies, especially, I think, Captain Grieg, I.M.S., Captain Gray, R.A.M.C., and Lieut. Pollock, R.A.M.C.

14477. Were you a member of any Plague Commission?—I am a member of the Plague Committee of the India Office at present.

14478. Have you at all considered the Reports of that Commission as to the transmission of plague by rat fleas?—Yes.

14479. And in your opinion is it established that the rat flea is the ordinary mode of transmission of plague from animal to man, and from man to man?—Yes, that is my opinion distinctly. The ultimate proof, of course, the giving it to man, cannot be made, but except for that every step in the proof has been taken.

14480. Have those conclusions been arrived at by experiments with animals?—Absolutely.

14481. Could you have arrived at these conclusions without experiments with animals?—Certainly not.

14482. And, so far, by means of that knowledge, are we put on the right lines in the way of prevention of plague?—Certainly.

14483. (*Mr. Ram.*) If I gathered aright from you you discovered this *micrococcus melitensis* in 1888?—1886. I think the report was published in 1887 for the first time.

14484. There was no Commission until 1906?—1904. It lasted for about three years.

14485. Was anything being done in all these years to attempt to find out the cause of Malta fever or to stop it?—Captain Hughes worked from about 1893 to 1897, and I should say that he worked at the fever, but was unsuccessful in discovering the true cause; and, as you can see from the statistics, nothing was done to practically limit the cases. That is to say, Malt fever cases were getting more numerous in the Arm and Navy up to the year of the Commission, and was on account of that probably that the Commissio was formed.

14486. Is there any cows' milk at Malta?—Ver little.

14487. There are some cows kept there, are the not?—Yes.

14488. Have any experiments been made with regar to their milk?—Yes.

14489. Has it been found to be free from the *micr coccus melitensis*.—No, there was a suspicion that som of the cattle were affected. I do not remember wheth it was actually found in the milk, but there was som suspicion that some of the cattle were affected by Malt fever.

14490. You have told us that there has been almo an entire elimination of the disease from the Army an Navy now. Have you any statistics to show that th native population are still as subject to Malta fever they were previously?—I have a letter from D Zammit, who is a member of the Board of Health, an he informs me that the number of cases of Malta feve in the native population is as great this year as ever.

14491. The goat continues to be used for milk?—Ye absolutely. The Government have been able to do ver little yet. Malta is a very conservative place.

14492. The evidence now seems to be that the *micr coccus melitensis* is swallowed; that the contaminatio is given to man by swallowing the micrococcus?—Th infection is taken by swallowing.

14493. And you have almost eliminated the idea dust and that sort of thing being the cause?—Yes, think so, or mosquitos. The mosquito theory was als strongly held for some time, but we could not get an proof of the mosquito being the carrier.

14494. You were asked one or two questions abou sera. You have not done much with sera yourself?—No, very little.

14495. As I understand, when you found animals use ful, and, indeed, necessary, was when you were anxiou to discover what the particular micrococcus was tha was the cause of this disease. And was it by exper ments upon animals that you were able to discover i to the elimination of other micrococci that might b present?—What I meant was, that in investigating disease, the important thing is to be able to stat definitely that you are quite sure that a particula organism is the cause of the disease, and it is ofte very difficult to be absolutely able to say this unles you have an opportunity of animal experimentation.

14496. When you find that a particular organism does produce certain symptoms, and is followed in variably by certain symptoms, you arrive at the con clusion that that is the cause of that particular dis ease, and in order to test it you want to try it o different animals to see the result of your differen experiments. Is that where the use of animals come in?—Yes, that is one use.

14497. That is one. Further, when the disease ha been established, and you are anxious to know th cause of the disease, do you then inoculate animal with the blood or other secretion of a person know to be diseased in order to see the effect upon tha animal?—I do not think, as a matter of practica work, that that is very often done. You mean fo purely diagnostic purposes?

14498. I want to get it accurately from you, if yo can say exactly where it is that experiments on animal come in to be useful. I think I have got one instanc that you explained in your examination-in-chief ver accurately. I am not sure that I follow the secon one as accurately. You spoke about taking the serum from some infected patient, and inoculating animal with it. Why did you inoculate these animals; t see what?—I do not remember that I did say tha I took sera from any particular man and put it int an animal.

14499. I thought you said that from a good man natives you took their blood?—From wild animals i Nagana.

14500. But you took the blood of natives in sleepin sickness in different parts of the country, some part where there was sleeping sickness and some parts wher there was not?—I only examined it through the micro scope.

14501. You did not experiment on animals?—No, that was purely microscopical examination; we found by microscopical examination of the cerebro-spinal fluid and the blood of sleeping sickness patients that practically in 100 per cent. we could find this particular parasite. We then took 200 people from a part of the country outside the sleeping sickness area, and examined their blood in the same way—not the cerebro-spinal fluid in that case, but only the blood, and we found it in none of them. If you will only consider for a moment, that is a very strong argument in favour of the trypanosome being the cause of sleeping sickness. That was the beginning of the work. You must remember that we were under the disadvantage of believing that this trypanosome was well known, and that it was the cause of a disease called trypanosoma fever, which was supposed to be quite distinct from sleeping sickness. On account of that, we had to work, as it were, against the difficulty of a preconceived notion—we had to get rid of that—and it was only by these experiments that we gradually lost the preconceived idea, and took on the new one.

14502. (*Sir Mackenzie Chalmers.*) Did you ever inoculate an animal with the blood of a person suffering from sleeping sickness and containing trypanosomes?—Yes, on many occasions. First, we had to find out if monkeys would take the disease.

14503. With the result that, having inoculated the monkey with the blood of a person containing trypanosomes, sleeping sickness was produced?—Yes, that was an experiment to help to prove that the parasite is the cause of the disease.

14504. (*Dr. Gaskell.*) Could you tell us at all how the Malta fever spreads, or the *micrococcus melitensis* spreads, from goat to goat?—No. We know that if we put into a small space a certain number of affected goats, and a certain number of healthy goats, after a certain time some of these healthy goats will be found to be affected by the disease by close contact of that kind. If you put a healthy monkey and an affected monkey together, so that they feed out of the same dish, and the urine may contaminate their food, if you keep the affected monkey and the healthy monkey together for some time, you find that the healthy monkey gets the disease; but if you keep them separate from each other, so that they can only touch each other, and the urine of one cannot get at the food of the other, then they do not take the disease.

14505. There is one question that I wanted you to make clear to the Commission, and that was that in these cases of bacterial infection, bacterial disease, one of the important points is to inject a particular bacillus or micrococcus into an animal in order to see that it gets that disease. Is not that so?—Yes.

14506. When you do that, do you not inject both the fluid substance and the bacteria—you inject both?—Yes.

14507. Then it is open, is it not, for people to say that the disease was caused by the fluid substance rather than by the bacillus?—That is the reason why we try to cultivate the bacilli outside the body for several generations on a neutral substance.

14508. You also mentioned another point, I think, which I want to bring out, and that was the Chamberlain or Pasteur filter?—Yes, that is merely to separate the solid particles from the fluid.

14509. Does it effectually separate the microbe from the fluid in which the microbe is contained?—As a rule, yes. You can take a fluid containing millions or billions of bacteria—say typhoid bacilli—and filter that fluid through a Chamberlain filter, and not a single bacillus will come through. The fluid that comes through will be absolutely sterile.

14510. And what is the effect of injecting that fluid that comes through into an animal?—It may give rise to a temporary rise of temperature. There may be a little poison in it, there may be a few toxins of the bacteria in the fluid, and that would give rise to a temporary rise of temperature and a feeling of seediness, but it would not give rise to the disease itself.

14511. Whereas, injecting the microbes, on the other side of the filter, would give rise to the disease?—Yes, if the animal is susceptible.

14512. I think it has been tried again and again, and we may take it as proved, may we?—Yes, I think you may take it as proved. There are some toxins, of course, so powerful that a little filtrate without any bacteria will kill animals, but it kills them as strychnine kills them.

14513. It would not necessarily give the kind of disease you were investigating?—No. Tetanus, for example, is a very powerful poison in that way.

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14514. In these cases of sleeping sickness in monkeys, and also in man, do you think there is much pain when the animal is suffering from that sickness?—I think man may suffer at first to some extent from headache and from a feeling of uneasiness, but I do not think he ever feels much pain. I think in a very few cases, say 1 per cent., acute symptoms of mania may come on on account of the poison affecting the brain, and that, I should say, was very uncomfortable. I mean to say that the patient is in a constant state of restlessness, talking, laughing and throwing himself about, and sleepless, but I do not think there is any pain. With regard to monkeys, I do not think there is any pain at all, at least so far as one can see.

14515. They always go into a lethargic condition?—They get into an absolutely lethargic condition; they feel nothing.

14516. Do they waste very much?—No; man does not waste as a rule; he remains quite plump to the end.

14517. We have had a picture shown us here in the Commission of a dog that was infected with sleeping sickness, with the trypanosome, at Khartoum. It appeared in a very emaciated condition, and it was suggested that this dog had been in terrible pain in consequence of the disease. I was wondering whether you could say that that was likely or not to be the case. There was emaciation there undoubtedly?—I do not think that sleeping sickness trypanosome would cause emaciation in a dog, and I am quite sure, from all I have seen of dogs inoculated with the sleeping sickness, that they did not suffer at all. They are very refractory to the disease. But was it certain that this dog had sleeping sickness? I should imagine that it was one of the other trypanosome diseases.

14518. (*Dr. Wilson.*) You admit that this disease, Malta fever, is very widely distributed?—Yes.

14519. And you contend that its mode of propagation is entirely through drinking goats' milk?—Yes, in Malta we think it is; probably in 999 cases out of a thousand. These are only figures which are not worth anything, of course, merely figurative; but we feel sure that most cases of Malta fever are caused by drinking goats' milk, but we cannot assert that for the rest of the world. The evidence up to the present time is that where there is Malta fever there is a grave suspicion of goats being at the bottom of it—in the Punjaub and in South Africa.

14520. Do you refer to Maltese goats entirely?—I do not think that the Maltese goat has found its way to the Punjaub. I think there is certainly a suspicion that the Maltese goat finds its way to South Africa, because if they want to improve the breed of goat you send to Malta for some of the Maltese goats.

14521. Then you would only put this as the probable mode of propagation—not a certain mode. You would not say that it is the absolute sole mode of propagation?—In Malta I should say that it was almost the only mode of propagation. I cannot speak for places that I do not know anything about. I have a strong suspicion that the goat is mixed up with it everywhere.

14522. But is not this microbial fever, because you call it a microbial fever, very singular in this respect, that you have the micro-organism excreted by an apparently healthy animal, and it produces all these febrile symptoms when taken in food?—By man do you mean?

14523. Yes?—Yes. There is nothing very singular about it, because the one animal is less susceptible than the other. A goat does not suffer from fever and inconvenience by taking this thing inside it. Man does. But the micrococcus can live in the goat, and, curiously enough, it finds its way after a time into the mammary glands. It is killed out in the spleen, killed out in the kidneys, killed out in the liver, killed out in the blood; but it gets into the mammary glands, and remains there and multiplies for years.

14524. But is the mammary gland in a diseased condition; have you found it so at all?—I am now thinking of tubercle bacilli in cows milk. In the Maltese goat you have a very highly developed mammary gland, developed beyond all nature, and it is, as it were, the place of least resistance. There must be some reason for it, but I put it broadly, only that here you have a gland developed out of all proportion, a gland that almost touches the ground when the animal walks

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along—a huge over-developed gland. You can imagine that an over-developed gland of that sort would have tissues of less resistance than ordinary normal sized and normal glands. Therefore it is that in the Maltese goat the *micrococcus melitensis* is killed out of all the other organs, and remains in the mammary gland chronic, and established, so far as we can see, for years.

14525. Do the glands and teats show any signs of disease?—No, you cannot tell by examining a goat that is has Malta fever.

14526. So that they entirely differ from the tuberculous glands in the cow, for example?—Yes. The tubercle, of course, forms nodules, and the *micrococcus melitensis* does not form any nodules; it does not form any tumour formation, as it were, because the little nodules of tubercle are like little tumours.

14527. When you were out in South Africa, had you opportunities for personally inquiring into the effects of anti-typhoid inoculation during the South African War?—I have had a good deal to do with it. I wrote a report upon the subject for the Government which caused anti-typhoid inoculation to be stopped for the time being, because I did not think it was being done sufficiently carefully, and with sufficient forethought.

14528. Do you think it is of value as a preventive?—We—that is, the Royal Army Medical Corps, and especially Colonel Leishman, Captains Harrison, Grattan and Kennedy—are working at it just now, and we are getting good results, but I would not like to say at present whether it will be found of great practical use in practice. We had a very good example the other day (I do not know whether you know of it, but it is a beautiful example) in the 17th Lancers. Every regiment going out to an enteric area is carefully inoculated before it leaves this country, inoculated twice, with this anti-typhoid fluid. The 17th Lancers were inoculated, went straight out to India, and fell straightway into an epidemic of enteric. There were 63 cases in that regiment; 62 of these occurred among the non-inoculated, and only one among the inoculated, and that one case had only been inoculated once instead of twice—the man refused to have the second inoculation. These men were all living together under the same conditions, with the same food, and the same everything, and yet that was the result. That, of course, is a beautiful result. But then you see the men were inoculated twice a very short time before they became exposed to the poison, and we do not know yet how long this immunity will last. There is no doubt that there is a certain amount of immunity attained in plague, cholera, and typhoid by injecting the dead bacilli under the skin. It imitates the disease to a certain extent. The disease of typhoid fever is caused by the typhoid bacillus, and the fever and general effect are caused by the poisons excreted by the bacilli, and also held up in the bodies of the bacilli. So that if you insert 50 millions or so of these bacilli under the skin, and they all dissolve up, that man gets a dose of typhoid poison just as he would if he had typhoid fever, only in a smaller extent. Therefore it seems to me quite natural that these inoculations of dead bacilli should bring about a certain amount of immunity.

14529. Has not Strong, while working in the Philippines, stated that he does not believe in the dead bacilli, and has taken to inject living bacilli?—That was in plague. If you heat typhoid bacilli to 65 degrees Centigrade in order to kill them you destroy the toxin that causes the immunity; you therefore kill them at as low a temperature as possible, 53 degrees, or thereabouts, and then do not get the same effect as with the living bacillus. Haffkine used the living cholera bacillus when he started. And certainly if we could use the living typhoid bacillus it would be better, but he would be a bold man who would start anti-typhoid inoculation in the British army at the present moment with living typhoid bacilli.

THIRTY-THIRD DAY.

Wednesday, 6th November 1907.

MEMBERS PRESENT:

The Right Hon. the Viscount SELBY (*Chairman*).

Sir W. S. CHURCH, Bart., K.C.B., M.D.	Mr. A. J. RAM, K.C.
Sir W. J. COLLINS, M.P., M.D., F.R.C.S.	Mr. W. H. GASKELL, M.D., F.R.S.
Sir J. MCFADYEAN, M.B.	Mr. G. WILSON, M.D., LL.D.
Sir M. D. CHALMERS, K.C.B., C.S.I.	

Captain C. BIGHAM, C.M.G. (*Secretary*).

Mr. GEORGE GRANVILLE BANTOCK, M.D., F.R.C.S.ED., called in; and Examined.

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14530. (*Chairman.*) You are a Doctor of Medicine and a Fellow of the Royal College of Surgeons Edinburgh?—Yes.

14531. And you practice in London as a consulting surgeon?—That is so.

14532. And you are surgeon to the Samaritan Free Hospital for Women?—Consulting surgeon now.

14533. And a Founder and Fellow of the British Gynecological Society?—That is so.

14534. And a Fellow of the Obstetrical and other societies at home and abroad?—Yes.

14535. And you have practised a great deal and written a good deal, more particularly on abdominal surgery?—I have.

14536. And you have been asked to give evidence here by the chairman of the Parliamentary Anti-Vivisection Association?—Yes.

14537. I believe you are not a member of the Society?—I do not belong to any society.

14538. But you hold some views which affect the question of experiments on living animals which are well known, and that is the reason why you have been asked to come, I believe?—I think it is more particularly because of my views on bacteriology that I have been asked to give evidence, because I have never taken any active part in the question of vivisection, either for or against.

14539. I only said that you held some views which bore upon the question of experiments on animals?—It has some bearing.

14540. I understand your general view to be—I am going to take you through your evidence, of course—that bacteriologists are mistaken in the view that a great number of the most important diseases are proved to be due to the presence of a particular bacillus in the body?—I have a strong belief in that respect.

14541. And it would follow, I presume, that in your view all experiments upon animals made for the purpose of ascertaining and identifying the particular bacillus are useless?—For identifying the bacillus they may not be useless, that is to say, the bacillus may be characteristic of a particular condition; but my contention is that it is not the cause.

14542. You do not deny that it exists?—No, and it may be diagnostic of the condition, but I say that the bacillus is never the cause. My contention is that the bacillus is an accident of the disease—that is to say, it results from the disease, that the conditions are favourable for its development; for we are swarming with bacilli of various kinds, and it only wants certain conditions to favour the development of certain bacilli. That is the view I hold.

14543. I presume that if your view were adopted by physiologists generally they would be obliged to come to the opinion that a great many experiments which they at present make would be unnecessary?—I think so.

14544. Therefore I am only pointing out how in my view, and I fancy in yours, your view about bacteriology affects the question which we have to consider, which is as to how far experiments on animals are justifiable?—Undoubtedly.

14545-6. You are prepared to state to the Commission your views generally upon this subject?—Quite so. Like a good many other surgeons who had constantly to perform serious and delicate operations, I took a very keen interest in the system of what was called antiseptic surgery, which was introduced by the present Lord Lister some thirty-five years ago, and like them I tried it in my practice. The system was founded on the hypothesis that germs floating in the atmosphere fell into wounds, there developed and multiplied and produced all the evil effects which sometimes followed surgical operations. In order to combat these lamentable results this system, when it was fully exploited, included operating under an antiseptic spray,

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and treating all instruments, ligatures and dressings on antiseptic lines. In the numerous operations which I had to perform, I soon found that the antiseptic (carbolic acid) spray had an injurious effect on the delicate lining membrane of the abdomen and other tissues, and I gradually began to reduce the strength of the solution until at last I used only ordinary London water to wash out or irrigate wounds, and finally depended on clean hands, instruments, ligatures and dressings, without the aid of antiseptics. In other words, I, in company with a very few other surgeons, dispensed with so-called antiseptics altogether as not only unnecessary but injurious, and I may be permitted to say that I took a not inconsiderable part in the somewhat acrimonious warfare which was aroused in the attack on Listerism by reading a paper on "Hyperpyrexia (or excessive rise of temperature) after Listerian Ovariotomy." In the course of my cross-examination I should like to elaborate that point, namely the results of my reading that paper at a society in London. As is well known, what is called antiseptic surgery, or Listerism, was ultimately discarded, and the system which was eventually adopted was the aseptic, which is the absolute negation of the antiseptic system. But aseptic surgery, which may be defined as clean surgery, is certainly not the outcome of bacteriological research, as many scientists and most people still blindly believe; but a commonsense revolt against the illogical details of surgical practice to which bacteriology gave rise. At the International Medical Congress, held in Berlin in 1891, Lord Lister acknowledged his error with a frankness which all must admire, though some, like myself, have difficulty in understanding the tenacity with which he has all along clung to the modern doctrine of bacteriology. In his address to that Congress Lord (then Sir Joseph) Lister spoke as follows in reference to surgical details in my own practice and in that of the late Mr. Lawson Tait. "Dr. Bantock, whose remarkable series of successful ovariotomies may seem to justify his practice does not, I believe, prepare his ligatures antiseptically. The success achieved by Bantock and Tait without, it is said, the use of antiseptic means, proves a stumbling block to some minds." No doubt, so long as they hold to the germ theory. "I can see that while the measures" (comprehended under the term cleanliness) "to which I have referred are, so far as they go, highly valuable, it must be in itself a very desirable thing to avoid the direct application to the peritoneum of strong and irritating antiseptic solutions." This latter is in itself a strong justification of my abandonment of carbolic acid. He continues: "As regards the spray, I feel ashamed that I should ever have recommended it for the purpose of destroying the microbes in the air. If we watch the formation of the spray and observe how its narrow initial cone expands as it advances with fresh portions of air continually drawn into its vortex, we see that many of the microbes in it, having only just come under its influence, cannot possibly have been deprived of their vitality. Yet there was a time when I assumed that such was the case, and trusting the spray implicitly, as an atmosphere free from living organisms, omitted various precautions which I had before supposed to be essential." He then describes how in a case of operation "the air passed freely in and out of the pleural cavity in a cloud of spray," and he arrives at the conclusion that "it is physically impossible that the microbes in such air can have been, in any way whatsoever, affected by their momentary presence in the air." "If then," he continues, "no harm resulted from the admission day after day of abundant atmospheric organisms to mingle unaltered with the serum in the pleural cavity, it seems to follow logically that the floating particles of the air may be disregarded in our surgical work, and if so we may dispense with antiseptic washing and irritation, provided always that we can trust ourselves and our assistants to avoid the introduction into the wound of septic defilement from other than atmospheric sources." What these sources are we learn from his address at Liverpool, in September, 1896, six years later. "Hence I was led to conclude that it was the grosser forms of septic mischief rather than microbes in the attenuated condition in which they existed in the atmosphere that we had to dread in surgical practice." Here I may be permitted in passing to give expression to my admiration of the character of the man who can confess his error with such candour and honesty, seeing that such a confession of error must detract from the credence we should otherwise give to his later views. Would that his disciples were like minded! To proceed: What then are the "grosser forms of septic mischief"? "If," in the words of the late Dr. Campbell Black, "they are what is vulgarly called 'dirt,' then we are all agreed that to remove dirt (not, however, by killing it) and to keep wounds clean is perfectly scientific and proper treatment." What is this but the doctrine of "cleanliness," which I have advocated for so many years? Thus it will be seen that it only required that Lord Lister should have taken one step more to fall into line with me. For while he gave up the theory of atmospheric germs, he admitted that we may dispense with antiseptic washing and irrigation, and virtually came to accept the principle of cleanliness—one of the two principles in the enunciation of which I played no unimportant part, and which are now generally accepted in the case of ovariotomy. So much for antiseptic and aseptic surgery; and if I have alluded to this phase of the subject at some length, it is because I wish to dispel the belief, which is so persistently advanced, that aseptic surgery is the outcome of bacteriological research. Bacteriologists have long since discovered that in order to convert filth or dead organic matter of any kind into harmless constituents, Nature employs micro-organisms or microbes as her indispensable agents. Thus, in the modern septic tank, which is now so largely used in the treatment of sewage, it is the action of the micro-organisms, whether ærobic or anærobic, which dissolves the sewage, and it is the continuous action of these microbes which converts all manurial matter into the saline constituents which are essential for the nutrition of plant life. In the natural purification of filth-polluted streams, or in the conversion of dead animal or vegetable matter into the flora of the vegetable world, it is admitted that the micro-organism plays a beneficent part, and so I am prepared to contend that however these innumerable and infinitely minute vegetable organisms may be designated, they always play a more or less beneficent part when they are found to be associated with disease, and that, however characteristic any micro-organism may be of any particular form of infectious disease, it cannot be classed as pathogenic, in the sense that it is the actual agent causing the disease. The microbe in its relation to disease can only be regarded as a resultant or concomitant, and in that respect is of more or less value to the physician in assisting him to diagnose the form or kind of disease with which it is associated. But even in this respect there are abundant sources of error. For example, the bacteriologists themselves admit that there is frequently great difficulty in differentiating the tubercle bacillus from other bacilli belonging to allied groups or species, and the diphtheria or Loeffler's bacillus from the Hoffman or pseudo-bacillus. Again, the true Loeffler's bacillus is often found in healthy throats, and I may say also as a gynecologist that it is sometimes found in the generative passages of healthy women. On the other hand, in what may be termed to be undoubted cases of diphtheria (clinically) it is very often not found after continued and careful examination, and similar difficulties and uncertainties present themselves with regard to other so-called specific micro-organisms. Thus, the staphylococcus pyogenes and streptococcus pyogenes, which are supposed to cause suppuration, have been found by various observers in the vaginal discharges of healthy women, as also the bacillus coli communis and the bacillus typhosus. Dr. Stoker's well-known treatment of ulcerative conditions by oxygen gas incontestably proves that the staphylococcus and streptococcus pyogenes were not only necessary for healthy granulation (or healing), but that any interference with them by means of germicides retarded the healing process. In several severe operations which have been performed by myself, and in which the healing process progressed satisfactorily, without any disquieting symptoms, I have had specimens of discharge examined by competent bacteriologists, and with one exception so-called pathogenic organisms were found to be present in all of them. Is it not, therefore, reasonable to conclude that these micro-organisms, while they may be necessary for processes of healing, are certainly not causative of disease? While it cannot be denied that there must be a virus or *materies morbi* to account for the spread of every communicable disease, I contend that another strong argument against their microbial origin is the fact that in respect to several of the most infectious

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diseases no specific micro-organism has yet been found, in spite of the most minute and persistent research. Thus, no specific or so-called pathogenic organism has been discovered in small-pox, vaccinia, measles, scarlet fever, whooping cough, or hydrophobia, and it seems very singular that the tetanus bacillus, which is believed to be the sole cause of tetanus or lockjaw should be found so abundantly in manured or garden soil, or on the surface of roads fouled here and there with horse-droppings. It is also worthy of note that in the disease for which microbal origin is claimed to have been most fully established, the pathogenic organism is usually associated with necrosis or death of tissue. Thus it is now generally admitted that the tubercle bacillus is not found in the sputum in the early stage of phthisis, but only when necrosis of the lung tissue sets in. The diphtheria bacillus, again, is associated with the septic or membranous throat, the typhoid bacillus with ulcerated intestine, the cholera vibrio with the inflamed intestinal canal, and the plague bacillus with the inflamed bubo or pneumonic lung. In all these instances is it not logical to conclude that these micro-organisms, instead of being pathogenic, are playing the part of nature's scavengers? On this point Dr. Vicentini says: "Considering the assimilating activity of bacteria on the one hand, and on the other their reducing activity at the expense of histological elements in decay, I have often asked myself whether this ubiquity of the bacterial elements is not, perhaps, destined to perform some important service, as, for instance, the expurgation of the fluids to act as scavengers in relation to decayed particles or cells of the body. The simple fact that bacteria are present in the products or morbid seats does not imply anything specific, so long as their totally extraneous origin and their quite independent entity be satisfactorily proved. Losing sight of this truth, as simple as it is essential, one risks falling into strange exaggerations, and is likely to include the most varied complaint (even the ingrowing nail, according to Regnault) in the list of bacteriological infections." In order to illustrate more fully my contention that so-called specific diseases have not a microbal origin, I will now deal with two of the diseases already named in which no specific micro-organism has been discovered—namely, small-pox and vaccinia—and two in which a specific organism is persistently paraded as the actual *causa causans* of the disease—namely, tuberculosis and diphtheria. With regard to the first two, I may here fitly quote from my pamphlet, to which I referred at the outset: "Investigators have, all the while, overlooked the fact that these diseases are due to a specific material poison which can be handled, and can be conveyed, from one subject to another, by inoculation. There is no mystery about this as there is about the natural mode of transmission—or should I not rather say propagation?—of these diseases, or of such diseases as typhus, typhoid, measles, scarlatina, etc. Take the case of small-pox, for instance. We know that if we insert an almost infinitesimal quantity of the contents of a small-pox vesicle, at the proper stage, under the skin of a perfectly healthy subject, an enormous multiplication of the poison takes place, as exhibited in the innumerable vesicles which appear on the surface of the body, each vesicle filled with a fluid possessing the same virulent properties as the original vesicle—in other words, an attack of small-pox, presumably under the influence of what we, in our ignorance, call a chemico-vital process. That this extraordinary state of things should be due to the action of a special form of bacillus passes one's comprehension and is incredible. It is not a mere local action, such as is attributed to the staphylococcus or streptococcus, when either of them is said to produce suppuration. It affects the whole constitution of the subject, and reminds one very forcibly of the effects produced by snake or other animal poison—which we know to be not the product of a bacillus, but of an organ specially constructed to that end. Vaccinia, under the same process, produces such a change in the constitution of the individual subjected to it, that, in at least a majority of cases and for an indefinite period of time, it renders that subject proof against the inoculation of itself and even of small-pox. And here I may be allowed to express the hope that my views will not meet with a "conscientious objection," but be examined dispassionately. What is the nature of this extraordinary change which is wrought in the constitution of an individual by an attack of small-pox, vaccinia, or other of the eruptive fevers, such as measles, scarlatina, etc., and which, in a large majority of cases, renders the subject insusceptible of a second attack? It is all a matter of conjecture, and, so far as I know, no one has offered even a plausible explanation. Is it a change in the actual constitution of the individual, or does it merely affect the nervous system only? Of the other two diseases I take the case of diphtheria first. This disease is said to be due to the influence of a specific bacillus—Loeffler's. How does it happen that this bacillus can be found in the throat of a subject weeks—yea, even months—after all trace of the disease has disappeared? This doctrine has suffered much discredit of late from the fact that this bacillus of Loeffler's is frequently present in some exanthemata (rash), and also in healthy persons. A still "more striking example is afforded in cases of tonsillotomy, wherein, upon the incised surface, a greyish membrane is formed, in which the bacilli abound without constitutional disturbance or any sign of diphtheria." I anticipate the argument that, if you allow some of the discharge from the throat of a subject of this disease to gain access to that of a presumably healthy individual, you *may*, but not necessarily, produce the disease in him. And you may point to a number of cases in which medical men, in the fulfilment of what they conceived to be their duty, have sacrificed their lives in the heroic attempt to succour their patients—as, for instance, in the course of the operation of tracheotomy. But the answer to this is the very valid one that you do not convey the bacilli only. You also convey the fluid in which they are bathed, in which I contend they live, and which, in my opinion, constitutes the real essence of the disease. As the vesicle in the case of variola (small-pox) and vaccinia is the outward manifestation of the disease and the contained fluid its essence, so in the case of diphtheria the condition of the throat is the outward manifestation, and the fluid oozing from its surface the essential element. Many observers of eminence and authority in this field concur in denying the connection of the Loeffler bacillus with diphtheria as cause and effect. I am bound to accept as matter of fact the statements made as to the association, even in a majority of cases, of the Loeffler bacillus with diphtheria, for they are not questioned; but to reverse the proposition, and say that their presence is the result of the disease, appears to me to be the more sound reasoning. The preceding argument appears to me to be unanswerable, and no one, as far as I know, has attempted to reply to it. But it will not be amiss to fortify it with some more *facts*. Thus Scharoz, writing in the "Deutsche Medicalische Woche," says: "There are certain cases which are clinically diphtheria, but in which the bacillus cannot be found." "It seems probable that widely distributed saprophytes, such as the bacillus, cannot alone be the cause of diphtheria." He quotes two cases in which diphtheria bacilli, so called, were obtained from wounds of the eye after cataract operations, in neither of which infection occurred. In a paper by Drs. Hewlett and Montagu Murray, published in the "British Medical Journal" on June 15th, 1901, the authors begin by saying that the purpose of their communication is to illustrate "the frequency with which children whose throats are presumably healthy may serve as sources of diphtheria," and, referring to appended tables, they continue: "It will be seen that the figures support two statements which are commonly made. The first statement is that the Klebs-Loeffler bacillus and pseudo-diphtheria bacillus exist in the throat in a large number of cases, especially in children, without producing any recognisable symptoms Out of 385 children under the age admitted for operation, or for some illness other than diphtheria, only 235 were free from both kinds of bacteria. In 92, or 24 per cent., the pseudo-diphtheria bacillus was found; in 58, or 15 per cent., the Klebs-Loeffler. Neglecting for the moment the presence of the pseudo-diphtheria bacillus, the pathogenic power of which is uncertain, these figures suggest that one out of every seven children, or at least one out of every seven sick children, is a possible source of diphtheria infection." They admit the possibility that "in some cases bacilli may have been present, but yet have escaped detection" (thereby underestimating the proportions), and "in view of this possibility it is interesting to note that in only seven of the cases was there any clinical evidence in favour of diphtheria, and that on the first examination in three of these cases no Klebs-Loeffler bacillus or pseudo-diphtheria bacillus was found in the throat." And one of "the morals deducible" from these figures "is that it is clear that babies and young children are not the harmless and innocent (*sic*) creatures usually imagined, and that kissing and other artificial

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(*sic*) demonstrations of childish (*sic*) affection should be discouraged." I can imagine the effect such a recommendation by the family doctor would have upon a mother. Then they give "some of the results obtained by other observers with regard to the occurrence of the Klebs-Loeffler and pseudo-diphtheria bacilli in the throats of persons with no sign of diphtheria"; thus "Loeffler examined 160 healthy school children, of whom four showed diphtheria bacilli in their throats." "Park and Beebe, out of 330, 250 are described as children, healthy throats examined, in eight, virulent Klebs-Loeffler bacilli were present." "Rober has made an extensive series of observations upon the presence of diphtheria baccilli in the healthy throat. There were 128 persons examined, who had been in contact with cases of diphtheria, and 600 who had, not knowingly, been exposed to the disease. Of the 128 persons, the diphtheria bacillus was found in ten, a percentage of eight, and of the 600 persons it was found in fifteen, or 2.5 per cent." Under these circumstances what ground there is for regarding the pathogenic power of the one bacillus as certain, and that of the other as uncertain, does not appear. Coming now to tuberculosis, I will not dwell on Koch's "tuberculin fiasco," which was hailed by the profession as a panacea for tuberculosis, but it should be borne in mind that this fluid of Koch's was intended to kill the bacilli, but it was soon found that, in too many instances, it had the very opposite effect—of killing the patients—twenty-seven or twenty-eight in Berlin alone, according to Virchow, in the course of two or three months. It was shown that the injection of this fluid into a healthy subject produced little or no effect, but into a tuberculous subject it produced, in a few hours, what was called, a violent "reaction," and that this reaction was, in many cases, attended with the production of a fresh crop of tubercles, especially affecting the serous and mucous membranes. Virchow, speaking of these new eruptions after the injection of Koch's fluid, and resting on the assumption that "all tubercles are produced by bacilli," says of one particular case of pulmonary tuberculosis in which "the so-called pericardium" was affected, that "there was no alternative but to suppose that the germs had reached the place by way of metastasis. How could we help thinking of metastatic processes here, and conjecturing whether, in fact, the bacilli had not been mobilised, and whether they had not been diffused *through the body* by a process of infection? And since, as you know, Dr. Koch himself considers the bacilli to be sufficiently refractory to his remedy—*and we have not found that they are destroyed*—the possibility must not be overlooked," that, "by a process of softening, whereby the products of disintegration are rendered more fluid" this metastasis takes place. Does not this lend countenance to the doctrine I hold—viz., that the injection of the fluid is only so much added to the amount of the specific poison with which the system is impregnated, and that hence the bacilli are found in all parts of the body, following their food, as fish find their way into the overflow in cases of flood, or advance and recede with the tide? The acute symptoms which follow the injection of this fluid point clearly in this direction, as does their subsidence, so soon after, on the exhaustion of the supply, provided the dose be not a lethal one.

14547. Could you tell me what metastatic processes are?—The transference of disease from one part of the body to another. What, then, should be the remedy? This, viz., not to endeavour to destroy the bacilli, but to maintain the vital forces and processes at their highest state of efficiency, by providing such as are already possessed by, or are predisposed to, tuberculosis, with pure air, abundant light, nutritious food, and, in a word, all the conditions that tend to the maintenance of good health. This is actually the method now in vogue. But not because, as has been put into the mouth of the Marquess of Londonderry at the opening of a hospital for consumption, "the best germicide to kill off the microbe was to provide air, light, and sunshine," for we now know that these conditions favour their growth. I call attention to the fact that many cases of so-called tubercular peritonitis have been cured by opening the abdomen, and removing the fluid contained therein. I specially direct attention to the case of a young subject, aged sixteen, on whom I operated on March 25th, 1895. In making the incision through the thickened parietes, and just below the umbilicus, I cut through several deposits of cheesy matter, and exposed a brain-like tumour, several pounds in weight, involving the omentum. As it was impossible to entertain any idea of removing the tumour, I sponged out about a pint of fluid lying in the lower part of the abdominal cavity, and then closed the wound, two or three of the sutures involving these cheesy masses. The wound healed by first intention. Previous to the operation the patient was much emaciated, her hair was falling off, and she suffered from a peculiar form of diarrhœa with very offensive evacuations. I put her on an absolute milk diet, not sterilised, until the evacuations assumed a healthy character, and improvement set in so rapidly that she returned home on the 26th day. She is now in perfect health, well nourished, and with an abundant crop of hair, and all trace of the tumour and of the deposit in the parietes has disappeared. That this was an example of tubercular disease I have no doubt whatever, if we are to put our trust in macroscopical appearances and the attendant symptoms. Will some bacteriologist be good enough to explain these facts on the basis of the germ theory? It is a remarkable fact that this young lady scarcely ever tasted milk. Had she been in the habit of taking milk, being the daughter of a farmer, it might have been contended that she had been infected in this way. But we are now assured that the cow is at least one of the chief sources of the spread of this disease by means of its milk. In his Harben lectures on "Consumption," the late Sir Richard Thorne-Thorne laid great stress on this aspect of the question. He stated that while, in the adult, tuberculosis had diminished 50 per cent. in the last quarter of a century, it still claimed a very large number of victims amongst children in the form of tabes mesenterica, and he attributed this prevalence to the fact that children are fed so much on milk. Now, if we look at the facts of the case, we find that tabes mesenterica occurs chiefly, if not wholly, amongst the children of the poor and indigent, and that it is extremely rare amongst the children of the well-to-do. The latter have an abundant supply of milk, and other nutritious food, while the former chiefly drink tea, and scarcely know the colour, much less the taste, of milk in their homes. Lest it should be thought that I am drawing upon my imagination, I quote from a paper read by Mr. R. Henry Rew before the Royal Statistical Society, on April 26th, 1892. After quoting from Morton, in his book, "The Diary of the Farm," 1885, that the consumption of milk "has very greatly increased of late years," Mr. Rew says: "I have been able to obtain some figures showing the average daily delivery of milk per family by retailers in a few fairly typical localities. From these I have calculated the consumption per head on the basis of five persons per family." He then gives the following table:—

STATISTICS OF MILK CONSUMPTION taken from the Books of Milk Retailers in various Districts.

No.	Description of District.	Approximate Number of Families Served.	Average Consumption per day, per Family.	Average Consumption per head.	
				Per Day.	Per Annum.
			Pints.	Pints.	Gallons.
1.	West-End of London	4,000	3·750	0·750	34·218
2.	North of London	2,000	3·250	0·650	29·656
3.	Manchester		2·666	0·533	24·318
4.	" (Middle-class District)	146	1·500	0·300	13·687
5.	" (Working-class District)	200	1·000	0·200	9·125
6.	Small Country Town	90	1·624	0.325	14·823
7.	Putney and Wandsworth	700	2·286	0·457	20·838
8.	Small Town (Health Resort)	500	2·000	0·600	27·375
9.	East End of London (one gallon of Milk divided into 37 portions		0·432	0·086	3·923

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I should just like to point out that amongst the well-to-do, the average consumption per head per day is in the West End of London 0.750 pints, or 34.218 gallons a year, whereas in the East End of London the average consumption per annum is 3.923 gallons.

14548. When it says that in the West End of London 4,000 families were served, does that, and do the following figures relating to the West End of London mean that those are the figures given by a particular trader in the West End of London?—I go on to explain that in the following paragraph. "With reference to No. 9 (the East End of London) it is necessary to explain that the figures are based on the fact that a gallon of milk is divided on an average into thirty-seven portions, each portion being, I believe, fetched by the consumer. I have assumed that each family consumes one of these portions every morning and evening. It will be seen that even on this assumption the consumption per head is infinitesimal, and it reveals, incidentally, a significant fact in connection with the dietary of the poor. I may, perhaps, add that the gentleman who was good enough to procure me that particular return is one of the highest authorities on the London milk trade."

14549. That did not seem to me quite to explain it. Does he put together 4,000 families supplied by a number of West End dairymen?—Yes, of course, the returns have been obtained from different milk dealers of families supplied by a number of dairymen, not by one dairyman.

14550. They are such round figures—north of London, 2,000; working-class district, 200; small town, 500—that they do not seem to be actual figures. However, you do not know about it?—I do not. This is a quotation from Mr. Henry Rew's book; he has given this table. Again tabes mesenterica is essentially a disease of mal-nutrition, and is often cured by nutritious diet, of which milk should form an essential part with cod-liver oil and some preparation of iron. And why does this form of the disease attack the digestive tract of children rather than the pulmonary? Because, says the lecturer (Sir Richard Thorne-Thorne), the tubercle bacillus gains entrance through the stomach. I venture to affirm that not a single case has ever been put on record in which the disease has been thus communicated. In this connection I may quote Dr. F. M. Sandwith, who has said that in Egypt women suckled their children for even two years, and all other milk was boiled, and among the cattle tubercle was rare, yet tuberculosis was common. His experience, moreover, had been that tubercular injections gave bad results in surgical tuberculosis. With regard to the tubercle bacillus of Koch, Professor Spina opposed the theory on the ground that if this bacillus were the true cause of tuberculosis the bacillus ought to precede the formation of tubercular nodules. Spina, Charrin, and Kuskow failed to find this bacillus in acute miliary tuberculosis, the last-named affirming that acute miliary tuberculosis does not at all proceed from the bacili of Koch's, and that such bacili only appear there second-hand, proceeding from pre-existing tubercular nodules. Professor Middendorp is of the same opinion, and in a letter to Dr. Vicentini says "the bacilli only accompany pulmonary phthisis in caverns communicating with the bronchi. . . . These microbes, therefore, cannot be called tubercular bacilli, as they are not found in tubercles as such, and have no etimological relation whatever with tuberculosis. For this reason I maintain that tubercular bacilli, in the sense of Professor Robert Koch, do not exist at all." With regard to plague, Mr. Hankin, Chemical Examiner and Bacteriologist for the North-West Provinces and Oudh, reported to the Plague Commission in January, 1899, that there was no doubt that cases of plague occurred among human beings in which no microbes were visible at the time of death. Ridicule seems to be the appropriate weapon to use against the theory that plague is conveyed to the human subject by rats through the agency of fleas! There was a time when the bacillus coli communis was regarded as a most virulent microbe, and by many it is still regarded as such, and yet it is now recognised by the best authorities as a normal inhabitant of the digestive tract, and necessary for the maintenance of health. In the case of typhoid fever, I have shown in the paper already referred to that in not a single instance in which the outbreak has been attributed to water has the bacillus typhosus been found in the incriminated water. I claim, then, to have shown that the poisons of variola, vaccinia, and several other acute infectious diseases, are not and cannot be the product of a bacillus; that Loeffler's bacillus is not a constant, and therefore cannot be the essential, element in the production of an attack of diphtheria; that the essential element in the case of typhoid fever is not the bacillus typhosus; that this bacillus cannot live but for a few hours in ordinary sewage; that not a single specimen of this bacillus has ever been discovered in sewer air, and hence that typhoid fever cannot be attributed to it because of its contained germs; that in the cases of the epidemics at Maidstone and King's Lynn there exists no proof of contamination of the water by typhoidal matter, as indicated by the presence of the bacillus typhosus; that there is no evidence worthy of the name that tuberculosis is due to the ravages of the tubercle bacillus; that the comma bacillus cannot be regarded as the essential element in the production of an attack of cholera, and that the same can be said of plague and its special bacillus; that the so-called pathogenic micro-organisms are constantly found under conditions consistent with perfect health, and that in more than one notable instance they not only appear to, but actually do, exert a beneficial influence. To all, then, who hold by the germ theory I would say, in the words of Oliver Cromwell, "I beseech you . . . . think it possible you may be mistaken."

14551. (*Sir William Church.*) Referring to the first page of your *précis*, eight lines from the bottom, am I to understand that only germs floating in the air were supposed to be dangerous when Lord Lister founded his system of antiseptic surgery, and that those which had settled upon other bodies were not dangerous?—That is not included. The Listerian system was founded on the theory that germs floating in the atmosphere fell into the wounds, and to counteract their influence the spray was used.

14552. But only those floating in the atmosphere?—Quite so. That was the basis of the Listerian method.

14553. What is your ground for stating that?—Professor Lister's statement.

14554. It is not Lord Lister's own statement?—Undoubtedly.

14555. Can you tell me where he states that germs only that are in the air are dangerous, and not those which have settled?—I think if you will just look over what Lord Lister himself said at Berlin you will find that that was the basis of the theory—the germs floating in the atmosphere.

14556. But are you not aware that Lord Lister, at the very time that he was investigating this question, was most particular with regard to the cleanliness of the skin both of the patient and of himself, and also of all the instruments that he used?—Sir Joseph Lister, as he was at that time, actually stated that he did not even aim at surgical cleanliness. He stated that at King's College Hospital after he arrived in London. And you see here what he says: "As regards the spray I feel ashamed that I should ever have recommended it for the purpose of destroying microbes in the air."

14557. Quite so, but that was an additional precaution, was it not?—That was the essence of the treatment, and he used the carbolic acid as a germicide.

14558. Before ever he used the spray what was his object in using germicides? Carbolic acid was what he used. What was his object in washing the skin, washing his own hands, washing the surfaces of the wounds, and keeping them dressed with carbolic acid?—I am not aware that he did it before he introduced the spray. In fact, I think that the use of carbolic acid and the use of the spray were simultaneous, and I believe the idea was founded upon the supposed effect of carbolic acid upon sewage matter—that the carbolic acid destroyed the microbes in sewage; whereas we now know that it does nothing of the kind. I believe that gave Lord Lister the idea of using carbolic acid for the purpose of destroying microbes in the air. Lord Lister seems to have had no idea of destroying microbes in the skin, which were already in the skin, and of which perhaps he was not aware at that time.

14559. But he states himself the necessity of cleansing the skin of the patient and his own hands and the instruments that he uses?—With carbolic acid.

14560. Yes?—Which he believed was a germicide, but now we know is not a germicide.

14561. Do you mean to tell the Commission that if you use carbolic acid in sufficient quantities it does not destroy the bulk of known germs?—Certainly

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it does, along with the tissues of the body to which it is applied. You cannot destroy the germ without destroying the tissues of the body, that is to say, that the germs will live in a solution of carbolic acid which will be destructive of tissues.

14562. Quite apart from the body, if you please, do you mean to say that it will not destroy germs in a solution in water or, in what you just now said, in sewage, if you used sufficient?—It will not kill them all; it may destroy a certain number, but carbolic acid to destroy germs must be used in such strength that it will destroy living tissue.

14563. That is not the point I am on at the present moment. You stated that it would not destroy germs in sewage?—That it would not destroy them all; that is to say it did not deprive the sewage of its septic matters; you could not apply it in sufficient quantity, and the practice, as you know, has been given up. The treatment of sewage by carbolic acid has been proved to be an absolute failure, and the very opposite method now is adopted of cultivating the microbes to eat up the sewage matter, not to kill it, but to eat it up.

14564. (*Chairman.*) You say in the second page of your statement that it was in 1891 that Lord Lister acknowledged his error?—Quite so.

14565. What is the date you fix for the introduction of the antiseptic treatment first by Lord Lister? —I really forget the date, but it was many years ago.

14566. You speak of his introducing the spray and the use of carbolic acid at the same time?—That was when he was at Glasgow.

14567. Is that what you call the beginning of the system?—That was the beginning.

14568. When was that?—I could not give you the exact date, but it must have been in the early sixties, if not in the fifties.

14569. As far back as 1860, at any rate?—Yes, undoubtedly.

14570. (*Dr. Wilson.*) You say about 35 years in your *précis*?—Yes; it would be quite easy to find the exact date when he introduced it at Glasgow.

14571. (*Sir William Church.*) Upon page 2 of your *précis* you say that you gradually began to reduce the strength of the solution until at last you used only ordinary London water to wash out or irrigate wounds, and you also, I think, mentioned Mr. Lawson Tait as doing the same thing?—Lord Lister refers to Mr. Lawson Tait.

14572. Did you use the water straight out of the tap? —Straight out of the tap.

14573. Without boiling it?—Without boiling it.

14574. Then you differ from Mr. Lawson Tait?—I do not think so. I think Mr. Lawson Tait did not always boil his water. I do not think he took any trouble in that respect.

14575. Do you think that boiling water sterilises it or not?—Sterilises of what?

14576. Sterilise the water—kills the germs in it?—You have to prove first that there are germs in the water before you can sterilise it or kill them.

14577. You know that Mr. Lawson Tait, although he did not mention it in his paper, did boil the water?—I do not think he did; he did not boil all the water, anyhow.

14578. And you never did?—Never. I had to use hot water, and if the water was too hot I added some cold water to it. I should like to explain my reason for my giving up the carbolic acid treatment. I lost a patient through acute inflammation of the kidneys as the result of carbolic acid poisoning; it was a case of ovariotomy.

14579. I think it is admitted that carbolic acid poisoning was frequent?—I was going to explain how I arrived at my views. A fortnight later I had another case in which the temperature went up to 106 within a few hours after the operation. I had to put the patient in a pack, and got the temperature down, and then I recognised that the symptoms were due to carbolic acid absorption, and I made up my mind to reduce the strength of the carbolic acid. I went from 1 in 50 to 1 in 60. I still got evidence of the absorption of carbolic acid by chemical test. Then I reduced it to 1 in 80; the evidence got less and less. Then I reduced it to 1 in 100, and then, as Lord Lister told us, that a strength of 1 in 40 was the weakest solution that was of any service, I thought when I had arrived at 1 in 100 that the experiment was at an end. And from that time my results began to improve. I do not know whether I am correct, but I think I am correct in saying that there is no surgeon in this country who has performed so many successive successful ovariotomies as myself in a public hospital, for at the Samaritan Hospital I have a record of 90 successive successful ovariotomies without the use of any antiseptic whatever. I think I was justified in every respect in leaving off the antiseptic treatment.

14580. You seem to place in contradistinction to each other the antiseptic treatment and the aseptic treatment?—Yes.

14581. But are they not one and the same?—Not at all. Antiseptic means the use of a substance with a view of preventing septic mischief, or destroying microbes. It is quite correct to say that you have done an antiseptic operation, but it is absolute nonsense to say that you have done an aseptic operation, because you do not know what the result may be. The patient may die of septic mischief within 48 hours.

14582. But surely is not the distinction between the two, if there is any (I do not admit that there is), that the antiseptic treatment was attempting to destroy germs which had got access to the body, and the aseptic is preventing their access?—Attempting to prevent the action of the germs was the idea of the antiseptic method.

14583. What do you understand by surgical cleanliness?—The use of nothing, and the absolute exclusion of dirt of any kind.

14584. But who showed us wherein dirt was harmful? How was that knowledge arrived at?—Dirt may contain specific poisonous matter apart from germs altogether.

14585. And may not specific poisonous matter be in the air, too, and so get on to the dirt?—I do not think it is possible that that specific poison can be in a dry air.

14586. How do you think that what you call dirt, I do not know quite what that is, becomes poisonous and dangerous?—Dirt may contain, as I have just said, some poisonous matter, whatever it may be. We do not know, and we have no means of knowing, what that poisonous matter may be. Even, you may say, you may find dirt free from microbes, and yet it will poison a patient, poison the wound.

14587. What is your authority for saying that?—My own observation.

14588. How do you know that the dirt had not germs in it?—I say it may have.

14589. I understand you to say that dirt quite free from germs is poisonous?—No, I say it may be free. You may not be able to find any germs in a particular portion of dirt, and yet that dirt will be poisonous to the wound.

14590. Would you give me your authority for that? —I have no authority beyond my own experience and observation. I do not think that any observations have been made on the subject.

14591. And are no organisms of any kind obtainable from that dirt?—You seem to misunderstand me. I have not said that dirt has been found free from germs, but I say that it may be free from germs and yet be poisonous.

14592. How do you explain what used to be so common in hospitals—hospital gangrene?—Shall I relate to you an experience of many years ago? When I first came to London, before I began practice, I went into one of the general hospitals of London, where I saw a very distinguished surgeon go up to a case on which an operation had been performed for hernia. He put his little finger into the wound, a suppurating wound, and he took a towel and just wiped it like that (*describing*). He went on to see other patients, and coming to one other patient of the same kind he ran his finger into the wound and did the same thing again. That is what I call dirty surgery; there was no attempt at cleanliness there.

14593. What do you think is the dangerous element in it?—Transferring the fluid from a suppurating wound to another wound.

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14594. So you did not believe in the possibility of contamination of wounds or the carrying of infection through means of the atmosphere?—Certainly not.

14595. Lord Selby wishes to know what would be the difference between a surgeon who acted antiseptically and a surgeon who acted aseptically. Did not Lord Lister himself introduce the term aseptic?—I do not think so.

14596. I think he did?—I think Lord Lister had ceased to operate by the time that that term was introduced.

14597. I think he introduced it himself, and he was very pleased to find afterwards that it was a word used by Hippocrates?—I was not aware of that, but I think it is immaterial even if it be so.

14598. (*Chairman.*) What I wanted you to do, if you could, was to describe to me and to others who do not understand these matters professionally, what a surgeon would do if he were performing a particular operation antiseptically, and how otherwise would he act if he were performing it aseptically?—In performing an antiseptic operation the surgeon would first of all wash his hands in some so-called antiseptic or germicide such as corrosive sublimate, carbolic acid, spirits of wine, or other things. A very elaborate process was invented by Dr. Howard Kelly, of Baltimore. He first of all washed his hands in a strong solution of permanganate of potash.

14599. I do not want every precise detail, of course, from you?—It was a process for the purpose of destroying bacilli or bacteria in the skin. And it has been found that that is an impossibility, because many of these microbes are too deeply seated——

14600. Instead of arguing upon each thing he does, will you just tell us what in fact he does?—Then he would have his instruments in a solution of corrosive sublimate or carbolic acid. He would then wash the skin, probably with germicide. In an operation upon the abdomen in which he would use a sponge, or any operation in which he would use a sponge, the sponge would be soaked or washed in a solution of carbolic acid or some other germicide. The aseptic surgeon uses no germicide to the patient except it may be to the skin. He does not allow the antiseptic to touch a wound or raw surface.

14601. Does he wash his hands?—Yes, he washes his hands.

14602. In the same sort of solution?—Yes, but he does not allow any germicide to touch the wound.

14603. (*Mr. Ram.*) Does he sterilise the instruments? —He may either sterilise the instruments or keep them in a solution of corrosive sublimate, or carbolic acid, or such like.

14604. (*Sir Mackenzie Chalmers.*) What does he do that for?—His idea is to destroy the germs.

14605. Is it effective in sterilising the instruments in the hands?—It is not effective in sterilising the hands or sterilising the skin.

14606. (*Chairman.*) I did not want to go into the question whether it was right or wrong, but I wanted to get first what actually was done. I do not gather from what you have told us so far that there is any difference between the two methods except that in aseptic treatment certain things are not applied to the skin of the patient?—And the tissues of the patient, that is the most important thing.

14607. Is that the difference?—Practically.

14608. (*Sir William Church.*) Did you in your practice make use of sterilised dressings?—No, I used the dressings that I got from the surgical instrument makers or the man who supplies these things. The gauze I used directly as I got it from the surgical instrument maker.

14609. You did not take any precautions to sterilise your wool, or your gauze, or your bandages?—None whatever. And I am not afraid of exposing a wound to the open air. I have operated on the breast and applied no dressing whatever. Any case where I could not draw the edges of the skin together I have kept exposed to the open air, and it has healed up perfectly.

14610. All surgeons used to do that at one time. They had to leave the wound exposed if they could not get a skin covering?—They did not leave it exposed; they put some dressing upon it, some ointment or something of that kind. But I purposely in a public hospital did that as an experiment, and everybody knows that the best method of treating a raw surface, if you graze your skin, for instance, is to let the blood dry upon it—do not apply any dressing to it, and it heals up forthwith.

14611. The answer that you gave me was what I wanted; that you in your own practice made no use of sterilised dressings at all?—Absolutely none.

14612. You used absorbent wool, or anything of that sort, as it came from the maker?—Yes, quite so, absolutely. That I have done now for the last 30 years, I should think.

14613. On page 4 of your *précis*, towards the bottom of the page, you say: "Thus in the modern septic tank, which is now so largely used in the treatment of sewage, it is the action of the micro-organisms, whether aerobic or anaerobic, which dissolves the sewage, and it is the continuous action of these microbes which converts all manurial matter into the saline constituents which are essential for the nutrition of plant life." Do you mean that plants do not suffer from parasitic organisms?—Undoubtedly they do. At least my observation in a greenhouse is of this nature, that it is only the unhealthy plants on which you find the aphides.

14614. By parasites I did not mean aphides, I meant micro-organisms?—I do not know that they suffer from micro-organisms. They suffer from visible organisms such as I mention, but I do not know that they suffer in any other way.

14615. Not from microscopical fungi?—Yes, from fungi; but fungi are not microbes, I believe.

14616. Am I mistaken or not in thinking that if you filter the fluid of small-pox vesicles it does not convey the disease?—I am not aware of any experiments of that kind. I know, as everybody knows, that if you take an infinitesimal quantity of the contents of a vesicle and put it under the skin you get an attack of small-pox.

14617. But you were not aware—I think I am correct—that filtered vaccine fluid does not convey the disease?—I am not aware of any observations in that direction.

14618. If that is correct it looks as if the disease was dependent upon some particular matter?—But you know that you do not prove anything. You do not prove the exact cause.

14619. Something must be left behind in the filter?—You prove that there is something, but you do not know what that something is. And it certainly is not a microbe.

14620. You say so?—No one has found the microbe yet, anyhow.

14621. I admit that it has not been found?—And you have no right to assume that it is the result of the action of a microbe.

14622. You also say on page 6 "it is now generally admitted that the tubercle bacillus is not found in the sputum in the early stage of phthisis, but only when necrosis of the lung tissue sets in." What is your ground for saying "it is now generally admitted." What are your authorities for that?—I have referred to several authorities, and I think that is the result of the reference to those authorities, that no one has yet discocered the tubercle bacillus in the early stage of the miliary tuberculosis.

14623. Is that so?—That is so, I believe. If you can refer me to any author or any experimenter who has discovered it I should be very pleased to know who he is, but, so far as my reading goes, the result is that no one has yet discovered the tubercle bacillus in the early stage of miliary tuberculosis.

14624. You mean before tubercles are found?—In the early stage of miliary tuberculosis.

14625. I wish to know what that is?—Miliary tuberculosis can be found in a very early stage. I do not know what the microscopical appearances might be; you know more about that than I do, but I take the word of the eminent men to whom I refer.

14626. That is just what I am asking. Who are your authorities that you quoted?—I have quoted some here. I do not know whether I am correct in using the word "generally," but I mean that it is extensively admitted.

14627. I will put my question in another way. Do you mean by the early stage of miliary tuberculosis a stage before you can see tubercles with the naked eye?—I do not know whether the term "early stages of tuberculosis" is confined to that stage in which it is not possible to see them by the naked eye, but they can be seen by the microscope.

14628. I wished to know what you meant by the early stages. But you do not maintain that when tubercles sufficiently large to be seen by the naked eye are found you do not find the bacillus in them?—I do not know whether the bacillus is found; I cannot say anything about that; but in a still earlier stage the bacillus is not to be found. When the tubercle is large enough to be seen with the naked eye it must be considerably advanced.

14629. I wanted to know what you meant by the early stages?—Yes, that is what I meant.

14630. You say that they are not early stages when the tubercles are visible to the naked eye?—I should say not by any means.

14631. Might I ask who Dr. Vicentini is?—Dr. Vicentini is an Italian doctor, living at Chieti, in Italy. He is a very good English scholar, and he reads and writes English perfectly, and when my paper on the "Modern Doctrine of Bacteriology," or the germ theory of disease, was first published, he sent his pamphlet to me. He had seen mine and read it, and was struck by the similarity of the views which he and I held, and sent me a copy of his pamphlet. I am sorry that I have not been able to bring it down with me, because in changing houses unfortunately I have mislaid it, but these quotations in inverted commas from Vicentini are from that pamphlet.

14632. Is he a skilled bacteriologist?—Evidently, yes.

14633. What has he published, could you tell me?—Unfortunately I did not put down the name of the pamphlet; but he has written a work on whooping cough and the micro-organisms associated with it.

14634. And when did he publish it, if you could tell me?—My paper was read in 1899, and Dr. Vicentini sent me his paper the same year; my pamphlet was published in the early part of the year.

14635. His work was published before 1899?—It must have been, as he sent me the printed pamphlet.

14636. I do not quite follow what you say at the bottom of page 7 of your *précis*. Why is it incomprehensible and incredible that smallpox can spread through the action of a special form of bacillus? Why should it not spread through a bacillus just as much as through a poison?—There is no condition that I know of in which such a state of things has been found or has been suggested even.

14637. But we do not know very much about it, and it seems to me no more incomprehensible or incredible that its spread should be due to bacillus than that it should be due to the poison?—To my mind it is utterly incomprehensible. I am giving my own opinion.

14638. You have given the reason, that it is something that is incomprehensible to your mind?—Yes, we do not know what the action of smallpox poison is in the human subject or the animal subject.

14639. Turning to page 9 of your *précis*, you say that this bacillus of Loeffler's is frequently present in some exanthemata, and also in healthy persons. Are you referring to Dr. Hewlett's paper on that?—Not alone, but the literature upon the subject is so full of that idea that I did not think it necessary to refer to any particular writer.

14640. Then what is your authority for saying that in cases of tonsillotomy the Loeffler bacillus is present?—I did not say that it is present, but that it has been found in cases of tonsillotomy.

14641. You say "A still more striking example is afforded in cases of tonsillotomy, wherein, upon the incised surface, a greyish membrane is formed, in which the bacilli abound without constitutional disturbance or any sign of diphtheria"?—I think you will find that I gave my authority for that.

14642. A greyish membrane is an exceedingly common thing after an operation for tonsillotomy; but apparently we do not get diphtheria with it. What is your authority for saying that "Loeffler's bacilli abound without constitutional disturbance or any sign of diphtheria"? You may have diphtheria after tonsillotomy too?—You may have; but I think I gave my authority for that statement. You see that is a quotation, "A still more striking example."

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14643. I want to know what your authority is and where you are quoting from?—I think Scharoz is the authority for that, but it is a quotation, as you see. You will find his name at the top of page 10. That is my authority for quoting it from the "Deutsche Medicalische Woche."

14644. Have you any evidence of fluids from these throats producing diphtheria without the presence of the bacillus?—No. I have no evidence; but there is the evidence that in a great many cases of true clinical diphtheria the bacillus is not to be found, and then again in a perfectly healthy throat the bacillus is to be found without any of the symptoms.

14645. But here you state "if you allow some of the discharge from the throat of a subject of this disease to gain access to that of a presumably healthy individual you may" produce the disease?—Yes, you may, but not necessarily.

14646. Have you any evidence of the disease being produced in that way?—I think you will find the answer to that in the following sentence, if you read it:—"You may point to a number of cases in which medical men in the fulfilment of what they conceived to be their duty have sacrificed their lives in the heroic attempt to succour their patients."

14647. Then they got the bacillus too?—There is no evidence that there was a bacillus. We know that in many cases of clinical diphtheria the bacillus is not to be found. You have no right to assume that in those cases where death has resulted from getting some fluid into the throat in the operation of tracheotomy the bacillus was present.

14648. But you have no right to assume that it was absent?—No. Then we are on an equal footing. You have no right to be top sawyer over me in that case.

14649. I thought you illustrated this as showing that it was the fluid that was the dangerous part?—I say that it is the fluid which is the dangerous part because the fluid is always there, but the bacillus is not always there.

14650. That leads me to ask another question. You say that there have been numerous cases of what you call "clinical diphtheria" in which the bacillus has not been found. What do you mean by "clinical diphtheria"?—Where you have all the symptoms characteristic of diphtheria.

14651. And followed by the after-results of diphtheria?—It does not matter what the ultimate termination of the case may be. What you have to determine is, is this a true case of diphtheria clinically or not? The patient may die or may recover. We do not require to refer to the after-results.

14652. Then you merely mean to state that it is (what everybody will allow) a very difficult question to decide whether the patient is suffering from diphtheria or not?—I imagine that there is some considerable difficulty, and I believe that many cases of sore throat are returned as cases of diphtheria at the present day.

14653. But to finish the clinical picture, can you bring before us any cases of what you say clinically were diphtheria which were followed by the ordinary sequels of diphtheria, that is to say paralysis, etc.?—I am not prepared to bring forward any cases on my own account, but I refer to the general literature on the subject, which is quite sufficient for me.

14654. Then what you mean by "clinically diphtheria" is a case in which there may be a difficulty in forming an opinion from the appearance of the throat and symptoms of the patient, whether it is diphtheria or not?—No, that does not represent my views, I mean by a case which is clinically diphtheria, that it must present the characteristic symptoms of the disease, whatever you may consider the characteristic symptoms.

14655. Then if I choose to consider the presence of the bacillus as a characteristic of the disease, I am right in excluding those cases which have not that from the category of diphtheria?—But that does not correspond with practice; that is not practical. It may be your view, but it does not correspond with the observation of the profession generally.

14656. You have given me no reason for proving that clinically these cases are diphtheria. You cannot tell

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me whether these cases furnish in about equal numbers of the ordinary sequels of diphtheria as those in which the bacillus is present?—I do not think I am called upon to express myself definitely on that point. You cannot exclude a case of diphtheria because the patient does not get paralysis after it.

14657. I only wished to know whether you can give me any instance of these cases in which the symptoms occurred without the bacillus being present?—I cannot from my own practice, but from the literature of the subject.

14658. You state here distinctly that by the inoculation of bacilli: "You also convey the fluid in which they are bathed, in which I contend they live, and which, in my opinion, constitutes the real essence of the disease"?—Yes, that is the poison.

14659. Still you cannot bring any evidence before us that these cases which have contracted the disease from the fluid alone without the bacillus have any of the ordinary sequels of diphtheria?—Not from my own observation. I take it from the literature of the subject.

14660. Then might I ask you who the "many observers of eminence and authority in this field" are to whom you refer on the same page, who "concur in denying the connection of the Loeffler bacillus with diphtheria as cause and effect." Could you tell me some of them?—There again I am dependent upon the common literature of the profession on the subject. I have not put down all the names.

14661. You cannot give me the name of any man of eminence?—I cannot. But that is the result of my reading. It is impossible always to remember the names of the authors you read.

14662. I should like also to know what proof you have that if you reverse the proposition you are correct?—It is a matter of reasoning simply. It is not a question of proof. I say it appears to me to be the more sound reasoning. It is not a matter of proof at all; it is a matter of opinion.

14663. Passing on to the bottom of page 11, you say "Coming now to tuberculosis I will not dwell on Koch's 'tuberculin fiasco,' which was hailed by the profession as a panacea for tuberculosis." Was it not rather hailed by the public than by the profession?—No, the profession also.

14664. Was not Koch himself averse to it, and did he not have his hand forced to a great extent?—Not by the public alone, but by the profession. I remember perfectly well that there were some American gentlemen over here watching my practice at the Samaritan Hospital, and they expressed a very strong desire to go over to Berlin to get some of this tuberculin.

14665. You admit that he himself was not desirous of tuberculin being brought out then as a cure?—I believe it is correct to say that.

14666. His hand was forced, as you say, by the profession; I thought rather by the public?—It was not by the public. I think the profession had as much to do with it as the public.

14667. That is a matter of no importance; but you admit that Koch himself did not wish it—it was against his wishes?—I have heard so. I believe I have read so.

14668. Do not you think that light has been thrown upon the action, or I might even say the dangers, of tuberculin treatment, by what we know now as opsonins?—I do not know anything about that subject; that has recently come up, and I have not paid any attention to it.

14669. You have not followed Sir Almroth Wright's work?—No, not with any degree of attention.

14670. What is your authority for stating that the bacillus of tuberculosis is favoured in its growth by air, light and sunshine?—I think it is a fact that those conditions are favourable to its growth instead of to its destruction; that is to say, so long as it has nutritive material to live upon.

14671. Surely that is not generally held, is it?—I am afraid that I hold several views which are not generally held.

14672. That is your own view?—That is my own view, certainly.

14673. You are not aware of anything contradicting it?—No, nothing.

14674. You are not aware that many experiments have been made as to the effect of light, especially sunlight, on the tubercle bacillus?—When isolated from the conditions in which they live it is very likely, but I am not aware of any experiments which have been made upon the tubercle bacilli in the natural conditions under which they live.

14675. Of course, directly you have them under investigation they are not in their natural condition?—That is just it. No allowance is made for that.

14676. I admit that they are not in their natural condition; but still are you not aware that experiments done with them in captivity, if I may use the term, do not support that view?—But I do not see how they can support any other view.

14677. Simply that the more light, especially sunlight and air, that you expose them to, the more rapidly they die?—As soon as you isolate them from the natural conditions under which they live, then, of course, they are likely to die very soon.

14678. One other question with regard to this very remarkable case of your own that you bring before us. On the same page you say: "That this was an example of tubercular disease I have no doubt whatever if we are to put our trust in macroscopical appearances and the attendant symptoms." Then, as a matter of fact, the tubercle bacilli were not seen under the microscope in that case?—No.

14679. Therefore you have no evidence that they are present there?—No, certainly not, but that the disease was tuberculosis I have not the slightest doubt, and no one has a right to question my opinion upon that point.

14680. Pardon me, I think everyone has the right?—From the macroscopical appearances I think one can perfectly safely say that a case is tubercle or not tubercle. I think you would bind us too closely if you denied us that privilege.

14681. But I think you admitted that this organism called the tubercle bacillus was at all events associated with a certain disease?—Not necessarily. In many cases of phthisis, in the last stage of the disease, you do not find the bacillus in the sputum; that is a fact.

14682. But you find it in the body, do you not?—I do not know. I did not go so far as that.

14683. On page 15, about the middle of the page, in the second paragraph, you say: "I venture to affirm that not a single case," that is of tuberculosis, "has ever been put on record in which the disease has been thus communicated"?—Yes, I affirm that most confidently.

14684. You mean by that, I suppose, only in man?—I am speaking of the human subject, of course.

14685. But do you mean to say that no case has been put on record in which the disease has not been thus communicated to animals?—I have not said anything of that kind. If you will look at my *précis*, I am speaking of children. I am speaking on the subject of tabes mesenterica.

14686. You say "I venture to affirm that not a single case has ever been put on record in which the disease has been thus communicated"?—By means of milk.

14687. That is to man, I understand?—Quite so. I am not dealing with animals.

14688. Do you hold the same with regard to animals?—I have nothing to do with animals.

14689. Would it modify your opinion at all if you learnt that, for instance, monkeys which have received only a single feeding or so with tubercle bacillus have contracted tuberculosis?—I have nothing to do with that question. I have asked over and over again for the details of a single case in which tubercle or tuberculosis has been conveyed to a child or a human subject by means of milk. I get no answer.

14690. And you put out of account all experiments on animals?—Yes, absolutely.

14691. May I ask who Professor Spina is? I am afraid I do not know his name?—He is an eminent Continental observer. Judging from the name, I should say he was an Italian.

14692. Would you tell me what he has written and where he has published his works?—I think you will find that that is a quotation from Dr. Vicentini.

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14693. I wish to be allowed to consult his works, to be referred to his works?—I must try and find that pamphlet of Vicentini's in which you will probably get the reference.

14694. Perhaps you will kindly send the Commission the information?—If I can find it I shall be very happy to do so.

14695. And Kuskow. Charrin's name I do know, but who is Kuskow; perhaps you will kindly send that name to us and reference to his works?—If I can find it I shall be very happy to do so. The work from which I have quoted by Dr. Vicentini is "Bacteria of the Sputum."

14696. And also if I might ask for Dr. Middendorp's work?—That also, I believe, I got from Vicentini.

14697. (*Dr. Gaskell.*) Of what nationality is Professor Middendorp?—German or Dutch. I think he is German.

14698. (*Sir William Church.*) I should like also to ask you why you say on page 16: "Ridicule seems to be the appropriate weapon to use against the theory that plague is conveyed to the human subject by rats through the agency of fleas"?—Yes, I think so.

14699. Why is ridicule the only appropriate weapon? Surely it is not?—I believe that one gentleman has given evidence here already on this question, and he said that one of the precautions which they took in avoiding the bite of the fleas was that they wore top-boots.

14700. (*Chairman.*) Who wore top-boots?—The experimenters, I think. I think if anything could be ridiculous that is ridiculous.

14701. (*Sir William Church.*) I do not remember it. I may not have been present. But why is ridicule the appropriate weapon to use against the theory that malaria is conveyed by mosquitoes?—No, not so much against the theory that malaria is conveyed by mosquitoes. But I contend that that does not explain the origin of the disease.

14702. We have no knowledge of the orgin of anything, have we?—I do not know, but the current idea is that malaria is produced and propagated by the agency of mosquitoes.

14703. Not produced—propagated by mosquitoes?—I will give you a case in point. In the Nile Valley there is a district of about 300 miles in length in which there are no inhabitants, yet it is pronounced to be one of the most deadly districts in the globe in consequence of malaria; but there are no inhabitants by whom the disease can be propagated by mosquitoes from one to another. That shows that we have not arrived at the origin of the disease; and it is well known that when you turn up virgin soil you get malaria as the result.

14704. That is an old belief?—And it is well authenticated.

14705. Quite so. There are explanations why that is the case, are there not: that in the first work of turning up the soil you often cause stagnant water, puddles?—That is not the explanation.

14706. But you do not think that ridicule is the right weapon to use against the belief that malaria is propagated by mosquitoes, do you?—No, I do not say that.

14707. Do you think that there is anything more ridiculous in the possibility of fleas conveying a disease than of the tsetse fly conveying a disease?—That is another thing altogether.

14708. I wish to know why this is ridiculous; why is ridicule the weapon to be used; and whether there is any difference between the possibility of the communication of plague by fleas and the possibility of the communcation of nagana by the tsetse fly?—The tsetse fly is on the same plane as the wasp; it develops its own poison. The wasp does not get its poison from outside, or the bee.

14709. The tsetse fly does not develop its own poison?—I think it does, and the same with the wasp.

14710. Then we will leave the tsetse fly, and I will ask you, do you think that there is such a thing as tick fever?—Very likely. I do not know anything about it.

14711. You think that with the exception of malaria the impossibility of disease being conveyed by infection is ridiculous?—I think in those cases that you refer to the disease is not a disease conveyed by the animal. The effects are the result of the poison secreted by the animal itself. In the case of the tsetse fly, for instance, and in the case of the wasp and the bee, they do not derive the poison from any other source than their own bodies.

14712. I will put it in another way. You think the explanation that has been suggested for sleeping sickness is incorrect altogether?—I do not know anything about sleeping sickness.

14713. What is your authority for saying that the presence of the bacillus coli is necessary for the maintenance of health?—I took that from the late Professor Kanthack, who read a paper at Liverpool, in which he pointed out that when the bacillus coli was absent from the intestinal track the individual was not in a healthy condition. You will find that in his paper.

14714. When was it published?—It was read at Liverpool before the British Association. I forget the exact date.

14715. Do you mean the British Association or the British Medical Association?—The British Association, because I think it was in the September when Sir Joseph Lister, as he then was, read a paper at Liverpool; it was at the same meeting.

14716. You do not remember the year?—It must have been about 1896 or 1897.

14717. Never mind, I will not trouble with the year. I daresay I shall be able to find Professor Kanthack's paper. But are you quite sure that your statement is right that the typhoid bacillus has never been discovered in water?—I believe so. There may be one solitary instance, but I think it will be a solitary instance which will prove the rule. It was not discovered at Maidstone. It was not discovered at King's Lynn. You know what an outcry there was in the case of Maidstone, and not a single bacillus was found in the water at Maidstone.

14718. I should like to ask you on what grounds you say that in the cases of the epidemics at Maidstone and King's Lynn there exists no proof of contamination of the water by typhoidal matter. Let us take Maidstone?—It was supposed that the disease had been communicated through the occurrence of a case of typhoid fever amongst the hoppers. Now the disease began in July, and the hopping season does not commence until September. The first case occurred at Barming; another case occurred about a fortnight afterwards in the neighbourhood of Barming, and no connection could be shown between those two cases.

14719. Might I not ask you whether there was not only in September, or whatever month you just now mentioned, but also in July, quite a possibility that the spring might be fouled by enteric discharges?—But there was no case of typhoid fever in the neighbourhood, and there had not been.

14720. Well, none was known?—But you cannot affirm anything unless you know it as a fact.

14721. But still you would admit that where you have a source of water supply which is capable of being fouled, it is possible—I do not say that it is the case—that it might have been fouled by a passer-by who did not stay in the neighbourhood?—You have got to prove it.

14722. Quite so; but you admit that the spring was found to be in a condition in which it was possible to be fouled?—No, I do not think it was. I went into that subject very fully in my pamphlet from which my *précis* is taken.

14723. Then on the last page of your *précis* you say that the so-called pathogenic micro-organisms in more than one notable instance not only appear to, but actually do, exert a beneficent influence. What is your authority for that?—A few years ago Dr. George Stoker was treating some cases of chronic ulcer. He had a patient, whom I saw myself, with a large chronic ulcer on each instep. These cases were treated by oxygen gas mixed with air. Mr. Jonathan Hutchinson was asked to see these cases, and he suggested that the one case should be treated with corrosive sublimate solution and the other with oxygen gas. The result of that was that in a short time the one treated with oxygen gas was found to be healing very nicely, whereas that treated with corrosive sublimate solution was making no progress. Then some gentleman went up from some bacteriological institute and took a cultivation from both these wounds. The one treated with

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oxygen gas showed that the wound was swarming with staphylococcus pyogenes; the one treated with corrosive sublimate was sterile. Dr. Stoker found by subsequent observation that the more abundant the staphylococcus pyogenes was the better the case was going on, and if in the course of treatment the case seemed to be getting slack in its progress he would take some of the secretion from the healthy wound and put it upon the one that was not making such progress, on the ground that it was desirable to increase the vitality of the staphylococcus or the streptococcus as the case might be. Then Dr. Stoker brought the case before the "British Medical Association," and he appended to it a microscopical or bacteriological examination. The Editor of the "British Medical Journal" refused to put in the bacteriological examination, and simply said that the bacteriological results were astounding. I say that that is extraordinary evidence in favour of the view that I take.

14724. I do not follow it, but it may be so. It seems to me it is evidence that the staphylococcus or streptococcus, whichever it was, acted as a healthy stimulus to the ulcer?—It is evident, because when there was no staphylococcus there the ulcer was not healing.

14725. And I would suggest that in both cases the rest had a good deal to do with it—the patient not using his legs?—The rest could have nothing to do with it, because they had already been under treatment by rest for a considerable time, so that rest had no part nor parcel in it at all.

14726. (*Dr. Gaskell.*) Who was the bacteriologist?—I do not know. He came up from one of the institutions. I do not know whether it was the Clinical Research Association or not, but he was a skilled bacteriologist and microscopist.

14727. And where is this statement of Dr. Stoker's published?—In the "British Medical Journal." I give it you very fully in this pamphlet of mine (*handing in a pamphlet*). All the facts are given there.

14728. (*Sir William Collins.*) Your work has been chiefly surgical, has it not?—Yes.

14729. Especially in gynecological surgery?—Yes.

14730. Have you ever yourself practised vivisection?—Never. I have never even seen an experiment.

14731. Do you think that you have derived any advantage in your surgical treatment from knowledge acquired by vivisection?—None whatever that I am aware of.

14732. And do I correctly understand you to suggest that in no case has any micro-organism been shown to be the true cause of any specific disease?—I am not aware of any case.

14733. Does that apply equally to such things as malaria and tsetse fly where a cause of animal origin rather than vegetables is suggested?—Quite so.

14734. As well as bacterial disease, so called?—Yes.

14735. I understand you to admit that you find the micrococcus in association with disease?—I believe so. I accept that as correct.

14736. What relationship, if any, do you think subsists between them?—That the micro-organism is the result of the conditions just as you find grouse growing on moors and partridges and pheasants on arable land and salmon in running rivers, and so on.

14737. Do you suggest that any of these bodies, vegetable or animal, which are alleged to be pathogenic in their character, proceed from the body itself, or are they wholly from without?—I think they proceed from the body. For instance, bacteriologists tell us that we are swarming with bacteria.

14738. Do you suggest that they originate *de novo*?—I think it is possible. I do not know. I have never studied that part of the question, but I am quite of the belief that some diseases originate *de novo*. But with regard to bacteria that is another thing.

14739. (*Chairman.*) Do you accept what the bacteriologists say, that we are swarming with bacteria?—I accept it because I have no means of disputing it.

14740. (*Sir William Collins.*) It is not unknown, is it, that fluids which are entirely free from micro-organisms can produce very violent inflammation. Take the case of jequirity, which is obtained from *Abrus Precatorius*. That will produce acute ophthalmia, although it is free from organisms, will it not?—I do not know. I have no experience of that.

14741. Has a great distinction been drawn between the aseptic system of surgery and the antiseptic system?—I draw a very marked distinction between the two, but the current view is that the antiseptic and the aseptic treatment are the same.

14742. Did Lord Lister at one time draw a great distinction between the two?—I am not aware that he did. I do not remember any reference on the part of Lord Lister to the aseptic treatment.

14743. Did Lord Lister include in the antiseptic system such things as frequent change of dressings, free ventilation, dependent openings, and a judicious selection of cases for operation?—I think very likely. Any sensible surgeon would do those things.

14744. Did Lord Lister at one time criticise Sir John Burden Sanderson's work, because Sanderson said that dry air did not contain putrefactive organisms?—I should agree with Burden Sanderson in that. I do not know whether Lord Lister objected to it or had anything to say on the subject.

14745. Was the carbolic spray, in the earlier years of antiseptic surgery, regarded as a most important part of the procedure?—A most important part of the procedure, according to Lord Lister's own showing.

14746. It was finally renounced, as I understand, because, as Lord Lister stated, it was founded upon a theory that was physically impossible?—To affect the microbes.

14747. Did not Lord Lister call attention to the fact that inflammation may occur independently of any atmospheric external influences?—I think so; I believe so.

14748. Did he include in antiseptic surgery such things as the dry dressing of McVail?—Lord Lister included dry dressings impregnated with carbolic acid.

14749. But what was McVail's dry dressing?—Probably like my own, simple.

14750. Was it lint spread with lard?—I should say not. That is not dry dressing. Dr. McVail was in the habit of leaving his amputation cases exposed to the air until the surface got glazed over. That was a peculiarity of his practice.

14751. And some other surgeons at one time did the same?—Yes, but I am speaking of McVail.

14752. Was Mr. Lawson Tait practically on the same lines with yourself?—Almost absolutely.

14753. Was Sir William Savory?—Yes.

14754. Can you mention any other?—Savory, Tait, and myself were the only three who ventured to oppose the antiseptic or Listerian method. It might be interesting to know the result of my reading of a paper at one of the medical societies of London on hyperpyrexia, after Listerian ovariotomy, in which I showed that my best results were with the non-Listerian method. The result of my reading that paper was that I was twice blackballed by that society, so strong was the prejudice at that time existing against my views and the views of Mr. Lawson Tait and Sir William Savory.

14755. I understand, then, that your practice, based upon your views in the matter of ovariotomy has shown an unrivalled success?—Quite so; and it was a case of unsuccess that opened my eyes in the first instance—that is to say, I lost a patient from acute nephritis—carbolic acid poisoning.

14756. That was not a very unusual occurrence at one time?—At one time, I believe, it was not at all unusual. The evidence of absorption of carbolic acid was at one time very conspicuous.

14757. Did Lord Lister say that if the air did not contain putrefactive organisms it would affect his surgical practice in a most important manner?—I do not remember that statement on Lord Lister's behalf.

14758. Does anybody use the carbolic spray now?—I do not think so.

14759. I see that attention has been called in Mr. Paget's book to the alleged relation between the work of Semmelweis and that of Lister. Have you anything to say on that?—I do not think there is any connection between the two. Semmelweis went in for extreme cleanliness after parturition, and I think sanitary arrangements generally, and he would not allow students from a *post-mortem* room to attend upon cases in labour.

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14760. Do you claim him as an aseptic or antiseptic authority?—I should call him aseptic, if you use the term at all, but I do not use the term aseptic, because I think it is a ridiculous term.

14761. I see on page 82 of that book of Mr. Paget's a quotation from Haller in 1849, in which he said: "The importance of these observations" (alluding apparently to Semmelweis) "is above all calculation both for the maternity department and for the hospitals in general, but particularly for the surgical wards"?—Yes; that was long before the era of the antiseptic treatment or bacterial observations.

14762. In regard to the question of tubercle, to which you called attention, whatever may have been the pressure upon Koch, is it not the fact that he published a book called "The Cure of Consumption"?—I could not say. I know of nothing but his announcement at Berlin. I do not know of any work that he has written on the subject.

14763. Have you not seen a work entitled "The Cure of Consumption: Further Communications on the Remedy for Tuberculosis by Professor Robert Koch"?—No, I have not seen it.

14764. Are you not aware of the claim there made that "phthisis in the early stages can be cured with certainty by this remedy"?—I believe that claim has been made.

14765. You do not accept it?—I do not.

14766. Do you know of any case of consumption that has ever been cured by tuberculin?—I am not aware of any case that has been cured by tuberculin. I am aware that cases of tuberculosis have been cured. I was taught that in my student days.

14767. As regards diphtheria and the presence or absence of the Klebs-Loeffler bacillus, I see that Dr. Newman, Medical Officer of Health for Finsbury, who has just been appointed Medical Officer to the Board of Education, in his work "Bacteriology and Public Health," says the organism may exist in the healthy throat without producing the recognised clinical symptoms of diphtheria. I understand that you agree with that view?—Yes, quite so; in fact, that is almost universally accepted now.

14768. He also states: "It should be remembered that about 20 per cent. of all cases of diphtheria offer no bacteriological evidence of infection. It therefore comes back to the point of broad judgment and common-sense. The clinical condition is the main fact for guidance, and the bacteriological must not usurp it"?—I agree with that.

14769. Then he says, "Whilst in 72 per cent. of notified definite cases of diphtheria the bacillus may be found, it has been shown that in apparently healthy persons who have not suffered from diphtheria the bacillus diphtheriæ may be present." You agree with that?—Yes, I agree with that.

14770. And that "Hewlett and Murray found 15 per cent. of the children in a general hospital had diphtheria bacilli in their throats"?—Those are statements which I quite accept as *bona-fide* fact.

14771. Do you think that suggests that there is the presence of the alleged cause with the absence of the alleged effect?—Yes.

14772. Or the presence of the alleged effect and the absence of the alleged cause?—It comes to that.

14773. The President of the College of Surgeons when he gave evidence before this Commission alluded to various instances in which surgery, in his opinion, had been assisted by vivisection experiments upon living animals, and amongst them was the treatment of septicæmic infections by sera. Have you anything to say with regard to that?—I have no experience of such treatment.

14774. In regard to the question of Listerism, the President was asked two or three questions by myself, so I will just read them to you and ask your views about his answers. At No. 7845 I asked: "As regards Lister's work, I suppose one would be right in thinking that there have been considerable modifications in the Listerian doctrine between 1876, when, as Mr. Lister, he gave evidence before the Royal Commission, and the present time." The reply was: "I do not quite follow that." I then asked: "For instance, in regard to the use of the spray and drainage, and so forth, there has been a very considerable alteration in technique, if not in the principles upon which Listerism was based?" The reply was: "The technique of the employment of facts which he has laid down, the technique of the employment of the treatment which is based upon the facts which he has discovered." Then again I asked: "Should I be wrong in thinking that Lister laid great stress upon the use of the carbolic spray and drainage?" And the reply was: "Quite so. Those are all modified now. The spray we know is an abolished thing." Have you any further remark that you desire to make in reference to that, arising out of your own experience?—I think Mr. Henry Morris maintained that abdominal surgery was very much indebted to experiments on animals. I entirely deny that. Abdominal surgery is the result of successful ovariotomy, and to that alone is due the success of abdominal surgery generally. It was shown that the peritoneum was not the sensitive membrane which it was always regarded as being. I recollect Sir John Erichsen asking me whether I would treat a knee joint in the same way as I treated the peritoneum, and I said, yes, certainly, I should have no hesitation in treating it just as I should the peritoneum. That was when I was not using the spray or carbolic acid, and experience has shown that I was right.

14775. Do you recall a case in which Lord Lister, in one of his classical essays, attributed suppuration following an operation on the knee joint to the fact that the spray had been interrupted in its course by the removal of the drainage tube?—I do not remember that particular case, but I remember one case in which the failure of a breast case was attributed to the fact that there was a pinhole in the macintosh covering the gauze dressing.

14776. Then, as regards cancer research, do you think it is idle to look for light from experiments on animals with regard to that?—I am afraid so.

14777. Has any light been obtained at present?—None whatever, so far as I know.

14778. And as regards hydrophobia, I do not think you have mentioned that. Have you anything to say upon that subject?—I should like to say that I know of nothing which is less creditable to the profession than the readiness with which they accepted the claim which has been set up on behalf of the Pasteurian treatment of rabies.

14779. Possibly you would desire to amplify that answer. I do not quite follow at the moment?—I think in the first year there were some 3,000 inoculations. There were only a certain number of deaths, and they claimed that in all the other cases in which death did not occur the cases should be regarded as cures. That is the claim that has been set up by the followers of Pasteur: that every case in which the disease is not manifested after inoculation is a case to be regarded as a cure.

14780. (*Chairman.*) Is that a publication of Pasteur's that you are quoting? Are you quoting any statement of Pasteur's to that effect?—No, not of Pasteur himself.

14781. We have had no evidence of it. We have had evidence about it, but we have had no evidence that such a claim is made?—The claim is made, I said, by the followers of Pasteur.

14782. We have had many witnesses who agree with him as to his method of treatment. I do not think that any of them have made that claim?—We must be guided in this matter very much by statistics.

14783. Statistics are very useful to prove or disprove a claim, but you were saying that the claim was made?—Yes, that claim was made. I could not give you the exact reference to the work in which it was made.

14784. (*Sir Mackenzie Chalmers.*) Who made it?—It is a general claim made by the followers of Pasteur—by the Institute.

14785. (*Chairman.*) A general claim made by the followers of Pasteur is a little vague. At any rate, it was not made by Pasteur. Some people who believe in Pasteur may have made it?—I have not spoken of Pasteur alone. I think much more blame is to be attached to his followers.

14786. (*Sir William Collins.*) I see in 1886 (Question 11659) the cases treated were 2,671, and the deaths 25, or a mortality of 0.94 per cent.?—I should like to read a letter that I have here. I paid considerable attention to this matter in the early period, but of late years I was so disgusted with the whole thing that I have not paid any attention to it. But I wrote quite

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recently to a friend of mine, Dr. Lutaud, of Paris, and he writes me this in reply to my request for the latest statistics.

14787. Is he the author of a work that has been referred to?—Yes, "Pasteur et La Rage." I have the work here.

14788. (*Sir Mackenzie Chalmers.*) What is the date of that work?—1887.

14789. Twenty years ago?—Yes.

14790. (*Dr. Wilson.*) What is the date of the letter?—The date of the letter is the 23rd of April this year: "I was not in Paris during the Easter holiday, and was detained in the country by an attack of 'grippe'; if not, your letter would have been answered long ago. I cannot send you exact statistics for the last two or three years; the numbers I have published at the time of the great Pasteur polemic were demonstrative—viz., that the annual mortality by hydrophobia was not less *since* the Pasteur treatment than it was *before*. For instance, the average number of deaths from 1850 to 1866, date of the introduction of the new treatment, was 30; since the discovery of the new method it was a little higher (about 35 per annum). The system adopted at the "Institut Pasteur" consists in inoculating the virus to everybody having been not only bitten but even licked by a dog without inquiring if the dog was hydrophobic; so they have a lot of customers which are cured of a disease which had not existed. So they establish beautiful statistics to strike the people. The Pasteur Institute is the property of a limited company exceedingly rich, and they can afford to insert advertisements or notes in daily papers; they are regular business people, selling every sort of virus, serums, drugs, even stuff to kill the rats and rabbits. A very rich man, Mr. Ohris, who died recently, has left them 25,000,000 francs. I will try to furnish you with more information, but I am afraid they will come too late." But I have been furnished with that information in the form of a document signed by Dr. Boucher, a well-known man in Paris.

14791. (*Sir William Collins.*) What does that purport to show?—Shall I read it to you?

14792. If you please?—"Before Pasteur's method"—I am reading the translation, but here is the original (*handing in the same*)—"the official statistics collected by Tardieu and Boulay, with the view not of advancing a method, but simply to give an account of the deaths caused by a disease, indicate from 1850 to 1872 inclusive 685 deaths from rabies—*i.e.*, a mean of 30 deaths per annum. After the method when thousands of persons (3,000 patients) were inoculated, whether they had been bitten by mad dogs, or, in the infinitely more general case, whether they were preventively inoculated, the number of deaths, from 1887 to 1890 inclusive, were 154—*i.e.*, 38 per annum. At the present moment it is impossible to get the number of deaths in the whole of France; one entrenches oneself behind the difficulty of picking out the different cases. This excuse has as its true reason the danger there would be for the Pasteurian theories in publishing these statistics, which are more remembered than before Pasteur's method. Yet it is easier to pick out the different cases occurring in the Department of the Seine. The discussions at the Academy, the municipal statistics, render this easy. Here is the result of the comparison between the manifestations of rabies before and after the method. Official statistics before the method, 1876 to 1885: Deaths from rabies 43, a mean of 4 per annum. Ten years after the method, 1897-1901 inclusive: 44 deaths—*i.e.*, 8.8—more than double."

14793. Would that be of French people only?—This applies to France only.

14794. Does it apply to French people only, or does it include those visitors who came to France to be treated?—I do not know. These are official statistics, and it does not specify whether they are French people only, or whether it includes foreigners.

14795. It would be important to discriminate, would it not?—I do not know that it would.

14796. I suppose there was not prior to the Pasteurian period that influx to France for treatment which has been notorious since?—That is so. "I have not the numbers for the years beyond 1901 except the official numbers of the statistics, which are false. One discovers this falseness from a comparison of the numbers furnished by the Academy, and those supplied by Bertillon, Chief of the Statistical Department. Thus:

		Deaths			Deaths
Statistics of Bertillon	1897	5	Statistics of the Academy Report to Council of Hygiene	1897	6
	1898	4		1898	8
	1899	7		1899	9
	1900	9		1900	9
	1901	9		1901	12

the year 1902 representing one death, after applying to Bertillon 1902 none, 1904 one, express numbers three times less, at least, than the reality." Then he goes on:—"I recall the statistics of Carlo Ruata for Italy. Before the method the mean of the deaths from rabies was about 60. After the method—*i.e.*, from 1887 to 1900—the total of the deaths is 1,193, giving a mean annual rate of 85." That is the result of my application to Luteau.

14797. I gathered from Dr. Martin's reply to Q. 11659 that rabies was regarded as uniformly a fatal disease, and that, according to Bouley, 40 per cent. of those bitten by presumably rabid dogs contracted the disease, whereas amongst those treated by Pasteur from 1886 to 1905 the mortality was in every year less than 1 per cent.?—Because they included cases in which it was not possible to prove that the dogs were mad. That was one of the oldest things I can remember. Youatt maintained or affirmed that of 20 persons bitten by really mad dogs not more than one would develop the disease; that is only a matter of 5 per cent. To put it down at 40 per cent. is a stretch of imagination, I should think.

14798. In hydrophobia has any micro-organism been found as the cause of the disease?—I am not aware of any.

14799. Or has it been claimed by anyone?—Not so far as I know.

14800. In that case is it the fluid that communicates the disease?—It is the saliva of the dog that communicates the disease, and how it happens that the saliva is not the source of the anti-rabic serum I cannot understand.

14801. (*Sir John McFadyean.*) Do I rightly understand you to hold that the experimental method as applied to physiology and pathology has been entirely barren of results?—I have never said so.

14802. But I ask, do you think so?—I suppose I must give a qualified answer to that. I cannot give a definite answer. I have considerable doubts as to the results.

14803. You could not mention to the Commission any case in which you are satisfied that useful knowledge has been obtained by the experimental method in medicine?—I am not aware of any. I have never availed myself of any knowledge obtained in that way that I know of.

14804. Do you think that the same might be said about the other natural sciences, or physics and chemistry. Has any useful knowledge been gained by experiments in those sciences?—Undoubtedly, in chemistry. Chemistry is an exact science. We hear a great deal said about the science of medicine, which is all nonsense. There is no such thing as the science of medicine. Medicine is an art. You might as well talk of the science of painting.

14805. But is chemistry not largely an exact science, because the experimental method has been extensively employed in the investigation of its problems?—Chemistry is undoubtedly an exact science.

14806. Would you tell me why in the case of biology, including physiology and pathology, all methods of investigation except observation should be barren of results?—Because you are not dealing with exact data.

14807. But do you not admit that by experiments you can at any rate limit the data. Would your argument not be fatal against observation as well as experiment?—Not at all.

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14808. Can anything be found out about a science which is not exact?—Yes, undoubtedly some things can be found out.

14809. Then you admit that some things can be found out by observation of things as they naturally occur—I mean physiological and pathological facts?—Yes.

14810. How is it that your argument against the experimental method does not apply to observation also. The science still remains an inexact one?—I cannot imagine an inexact science, you know; it is a misuse of the term "science."

14811. Call it anything you like; I cannot find another word handy in my mind that will cover what we both mean. But leaving that out of account you admit that observation has led to the discovery of a great many important facts in physiology and pathology?—Undoubtedly.

14812. And yet this branch of knowledge which you object to being called a science is inexact?—Yes.

14813. Why should one be able to learn something by observation and yet be unable to learn anything from carefully planned experiments?—All knowledge as the result of observation is open to error.

14814. Surely?—Unless you can come to an exact science you cannot rely upon the result of any observation.

14815. You mean to say that even the beliefs which are most firmly held that are founded on observation, may be wrong?—Yes. Take the case of Listerism, for instance. At one time, as you know, Listerism was almost universally practised.

14816. Is this an attempt on your part to discredit observation also?—Yes, it may be taken as such. Everybody, the whole world almost, was impressed with the advantages of Listerism.

14817. I really do not want to go into that?—I take it as an illustration.

14818. Would you forgive me for saying that it does not appear to me to be an illustration of the point which I was trying to elucidate at all. I wanted to learn from you how it came that one might learn a good deal from observation of naturally-occurring cases, but that one could learn nothing, and as a matter of fact in your opinion we have learnt nothing, from observation of experimental cases?—I did not say that we have learnt nothing, but I should like to know definitely what I have learnt as the result of experiment, for at present I cannot recall anything.

14819. You do not come here to give evidence except with regard to diseases of the human subject?—Quite so.

14820. You have no knowledge whatever about diseases of animals?—No, I do not profess to know.

14821. We have had a good deal of evidence laid before us to the effect that things of great practical value with regard to the treatment and prevention of animal diseases have been discovered by the experimental method. You cannot offer any evidence to lead us to discredit that evidence?—No, I am not aware of anything one way or the other.

14822. You make the following statement in your *précis* as explanatory of what the hypothesis was on which the special treatment introduced by Lord Lister was founded; you say:—"The system was founded on the hypothesis that germs, floating in the atmosphere fell into wounds, there developed and multiplied, and produced all the evil effects which sometimes followed surgical operations. In order to combat these lamentable results this system, when it was fully exploited, included operating under an antiseptic spray, and treating all instruments, ligatures, and dressings on antiseptic lines." It seemed to me that in developing your argument you chose to overlook the fact that an important part of the precautions advised by Lord Lister was directed not against atmospheric contamination, but against the contamination that was attached to instruments and to dressings and to hands?—That was not the basis of the Listerian system of antiseptic treatment.

14823. Observe I am quoting from your *précis*?—Yes, I affirm that what I have said there is correct; in fact, you have it from Lister's own address at Berlin.

14824. But I am taking it that you have not misrepresented him?—No, I have not misrepresented him.

14825. You have said that this system, when it was fully exploited, included the treatment of instruments, ligatures, and dressings antiseptically?—Yes.

14826. Does it not appear obvious that it was not directed solely against atmospheric contamination; that the treatment comprised two things? It was directed against what were supposed to be injurious germs in the air; but it appears to have been directed, and I certainly always understood that it was directed against injurious germs that were supposed to be practically ubiquitous and therefore likely to be found on hands and instruments and dressings. Is that not so?—In the early stage of the antiseptic treatment there was no idea of germs or bacteria coming from the skin; it was only of recent years that that was developed, when it was found that no system of antiseptic treatment of the skin could get rid of these bacilli. The staphylococcus albus in the skin is so deeply seated that it is absolutely impossible to remove them altogether.

14827. What do you mean by recent? You said it is only recently; do you mean the last 30 years?—Yes.

14828. That is not very recent?—As it happens, unfortunately, all this matter is ancient history, you know, because it is 30 years since I first began my opposition to Listerism, and that is a quarter of a century ago and more.

14829. I am sorry to say that it is 30 years since I was a medical student myself, but I have a very good recollection that at that time, at any rate, an attempt was made to destroy the hypothetical germs that were on the instruments?—But the main idea was the germs floating in the atmosphere.

14830. Why do you say that that was the main idea, when you admit that the system always comprised the two things?—I believe those were added afterwards. I believe that the first principle was that germs were floating in the atmosphere and got into the wound.

14831. Would you deny that at least 30 years ago, and within five years after Lord Lister's name was associated with this system, he in operating invariably endeavoured to destroy the germs that were supposed to be attached to instruments and hands?—I never got the idea that that had anything to do with Lord Lister's conception in the first place.

14832. But that was especially Lord Lister's practice?—Yes, afterwards; but the first thing that he did was to adopt the spray—that was the basis.

14833. (*Chairman.*) You told me that it was before 1860?—I forget when Lord Lister introduced the treatment. Perhaps Sir John can tell us.

14834. (*Sir John McFadyean.*) No, I cannot tell you. I am really prompted to put these question largely from my own experience. I wanted to know whether you could tell us of anybody who before Lord Lister habitually treated his hands and instruments and dressings with the object of destroying germs that were supposed to be attached to them?—I am not aware of anyone who did it before Lord Lister.

14835. So that the credit of introducing that belongs to Lord Lister?—Undoubtedly; that is the antiseptic treatment as distinguished from the aseptic.

14836. (*Sir William Collins.*) I thought you told me that Semmelweiss did it before?—Yes, cleanliness.

14837. (*Sir John McFadyean.*) You claim to have been rather a pioneer yourself in aseptic surgery?—I had an opportunity of telling the Commission before you came in that in consequence of my opposition to the Listerian method I was twice blackballed by a very distinguished Society in London, and that Sir William Savory and Mr. Lawson Tait and myself were the only men who had the courage to oppose the Listerian method in this country.

14838. But what I meant was that you claim rather to have been a pioneer in teaching that dirt was the thing to be avoided?—Well, scarcely. I think that phrase is due to the late Dr. Campbell Black.

14839. But you quote him with approval?—I quote him with approval, undoubtedly.

14840. Will you tell me what you understand by dirt?—That I have explained already in your absence. My view is that dirt may or it may not contain germs, and that dirt with the absence of germs may poison the wound if it is admitted into it.

14841. What is the chemistry of that sort of dirt?—I cannot tell you.

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14842. It is capable of analysis?—It may be, but I do not know that anyone has ever examined it, and I doubt whether chemical analysis would find out a poison unless it was of inorganic origin. If it was organic the probability is that a chemist could not find it.

14843. It is something so subtle that it is past finding out, you think?—I do not know that it is past finding out, but it would be a very difficult matter to find it out.

14844. Has it not occurred to you that you, or somebody whom you could set to do it, might make himself famous by discovering what is the precise nature of this thing in dirt that is capable of producing the formidable effects with which we are familiar in wounds not treated aseptically?—I have had no ambition in that line to make myself famous by chemical discoveries. I am satisfied to rest upon the results of my practical work.

14845. Do you contend that any sort of what is popularly called dirt is capable of causing suppuration in wounds?—I do not contend for anything of the kind, and I do not see how one can contend for it either. One must prove the absolute product of the application of what is called dirt.

14846. But I understand you to maintain that the great thing is to exclude dirt; that the presence of germs is of little or no consequence; that the dirt must therefore be the noxious element?—To give you an example of that, supposing that a man falls down on the road and cuts his head, and a lot of dirt, take, for instance, sand, gets in without anything else; if you attempt to sew up that wound it will not heal by first intention, simply by reason of the presence of the sand, which is not poison, but which simply produces local irritation.

14847. But may I point out that you are not entitled to draw any such conclusion unless you can give me a sample of this sand or dirt from the street that has not got germs. In discussing whether it is the dirt by itself or the germs that cause the evil, you must not cite a case in which the two are used concurrently?—You can wash a wound and deprive it of all soluble matter.

14848. Deprive it of all the dirt?—Of all soluble matter; but you cannot deprive it of all the sand.

14849. But bacteria are not soluble matter?—But they are washable; they can be removed by washing.

14850. So can the finer particles of dirt?—No; probably the large particles of dirt will be removed, but the small particles will remain, and if those particles be sand you have got a local irritant in the wound, and it will not heal by first intention.

14851. Would you deny that it is possible to inject sand and dirt from the road, provided that it has been freed from its germs, under the skin in considerable quantities without exciting any very serious result?—I think it is very probable that if you inject sand or any inorganic material into a wound it will produce some irritation.

14852. But would you admit that the sand or dirt when treated by any means that can be relied upon to kill bacteria seems to have lost that injurious power which it had in its natural state?—Can you refer me to any experiment of that kind, for I know nothing? There is nothing within my knowledge that would lead me to accept that statement.

14853. You think it is not possible to treat sand by heat, for instance, or by steam, so as to render it absolutely incapable of setting up any serious lesion?—I do not think it is possible, from my experience.

14854. I venture to differ from you. You said that you did not know of any experiments bearing on the matter?—No, I do not.

14855. You have not tried it yourself?—No, and I should not think of trying it either.

14856. On page 6 of your *précis* you say: "It seems very singular that the tetanus bacillus, which is believed to be the sole cause of tetanus or lockjaw, should be found so abundantly in manured or garden soil, or on the surface of roads fouled here and there with horse-droppings." I was not able to follow that reasoning. What is singular about it?—That the disease should be so rare while the presence of the bacillus is so common and so almost universal; that is singular.

14857. But does not that cease to be singular when one adopts the view that the tetanus bacillus is harmless except when it is introduced into the tissues through a wound. What is singular about that theory?—How do you account for it—if you will excuse my asking you a question—for the occurrence of tetanus after an ovariotomy, for instance? Can you explain the like of that?

14858. Certainly?—Do you mean to say that that is because the tetanus bacillus has been introduced into the body?

14859. If you operated, or any of those who hold the same views as you do, I should think it a perfectly natural consequence to happen, now and again at any rate, because you omit the only precautions which can ensure that tetanus will not set in; and even supposing that you adopted the precautions which are generally advised, it is not contended by anybody that there might not sometimes be a loophole which allowed the tetanus spore to creep in?—That is all assumption.

14860. That is the best answer that I can give to your question?—Very good; it must be taken as assumption, but not as evidence.

14861. What is there remarkable, may I ask, in the fact that the tubercle bacillus is not found in the sputum in the early stage of the disease?—I stated that to show that the tubercle bacillus is not the cause of tuberculosis.

14862. Is that the only evidence that you have got to show that it is not the cause of tuberculosis?—I think that is sufficient; it ought to satisfy any mind.

14863. Is it not perfectly natural that the tubercle bacillus should not be found abundant in the sputum, at any rate until the lesions which it has caused begin to break up?—But it is not found at all.

14864. Who said that?—Many people have said so.

14865. But I think that a great many people say that the bacilli are found?—I have read as much as I have seen on the subject, and I have never met with a man yet who has said that he has found the tubercle bacillus in the sputum before necrosis of the tissues has taken place—that is to say, in the early stage of the disease. It is denied by men of considerable authority on the Continent. I do not know whether there are any in this country who deny it.

14866. Yes, I think a good many deny it, and I think myself it is entirely contrary to the fact. You are familiar with the chain of evidence on which it is held that particular bacteria are the actual cause of disease. You are familiar, no doubt, with the evidence which Professor Koch produced many years ago to prove that what has since been called the anthrax bacillus is the actual cause of anthrax?—I have never paid any attention to the question of anthrax; I know nothing at all about it; but if I were asked for an explanation of the occurrence of anthrax I should say that when an individual suffers from anthrax, the active poison of anthrax has somehow been introduced into his body. I hold the same view on that question as I do on tuberculosis, and on other diseases which are attributed to the action of bacillus.

14867. But that is hardly going into the matter as minutely as I wanted. If you are not familiar with the evidence on which it is held that the anthrax bacillus is the cause of anthrax, I will briefly tell you what it is. It is that when you get a case of anthrax you can find this thing which is called the anthrax bacillus generally in enormous numbers, that you can induce this organism to grow outside the body in a great variety of artificial solutions of a comparatively simple character, and you can go on cultivating it and transferring it from one tube to another for years, and whenever you like you can produce what appears to be a case of anthrax exactly similar to the original natural one by injecting a little of this culture under an animal's skin. It dies, and it is just like the original case. It has this anthrax bacillus in its blood. When that can be done repeatedly, will you point out where is the fallacy in concluding that the bacillus is the cause of the anthrax?—In the first place the bacillus has not been freed from the medium in which it lives.

14868. Allow me to interrupt you by saying that it has been freed from the medium in which it lives many and many a time?—Washed and absolutely freed?

14869. You can wash it repeatedly for days, and it will still be found to have the same effect?—And when

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it comes to the period of injection, do you inject the live bacteria or the fluid?

14870. You can inject the solid bacteria and nothing else?—Because you know in the case of Koch's fluid there are no living bacteria introduced at all.

14871. Nobody has pretended that there are any in tuberculin, so far as I know, but let us stick to the case of anthrax, or if you like take tuberculosis. You can cultivate this thing called the tubercle bacillus, and can carry it on for years and years thousands of miles away from any case of tuberculosis; you can wash it as long as you like, and you can introduce it into an animal and you set up what appears to be an ordinary case of tuberculosis. What is the fallacy in concluding therefore, when that is done repeatedly, that that thing which is called the bacillus of tuberculosis is the actual cause of the disease?—With regard to anthrax I know nothing at all about it, I have not studied the subject; and in the case of tuberculosis, so far as my reading and observation go, the introduction of the tuberculin was not the introduction of a living bacillus, and I am not aware of any experiments in which a live bacillus has been introduced, freed from the medium in which it lives.

14872. You think that that has not been done in any other instance either?—I do not contend for anything beyond what I know. I am not aware of any experiments to that effect.

14873. Would you deny that cases of tetanus have often been experimentally produced by using the organisms freed from their liquid surroundings?—I could not deny anything if it is stated as a positive fact. I am not prepared to deny what is stated as a positive fact.

14874. (*Mr. Ram.*) In the event, might I ask you, of your being satisfied of the introduction of such an active actual bacillus, and that the consequences were that it produced tuberculosis, or anthrax as the case might be, would that modify the views which you have been expressing to us to-day?—I should have to be satisfied that the bacilli alone were introduced and not the medium.

14875. I am supposing that to be the case. If you were satisfied of that—that they introduced the actual bacillus—would that modify all the opinions that you have been expressing to us to-day?—Probably, yes. I think if that were shown to me to be an actual fact, I would accept it as a fact, but there are so many theories.

14876. (*Sir John McFadyean.*) You contend for the existence of what is called a materies morbi as distinct from bacteria, and you apparently contend that when you get noxious effects from injecting cultures of bacteria it is something in the liquid that is to be blamed?—That is my view.

14877. Has this something in the liquid the power of multiplication?—Probably, as in the case of small-pox.

14878. You think that you can really figure to yourself a non-living liquid material which has the power of indefinite multiplication?—Undoubtedly. Small-pox is an example; vaccine is an example.

14879. You are begging the question?—I do not see it.

14880. In a very flagrant way, because it is contended that in all probability the materies morbi in vaccinia is a bacterium?—You say in all probability, but that is not a fact.

14881. I never said that it was. I said that it was a fact with regard to many other diseases, tuberculosis, anthrax, and so on, and it may yet be made a fact with regard to vaccinia?—But you have no right to calculate upon a possibility. You may calculate to some extent upon a probability.

14882. But I contend that it is a fact with regard to many other diseases. This same thing, whatever it is, has got a far greater power of multiplication in anthrax than in vaccinia or in variola?—I do not know how that is possible.

14883. Are you familiar with anthrax in animals?—I know nothing about anthrax.

14884. Would it surprise you to know that if you inoculate an ox to-day it may be dead in 48 hours, but the thing has multiplied to such an extent that every drop of the animal's blood if injected into another ox would kill it with anthrax?—That may be; that is analogous to the case of small-pox.

14885. Then that involves that during the morbid process what has been multiplying is not the bacterium, but some chemical liquid?—Some liquid. We will not call it chemical or anything else; but it is an unknown liquid.

14886. (*Sir Mackenzie Chalmers.*) Is there any known chemical poison which multiplies in the human body?—No, certainly not. The chemical process applies chiefly to inorganic matter; it does not apply to organic matter. There is something else introduced in the case of organic matter which is absent in the case of inorganic matter; that is vital force—life.

14887. Take the case of a vegetable poison, that is organic?—That is organic.

14888. Is there any vegetable poison that multiplies in the human body?—No. That is a very different thing from poisons existing in the human body. It is a totally different thing.

14889. But there is no instance of either an inorganic or vegetable poison which when introduced into a wound multiplies in the human body?—No.

14890. (*Dr. Gaskell.*) By multiplication or growth in the human body, do you mean necessarily something living?—Yes, apparently. Take the case of small-pox. You know that if you introduce an infinitisimal portion of the fluid under the skin you have an enormous multiplication of the actual poison, which is an exact repetition of the poison that you introduced under the skin.

14891. (*Mr. Ram.*) Which involves something living?—It involves a living subject.

14892. Exactly?—If you put it under the skin of a dead subject there will be no effect.

14892 (A). No, I mean does the multiplication, the growth, involve something living?—It involves the life of the subject.

14893. I do not mean that at all. You say that the multiplication, the growth, is not a chemical process; there is no evidence of connection with vegetable alkaloids. By that word growth, do you necessarily imply that the morbific material must be a living substance?—I do not know what it implies. There is a fact that you cannot explain.

14894. Can you have growth without life?—You must have the life of an individual. In my ignorance I call that a chemico-vital action, because you require a living subject in which this poison may be developed.

14895. (*Sir John McFadyean.*) But I submit that it is not quite fair of you to say that there is a fact which you cannot explain, because a minute ago I took an absolutely analagous case—namely, anthrax—in which there is the same apparent enormous rapid multiplication of something, and I put before you the chain of evidence on which it is held that it is not the liquid that multiplies, but that it is the thing which we call the anthrax bacillus, and you were unable to break down that chain of evidence?—In the case of anthrax I say that I do not know anything at all about it, but I stick to the case of smallpox, in which you have not shown the existence of any organism.

14896. But you also have not shown that there is not an organism there?—It is for you to show that there is an organism. It is for me to deny the existence of it until you can prove its existence.

14897. (*Sir John McFadyean.*) On page 7 of your *précis* you refer to this question of the rapid evolution of the morbid process in vaccinia or smallpox, and you say that it is an extraordinary state of things that this should be due to the action of a special form of bacillus which passes your comprehension and is incredible. That is going far further than saying that it has not been seen. You say that it is not credible. Again, I put to you the case of anthrax, which is one on all fours, and which you agree is not incredible. I have given to you the facts on which it is held to be proved, and you yourself admitted that you could not point out what the fallacy was. If it is true in anthrax, why should it be incredible in the case of vaccinia, if one merely assumes that the thing has been overlooked—that our methods so far have not been capable of detecting this thing—it may be too small for one thing?—But you have no right to assume its presence or its existence until you can prove it.

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14898. Not even by analogy?—No, not even by analogy. Analogy is a very unsafe thing to go by.

14899. Then on page 8 you discuss what happens in a patient affected with smallpox, including the fact that the patient generally becomes insusceptible to a second attack, and you say: "No one has offered even a plausible explanation." Do you think that is quite fair. Have you read the recent literature on protective inoculation and vaccination?—I am not aware that any observations have shown the mode by which this change is brought about or the nature of the change that is brought about as in the case of vaccinia or smallpox.

14900. Remember, please, that you are not the first scientific witness before the Commission?—I do not profess to be a scientific witness. I profess to be a practical witness.

14901. What I want to point out to you is that theories as to how this is brought about have been placed before the Commission?—Quite so.

14902. And it seemed to me to be rather too strong language to tell the Commission that these theories were not even plausible?—In my opinion. I am only expressing my own opinion, and I have a right to express my opinion on any subject.

14903. (*Sir Mackenzie Chalmers.*) I do not want to go into technical details; I only want to know whether I rightly appreciate your argument, because you have gone into some rather technical matters. You said that medicine is not and cannot be an exact science?—Quite so.

14904. It is a practical art?—Yes.

14905. Do you therefore go so far as to say that it has not made any advances towards being an exact science?—It never can, because in the first place we do not know what life is, and we are deprived of one of the most important premisses. It is absolutely impossible that medicine can become a science.

14906. Would you treat it practically then as a handicraft?—It is an art and a very empirical one too.

14907. Is not that almost going back to the old days of surgeons and barbers?—I do not know that it is. We have learnt by experience. We are trading in fact upon the experience of ages.

14908. But we are not making any scientific progress; we are only building up empirically?—Building up facts.

14909. But when we are building up facts, as it builds up facts does not an art tend to become a science?—Not a bit of it.

14910. When you generalise from facts and arrive at even an intermediate law, is not the art becoming a science?—Not a bit of it. I could give you plenty of illustrations.

14911. If I have understood your argument aright, you agree that in certain specific communicable diseases you have concurrently specific bacilli or micro-organisms?—For example?

14912. For example, anthrax and Malta fever?—In Malta fever that is not proved to demonstration yet.

14913. I understood you to say that there were certain specific micro-organisms or micro-cocci or bacilli; that with these specific infectious diseases you do in many cases find these specific bacilli?—Undoubtedly, and I say they may be characteristic of the disease, but they are not the cause of the disease.

14914. They are either concomitants or inseparable accidents, as the logician would say, or they are caused by the disease, instead of causing the disease?—You might just as well argue that a river is the result of trout being in it or salmon.

14915. Therefore you say that cause and effect have been transposed by the bacteriologists?—Yes. In some rivers you will only get trout and salmon; in other rivers you get other fish, such as dace and those fish that live in still water, where salmon and trout will not live. And in the same way you have grouse living on moors, while you have partridges living in arable fields, and so on.

14916. But they are not the cause of the fields?—Just so.

14917. (*Sir John McFadyean.* But you can get the fields without the grouse and the rivers without the salmon?—Yes, that is just it. You have got the disease without the bacillus. That is all right.

14918. (*Sir Mackenzie Chalmers.*) Have you followed carefully the researches of bacteriologists; do you read their literature carefully?—I have got so sick of the matter of late that I have not done so.

14919. For how long have you not done so—for eight or ten years?—More recently than that. I have paid some attention to the matter.

14920. But you have not studied carefully recent researches?—No.

14921. You think it is a waste of time?—Yes, quite so, because my work has been the practical work—the work of a surgeon, and I have never found any benefit from bacteriology.

14922. It is interesting to ask, is there any large body of medical opinion holding the same views as you do?—I do not know. But you know at one time I was in a minority of three against millions.

14923. *Athanasius contra mundum?*—Yes, and I was accused of that the other day in a letter in the "Medical Press and Circular," because I denied the occurrence of tuberculosis as the result of drinking milk. I asked for one single, solitary instance in which it could be shown even that there was a suspicion that the disease had been thus communicated, and the writer said it was a case of Dr. Bantock *contra mundum.* I pointed out to him that that was the position that I occupied some quarter of a century ago when I began my attack on Listerism.

14924. And now the *mundus* has come round to Dr. Bantock in that respect?—On the question of Listerism undoubtedly, and I do not get the credit for the part that I played in introducing the so-called aseptic system or the system of cleanliness; but yet there are some, who are not prejudiced, who admit the part that I played in bringing it about. As you know, Listerism now is as dead as Queen Anne.

14925. I want to come to another point, please. You mentioned the case of smallpox. Do you hold with the protective effect of vaccination?—I do, as the result of experience.

14926. As a mere matter of accumulated experience?—Yes.

14927. Do you see any objection to using animals for the purpose of providing vaccine? Do you think it is justifiable?—I have myself vaccinated a calf, taken the lymph from that calf, and vaccinated children with it, but that is the only thing I have ever done in the way of experiment.

14928. We may take it generally that you have come here on the recommendation of an anti-vivisection society?—Will you allow me to say, with which I have nothing at all to do.

14929. I only wanted to know how far you and they agree except upon one point. Do you think it is justifiable to use animals to prepare vaccine—you do?—I have no sentiment in the matter of experiments upon animals. With me it is a question of utility, and if I can be shown that certain experiments are valuable in increasing our knowledge I have no objection to them. I do hold some objections which perhaps will come out in my examination.

14930. Let me take you to another thing. You have told us a good deal about diphtheria; have you had any experience with the antitoxin?—No.

14931. Do you think its use is justifiable or unjustifiable?—It may be justifiable, but it has yet to prove its usefulness.

14932. Have you seen cases treated with the antitoxin?—I have not, and I should not think of treating a case with the antitoxin.

14933. You would not use it yourself?—No, I should not.

14934. But in the case of people who believed in its beneficial effects, do you think that they are justified in using animals in preparing the antitoxin?—It does not matter to me how the antitoxin is prepared. If it can be shown to be useful I would admit its use, of course.

14935. I want to go one step further. A good many new drugs come into use. Do you think it is justifiable before those drugs are tried on human beings to try them on animals?—Whether it is justifiable or not, I would say that in my opinion it is useless. I am inundated with new remedies every year, a thousand and one—"The Arabian Nights" was nothing to it—and scarcely one of these remedies that are so much vaunted is of any use.

14936. That is very likely?—And these drugs, mind you, as I understand, are sent out on the faith of certain results obtained by experiments on animals, not on the human subject.

14937. Surely among a thousand drugs, you may every now and then get one which is of great use?—As I have said, I think the thousand and oneth is the only useful one; and even in the case of those that are useful their application is very limited. A number of them were mentioned here by a witness—veronal, trional, sulphonal, and some other things with names as long as your arm, but none of those things are of any use. I have tried several of them, and they are absolutely useless, if not dangerous. Sulphonal, as you know, is a dangerous remedy because it is cumulative.

14938. I only wanted to get a general principle from you, to know what your opinion is. You think that researches in pharmacology are useless?—Yes.

14939. And it is better to stick to the old familiar remedies which we know, on the whole?—The longer I live the more I am reduced in the number of my remedies, until I can now count them on the fingers of my two hands. And there are fallacies about old remedies. Many of the old ones you will find, if you investigate them, are perfectly useless. So that as I say I can reduce the remedies that I use now to the number of fingers and thumbs that I have got.

14940. That simplifies medicine, does it not?—Immensely.

14941. Did you read Dr. Cushny's evidence given before this Royal Commission?—I fancy that I did, but I do not remember the trend of it.

14942. He gave evidence on the usefulness of animal experimentation with new drugs, and he drew the conclusion that it was of great benefit. I do not know how far you agree with him?—I do not agree with that. And I think Sir Lauder Brunton gave evidence to the same effect.

14943. He did?—I do not agree with that at all.

14944. Did you read Sir Lauder Brunton's evidence?—I did.

14945. You disagree with his conclusions?—Undoubtedly, particularly on the question of surgery.

14946. On the question of physiology, do you think that physiological experiments on animals are justified or not justified; do you think that we can learn anything from them?—I doubt it very much.

14947. Do you think that we ever have learnt any thing from physiological experiments?—I am afraid not. I am afraid we have not got any knowledge gained by observation beyond the case of Alexis Martin. I do not think we have got beyond that.

14948. That was at the end of the eighteenth century, after the Canadian war?—It was a long time ago anyhow. I think the date has been given in someone's evidence.

14949. May I take it, then, that the very numerous physiological experiments on animals since then have taught us nothing?—I think so; that is my opinion.

14950. You mentioned one other matter that I should like to ask you about; you mentioned the theory that plague was communicated by means of the rat flea as the mechanism of communication?—Yes.

14951. Have you read the two reports of the Plague Commission?—No, I have not.

14952. I am sorry that you have not, because the arguments to a mere layman seemed sound, and I should like to know where the fallacies were?—Well, there was one piece of evidence given before this Commission quite recently which freely excited my risible faculty to a very large extent. It was that when the experimenter was engaged in collecting fleas from the rats he wore top boots. I think when it comes down to that we have got the *reductio ad absurdum*.

(*Sir John McFadyean.*) The Commission must be devoid of a sense of humour, then.

(*Witness.*) I think that is in Dr. Martin's evidence.

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14953. (*Sir Mackenzie Chalmers.*) Did you read his account of the experiments themselves, and the way in which he tested his theory?—No, I am afraid I did not, for as I say, I am rather sick of the thing.

14954. I may take it that you did not read the actual experiments?—No. I quoted the evidence obtained in India, where it was shown that patients who had died from plague presented no bacilli in the body. I stated that Mr. Hankin, Chemical Examiner and Bacteriologist for the North-west Provinces and Oudh, reported to the Plague Commission in 1899 that there was no doubt that cases of plague occurred among human beings in which no microbes were visible at the time of death; and I think that an actual observation like that is worth any amount of theory.

14955. There were also experiments made on the opposite side by Dr. Charles Martin on one day, were there not, where he excluded the flea and the animal did not take the disease; but where the flea got access to the other animal, the other animal did take the disease?—But it is absolutely impossible to ascertain all the conditions that surround a case of that kind, and unless you can do that, you cannot rely upon the observations.

14956. As you have not read the observations, I do not think there is any use in discussing it?—No, I do not think so.

14957. (*Mr. Ram.*) Do you think that the fact that it is impossible on certain occasions—say, in these cases of plague—to discover the bacilli in the patient, warrants you in saying that there is no such thing as a bacillus?—No, but what I maintain is, that it does not warrant the supposition that the bacillus is the cause of the plague. That is my argument.

14958. If there is a failure in certain cases to discover the bacillus, that does not preclude the discovery of the bacillus in other cases?—Certainly not.

14959. I think you have told us that many years ago you formed a strong opinion against what I may call the bacillus theory?—Quite so.

14960. And that for several years past, at any rate, you have not even read the researches made by others in the matter?—Quite so.

14961. And you have never yourself performed any experiments with the view of ascertaining the presence of bacilli or otherwise?—Never; I have never bothered my head about it.

14962. You know that we have had witnesses before us who have spent the whole of their lives in such experiments as these, and many witnesses of great distinction have laid before us evidence which, in their opinion, at any rate, warrants them in thinking that they have discovered the actual bacillus in the case of certain diseases capable of communicating the disease. Do you ask us to reject all that evidence in favour of the opinion of one witness who has never experimented, and has not lately even read the researches about the matter?—I ask you to accept my opinion for what it is worth. I am a practical man. I have been engaged in surgery for more than a quarter of a century, and my views are the result of practical work and observation in that work, and as the result of that I have come to the conclusion that the bacilli play no part in the causation of disease. They may be associated with the disease, and they may even be a characteristic of the disease, but they are not the cause of the disease.

14963. I think you told me just now, very candidly, that if you could have brought to your knowledge the fact that the undoubted bacillus by itself, apart from the surrounding fluid, on being injected into an animal caused the disease, that would lead you to modify all your evidence?—I would not say positively that it would.

14964. Then what is your attitude of mind with regard to that?—I cannot say. I should have to know all the circumstances, all the conditions surrounding the experiments, before I could give my adhesion to any particular doctrine.

14965. The case which I am putting to you is the case which has been very frequently put to us—that of the bacillus of some disease separated from any surrounding fluid, washed repeatedly, isolated, if I may so say, so as to be the bacillus pure and simple, injected into the blood of an animal, and causing imme-

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diate symptoms exactly identical with the disease of the person from whom it originally came?—Well, I have considerable doubt as to whether that has been done or not—whether the bacilli have been deprived of all the material by which they are surrounded. I say I have considerable doubt as to the absolute success of the cleansing process.

14966. But when you say you have doubt about it for many years, at any rate, you have not taken the slightest pains to find out what has been done in the matter?—No, I have not, and I do not profess to give any opinion on that subject. I have been so satisfied with the evidence that I have had, that I have ceased to take any trouble in the case of bacilli.

14967. Your evidence and such experience as you have had (and I know that it has been very large indeed, and very successful) have been entirely on the human subject?—Yes.

14968. And you have made no sort of experiments with the view of ascertaining the existence or non-existence of any bacillus?—Quite so.

14969. Now, with regard to this aseptic or antiseptic treatment, you have said that you were one of the pioneers of that which is practised so much now—namely, the protective measures taken by being cleanly?—Yes.

14970. By being cleanly, do you mean anything more than merely the use of water?—And soap.

14971. Against what do you guard when you are cleanly?—Dirt; and that is the only explanation I can give you.

14972. And what in dirt is it that you desire to guard against?—I do not know, nor does anybody know.

14973. You think that if you were not cleanly you might infect the patient with some disease with which you had been recently in contact?—Yes, undoubtedly.

14974. There is a something which some people call germs and other people call bacilli——?—Not generally so.

14975. May I finish my question? Am I right in thinking that there is in dirt a something which some people call germs and some people call bacilli, and of which you say that you do not know what it is, against which you wish to guard?—Undoubtedly.

14976. And against which, if you did not guard, you might infect the patient?—Quite so.

14977. You told us of the case of some surgeon who in old days went to a man who I think you said had been operated on for hernia. He inserted his finger into the wound; the wound was suppurating. He then merely wiped his finger on a cloth or towel, and he afterwards inserted the same finger unwashed into the body of another patient?—Yes.

14978. In your opinion was that a dangerous thing to do?—Under the circumstances it was not a dangerous thing to do, because the two wounds were in the same condition; but if he had gone to the first case and just wiped his finger and then gone and performed a fresh operation and allowed some of that material to get into the wound, then I should look for disaster. But the two wounds were in the same condition.

14979. So that the only safeguard was in the condition of the patient and not in the care of the surgeon?—Yes.

14980. Supposing that the second patient to whom he had gone was a patient with a clean uncontaminated wound, what would he have given to him?—Inflammatory mischief, followed by suppuration and goodness knows what.

14981. Why would he have given it to him?—Because he would have introduced some poisonous material apparently into the wound and otherwise dirt.

14982. He would have wiped off, by the process that you have told us, anything in the nature of fluid?—Not all of it. He would get some of it under the nail, for instance.

14983. And you think that that fluid might produce very serious consequences?—Yes.

14984. Why is it easier to suppose that the fluid would cause mischief than to suppose that in the fluid there would be a bacillus that would cause mischief?—You must prove that the bacillus was there first of all.

14985. I do not think that that is quite an answer to my question. I should be glad to have an answer to it?—Will you kindly repeat it?

14986. Certainly. Why is it easier to suppose that the fluid, as you have just told me, would cause mischief to the second patient, than to suppose that it was a bacillus communicated by the finger which was the cause of mischief?—Because the fluid is visible to the eye and you know the fluid is there, but you do not know that there are any bacilli in it; and, as we know, in the case of a chronic ulcer the application of the active staphylococci is of benefit to a wound that is not healing up well.

14987. But are there not thousands of cases in which things which are invisible to our eyes are most undoubtedly potent factors of either good or evil?—But how can you speak of them as being potent factors if you do not know that they are there? You only suppose that they are there.

14988. If you take a tiny drop of what you have been speaking to us about—from a vaccine pustule—and you put it into the body of an animal or a man, it produces vaccine?—It reproduces itself; it does not produce septicæmia.

14989. And you suggest that it is because we can see this tiny drop that it produces it. What does it matter whether we can see it or not?—It does not matter; all you want is to know that you put it there. It does not matter whether you see it or not. But you know that you put it there. You take it on the point of your lancet and introduce it.

14990. You are so confirmed in the view that you hold that there is no such thing as a bacillus being the cause of disease, that you think there ought to be no further research into the matter at all?—I am very much of that opinion.

14991. That is not the method by which medicine and many other sciences in the past have made advances?—I am not aware of the influence which these experiments have had upon the advance of practical work.

14992. But you have told us that you have not latterly, at any rate, tried to inform yourself?—No.

14993. Still, you think that no research as to the nature and method by which disease is communicated is advisable?—I would not go so far as that.

14994. Then how far do you go?—I would simply go to the limitation of experiments. I am not an opponent of vivisection. I might say that I should be almost sorry if vivisection were abolished, because it may be useful yet. It has yet to be proved that it has been of material service, but it may yet be so. The only opposition that I should offer to vivisection would be that operations should not be performed before a class—that experiments of which the results are already known ought not to be repeated; and that experiments with a view of ascertaining the action of remedies ought to be very much curtailed. That is the extent to which I should go.

14995. I am much obliged to you for the answer. I think it helps us very much, having regard to what we here have to investigate.

14996. (*Dr. Gaskell.*) On page 3 of your *précis* you give Lord Lister's words: "Hence I was led to conclude that it was the grosser forms of septic mischief rather than microbes in the attenuated condition in which they existed in the atmosphere, that we had to dread in surgical practice." Do you intend us to believe that by those words Lord Lister meant something that was not living germs?—I do not know what he meant. You must apply to Lord Lister himself for the meaning of his words.

14997. I heard the lecture myself; I was there?—This is taken from the report of his lecture as published in the "British Medical Journal."

14998. I know it. I was there at the time in Liverpool and heard it?—But one might make a mistake in one's recollection of a thing; but one cannot make a mistake in copying down the exact words of the address, and that is what I have done here.

14999. But I want to be quite certain that you do not want us to believe that that term, "the grosser forms of septic mischief," in any way implied that Lord Lister had given up the germ theory?—I cannot answer that question. I do not know what his ideas were. He gave up the idea that the spray could be of any use in affecting germs floating in the atmosphere.

15000. But in saying "the grosser forms of septic mischief" he was putting that against the next sentence, "the attenuated condition" of microbes in the atmosphere?—He said: "Hence I was led to conclude

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that it was the grosser forms of septic mischief, rather than microbes in the attenuated condition in which they existed in the atmosphere, that we had to dread in surgical practice." What is the meaning of "the grosser forms of septic mischief"?

15001. Throughout the whole of his lecture was it not the case that he dilated upon the influence of the germs in producing suppuration and disease, which were "the grosser forms of septic mischief"?—I forget what he said throughout his lecture. That was the part that interested me, because it had a direct bearing on my own practice.

15002. But nobody who listened to that lecture could doubt for a single moment that Lord Lister meant throughout to hold on to the germ theory of disease?—At the same time I should just like to point out to you that Lord Lister certainly said at Liverpool that the condition was one of great importance; that it was not the germ or the bacillus alone, but that it was the condition necessary for the action of the bacillus.

15003. There was another point, too, that I wanted to be quite clear about, which you did not seem to me to make quite clear when Sir William Church asked you a question. You said, speaking of the microbes of tuberculosis, that we now know that air, light, and sunshine favour their growth. I do not understand about that "now know"?—As the result of experiments on sewage we now know that if you expose the sewage matter to the air it favours the action of the microbes.

15004. Then you refer in that "now know" to microbes in sewage, and you are not referring to the microbe of tuberculosis?—But it applies throughout.

15005. Does it apply throughout?—Yes.

15006. (*Sir John McFadyean.*) By analogy?—Yes.

15007. (*Dr. Gaskell.*) You have no experience that there is some difference, that some microbes like light and some microbes like dark?—I am quite aware that some flourish more in light and some in dark.

15008. And are you not aware that some will be actually killed out by exposure to light and sunshine?—No.

15009. You are not aware that there is a great difference in microbes in that respect?—I am not aware that they are killed off, for instance, when they get into a river and are exposed.

15010. (*Sir William Church.*) I understood you to say in answer to me that you distrusted those experiments in which bacilli were exposed to light and air, because they were done under unnatural circumstances?—Unnatural conditions.

15011. And I ventured to use the term that the very fact of their being in captivity was unnatural. Therefore, if you consider that an unnatural circumstance, you doubt all these experiments?—Yes.

15012. (*Dr. Gaskell.*) You have not seen any experiments of the kind?—No, none.

15013. They are very striking?—To some minds.

15014. I wanted to be clear, if I could, with respect to this fluid that accompanies the microbes, and which, when injected, reproduces itself, and so causes disease. That I understand is your position?—No, I have not said that.

15015. That it is something in the fluid?—I have not said that. The only instance that I have given of a reproduction of itself is in the case of smallpox. I have not said anything about the fluid of diphtheria reproducing itself.

15016. What I wanted to understand more clearly than we have yet obtained, was what is meant by the words "reproduces itself." All bodies can be put into two categories, either living or non-living. A living body we know reproduces itself. I do not understand what you mean by a non-living body reproducing itself, therefore when you use the term "reproduce" do you mean a living substance or a non-living substance?—You mean an organism or a substance.

15017. A living substance or a non-living substance?—I have made use of the word reproduction in the case of smallpox for this reason: that when you introduce an infinitesimal portion of the contents of a vesicle under the skin you have an enormous multiplication—call it multiplication or reproduction, whichever you like—of the poison; it is incalculable.

15018. I do not understand what you mean. If it was a salt in water, of any kind whatever, would there be that enormous reproduction?—Certainly not; I doubt whether there is any reproduction in the case of snake poison. We do not know the action of snake poison; we do not know how it acts upon the blood, whether it is multiplied or whether the initial dose is sufficient to destroy the blood and kill the subject; but we do know in the case of smallpox, because we can get any quantity of poison possessing precisely the same properties. There is no analogous condition, that we are aware of, to that of smallpox.

15019. Do you not rather mean by its reproducing itself that it may possibly be a spreading of the small thing rather than an increase in quantity? Snake poison is the spreading possibly of the initial poison that is put in?—Spreading throughout the body.

15020. Carried about in the blood and fluids?—Yes.

15021. Do you not mean that that is what happens with each vesicle?—No, I do not think they are at all analogous. I think that there is no resemblance between them. Smallpox and vaccinia, which are probably the same thing, stand by themselves; there is nothing else like it in Nature.

15022. Now I have got it. Then it is a process of reproduction which is dissimilar to any other in Nature?—Undoubtedly, but no one can explain the nature of it.

15023. I understand you to suggest to us that if you have a fluid in which certain organisms, bacteria or microbes, are found, in your opinion the mischief is done by the fluid parts of that fluid rather than by the microbes themselves?—Undoubtedly.

15024. How would you test it? If you separated the fluid from the microbes would you expect the mischief to be done by the injection of the fluid part or by the injection of the part that contains the microbes?—I should say by the injection of the fluid part most likely.

15025. Are you aware that there is such a thing as a Pasteur filter which prevents these small microbes passing through?—I am not aware that there is a filter which will prevent microscopical objects from passing through.

15026. It is so; it has been shown again and again?—Well, of course, if you say that it is so, I am not prepared to say that it is not so.

15027. In that case if it was possible to do that you would expect the fluid part to give the disease rather than what is left behind?—Yes, that is what I should expect.

15028. You are not aware that that experiment has been done over and over again with just exactly the opposite result?—I am not aware of that.

15029. Would you tell me what is a trypanosome?—I do not know anything about it.

15030. You have never heard of it?—I have already said that I do not know anything about sleeping sickness or the cause of it.

15031. But you said that the tsetse fly developed its own poison?—Yes.

15032. Did I rightly understand you to mean that in the case of both nagana and sleeping sickness it was the poison of the tsetse fly that gave the disease?—I am not aware that the tsetse fly is the cause of that sleeping sickness. I know that it has the effect of killing horses and cattle in certain districts, but beyond that I am not aware that it has any influence upon the human subject.

15033. In the case of killing horses and cattle, I understood you to say that it was the poison that the tsetse fly developed that killed them. Is that your opinion?—In certain districts of Africa the tsetse fly kills horses and cattle, but I have never heard that the tsetse fly produces any effect upon man. If you read Gordon Cumming and Livingstone, and all those earlier explorers, who tell us everything about the tsetse fly, we never hear anything of sleeping sickness or any disease communicated to the human subject.

15034. Is it your opinion that it is the poison produced by the bite of the tsetse fly that kills those animals?—I believe so most thoroughly.

15035. You are not aware of anything to the contrary?—No.

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15036. In fact, you know nothing about trypanosomes?—I will not allow that I know nothing about them, because I have read extensively the writings of Gordon Cumming and Livingstone and other explorers who have told us about the ravages of the tsetse fly on horses and cattle, but not on human beings. Human beings were not attacked at all.

15037. If you or any of your friends were bitten by a mad dog, would you go to Pasteur?—Not a bit of it.

15038. You would not give him a chance?—No.

15039. You gave us some statistics with regard to hydrophobia. Those were all general statistics were they not?—No, they are very specific.

15040. They are statistics as to the number of deaths in certain times?—I think those statistics are specific, because they refer to particular times, and to a particular district of France.

15041. But they were general statistics, they were not case statistics?—They were not case statistics. I do not consider that the giving of cases is a statistical statement at all.

15042. When you first performed surgical operations for ovariotomy would you have considered it fair to you that the general statistics as to deaths from ovariotomy should apply also to your case?—No, certainly not.

15043. Would you not have felt very much aggrieved if it had not been a case there of case statistics rather than general statistics? Would you not have considered it unfair unless the person who drew up the statistics had said, Dr. Bantock has had so many cases of ovariotomy, and has had only so many deaths? —General statistics, lumping together the statistics of half a dozen operators, would be absolutely useless. What you wanted to ascertain in the earlier stages of the operation was what was the success obtained by any particular operator, and what was his method of operating.

15044. Is not that what you want to obtain with regard to the Pasteur Institute and hydrophobia?—I do not see the analogy.

15045. And not the number of deaths that took place in a certain year throughout France?—I think if you can show that there was a certain number of deaths during a certain number of years before the introduction of the Pasteurian treatment, and that the deaths had not been reduced over a similar number of years after the introduction of the method, that is a fair comparison.

15046. Would you like that sort of comparison to have been made in your own case with ovariotomy. I should not have liked it if I had been you?—I should not like my cases to be compared with the results of anybody else certainly, but I am quite open to have my results in a certain number of years compared with the results of another number of years. I think it would be very unfair to credit me either with the successes or the failure of any other operator. I must stand on my own feet.

15047. (*Dr. Wilson.*) With regard to that, of course, in dealing with cases of ovariotomy you cannot compare any results at all with the results obtained or supposed to be obtained by the treatment of a disease like hydrophobia?—Oh no. I do not see how you can talk of them in the same breath or the same week.

15048. Returning to the effects of bacteriology on surgery, I do not think your contention has been made quite clear. Your contention is that bacteriological views rather led surgeons astray in the first instance?—Undoubtedly. If the bacteriologists claim that the study of bacteriology has had any influence upon surgery, then I say that that influence has been a gigantic mistake in the form of Listerism.

15049. Was the term Listerism applied to what is known as antiseptic surgery?—The name was given to the method almost immediately after its introduction.

15050. And am I right in defining antiseptic surgery in this way, that antiseptics, carbolic acid say, were used as a spray to prevent the wounds becoming infected by the microbes contained in the air, and that antiseptics were also used for instruments, dressings, and ligatures? Was that antiseptic surgery?—That was antiseptic surgery as we understood it when Lord Lister invented it.

15051. And it was discarded eventually, just because the use of the antiseptics which were supposed to kill the bacteria injured the wounded tissues of the body?—Undoubtedly that was the sole reason why it was discontinued.

15052. And nowadays, of course, the spray is never used, nor are antiseptics?—No.

15053. And you, Sir William Savory, Mr. Lawson Tait, and others, from your own painful experience by trying this antiseptic method, revolted against what is called Listerism?—Quite so, and I might instance the late Dr. Thomas Keith, who never improved his statistics by one iota after he had adopted the Listerian method; and the result upon himself was that after a long operation under the spray he invariably got an attack of hœmaturia, and he ultimately died from kidney disease.

15054. But Listerism, or antiseptic surgery as I should call it, was advocated most strongly at the time by leading surgeons of the day?—All over the world.

15055. I may quote this passage from your pamphlet giving an extract from an address by the late Sir William MacCormac—he was then Mr. MacCormac: "The lecturer said, 'other methods I would term inexact. The Listerian method is, in contradistinction, an exact method'"?—That is so.

15056. That is in using the spray, and so on?—Yes.

15057. "'Founded on a special theory, and carried out in all its details, in almost precisely the same manner in each case: that the access of ordinary air is very generally the cause of inflammatory or putrefactive changes in wounds," and so forth"?—Yes.

15058. Those were the words used by Sir William MacCormac?—Yes.

15059. So that the consensus of opinion, as it is called, in those days was that the spray must be used for successful surgical operations?—Quite so.

15060. And then, afterwards, the system which was introduced by Mr. Lawson Tait, yourself, Sir William Savory, and others, known as clean surgery, came to be generally adopted?—I may say that the only two operators were Mr. Lawson Tait and myself. Sir William Savory had delivered a lecture at Cork upon the subject, but I do not know how far he carried out his views in practice.

15061. And on account of your strong protest you were blackballed because you referred to it?—That was after I strongly opposed the Listerian method, and advocated the practice of cleanliness; but I did not use the word aseptic.

15062. (*Chairman.*) I daresay it was so, but it would be very difficult for anybody who has been blackballed to say precisely why it was?—I know, my Lord.

15063. I have no doubt that it was an operating cause?—Yes, there is no mistake about it.

15064. (*Dr. Wilson.*) And you define aseptic surgery simply as clean surgery?—Clean surgery.

15065. (*Chairman.*) I should like to ask you a further question. I understood you to say that if you could imagine yourself converted, or perverted, you would say, to the opinion of those bacteriologists who believe and have satisfied themselves that a bacillus is the cause of a certain number of ascertained diseases, then you would think that the pursuit of bacilli for the purpose of discovering and identifying them as the cause of disease by means of experiments on animals would be quite justifiable if you can imagine yourself of their opinion as regards the cause of disease?—I should like to answer that by saying that at present I cannot conceive of my conversion to the doctrine.

15066. But might I assure you that you are not committing yourself to any admission by saying that, but simply putting yourself in the position of agreeing with probably nineteen-twentieths of the experimental physiologists of this country: if you could imagine yourself reduced to that, what would you say?—If I could imagine myself; but I cannot.

15067. Then I will put the question in another way, though I should have thought that you might with ease have allowed your mind to be sufficiently elastic as to follow me; but as you cannot do that, take the case of those who do strongly believe it, do you see any reason why they, holding that opinion, should not endeavour to follow out that line of discovery by experiments on animals?—I see no reason. I see no objection, and I see no reason to object to that.

15068. And also in the same way in the discovery of remedies by those means—holding those opinions?—Quite so.

15069. Whether these diseases are caused by a bacillus or by a fluid is a thing which cannot be proved either way, you say. Supposing that by experiments on animals you could prove that it was not the fluid but the bacillus, or that it was the fluid and not the bacillus, would you think that experiments for the purpose of ascertaining that very important matter and making it perfectly clear, justifiable?—I should have no objection to experiments of that kind, but I do not think that they ought to be continued indefinitely.

15070. I will not ask you to repeat the conditions that you applied; you applied those conditions that you mentioned before?—Yes.

15071. You said that ridicule was the only answer to be given to those who thought that the plague was produced by the rat flea, and you gave us a reason for that that one witness had said that the doctors who were engaged in examining into the plague question in India wore top-boots to prevent their being bitten by the fleas?—Yes.

15072. That is not all that Dr. Martin, the witness in question, said. His answer is at No. 12111. He is asked, "Incidentally if the rat flea is the ordinary way of communicating the disease, how is it that doctors continually handling these animals in transferring the fleas from one to another, were not bitten?" His answer is "I imagine they were occasionally bitten." And then he added, "For one thing, they wore top-boots," but he goes on to speak of other precautions. I do not know whether you have read the evidence?—Yes, I have.

15073. I suppose that if these fleas do convey the plague it is very desirable not to be bitten by them?—Undoubtedly.

15074. And if there are fleas frequenting rats upon earth floors in a house in India, would you not think it was a very prudent thing to avoid contact with the fleas hopping about; would you not protect your legs?—Well, they will not convey the disease.

15075. But supposing you believed that the rat flea did convey it, would you think that it was a ridiculous thing, if you were going in the course of your duty into a place where you knew there had been plague and where you thought rat fleas were likely to be, instead of putting on a pair of light shoes to put on a pair of top boots?—I should think the protection very insufficient.

15076. It is better than shoes, is it not?—I do not know. The fleas would not attack you on the legs only.

15077. Of course they might fall off the ceiling, but they would be more likely to be on the dusty floor, would they not?—Yes, but I do not accept the doctrine that plague is due to rats.

15078. I know you do not, but you said that one of the reasons why you thought that ridicule was the only answer to that suggestion was that these doctors wore top boots when they went into places where the floors were likely to be frequented with fleas. That is not a very formidable reason, is it?—No, but I probably had other reasons in my mind when I reduced it to two lines.

15079. Dr. Martin goes on to say, you know, that other precautions were taken when they were likely to come in contact with the fleas.

15080. (*Sir William Church.*) The name of an old colleague of mine has been mentioned frequently to-day, Sir William Savory. Of course, he did not accept Listerism in full at first, but are you aware what Sir William Savory's views were with regard to, if I may so call it, the germ theory and suppuration?—I had no communication with Sir William Savory, except some little time after I was black-balled by the society.

15081. Do you know anything more of his views than what you have gathered from the lecture he read in Cork?—No more, except in conversation with him.

15082. And were you in the habit of conversing frequently with him afterwards?—Not frequently, occasionally.

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15083. (*Sir John McFadyean.*) If I may ask you one more question, do I rightly understand you to contend that the term Listerism or antiseptic surgery, as introduced by Lord Lister, should be held to cover only the use of the spray intended to destroy atmospheric germs?—And all the other arrangements that he devised.

15084. Do you admit that he also was largely instrumental in introducing methods directed to destroying what he supposed to be the germs on instruments?—That is part of the system.

15085. I think that any person would have gathered from some of the answers which you gave to Dr. Wilson that the aseptic surgery which is now practised universally in London and all over the civilised world is practically what you call clean surgery and claim to have introduced rather than what Lord Lister introduced?—Undoubtedly.

15086. At what period did you begin to apply antiseptics to your instruments?—When I began to practise.

15087. What was the antiseptic that you employed?—Carbolic acid.

15088. Have you given up the use of things intended to destroy the *materies morbi* on instruments?—Absolutely, for the last quarter of a century.

15089. But you do not wish the Commission to believe that that is the sort of surgery that is practised in the London hospitals?—I do not know what the practice is.

15090. (*Chairman.*) I asked you to describe exactly how operations were performed when antiseptics were in vogue and then when the aseptic treatment came in, and you described to me that in the first case first of all the spray was used, which has been abandoned since, and that the hands of the operator were washed with some antiseptic, and that then antiseptics were used, some of them of an irritating kind like carbolic acid, upon the wounds in dressing. I asked you then to describe what was the aseptic treatment, and you said that it was the same, with the exception that the spray was discontinued and also the bringing of anything like irritant antiseptic such as carbolic acid into contact with the tissues; but that with that exception the proceedings were the same as heretofore?—That is so. The distinction between the two is as plain as a pikestaff.

15091. But it is rather different from merely saying soap and water.

15092. (*Sir John McFadyean.*) You really do not want us to believe that in the great hospitals of London at the present time anything approaching your system of clean surgery is practised?—I never contended that.

15093. They would not be content with washing their instruments and their hands with soap and water?—I never contended anything of the kind.

15094. Is that a part of Listerism?—It is not a part.

15095. You say that you began to direct your attention to instruments when you began Listerism?—Yes.

15096. Surely that is a part of Listerism. If not, I am asking who deserves the credit of having introduced it?—Allow me to say that many eminent surgeons do not use any antiseptics in connection with operations at all; that is to say, they keep their instruments in water which they call sterilised.

15097. Hot water, you mean—boiling water?—Just so, water that has been boiled.

15098. Do you want to deny the use of the adjective "aseptic" or "antiseptic" to boiling water?—I say that the use of the term "aseptic" is nonsense, because you cannot say that you have performed an aseptic operation. You can say that you have performed an antiseptic operation, but you cannot say that you have performed an aseptic operation, because you have to wait for the result.

THIRTY-FOURTH DAY.

Tuesday, 12th November 1907.

PRESENT:

The Right Honourable the Viscount SELBY (*Chairman*).

Sir W. J. COLLINS, M.P., M.D., F.R.C.S.	Mr. A. J. RAM, K.C.
Sir J. McFADYEAN, M.B.	Mr. W. H. GASKELL, M.D., F.R.S.
Sir M. D. CHALMERS, K.C.B., C.S.I.	Capt. C. BIGHAM, C.M.G. (*Secretary*).

Mr. J. N. LANGLEY, M.A., D.SC., F.R.S.. called in; and Examined.

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15099. (*Chairman.*) You are Professor of Physiology in the University of Cambridge?—Yes.

15100. And in that capacity you have both to experiment on animals, I presume, and also to teach with regard to experiments on animals?—Yes.

15101. And do you give demonstrations before students?—I generally give three or four in illustration of my lectures in the course of the year.

15102. You have given us the heads of the points on which you desire to speak. I believe first you desire to say something about your personal experience that experiments on animals under the existing Act rarely cause pain?—On that point I wish to state the evidence which has come before me; it is almost entirely in connection with the University of Cambridge. In such a matter it seems to me better to give one's personal experience. I have had experience of the physiological laboratory in Cambridge ever since it was founded, and in that time all the experiments within my knowledge have been performed under the influence of anæsthetics. There are two classes of experiments which are carried out; the first are those simply under a licence in which anæsthesia is always maintained throughout the whole of the experiment and the animal is killed. In those I do not see how, short of gross carelessness, there can be any pain inflicted; and such carelessness, in my experience, does not occur. The other class of experiments come under a certificate; an anæsthetic is given, and then the animal is allowed to recover from the anæsthetics; the pain inflicted is that which occurs in a surgical operation, the experiment is done under the aseptic treatment, and it is done, moreover, on an animal that is healthy. An animal that is healthy recovers from an ordinary skin wound or a surgical operation with extraordinary rapidity. I myself have seen animals in an hour or two after the skin has been sewn up take their food in the ordinary way, walk about perfectly content; cats will purr. What I would call the more ordinary physiological experiment which comes under this certificate really does not inflict anything that can be called pain.

15103. What do you call the more ordinary physiological experiments?—Those are the more frequent ones.

15104. You mean that they may be severe?—Those that are severe I should put under the second class, which I would now speak of. In some of those experiments, possibly—such, for example, as where there is a section of the spinal cord, undoubtedly there is some pain during the process of recovery. That pain is less, I think, than in a human being, but it does occur and is of the same nature as in man. So that what I mean with regard to the class made under the certificate is in the first place that the great majority are quickly recovered from and do not inflict pain, although they come under this certificate which implies that they do. Then, secondly, there are some operations which do inflict pain, but the pain in that case is never, I think, as great as is common in a surgical operation on man. And on the whole, so far as my experience has gone in the Physiological Laboratory at Cambridge, the total amount of pain that has been inflicted is exceedingly slight. There are very few cases where I could have said that the animal had been suffering pain after any operation where the anæsthesia under the certificate has been allowed to cease. I may also mention that during the whole surgical part of the operation the anæsthesia is strictly maintained, that there is no question about doing anything at all on the animal unless it is anæsthetised, and properly anæsthetised. All the skin stitches, for example, are inserted under anæsthesia; nothing is done to an animal except in that condition; then it is put aside and slowly recovers from the anæsthetic.

15105. Do you make any distinction as regards the keeping up of the anæsthesia, where the animal is going to be allowed to come to after the operation finally, between the treatment of an animal and of a man?—None.

15106. You would keep the anæsthesia on just as completely and as long in the case of an animal as you would in the case of a man?—Absolutely.

15107. Does that include the sewing up of the wound and the dressing? In the case of a patient, I suppose you would put them comfortably up in their beds before allowing them to come to?—Yes.

15108. Is that the same process, leaving out that last element?—Yes, entirely the same. Everything that is done to the animal is done under anæsthetics. It is then put aside, and after everything is over it slowly recovers from the anæsthetic. In fact, generally myself I have given a little morphia or a little chloral, or something to prevent it from coming to until some time after the operation, because an animal

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will stand more anæsthetic than a man; if it dies there is not the same serious mischief done, of course; so that on the whole one really gives more and prolongs the anæsthetic more in an animal than one does in man.

15109. You say that is your experience in your laboratory?—That is my experience in my laboratory in Cambridge.

15110. But you have studied elsewhere also?—I did work in Heidelberg for about three months, but there were not many experiments going on there at the time, and that is, of course, outside the operation of this Act.

15111. I was going to ask you whether your experience extended in that sort of operation outside Cambridge?—I have seen them in various laboratories outside Cambridge, and they have all been conducted, so far as I have seen, in exactly the same kind of way.

15112. (*Sir Mackenzie Chalmers.*) Just to clear up one point, it has been suggested to us that under Certificate B, it is a frequent course simply to administer the anæsthetic for the purpose of a cutting operation only, and that other exploiting and the painful sequelæ of the operation immediately connected with it are done after the animal recovers. Have you ever known a case of that sort?—It has never even occurred to me. The thing seems to me so foreign from anything that I can imagine is likely to occur in the laboratory that I hardly know how to answer the suggestion.

15113. It would not occur to you to construe that Certificate B as allowing that?—Certainly not; it never occurred to my mind that it could be done. Then with regard to the anæsthetics that are used, no one, I suppose, has any doubt that nearly all the anæsthetics which are used are sufficient to maintain anæsthesia practically indefinitely. I should think that could be taken now as an established fact.

15114. (*Chairman.*) Although it may be taken, amongst scientific men, as an established fact, there are other people who do not understand it to be so. You say that for an indefinite time the anæsthesia may be prolonged. In a case where you are going to give an ultimately lethal dose, but intend to operate in the meantime on the living animal, how long a time can intervene between the giving of the dose which produces anæsthesia and the death of the animal?—Of course, one has never carried on an experiment in that way. All that one can say is that one has never seen any sign of a tendency to recovery of consciousness in consequence of lapse of time; but generally it is the other way, that the anæsthesia becomes deeper and deeper, and the animal more and more immobile the longer the anæsthesia is kept up, and I have seen for something between eight and ten hours' complete anæsthesia maintained.

15115. (*Sir Mackenzie Chalmers.*) With or without artificial respiration?—Without artificial respiration.

15116. (*Chairman.*) So that if you required two, three, or four hours for an operation on an animal, and it was intended it should never come out of the anæsthesia, would you have any difficulty in keeping it under anæsthesia, yet still alive for the period?—Absolutely none—that is quite certain.

15117. (*Mr. Ram.*) When you said the animal would become more and more immobile, did you mean more and more incapable of reflex action?—Yes.

15118. Never at any time capable of conscious action?—Never at any time.

15119. (*Chairman.*) You were speaking about anæsthetics?—Of morphia I have very little personal experience. I have not done many experiments under morphia, but in those which I have done I am quite confident that no pain was inflicted. The animal in those cases lay perfectly still; when severe things like cutting the skin were performed, they did not cause any movement. I purposely tried this in consequence of a doubt which has been thrown out with regard to the anæsthetic properties of morphia. This was an animal that was not fastened, so that there could be no question that if it wanted to get up and run away it could do so; and there was no movement. As I say, I have not done many of those experiments, so that I have not great experimental experience in the use of morphia, but within my personal experience I am confident that pain was not inflicted by the operations of cutting the skin, or by the operations of cutting a nerve which would, without the morphia, have caused very considerable pain. Another point that I should wish to call special attention to is this question of the difference between reflex actions and ordinary voluntary conscious actions. It is, of course, known in man that during anæsthesia in the absence of pain and absence of consciousness movements may occur. I suppose that I need not enter into that; it does not come specially within my province; but, of course, one can say something about it on general grounds if it is considered desirable. But I should take it that it would be accepted that the experience of anæsthetists has shown that there is a degree of anæsthesia which allows movements in man to occur which are not accompanied by consciousness.

15120. Or by pain?—Or by pain. What I wish to point out is that in animals this limit of range between the disappearance of pain and the disappearance of reflex action is far wider than in man. That is partly, mainly I suppose, because in man the forepart of the brain, which is concerned with consciousness and with pain, is so much more developed relatively to the rest of the nervous system. The rest of the nervous system is really a mechanism for carrying out a number of movements that is controlled by the fore-brain, which is concerned with pain and with consciousness. Now, the greater the normal working of the forepart of the brain, the less is the automatic, the intrinsic, power of the lower part of working by itself. In a very low vertebrate as the frog, all the mechanism of movement is carried out by the spinal cord and lower centres—it can be carried out purely by reflexes. The more the higher centres of the brain, which we may call those of the fore-brain, are developed, the more the lower centres cease to act of themselves. So that a suppression of the higher leads to a greater suppression of the lower. Or, in other words, when in man you suppress the activity of the forepart of the brain, then there is a greater diminution in the ordinary power of activity of the lower parts; they go so far hand in hand. The lower you go in the scale, the less effect is produced by suppressing the higher parts, those concerned with pain and with consciousness. Now anæsthetics are substances which have a special action upon the fore-brain. That is really what is meant by an anæsthetic; it is a chemical substance which acts primarily upon a particular portion of the central nervous system, which is that of the fore-brain. Anæsthetics amongst themselves differ very greatly as to their power of acting upon the lower centres. Chloroform, as I have mentioned, first abolishes pain; then it abolishes consciousness; after that there is a rather narrow limit of increase of dose at which it begins to act upon the lower centres, but for some time after nearly all the ordinary reflex actions have gone, respiration, which is part of the mechanical apparatus, which is part of the working of these lower centres, still goes on. And, moreover, it can be influenced by decreasing the amount of oxygen in the blood; it can be influenced by various effects upon the nerves, so that the mechanism is obviously working in the respiratory centres just as it also works, but to a less degree, in the whole of those lower centres of the spinal cord. The danger of chloroform is in the narrow limit between its acting on the higher and on the lower centres. If you give a little too much, it not only destroys pain and consciousness, but it stops the action of the rest of the brain and of the lower centres. If you give a little too much, it stops respiration; and remember in man respiration lasts a little longer than most of the other reflex mechanisms. Morphia is immensely more selective. Morphia picks out the pain centre, and it leaves these other lower mechanisms for a time practically intact. Moreover, it not only does that, but it stimulates a certain part of these lower centres, quite special ones, and they are those which do not give rise to pain in normal life. In an animal under morphia this selective action can be shown (it is obvious) in this kind of way. If a small point of the skin is taken between the fingers and pinched hard, nothing results; if a knife is taken and the skin is cut there is no movement; but if you give a loud rap on the table close to it, causing vibration, then there will be a more or less convulsive start of the whole animal. That is not a painful sensation.

15121. That is caused by the action upon the animal's nerves?—Yes, it is caused by the action on its lower mechanism. The evidence that is really produced by the mechanism is that as you increase the

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morphia the special reflex to a certain extent increases, and it continues (when it is pronounced) after the whole of the fore-brain has been removed. It would continue in a portion, in the hind limbs for example, after not only the fore-brain, but the whole brain, and the top part of the spinal cord, had been removed, so that there is little more (in that case, of course, artificial respiration must be kept up) than a fragment of the nervous system with the hind limbs depending from it. In that state, if you gave a sharp rap causing vibration, the hind limbs start in exactly the same kind of way that the fore-limbs and body do in the intact animal under morphia.

15122. Is that an experiment that would produce the same results in man as it would in any animal?—I believe that you would get the same sort of thing in man if the observations were made in the same conditions. In man many people have described the convulsive action which occurs near the point of death with large doses of morphia. It is well known, in fact, that there are certain twitchings of the muscles, and that these ,in certain particular cases, spread into real convulsions with excess of morphia. It is in a state of deep coma that this occurs. In fact, not uncommonly death results from the convulsions which are produced in coma. The convulsions are, I am certain, the same kind of thing as this increased reflex that is produced in the lower animals. In man, what is done to keep a man alive when an excess of morphia is given is to take him and shake him. He is walked up and down the room. It is old fashioned; it is not done as much now, but it was an empirical experiment, that to save a man from morphia you marched him up and down, shaking him at intervals and shouting at him. So far as any painful stimulus was concerned, he was in a state of profound stupor, and took no notice; but the shaking produced some kind of evidence that all this lower reflex mechanism was still reacting. I am quoting this not so much on the action of morphia itself as an anæsthetic, as in relation to the degree of movement that can be obtained after complete loss of consciousness. What I want to insist upon is that each anæsthetic has special properties, that the nervous system consists of groups of nervous centres performing different functions. Anæsthetics form an incoherent group of chemical bodies which have this in common, that they act first on the central nervous system in the part where pain and consciousness lie; after that there are endless differences in the other effects; they may, like chloroform, rapidly sweep through all the rest of the central nervous system and destroy the function of that too; they may, like morphia, linger in the higher nervous system and only slowly proceed to abolish the lower. Further movements which are reflex, and which may be produced in anæsthesia, are the movements which are produced by a diminution in the amount of air, or a diminution in the amount of oxygen. In animals in a state of deep anæsthesia, if asphyxia is produced; that is, if the supply of air is cut off, you may get more or less struggling movements which diminish with the depth of anæsthesia, but in perfectly complete anæsthesia it will cause rather strong movements; the reflex action upon this respiratory mechanism is seen though to very much less extent in man also. The evidence that in animals it is reflex is shown by removing the obstruction. Almost immediately the air gets into the lungs and the blood gets oxygenated, the movements cease and the animal is perfectly quiescent.

15123. Is it the case also that the animal has become insensible?—Absolutely insensible. The anæsthetic is given first; it is given to a stage in which the animal could be tossed about without any indication of movement. Then the trachea is stopped, the breathing is stopped by stopping the inlet of air, and movements will begin. The air is allowed once more to get to the lungs, and the movements stop. The trachea is stopped, once more, *i.e.*, the supply of air is cut off, and the movements begin again, and so on; it will go on in this state of anæsthesia constantly with the regularity of the ticking of a clock.

15124. In ordinary suffocation not arising from anæsthetics, does a man become unconscious before he dies?—Yes.

15125. There is a space?—Yes.

15126. In that space does he struggle?—He struggles early, and for some time after unconsciousness is produced; unconsciousness comes on rather early in suffocation, so far as can be judged by external appearance in man and by what is said by those who recover, but the movements in suffocation go on practically up to death, and usually there are rather slow laboured respirations at long intervals at a time when consciousness must have long gone. It varies greatly in different individuals. In the case of an animal that is anæsthetised, and then is asphyxiated, there is a regular series of ways in which the nervous mechanism works, and the last are inspiratory gasps which occur at an interval of perhaps 10 or 20 seconds, long drawn, and then they diminish and slowly die out. During that time and long before that the animal is completely unconscious. I have mentioned above methods in which various movements are obtained in anæsthetised animals, and which in the description of experiments in physiological accounts give rise to great misconception. There are many people who think that when in the account of an experiment it is said that the animal moved, or that the animal struggled, it implies a return to consciousness. That is not the case. It is perfectly certain that all the varied kinds of reflex movements and the general movements of the body may be noticed long after all pain and consciousness have gone. As a last example a small reflex movement may be produced by slight tension of the limbs, sometimes when the feet of an animal are fastened there may be a rhythmic movement of the hind limbs.

15127. You mean while the animal is under anæsthesia, but fastened, to counteract any movement?—To steady it and prevent it from rolling over.

15128. In order to prevent reflex movement?—Yes, merely to steady it. If the cords are relaxed to a certain extent the movements stop. If you gently tighten them the movements begin. If you relax them again they stop, and so on. It is the effect of slight tension; moreover, that occurs with a portion of the spinal cord only, without any brain and without any medulla in the animal, in which practically everything has been removed, and I have not the faintest doubt that that would occur with artificial circulation; if you could remove all the front part of an animal, and take only the posterior part, and inject arterial blood through it, you would get the same regular movements steadily occurring on a particular kind of tension.

15129. That, I think, is what you wish to say about anæsthetics?—Yes.

15130. You, of course, have formed some opinion as to the value and importance or the reverse of using experiments on animals for the purposes of physiological and medical knowledge?—On general grounds it seems to be almost self-evident that as physiology is the study of the mode of action of the living organism and of its various parts, that they must be studied living, because the function ceases when life ceases. Anatomy, the use of the microscope, observations in health and disease, offer suggestions as to functions, but these must necessarily be tested by experiments, for otherwise they leave room for endless conjectures. To take a single example. The functions of certain nerves can only be determined by actual experiments on animals. It is impossible to discover where a vaso constrictor or a vaso dilator nerve runs except by trying it. There is no microscopical difference, there is no anatomical difference. The only way of noting the difference between a vaso dilator and a vaso constrictor nerve is to try and stimulate the nerve and see what effect is produced.

15131. (*Sir Mackenzie Chalmers.*) That is not necessarily a sensitive nerve?—No, a peripheral nerve, a nerve removed from the centre and running to the periphery of the organism, as to a muscle or gland, will serve for experiment.

15132. Is that necessarily a sensitive nerve?—Not necessarily.

15133. (*Chairman.*) In point of fact, was the distinction between those nerves known before experiments on animals?—It was not known before experiments on animals, I think.

15134. Was it known that there were two sets of nerves, apparently the same, but having different functions or action?—So far as I know it was not known. I have not discovered any reference to it before the experiments of Claude Bernard.

15135. It is now known by those means, I under-

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stand, that there is that distinction, and you can discover which nerves act in a particular manner?—You can discover which nerves cause dilation of vessels, and which cause them to contract. And in the same way nearly all we really know of the functions of the different parts of the body has been derived from experiments on living tissues and living organs and living animals. With regard to the applicability of the results obtained on anæsthetised animals to unanæsthetised animals, it has been said that observation made on animals under anæsthesia are not applicable to ordinary life, because they are abnormal. The use of the word abnormal in that sense is purely misleading; it depends on a juggle with words. It seems to be supposed that anything that can by any chance be labelled abnormal must therefore be inapplicable to normal life. It is obvious that that is not the case, because one of the ordinary meanings of the word "normal" is "average."

15136. I do not understand why the words normal and abnormal are used. The question is rather, is it not, whether you cannot necessarily apply to human life a conclusion which you may safely draw from animal life? You would not say that if the life of an animal, or its construction, differed in some respect from that of man, it was abnormal?—No, I take it that one objection that has been made to experiments is that the anæsthesia modified so much the whole character of the living tissue, that you could not apply the results to the normal unanæsthetised animal.

15137. I see. I thought you were arguing simply from animal to man?—No, the relation of animal to man is, I think, the second line of argument; but the first, which is more general, is that you cannot use any result obtained under anæsthesia. Of course, that depends upon a purely verbal juggle with words. The abnormality consists solely in the alteration in pain and consciousness. As I have indicated, the nature of anæsthetics is, to begin with affecting certain parts of the nervous system, and it is only subsequently that they go to other parts of the nervous system, and it is subsequent to that that they proceed to the muscles and nerves and the various glands in the various parts of the body. So that, in fact, nearly all the parts of the body are normal, and in the ordinary condition; their state is one which allows conclusions to be drawn as to the condition in ordinary life, except as regards the particular circumstances of pain and consciousness. The animal is abnormal. The tissues are not abnormal; they are perfectly normal. So that in that way all the muscles, the blood vessels, and viscera—nearly the whole body except just this part of the brain connected with pain and consciousness—is in a condition which allows you to definitely state what is the condition in ordinary life. With regard to the applicability, then, of experiments on animals to man, it is to be noted that the tissues of man and the organs resemble very nearly those of all other vertebrate animals, so that there is a probability that the function of any one tissue or organ in any one vertebrate would be approximately the same as that which it has in other vertebrates. Further, if you find a function in a number of different animals, the probability, then, is very great that there will be a similar function in man, and the more animals that you can show have the same kind of function, the more it is necessarily applicable to man. As a matter of fact, a similarity, and often a very close similarity, has been shown over the whole range of physiology to exist between man and the vertebrates. The normal working of the parts of the human body differs in most cases only in detail from that of the higher mammals, so that experiments on animals give to a large extent positive evidence with regard to the condition of man. But they do more than that, because they clear away a number of erroneous hypotheses. If we look back to the history of medicine, it is clear that treatment is always open to a number of erroneous modifications, until a definite reason is given why a particular method of treatment should be applied. As soon as a reason can be given, then a particular line of treatment is rapidly adopted. It satisfies the human mind to have something which it can positively say is the cause of a particular thing. Until that can be given, so long there is room for numerous conjectures, numerous side issues, and numerous different forms of treatment. That is really indicated at the present day very largely by the different treatments in nutrition. One will recommend the constant feeding on meats; another constant feeding on this or that diet. These different treatments which are considered to be useful in particular functional processes mark the fact that we do not know the scientific reason for the dietetic methods, and, in consequence, all the possibilities of treatment are open and have followers; so that the greater the number of possible ways of accounting for a fact, the greater is the number of erroneous methods of treatment. Physiology, I think, then, aids medicine and surgery in those two ways, in that it brings positive results which are in the highest degree probable for man, and then that it clears away a number of erroneous hypotheses, and gives some firm ground for explaining current practice, and so leads to the adoption of the practice which is really the best and the most useful for the purpose.

15138. Are experiments upon animals, in your opinion, necessary in order to correct a great many errors which arise from hypothesis or suggestions?—I think they are absolutely necessary. The usual method is to disprove first one hypothesis, and then to disprove another, and gradually to arrive at truth in that way, but each step in the process, I think, is valuable in its application to the ordinary treatment of medicine and surgery.

15139. Of practical value?—Yes, of practical value. One case of a particular advance in surgery which I should like to bring before the Commission is that of the effect on the surgical treatment of diseases of nerves and of severed nerves, which has been brought about by experiments on animals.

15140. Do you mean so as to enable you to know how you should treat a case of an accidentally severed nerve in a man?—Yes. I think that has come from experiments on animals. The earlier relation I may treat quite briefly. Animal experiments disproved the old medical and surgical belief which came from Galen that nerves could not be healed at all. That was current till about 1770, and then Cruikshank and a number of other observers, by experiments on animals, which went on to 1822—they were, in fact, on dogs—showed that nerves could heal when joined together. That was adopted in surgical practice, and it became the custom then to join up the central end with the peripheral end of a severed nerve, and in a very short time it was found that the difficulty in certain cases was not to cause nerves to join, but to prevent their joining too rapidly in particular instances, as, for instance, in neuralgia. A modification of the treatment was introduced in the case where a large portion of a nerve was cut out. That, again, so far as I have been able to discover, was first introduced by Philipeaux and Vulpian in 1869, who in dogs introduced a portion of a foreign nerve between the central end and the peripheral end of the cut nerve, and found that the piece introduced could aid and help regeneration. That also is a constant practice in the surgical treatment of nerves.

15141. Were all those experiments made without anæsthetics?—I should think the earlier ones were made without anæsthetics, but the last that I have mentioned, those of Philipeaux and Vulpian, I think were experiments under anæsthetics. The earlier ones undoubtedly were without anæsthetics.

15142. They would be very painful operations, I presume?—That depends upon the nature of them. The simple connection of a severed nerve I do not think would be necessarily painful.

15143. Some of them, at all events, would be painful operations?—Yes, some of them would be.

15144. (*Sir Mackenzie Chalmers.*) They were foreign experiments?—They were made in France. The earlier experiments of Cruikshank were published in the Transactions of the Royal Society in 1789, I think; they were by an Englishman, in 1770.

15145. Can those operations be done now under anæsthetics?—Yes.

15146. (*Chairman.*) They could not be done then under anæsthetics?—They could not; there were no anæsthetics discovered at that time. The two main points then were, first, the actual union of a nerve after having been severed, and then the introduction of a piece of nerve. A further development then was in joining the central end of one nerve to the peripheral end of a different nerve, which is necessary in certain operations. For example, when a facial nerve is injured in the skull, as it is sometimes by an abscess, or by other causes, such as bullet wounds

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and so on, the central end cannot be got at—it is too deep down and in the bone—for the purpose of joining on to the peripheral end. What is done now, consequently, is to take the central end of another nerve and to join it on to the facial nerve, and that, it is found, produces recovery from facial paralysis more or less complete. This operation is one which can be traced with remarkable closeness immediately to operations which were made on animals. There was first a series of experiments which showed that the thing was possible—that is to say, physiological experiments which were purely of a scientific nature. The first of these was a very early one by Flourens, in 1824-1828, who performed the operation of the cross-union of nerves in the wing of a fowl. He took the nerves which run to the upper portion of the wing, and joined them to those which go to the lower portion, and *vice versa*, and he found that in no long time the fowl could use these nerves, and could fly—that they had actually joined crosswise—that the central end of one nerve had joined on the peripheral end of another and different nerve. Then physiological experiments on other nerves were made by Bidder in 1842, and by Philipeaux and Vulpian in 1863. That then led to some operations in France by Desprès in 1875. Further experiments on animals of a more recent kind are those of Rawa in 1885, Stefani in 1886, Howell and Huber in 1892, Cunningham in 1898, and Kennedy in 1901. All of these joined nerves which were approximately of the same kind. Schiff and Reichert in 1885, and myself in 1898, tried nerves of a different character; those in my case were apparently of a thoroughly different nature, and it was found that there also the central end could join the peripheral end. So that it was an established scientific fact that almost any motor nerve could join the peripheral end of another motor nerve. It took a little time before this purely scientific physiological deduction was translated into actual practice; that was done in France, Germany, America, and England with a few years; in nearly every case the deduction for the operation in facial spasm and facial paralysis in man was made as the result of the experiments mentioned above. Up to about 1898 facial paralysis in man was regarded as incurable, and certain kinds of facial spasm were also regarded as incurable. Nobody thought of doing anything except ameliorating the methods of treatment. Nobody supposed for a moment that they could be healed. The first successful operation was by Kennedy in England. The operation was performed in May, 1899. The paper giving an account of it was read before the Royal Society on November 22nd, 1900, and published in the Philosophical Transactions, 1901. The case was that of a woman who was admitted to the Glasgow Western Infirmary on account of chronic spasms and incessant muscular twitchings on the right side of her face, which had lasted ten years. As a result of that operation, the tonic spasm disappeared, and control over the muscles of the face was regained. Kennedy definitely states that he performed these operations because of the general physiological result that was known, and because of the experiments on animals which he made himself directly for the purpose.

15147. Was that a new operation on man?—Yes.

15148. And was it successful?—Quite successful.

15149. What was the condition of the person before the operation?—In Kennedy's case all one side of the face was in constant twitching, a very uncomfortable thing; there was tonic spasm which gathered up the corner of the mouth across the face.

15150. Was it disfiguring?—Most disfiguring; it would practically prevent the subject entering into a great many of the ordinary occupations of life.

15151. And after the operation did these spasms entirely disappear?—They entirely disappeared.

15152. And the face resumed its normal appearance?—Recovery is rarely absolutely complete; there is generally some sign left. But control over the face and control over the muscles was restored—that is to say, all this drawing of the face and the mouth to the side disappeared.

15153. But there are some traces left of the disfigurement?—I think in all these instances there will be some traces left. But Kennedy said that what induced him to try the experiment—which was, of course, rather a daring thing to do on man—was this: "Being convinced that voluntary co-ordinated movements had been restored after nerve-crossing in the dog, I decided to treat the case by dividing the facial nerve and grafting its peripheral segment on to the spinal accessory nerve, in the hope that the same result of nerve-crossing would result in man as had proved to take place in the dog."

15154. You have in print before you, I believe, the remaining evidence of the success of these operations?—I have. Subsequently several cases of facial paralysis, treated by joining the spinal accessory nerve with the peripheral end of the facial were described by C. A. Ballance, H. Ballance, and Purves Stewart ("British Medical Journal," May 2nd, 1903). In this paper Mr. C. A. Ballance mentions that he first performed the operation in 1895. The result was not known for some years, as the patient was lost sight of. Mr. Ballance informs me that he felt justified in making the operation "by the fact that the cross-union of nerves had long been known by physiologists to be a successful operation." In France the first operation for facial paralysis was performed by Faure [Faure and Furet, "Gazette des Hôpitaux," 71 An., 8 Mars., p. 259, 1898]. Furet suggested to him [cp. Bréavoine (op. cit. infra)] that the hypoglossal nerve should be joined to the facial. The choice of the hypoglossal may reasonably be attributed to this nerve having been used in the well-known experiments on the cross-union of nerves by the French observers Philipeaux and Vulpian.* On discussing the question, they came to the conclusion that the spinal accessory would probably be a better nerve to take than the hypoglossal. The operation was performed early in 1898. Nine months later Faure ["Revue de Chirurgie," T 18, p. 1098, 1898. Improvement in this case was noted at a later date, and in 1901 (Congès de Chirurgie, Paris, 1901) Faure describes it as a partial success] reported that no recovery had taken place. He argued, however, that success would be obtainable in proper conditions, since it had been shown that different nerves of the same nature can become functionally connected. The failure of the operation could not, however, but raise some doubt as to its feasibility, and this led Barrago-Ciarella [Barrago-Ciarella, "Il Polichnico," 1901: "Per consiglio del mio maestro, che vivamente ringrazio, ho voluto tentare la risoluzione del quesito che Furet e Faure vollero sciogliere, limitando pero questa mia prima ricerca al fatto, se cioè nella procurata paralisi facciali mediante la sezione del nervo alla sua uscita dal forame stilo-mastoides con l'immediata sutura del moncone periferico di questo all'estremita centrale dell'accessorio, resecata al momento del suo ingresso nella faccia interna dello sterno-cleido-mastoides, sia possibile ripristinare la perduta funzione dei muscoli innervati dal 70"] to test in dogs how far return of function could be brought about when the spinal accessory or the vagus was joined to the peripheral end of the facial nerve by primary suture. He made three experiments on dogs, and found that recovery of motor power occurred in each case. At Faure's suggestion, Bréavoine [Bréavoine, "Travaux de Neurologie Chirurgicale," VIme. An., p. 92, 1902] in 1901 drew up a statement of the observations made in various countries up to that date: "Dans le but de faire connaître en France les heureux resultats de cette opération, tant sur les animaux que sur l'homme." In America the first operation was performed by Cushing [Cushing, "Annals of Surgery," May, 1903] in May, 1902. The patient was a man of thirty years of age, in whom the facial nerve had been severed by a bullet wound. "Motor paralysis was complete on the right side of the face and in the platysma as well." "Bell's sign, the rolling of the eyeball on attempted closure of the eye, inability to pronounce distinctly the labials, lacrymation, and the other many discomforts attending on the condition of facial palsy, were present." As a result of the operation the discomforts disappeared, the muscles were saved from atrophy, and voluntary control over them regained. The operation was suggested to Dr. Cushing by experiments made upon cats. He says (p. 4) the "suggestion for such an operation came to the writer through cognizance with Langley's experimental work on the transference of nerve function after anastomosis between the vagus and the sympathetic." Other similar operations have recently been made by Korte in Germany, by Dr. Tubby in England, and probably by others elsewhere. It may

* *Faure and Furet refer to Lebierant, who refer to Philipeaux and Vulpian.*

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safely be predicted that the operation will alleviate human suffering and distress all over the world. But there are several questions still to be settled before it can be definitely ascertained what is the best method of operation. Thus it is probable that if the whole spinal accessory were cut through, the central end of the trapezial branch joined to the facial, and the remaining central branch joined to all peripheral branches of the spinal accessory, the maximum of independent voluntary control would be regained, with little or no permanent paresis of the muscles supplied by the spinal accessory. The further experiments, if not made on animals, must be made on man. Facial paralysis in an animal is much less distressing than in man, since a large factor in man, the consciousness of a distorted face, is absent in animals. Since I wrote the pamphlet from which this evidence is taken, *i.e.*, last year, my attention has been called to further operations which have been suggested by these operations, so that the effect is accumulating.

15155. Operations on the nerves?—Operations on the nerves in man.

15156. Had these operations ever been attempted before on man?—No. The operations which are now being carried out are those on birth palsy. When a child is taken out by instruments, it sometimes happens unavoidably that the nerves of the brachial plexus or elsewhere are torn, and that leads to permanent paralysis, which is called birth palsy. This has suggested that one of the central ends of a sound nerve of the brachial plexus should be joined on to the peripheral end of a nerve which has been injured; that has been done, and with success; the child has been saved from lifelong paralysis in that way. There is another disease, called acute anterior poliomyelitis. This is a disease of a certain number of cells in the spinal cord, which causes partial paralysis. There again the nerve which has these unsound diseased fibres is joined on to the central end of a portion of a sound nerve and that has also led, so far as it has been carried out, to satisfactory results.

15157. When you speak of the central end of a nerve, what exactly do you mean?—I mean the end that is connected with the spine. Then again in athetosis, which is a continued spasm of certain muscles, the flexor muscles of a limb are sometimes very much stronger than the extensors, and cause various twitchings and tonic spasms. What has been done has been to take the nerve from the flexor side and join it on to the extensor side, so that the excess of action which goes to the flexors is diverted to the extensors, and a balance is fairly maintained. That also, so far, is a success.

15158. How do you regulate the extent of the operation in accordance with the extent of the excess?—That can only be done, a little empirically, by estimating how much stronger the muscles are on one side than on the other, and taking the corresponding portion of a nerve to join across; the one is grafted on the other. A portion of the flexor nerve might be cut out, and the extensor nerve drawn across and joined on to the flexor nerve.

15159. Has that been done?—The operation has been done by Spiller, Frazier, and Van Kaathoven, and the account of it is to be found in the American "Journal of Medical Science," 1906.

15160. Is that of practical benefit to the person who has it done?—It is.

15161. Is the defect from which he was suffering a serious one?—Yes, very serious. These operations are of the very greatest benefit to people on whom they are performed.

15162. It is not a very common operation, is it?—I can hardly say how common they are. At present there are certainly not a great many operations being performed; it is entirely recent.

15163. In your view, are all these operations the consequences of experiments on animals?—I could give you a statement of Dr. Frazier, who wrote the paper in the "American Journal," and who is partly responsible for these operations.

15164. As you have given us the reference it will be enough, I think, if you can say that he does or does not attribute them to experiments on animals?—He attributes them to experiments on animals.

15165. And that will be seen on reference to his paper?—Yes. Lastly, there is the operation for partial hemiplegia, which is allied to these, and which hitherto has been quite intractable to treatment.

15166. Has that been remedied by operations?—In the few experiments which have been made the operation has distinctly benefited the patient.

15167. It may be said that these operations might probably have been discovered without experiments on animals, although, in fact, the possibility of them was discovered by that means. Does it occur to you that such might have been the case?—It is impossible to say that they might not have been discovered in the next century, but they would not have been discovered in this. One can only judge by seeing the progress of past inquiry, and the progress of past inquiry when it has been confined to man without the co-operation of experiments on animals has been so slow that I think we may legitimately say that without experiments on animals, these things would not have been discovered within any reasonable time.

15168. If they were discovered?—Exactly. It only means that experiments must be made on man or on animals, it is a choice between the two. No doubt, if experiments were made on man the same kind of things could be discovered, because, as I have already indicated, I think that many of the higher mammals are so closely similar, that what is applicable to one is applicable to another.

15169. Have you had any experience of the use of curare?—I have had experience of the use of curare for particular experiments, and having obtained a knowledge of the way to administer anæsthetics, I am confident that there is no difficulty in keeping up the anæsthesia whilst curare is given.

15170. And what seems to be very important, is there any difficulty in the operator knowing that the effect of the anæsthetic is complete throughout the whole time of the operation?—He must start with complete anæsthesia, and he must know from past experience the amount of the anæsthetic which it is necessary to give in order to maintain it.

15171. Must he start with the anæsthetic before he gives the curare?—Yes, before he gives curare the anæsthetic must be given.

15172. And then he must satisfy himself that the animal has arrived at complete anæsthesia?—Yes.

15173. And with a dose that will last for a certain time?—Yes, and which must be repeated. For instance, with chloroform, the chloroform is given at the intervals, and in such doses as his previous experience has shown will maintain complete anæsthesia.

15174. Assuming that he uses adequate care, the care that you have a right to expect from a skilled anæsthetist, would he have any difficulty?—I do not think he would have any.

15175. But he would have to rely on the dose and on his watchfulness?—Yes.

15176. That is to say, he could not see from any signs in the patient anything that would satisfy him?—There is the state of the blood pressure which will indicate, to some extent, the reflexes on the vascular system.

15177. Would pain felt cause an increase of blood pressure?—It would cause a rise of blood pressure, but I should myself always rely on the previous experience of the anæsthesia—how it is produced and the depth of it, and see that it is maintained in just the same way as if the curare were not given.

15178. Is that the precaution you have seen adopted when the curare has been used?—Yes, that is the mode. An anæsthetic is always given in my experience.

15179. Are there any operations on animals to which you can point in which you would say that the use of curare was necessary?—There are two operations which I can mention, which I have myself performed. One is in investigating the origin of the class of nerves called sympathetic nerves, from the spinal cord. These nerves issue from the spinal cord with nerves to the muscles, and in investigating where they originate, the nerve is severed from the spinal cord, and the end which is not connected with it, but connected with the periphery, is stimulated; that is to say, stimulation in a live unanæsthetised animal could not give any pain. That experiment would give extremely uncertain results unless curare were given, because the muscular movement which is caused by the other nerves mixed with the sympathetic nerves obscures the action of the sympathetic nerves themselves. They must be seen by themselves in order

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to be distinctly appreciated—for example, whether they cause constriction of blood vessels. If a muscle is tight and contracted, of course, it causes blanching or squeezing of the blood out of the blood vessels, and so there is a pallor which might be attributed to the sympathetic nerves, but which is really due to the other class of nerves. Then another kind of experiment that I have had occasion to try is that of showing certain nerve fibres going to the sphincter of the stomach, which cause dilation of the sphincter, which cause it to relax instead of causing it to contract. That experiment was done on a rabbit, and in the rabbit there are fibres running to the sphincter which do not belong to the sympathetic system, but which are of the same character as the muscles of the limbs. Those must be put out of action by curare before you can determine whether the sphincter contracts or dilates, or is inhibited when the nerve is stimulated. One could have got no satisfactory proof of the presence of inhibitory fibres in the sphincter of the stomach unless curare had been given. When curare is given the proof is absolutely clear and one or two experiments settle the question.

15180. We have had suggestions made that even if experiments on some animals are permitted they ought not to be permitted on dogs. Have you anything to say on that point?—In all experiments on animals, of course, one would naturally prefer, for various reasons, sentimental reasons, to avoid the use of higher mammalia. The same sentiment which makes one hesitate to experiment at all applies with increasing stress the higher you go in the scale of animals, ending, of course, in man himself; but experiments on dogs I think sometimes are absolutely necessary. Of course, on general grounds it is clear that physiological laws must have a broader basis the more different kinds of animals are investigated, and to remove dogs from the category would be to limit the general application and determination of physiological laws. Professor Starling has already given a number of instances in which dogs are required for experiments, and I need not repeat those. I would only mention two which are of immediate interest to me in my own work. One is in relation to the sympathetic nerve fibres which run to the lips and to the gums. In no other animal can that be properly investigated, since vaso-dilator fibres in this region are much more developed in the dog than in any other animal, and it is necessary, I think, to perform experiments on dogs in order to arrive at any satisfactory result.

15181. With what object?—To find out the general relation of the vaso-dilator nerves in the sympathetic system. It is the only case, as it happens, which is definitely known, which can be shown, in which there are vaso-dilator nerves in the sympathetic system at all; and the following out of that problem may lead to very interesting and very important results. At the present moment, of course, one cannot definitely say what is likely to come from it.

15182. It is research really?—It is research. And the second instance also is a piece of research work of the same kind, that is to say, as to the presence of vaso-dilator fibres in the posterior roots of nerves. These may be of great physiological importance, but there is so little known about them at present that it is almost open even to doubt their existence, and it is certain that the investigation of them, so far as one can see at present, must be performed on dogs, and that no other animal will adequately meet the case.

15183. As regards teaching by means of demonstration experiments on animals, you practice that to a certain extent yourself, and therefore I presume that you approve of it—that you think it necessary?—I approve most strongly of giving a certain number of demonstrations to students. I think they ought to see in the first place an experiment, under anæsthesia, so that they may judge of it themselves, and form a clearer idea of the working of the body. They cannot see it in any other way. It is always recognised that wherever you can show a thing, the fundamental truth of it is much better appreciated if it is seen rather than if it is simply read about, and in my experience the students take the greatest, but a serious interest in the demonstration, and I do not understand at all how anyone who has been familiar with students attending those demonstrations can say that they have any demoralising influence whatever.

15184. Have you seen any symptoms of it at Cambridge?—Absolutely none.

15185. Your classes almost entirely consist, I presume, of those who are either going into physiology as a profession, or are going to practice medicine as a profession?—Yes.

15186. About how many would there be in a class?—About 100.

15187. Would only a certain number of those be present at demonstrations?—They would all be present at demonstrations—*i.e.*, the demonstrations are open to all.

15188. Could they all see the demonstration?—If it is a case where they cannot all see, it has to be repeated in two sections.

15189. (*Sir Mackenzie Chalmers.*) That is to say on two animals?—On two animals.

15190. (*Chairman.*) Have you seen any sign of heartless conduct on the part of students—exhibitions of jesting, I mean?—I think it is quite the reverse; that they are all extremely anxious to assure themselves that the animal which is there has no trace of pain. One of the things which I generally show in a course is some of these reflexes during anæsthesia, because as a rule they are not satisfied on the first view that the movement can be carried out in a wholly anæsthetised animal. They always press home the point, and that is usually shown to them in such a manner that they can satisfactorily convince themselves of it. I am sure that there is no levity of demeanour; that they are extremely impressed with the necessity of avoiding the infliction of any unnecessary pain.

15191. You think that the public opinion of the students would be quite against any exhibition of levity?—Entirely. It would, I think, be impossible.

15192. Have you any suggestions to make with regard to amendment of the present Act?—I have some suggestions to make on the lines that I think that if precautions have been taken to prevent severe and prolonged pain, and to prevent any unnecessary pain, it should be the object of the Act rather to encourage than to discourage experiments on animals. I think in the first place that the interference with research, which is caused by the necessity of taking out certificates in addition to a licence, should be lessened. I would suggest that after a licence has been held for, say, ten years, it should ordinarily be endorsed with permission to make experiments under any of the certificates, a return, of course, being made of such experiments as are performed. In the next place, I am inclined to think that the injection of non-irritating substances under the skin, and feeding experiments when they are not calculated to give pain should be excluded from the Act altogether. I think it creates a good deal of prejudice to see that a large number of experiments are made, many of these which are not experiments of a painful nature at all. That experiments merely involving a prick of the skin should be massed together as experiments requiring the sanction of an Act of Parliament does not seem to me to be a satisfactory thing. Then again, I think the necessity of taking out two certificates for an operation on a cat or dog, that is to say, B and E E, or, in the case of a horse, ass, or mule, B and E E and F, should be got rid of, and that one certificate should be substituted. I should, by the way, add the monkey to the same class as the cat or dog.

15193. (*Sir Mackenzie Chalmers.*) Do you mean that the ordinary Certificate B should include the right to experiment on dogs without a special certificate?—If you want to experiment on a dog, there should be a separate certificate, that you should not have to get Certificate B first. Now one has first to get B and then E E.

15194. (*Chairman.*) You would have Certificates A, B, and E E all on one piece of paper, you mean?—So far as I understand the Act you cannot do an experiment on a dog without having certificate B.

15195. (*Sir Mackenzie Chalmers.*) You might have A and E E?—Yes, but that means two.

15196. You might have A and E E or B and E E?—Yes, but you must have two. If anyone is researching and only wants to do experiments on dogs, why should he get Certificate B, which is for experiments on other animals?

15197. Certificate B covers animals generally which are to be experimented on with anæsthetics, and then are to be allowed to recover?—Yes.

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15198. Then, if you want to go into the particular category of dogs you get Certificate E E as well?—Could you not go at once to the category of the dog without the intermediate stage of animals generally?

15199. If you had Certificate E E only, the persons who give the certificate would not know whether you want E E plus A or E E plus B?—The certificate might be headed A plus E E; then you would obtain it direct, instead of getting two sets of signatures and having two certificates to fill up. There are various ways in which it might be done.

15200. (*Chairman.*) You mean that it would save time and trouble in applications?—It would save time and trouble to the people who have to sign. It is rather hard, for instance, to take up the time of the President of the Royal College of Surgeons in giving signatures, if it is unnecessary.

15201. And it would save time in getting the certificate, and not waiting for it?—Yes.

15202. Are there any other alterations that you would suggest?—In the certificate for teaching purposes I think that the word "absolutely" before "absolutely necessary" should be struck out, because it is so much a matter of argument what you can say an experiment is absolutely necessary for. It comes to this, that for teaching purposes at present, only experiments which are absolutely necessary can be performed, and the word "absolutely," I think, throws an unfair onus on the holder of the certificate. If it comes to a court of law he does not know what view will be taken as to the meaning of "absolutely" in that sense. Because from the physiological point of view there is nothing that is absolutely necessary. There are things which are advisable, and there are things which are more or less necessary, but I doubt whether in any scheme of education, short of learning to read and write, you could say that anything is necessary, if it is pressed home to a point.

15203. You prefer something nearer to the word "desirable"?—I think if you say "necessary," and leave out the "absolutely," that would be adequate. People would then understand it; they could say it is necessary to their functions. There are many things which you can say are necessary, but I think there are very few things which you could say are absolutely necessary.

15204. (*Sir Mackenzie Chalmers.*) The Act assumes that certain experiments will be absolutely necessary, or it would not have made that provision?—Yes, it is an alteration in the Act which I am suggesting.

15205. The Act assumes that certain experiments must be absolutely necessary, because it makes those exceptions, and leaves you to determine what they are?—I suggest that that is throwing an unfair burden upon the licensee.

15206. (*Chairman.*) I understand that your view is that the condition should not be that you have to prove the necessity, but the desirability in the interest of science, or some words of that sort?—Yes.

15207. (*Sir Mackenzie Chalmers.*) What word would you substitute?—I should leave out "absolutely."

15208. You would say "necessary"?—Yes.

15209. But not "absolutely necessary"?—Not "absolutely necessary." It seems to make too much of a different grade of proof required in one case than in the other.

15210. (*Mr. Ram.*) It would eliminate a large number of cases if you were to keep the word "absolutely"; there are cases which you might say would be necessary?—I think it would remain purely arguable. Then a point which has probably been brought up already is that an animal which has been operated upon under anæsthetics and is allowed to recover should be allowed to be kept in any place, provided that notice of a change of place is given to the Inspector, and that the Inspector has the right of entry to the place, wherever it is. That is really in the interests of the animals; especially in the case of dogs; that is to say, if they are kept in the physiological laboratory it not infrequently happens that the place is less satisfactory than would be some place outside a town.

15211. (*Chairman.*) You mean for their recovery?—For their comfort.

15212. Do you mean by reason of the dog associating the place with the operation that it has undergone?—No; it is for their comfort and well-being.

15213. (*Sir Mackenzie Chalmers.*) You mean that your laboratories are not good places in which to keep the animals; they are necessarily rather dark and rather confined?—Yes.

15214. (*Mr. Ram.*) And you would give them a happier life elsewhere?—Yes, much happier. I have known people, for instance, who would like to take them home and see after them there, but they are not allowed to do it.

15215. (*Sir Mackenzie Chalmers.*) To give them fresher air, in fact?—Yes.

15216. (*Chairman.*) I see what you mean. You would take, I suppose, their address before they left?—Yes. Then I am inclined to think that whilst the animal under anæsthesia under a licence should, as now, be killed, of course, before recovery, anyone holding a licence ought to be able to experiment on that animal during the anæsthesia, because it would save another life.

15217. When it is going to be destroyed?—Yes.

15218. (*Mr. Ram.*) Under a lethal dose?—Yes.

15219. (*Sir Mackenzie Chalmers.*) Anyone holding a certificate, you mean?—Yes, because it merely saves another animal being killed, and, so far as that goes, I take it that is an advantage. If the animal is properly under anæsthesia, I cannot see that there is any objection to anyone who is properly licensed to do an experiment beginning at that stage instead of beginning a little earlier.

15220. (*Chairman.*) Would you have the second operation that it is to be subjected to described, as well as the first?—I think that anyone who does an operation must make a return of what he has done, but that in counting up, in the summary which gives the number of experiments, it should be counted as one experiment, because the number of experiments is to be considered as the number of animals, and you want to get the statement of the number of experiments to coincide with the facts of the case as to the number of animals which have been experimented on.

15221. (*Sir Mackenzie Chalmers.*) Or, rather, with the number of times that anæsthetics are administered. Surely it is two experiments, if you put an animal first under an anæsthetic and experiment upon it, and then let it recover, and then do a subsequent experiment?—Yes; I am talking, of course, of an animal experimented upon under a licence, and never to recover.

15222. (*Chairman.*) You mean when a dog under a licence has been given a dose of anæsthetic, from which it is never to be allowed to recover?—Yes.

15223. Whilst it is under that anæsthetic you would, if it was desirable in the interests of science, perform more than one operation?—Yes.

15224. (*Sir Mackenzie Chalmers.*) There is nothing in the Act to prevent that at present, is there?

15225. (*Chairman.*) I rather think that a witness has objected to that as illegal. (*To the Witness.*) Is there anything else you wish to say?—The last point I am not quite clear about, but apparently under the Act, a technical infringement of the Act of a very minor character may really mean an action at law with a threatening of a penalty of £50 and imprisonment. In an Act of this kind, which is to prevent pain, but at the same time to forward science and humanity, I think that no merely technical infringement should be liable to such a penalty—that it should be excluded from the Act and dealt with by the Home Office. Some of these infringements are really comparable to, say, in a Government Office, coming at 11 o'clock instead of 10. There may be ignorance of a thing, but no one would think of threatening a public employee with a fine of £50 or imprisonment for six months because by chance he came at 11 o'clock instead of 10 in the morning.

15226. (*Sir Mackenzie Chalmers.*) Practically that is secured, because no prosecution can be instituted against a licensee under the Act for anything done in contravention of the Act except by leave of the Secretary of State?—I am aware of that, but could it not be made a little more clear? I think that there are persons who are anxious to take any chance infringement of the Act, of however technical a nature, and

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claim of right that under the Act, as it stands, there can be a prosecution, and therefore the Home Office ought to prosecute. It seems to me that people ought not to be put in that position.

15227. I do not think that the Act contemplates the Home Office prosecuting, because the Home Office is the authority that gives leave for a prosecution.

15228. (*Chairman.*) I think what you mean is that there might be pressure brought to bear upon the Home Office to allow a prosecution?—Yes.

15229. And you think that a prosecution in such a case ought not to be allowed?—That is so.

15230. It is not a penalty of £50 necessarily; it is not exceeding £50. However, I see what your point is. Is that all that you wish to state?—That is all.

15231. (*Sir William Collins.*) How long have you held a licence?—Since the beginning of the Act, so far as I can remember.

15232. Since 1876?—Yes.

15233. What certificates do you hold?—Certificates B, C, and E E.

15234. (*Sir Mackenzie Chalmers.*) You do not do experiments under Certificate A?—Never.

15235. (*Sir William Collins.*) And I think you told us that your experiments have been uniformly painless?—Uniformly painless.*

15236. What animals have you employed?—A variety of animals—birds, rabbits, cats, and, rarely, dogs—very rarely dogs.

15237. Could you give us any idea of the number?—Of course, they are all in the return. Last year I should think none on any of those animals which I have mentioned. All my experiments last year under Certificate B have been on frogs. Perhaps something between twenty and forty experiments, including all kinds under a licence and under certificates, in a year, including frogs, rabbits, cats, and dogs—the whole thing.

15238. And did I rightly understand you to say last year none except on frogs?—Last year and this year. (The details are in the return that I have made). I have done very few experiments except on frogs. The number varies from year to year according to the line of experiments that one is carrying out.

15239. Have you employed any dogs this year?—Two.

15240. Do you employ dogs for demonstration purposes?—No.

15241. Do you think it necessary to do so?—For some experiments I think it is necessary. These demonstrations that I give are generally on the cat and the rabbit.

15242. Do you give a complete course of physiology?—Yes, I give a complete course of physiology. Perhaps my demonstrator may show an experiment on a dog. He does some in addition to those that I do.

15243. You do not yourself employ dogs for demonstration?—In the laboratory they are used sometimes for demonstration, but it is not very common.

15244. For class demonstrations?—Yes. I should think, perhaps, one a year.

15245. For what demonstrations?—For the flow of chyle, because that can hardly be seen in any other animal than a dog, to show that after feeding there is a large amount of material passing from the various parts of the body, absorbed from the intestine and flowing into the blood, and that it is milky. The amount of the flow is a very impressive thing in absorption.

15246. (*Mr. Ram.*) Would the dog be under a lethal dose of anæsthetics?—Yes.

15247. (*Sir William Collins.*) Professor Gotch, of Oxford, told us that he did not use now dogs at Oxford for his vivisection experiments?—No doubt the practice varies in different places.

15248. I was anxious to get from you whether you think that it is essential to employ dogs for purposes of demonstration to students?—It is certainly essential if they are to understand how the lymph flows in the body that they should see it. You can explain it, but that is a thoroughly different thing from seeing that particular experiment.

15249. Is that the only experiment that you show on a dog to a class?—So far as I remember, it is the only experiment that we show to a class. We might have, for instance—I do not know that it has been done, but it might be done for the more advanced class—we might have an experiment on the vaso-dilator nerves to the lips or gums.

15250. As a matter of fact, have you demonstrated that before a class?—Not to my elementary class. Whether I have done so to the advanced class I should hardly like to say. I should have to look up my records.

15251. You have drawn a distinction between painful and painless experiments. We have been told that the Home Office returns formerly made such a distinction, but that of late years that distinction has been given up, and I understood for the reason that it was sometimes difficult to make such distinction. What do you say in regard to that?—The degree of pain is, of course, always a relative thing. I do not think that there is any severe pain in the operations of which I have any personal knowledge. There is discomfort when a wound heals; there is certain to be discomfort; and it is rather a matter of nomenclature where discomfort passes into pain. I have indicated the difference, so far as I could, by saying that some of my experiments were clearly not painful, and some of them were of the nature of a severe operation in a hospital, and those I should think possibly at times do cause pain, as they do in a human being.

15252. Your work is limited to physiological research, I understand?—Entirely.

15253. You have suggested certain possible amendments in the Act, but I did not quite gather whether you suggested that the Act itself was undesirable or should be repealed?—I would much rather have the Act than no Act. I think it would not be fair to the animals to allow anyone to experiment upon them without control. I think it is a very natural desire that there should be some control of experiments, which might, if carelessly conducted, inflict pain, and I thoroughly approve of there being some control.

15254. And you desire that anæsthesia should in all possible cases be employed?—Most certainly.

15255. And you believe that, at any rate within your own experience, it is so employed?—I believe that it is so.

15256. We were told by a lecturer on physiology, in answer to Question 14115, that the Vivisection Act was entirely opposed to the advancement of physiology, and when he was asked whether that was an opinion held by others engaged in physiological research besides himself, he replied "Yes, I think so." What do you say in regard to that?—It cannot, I suppose, be denied that the working of the Act has to some extent interfered with the advancement of physiology, because there is the difficulty and trouble of getting certificates. In research of any kind the way in which it is carried out is really to go red hot on ideas as they occur to you. If you have to wait for three weeks to get a certificate, a particular idea which occurs to you is probably never carried out, and you go on with the humdrum and immediate things which are in your line of work. If you have everything ready, then some idea which may be very fruitful you can test by observation, you can test it if you can do so at the moment, but if it has to be put off you probably never test it. So far as that goes, I suppose it would be said to interfere, but I certainly do not think that the interference with the advance of physiology has been a really serious one. Physiology has, in fact, advanced very greatly in England during the operation of this Act, and, I believe, with a diminution in the amount of pain which has been inflicted.

15257. May I take it from that that you are not of opinion that the Vivisection Act is entirely opposed to the advancement of physiology?—I should not say that it was entirely opposed. I should say that it was only partially opposed to it.

15258. A great deal of your research work has been in regard to the nervous system?—A good deal of it.

15259. And you have drawn a distinction between

* *This refers to actual experiments and operations the state after an operation is referred to in Q.* 15251.

reflex actions and conscious action in your evidence to-day?—Yes.

15260. I do not think you gave us any description or definition of what you mean by reflex actions?—If you take a sensitive plant, and you touch it, the leaf shuts up; that is one form—the simplest form of reflex action; so if you take a fragment of the nervous system, separated from all the rest, you get similar little movements of a simple character in response to external stimuli, and if you take a little more of the nervous system, or get a little more complex piece of the nervous system, you get a little more complex movement; so that one follows the reflex actions up from the plant, where they occur in a very simple form, right through the animal kingdom, and it is on the basis of comparison of the behaviour of the simplest forms of life with the most complex that I should draw the distinction between reflex actions and those which are accompanied by consciousness. Eventually consciousness can only be judged of by ourselves. We cannot have any thorough knowledge of anything conscious except as it occurs in man, and in man conscious pain only occurs when he is in possession of certain parts of the central nervous system. If those go there is no longer any power of appreciating pain. Thus you can get an absolute lack of pain where you have a body of which a portion and the nervous system is cut or where there is a lesion of part of what is called the internal capsule; this gives complete anæsthesia on the opposite side of the body. The reflex actions can then build up the mechanism out of the simple cell, but except with certain physiological structures as a basis, there can be no consciousness and no pain.

15261. Is reflex action ever conscious?—It is never conscious.

15262. So that an essential part of reflex action is that it is unconscious?—That is a necessary part of a definition except that reflexes may in their operation also set in action a portion of the brain which is concerned with consciousness, so that there may be another chain set in action, but the reflex in itself goes on in its integrity entirely and absolutely independent of consciousness.

15263. Then when you speak of reflex action, *per se* it is implied in the definition that it is an unconscious act?—It is implied. The term, of course, is often used loosely.

15264. We have often had the term "incomplete anæsthesia" mentioned in the course of the evidence; have you any definition which would attach to that?—If it was incomplete, I should usually say that sufficient anæsthetic had not been given; but those words are not used with very great scientific accuracy.

15265. It is not a recognised physiological term?—I do not think we use it in pure physiology with any frequency.

15266. I understand you to say that the margin between the disappearance of consciousness and the disappearance of reflexes under anæsthesia was different in animals, and different with anæsthetics.

15267. Is that margin very much greater in the case of animals than in that of man?—Much greater. With chloroform you have to watch very carefully to prevent the respiration stopping, which is part of the reflex really, and not conscious; it does not give rise to ordinary sensation unless you attend to it.

15268. Would the teaching to students of anæsthesia in animals be of use in their carrying on such practice in the case of a man, or is the difference in this margin so considerable that it would make it unwise to use such instruction?—I think that would depend upon the intelligence of the student. With a good student I think it would be a great advantage to him, but with a student who would go simply by rule of thumb, it might be rather dangerous.

15269. One witness suggested to us that hypnotism might be employed in lieu of anæsthetics on animals. What would you say with regard to that?—I have done a good deal of hypnotism, but I never succeeded in hypnotising an animal or a lunatic.

15270. The lecturer on physiology at Guy's Hospital said, at Question 14149: "I say that the introduction of an anæsthetic may produce complications, so that more experiments may be necessary than if the experiment were done on the animal in a condition of hypnotism, which I offered to show to the Commissioners. That, I think, is one very important point. These animals pass into a condition, so far as one can see, comparable with hypnotism. If you give them anæsthetics you are introducing a complication which you could remove, and therefore, without anæsthetics, you actually save life and actually diminish the infliction of pain." What do you say with regard to that?—You can produce a condition which is similar to that of hypnotism in man in animals, but in an animal that condition cannot be maintained for more than a short time; in certain animals it cannot be maintained for more than a few minutes, and it is always uncertain; you could never rely upon keeping it up. It is not a condition in which you could operate on an animal. In a dog, so far as I know, it has never been produced at all. It is done with pigeons and with other birds and with frogs, but no serious operation could, in my opinion, be performed on an animal in a state of hypnotism. I should be extremely interested to see the condition of an animal where anyone would suggest that you could really substitute hypnotism for anæsthesia.

15271. The same witness, in answer to Questions 14119 to 14121, gave the following evidence: "Did I rightly gather that in your opinion anæsthetics are unnecessary?—(A.) Yes, I am convinced that they are unnecessary, not only in the case of animals but of men. (Q.) For vivisection and for surgical operations?—(A.) For vivisection and for surgical operations, and I mentioned the case of midwifery. (Q.) Would it be going too far if I were to express your opinion as being that pain is a good thing in itself?—(A.) I say that pain is a beneficent and protective mechanism. I have written that in an article which I have recently published, and I am prepared to defend it. It is not a new view, of course, at all, it is recognised by surgeons, I believe, that a certain amount of pain is a protective mechanism." Is that a view that is generally held by physiologists?—There are two points there, the one with regard to the application of hypnotism to animals, the other with regard to the beneficent action of pain.

15272. Hypnotism was not mentioned in the last questions that I read to you?—In the development of the race pain, of course, has been of value, but what we are all hoping to do at the present time is to diminish the amount of pain in the universe as much as possible. Its beneficence has narrow and restricted limits. In the struggle for evolution, no doubt some excess of pain was almost necessary for the selection of certain individuals from others, but as things are now, I think it can only be said that the less pain there is in the world the better it would be. That seems to me a perfect truism.

15273. Then are you, or are you not, of opinion that for vivisection and for surgical operations, anæsthetics are necessary?—I think that they are entirely necessary.

15274. The return of movements in an animal or in man under an anæsthetic might, or might not, be indicative of returning consciousness, I gather from what you have told us?—The first movements which return after complete anæsthesia will always be unaccompanied by consciousness. The later movements may or may not be accompanied by consciousness.

15275. But in ordinary surgical anæsthesia is not the return of movement not infrequently a reason for administering more anæsthetics?—Yes, it is the beginning sign that more is required, but not that consciousness has returned.

15276. In regard to surgical operations for reuniting a nerve which has been severed, and other experiments to which you referred, do I rightly understand you to say that they have all been dervied from vivisection?—Certainly not. The main leading things have been derived from vivisection, and they have suggested operations on man. The two have been going on concurrently ever since the beginning of the work.

15277. Do you trace the ordinary union of a divided nerve directly to vivisection experiments?—The adoption of it by the surgical profession as a practice was due to the advocacy of it by prominent physicians and surgeons on the basis of vivisection. People had tried the thing long before. I suppose that from the time of the Greeks you will probably find some evidence of surgical operations on the nerves, but up to the end of the last century there were many different views about it, and the prevalent medical view was that it was impossible to cause regeneration of the nerve; and the thing which turned the tide and really directed attention was the early experiments of Cruickshank, Houghton and Fontana.

15278. What date do you put upon them?—From 1770 to 1822.

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15279. You said that the erroneous opinion of Galen had been refuted by these experiments, as I understood you?—Yes, that is putting the thing in a concise fashion.

15280. Should I be wrong in thinking that Galen's opinions, whatever they may have been worth, were not derived from experiments upon animals?—I am afraid I could not offer any opinion as to where Galen derived his opinions from; I know so little about him.

15281. I see that the last Royal Commission says that Galen, in his writings, describes with painful particularity a great number of different kinds of tables which he had on which animals might be fastened, and severe experiments, which he describes, might be performed upon them. Apparently Galen's views, whether right or wrong, were derived from experiments on animals?—Does it say that this particular view of his was derived from experiments on animals?

15282. I was merely asking, as you referred to Galen, whether your attention has been called to that particular passage in the Report of the last Royal Commission?—It may well be that Galen's attention was directed to other points.

15283. When was the first successful reunion of a divided nerve for surgical purposes?—I think it would be impossible to say.

15284. Have you made any researches to ascertain that?—Yes, but the facts are scattered in so many medical papers; there are thousands of papers (of course, there were fewer in the earlier times), that it would take years to settle that point, I think.

15285. Was it a well-understood procedure thirty years ago?—Yes.

15286. Is the regeneration of nerve fibre fully ascertained now?—There are some theoretical conclusions which are still very much in dispute as to how exactly it takes place, but the general facts of regeneration are well established now.

15287. Are there not two rival schools known as the central and peripheral schools respectively?—Yes.

15288. Are you able to decide between them?—I have my own opinion, but I do not suppose that I should convince the other people.

15289. Are you a centralist on a periphalist?—I am less certain than most people, but so far as I go, I am a centralist.

15290. The researches of Ballance to which you have referred rather tended in the opposite direction, did they not?—They did.

15291. Have both schools appealed to experiments on animals for verification?—Yes, both of them.

15292. (*Sir Mackenzie Chalmers.*) I think you are a pure physiologist?—Yes.

15293. You are not a medical man?—No.

15294. Is there a school of physiologists springing up in England who are not medical men—who go in for physiology as a profession, so to speak?—Partly as a profession, and partly because they are men with independent means, who are interested in research for its own sake, and are able to live independently, and are not bound to go into the medical profession.

15295. The question has been raised from this point of view; it does not apply to the man who is chosen as the professor of a great university; but on what conditions ought licences to be granted to people who wish to practice physiology and have not had a medical training?—If they have had a scientific training I should have thought that that was perfectly sufficient. If they have been through courses of physiology in a laboratory, and are working where others are already conducting research, I think every precaution would be taken that there would be no misuse if a licence or certificates were given them.

15296. Do you think that you would have the same guarantees for care and humanity as you would have in the case of a member of the medical profession?—Naturally you cannot have exactly the same guarantees at the beginning as you can after experience; but I think that the guarantees are adequate. I think it would be exceedingly rare that there would be any misuse of the power.

15297. You think that the present authorities who recommend for a licence or grant a certificate have the means of ascertaining the character and experience of a man before they send in their recommendations?—One of those who signs the certificates, I take it, in nearly all cases knows enough to be able to state that the experiments will be conducted with care and proper precautions, but not necessarily both of those who sign.

15298. Another point that has been raised before us is this: It has been suggested that senior students who are going eventually to be teachers of physiology should have licences authorising them to perform experiments on animals under the supervision of the recognised head of the laboratory. Is that important or not, in your opinion?—The object being to fit them for research, or to fit them for study, or to fit them for a medical career? They are rather different aspects.

15299. It may be one or the other. Every one has to begin performing experiments. The question is whether it would be right that a, so to speak, partial licence should be given to a man who is a student to begin, not on his own account, but actually to do the operations himself under the supervision and teaching of a professor?—Yes, I think so, if care is taken that he is supervised.

15300. It was suggested by one witness that the proper precautions would be these: That the animal should be anæsthetised by the professor, that in an anæsthetised condition it should be handed over to the student manually to do the operation, with the professor supervising and seeing that the anæsthesia was kept up?—I think that for many students it would be extremely useful, and if it were conducted in that way I do not think that there would be any danger of any pain being inflicted by lack of knowledge or carelessness; but I do not think that I would give indiscriminate licences to students merely to practice.

15301. No, certainly not. But do you think that, under those precautions, there would be any real, substantial advantage gained? Do you think it is at all necessary?—I think there would be an advantage gained.

15302. I want to ask you one or two questions about Certificate B. You have told us that not merely the initial operation, but the whole of the operation, was performed under anæsthetics?—Yes.

15303. And then the animal allowed to recover?—Yes.

15304. Was it your practice, or is it within your experience, that when the animal is allowed to recover there are any supplementary operations ever required?—None, in my experience.

15305. It is not necessary again to put the animal under anæsthesia; so far as you have been concerned it was not necessary to do any further experiments upon it?—I can conceive that there might be such a case. I cannot recall any case of my own* in which it was necessary to give anæsthetics again, or to do anything with the animal until it was killed under the anæsthesia.

15306. If, after an animal has been allowed to recover under Certificate B, it is found to be suffering, what do you do with it?—It is killed. With regard to your former question, one might, for instance, in nerve surgery, want to give anæsthesia again, and to cut the nerve again, and leave it to recover a second time. That is to say, having observed regeneration, it might be necessary, in order to see how much fibre had regenerated, to cut the nerve and then observe the tracks under the microscope after they had been altered by the process of degeneration; so that there a second operation and a second giving of anæsthesia would be necessary.

15307. As you construe the certificate, if that had to be done, anæsthesia would be applied again as a matter of course?—Yes.

15308. It has been suggested to us by some witnesses that certain animals, having very much more acute sight and hearing than human beings, have also a more acute sense of pain. Is there any justification for that suggestion?—Every-day experience of the behaviour of animals which have large gashes and skin wounds is directly opposite to that. I should say that that is a question on which every one can form his own opinion.

15309. Physiologically, is there any connection between either the senses of sight, hearing, or smell, and the sense of pain?—Physiologically, all the apparatus for discerning pain is much less in the lower animals than it is in the higher.

15310. You may have increased sensitiveness of

* *On reference I find that I have, in a few cases, given anæsthetics a second time, in investigation on nerve degeneration and regeneration.*

hearing and of sight quite independently of that of the perception of pain?—Quite.

15311. In your experiments I suppose that you use general anæsthesia, not local anæsthesia?—I generally use the A.C.E. mixture.

15312. Taking the A.C.E. mixture or taking chloroform by itself, does the animal suddenly pass from acute sensitiveness to pain to absence of pain, or is it a slowly progressive diminution?—It is a very gradual process, beginning with diminution of the power of feeling pain, and going on to disappearance of pain, and then a gradual diminution of consciousness, and so a gradual action on all the rest of the nervous system. It is gradual throughout.

15313. There is no sudden crossing of the gap between consciousness of pain and unconsciousness of pain?—No, none.

15314. Have you by chance read Miss Hageby's evidence of some physiological demonstrations which she saw in London University?—I have looked through her evidence. I cannot say that I have read it.

15315. Her impression was that the animals there were imperfectly anæsthetised, because she saw certain movements. Can you form any opinion yourself as to whether those movements were reflex or conscious movements?—From what I have seen myself in Professor Starling's laboratory, I should feel certain that they were reflex movements. But I have not read the exact account with sufficient care to give evidence upon it, and can only answer upon general grounds.

15316. You have told us that your own experiments have been chiefly on nerves. There are many nerves, I suppose, which are wholly unconnected with the sensation of pain, which you can stimulate without producing pain?—Certainly.

15317. But are the nerves themselves which are not the subjects of pain so inextricably woven up with other nerves which are there, that you cannot stimulate them without causing pain?—If a nerve that is severed from the central nervous system is stimulated, it cannot by any possibility cause pain. The majority of stimulations in experiments on animals are stimulations of the peripheral end after it has been cut away from the central end.

15318. (*Mr. Ram.*) And that has no sensation?—It has absolutely no sensation. It is impossible that it should have any sensation.

15319. (*Sir Mackenzie Chalmers.*) We have been told by some witnesses that in operations on animals which involve opening the intestines, the intestines themselves are very slightly sensitive. Is that so?—In ordinary cutting operations they are always insensitive, and it is difficult to find that they react at all in such cases as cutting with the knife. On the other hand, it is known that when they are in an inflammatory condition, they are painful.

15320. For instance, the intense pain of peritonitis?—Yes, that, I take it, is one of the most painful diseases, but there are numerous instances in which the intestine is cut under very light anæsthesia, just over the border of loss of consciousness, when it produces no indication at all that severe pain such as might cause a return of consciousness is produced, there is nothing there to indicate that the section or suture of the intestine is capable of giving rise to pain.

15321. Then in these insensitive internal organs, how do you get intense pain of cramps, the cramps of cholera, for instance?—In that case, they seem to be sensitive to a particular kind of stimulus which one may regard as a special stimulus of the intestine; thus a certain degree of strong contraction causes pain, but cutting and burning are not painful as a rule in the viscera.

15322. Are the same nerves sensitive both to heat and cold and to pain?—No, I think they are a different set. That is one of the questions on which there is considerable difference of opinion still as to whether where the stimulus increases in intensity in a nerve which gives rise to cold, say, or to heat, and then it passes into pain; or whether there is a different form of nerve fibre which comes into action when pain is concerned. That is a matter which is not definitely settled, but in which the balance of opinion is in favour of a specific different kind of nerve.

15323. (*Sir William Collins.*) Do you mean a morphological difference of fibre?—No doubt it is connected with morphology—that one kind of nerve cannot give rise to pain under any circumstances, it only gives rise to cold; and that a nerve which gives rise to pain cannot under any circumstances give rise to cold, it only gives rise to pain.

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15324. (*Sir Mackenzie Chalmers.*) The two are so closely interwoven, then, that you cannot separate them?—They run together, and it is difficult to separate them. You can get little points of the skin which only feel cold, and other little points which only feel pain or pressure.

15325. It was suggested by a witness last week that in certain cases a slightly painful operation is preferable to the use of anæsthetics. I suppose that often in mankind that is very common?—I should think it was common, and I think that it is so in some experiments on animals—all those very slight experiments. It is obvious that the puncture of a needle is perfectly trivial, whereas the giving of anæsthetics is generally accompanied by discomfort.

15326. It depends, therefore, upon the nature of the operation whether an anæsthetic should be given or not?—Yes.

15327. But I suppose you would say that as an animal cannot speak for itself, the safe thing is to give an anæsthetic whenever there is a cutting operation?—Yes, that must be regarded as a guarantee for the sake of the animal, because the infliction of discomfort, after all, is not a serious matter, whereas the casual infliction of pain is.

15328. A human being can be asked whether he would rather have the slight pain of an operation or the discomfort of an anæsthetic, whereas an animal cannot?—That is so. Where the operation is clearly not painful, as in a simple puncture with the needle, I think it is obvious that anæsthetics should not be required.

15329. That is to say, under Certificate A?—Yes.

15330. You mentioned showing certain what I may call advanced experiments to certain advanced students. Do you allow any of your students to see some of your research experiments, or only the demonstrations?—Occasionally they may see research experiments.

15331. And that obviates the necessity of having a Certificate A?—Yes.

15332. (*Mr. Ram.*) There are a few points that I want to ask you to be good enough to tell me about, please. In the nerve regeneration operations that you have told us of, would the process of healing be necessarily painful?—No.

15333. Sometimes?—I do not think that the process of healing of a nerve is ever painful unless there is inflammation, or unless there is sepsis.

15334. If you use an antiseptic there is no pain?—There is no pain, I think.

15335. You spoke of a certain number of operations in which, when the animal recovered, there would be discomfort, and of a certain other number in which the discomfort would pass into pain. What sort of operations would those be; can you indicate a specimen?—Operations on the central nervous system, I think, must generally be accompanied by some kind of pain, but certainly not always.

15336. Then the number of cases in which during recovery any pain is liable to be felt is, in your opinion, very small?—I think it is small. In the removal of a portion of an organ, for example (supposing it were endeavoured to discover with how little of an organ an animal can live with) there may be pain. I think an experiment like that, to the minimum of organ left, will probably cause some pain. Some of the experiments made in the extirpation of thyroid, which led to the introduction of thyroid treatment, I think caused pain.

15337. And would that be pain merely while the wound was healing, or even after recovery, in consequence of the altered state of the organ?—In the case of the central nervous system, it would be while the wound is healing. In the case of the removal of an organ, it might be the result of the actual removal—in the alteration in nutrition that was brought about.

15338. Then probably in that latter case the animal would be in some discomfort or pain as long as it lived?—In some cases I think it might be.

15339. What would you do yourself in your own practice in such a case as that. Should you consider yourself justified in keeping the animal alive indefinitely?—Not if it were in severe pain.

15340. You would judge of the amount of pain?—Yes.

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15341. And you would put the animal out of pain if you considered the pain severe?—Yes.

15342. That, I suppose, is a matter which must be left, at all events in the present state of the law, to every operator to decide?—I think so, but, as I have said, I have never seen such a case, and I think they are rare.

15343. But if you found such a case you would yourself put the animal out of its pain?—Yes. Such experiments as those are not made generally for scientific purposes, but are directed to the study of some disease which it is desired to elucidate in man.

15344. And to that end it is necessary to allow the animal to live?—It is necessary to allow the whole disease to work through.

15345. Have you ever found the inspection which at present exists under the Act any nuisance or hindrance to you?—No, I cannot say that I have found it any nuisance. The Inspector comes at irregular times and walks round the laboratory and asks to see everything that is there in the ordinary course. That takes up an hour or so in the morning.

15346. That is the only trouble?—Yes, that is the only trouble.

15347. Save in so far as there might be a hindrance to you in your work and in occupying your time, should you mind having the inspection increased?—No. Of course there is a limit. It would be a great hindrance not to be able to experiment unless someone was there.

15348. I was not upon that?—But if by an increase of inspection you mean that more people could come in at any time they liked I have no objection whatever to that.

15349. Do you think that it would be any hindrance to scientific research if an animal which had undergone an operation which might cause pain and survived was to be inspected periodically by the Inspector?—No, I think there is no reason why the Inspector should not inspect as much as he chooses.

15350. Now, with regard to morphia, if I understand you rightly, morphia not only does not prevent, but actually at times occasions reflex action?—At times it occasions reflex action; that is to say, it does not in itself occasion it, but it puts some of the lower parts of the nervous system into a condition in which they react more readily than if they are in a normal condition.

15351. Morphia given in sufficient quantities is an absolute anæsthetic?—I think it is an absolute anæsthetic.*

15352. But an animal under absolute anæsthesia from morphia would be more likely to show reflex action than under any other anæsthetic?—Most animals if you give them enough go into convulsions under morphia.

15353. Is that also the case where morphia is used in conjunction with other anæsthetics?—No, it is not the case.

15354. The other anæsthetics control the reflex action?—Yes.

15355. Now with regard to curare, in your opinion, are there certain cases in which if the necessary object is to be attained, curare must be employed?—I think it must be employed.

15356. Those cases, some witnesses have told us, are very few. Do you agree with that?—It depends upon the line of research. Taking the whole field of research they are very few; but any particular line of research might want many.

15357. It would depend upon what the object of the operation was?—Yes.

15358. And it is principally, is it not, in operations dealing with nerve investigation that curare would be necessary?—Yes.

15359. When curare is used, I rather gathered from your evidence, that many of the indiciæ of pain are absent?—They are.

15360. Therefore, as you expressed it, if I understood you rightly, the operator, in order to be sure that the animal is not suffering when under curare, must be certain that the dose he had previously given of anæsthetic is sufficient to obviate all possibility of it?—Yes.

15361. In the hands, therefore, of a careless or inexperienced administrator of anæsthetics, or operator, the presence of curare might involve suffering unperceived by the operator?—I think it would be gross carelessness, because the operator must be aware of the necessity of giving continuous anæsthetics.

15362. Do you see any objection to its being enacted that there should be a separate certificate to authorise the use of curare?—No. I think, of course, that all increase of certificates is so far a drawback that it tends to prevent people from doing experiments which they otherwise would do; but if it tended to alleviate public opinion on the question I should not in any way object.

15363. It was on that ground that I was suggesting it to you?—In itself I think it would be unfortunate, but there are many things which are unfortunate which have to be done, because of public opinion, which does not understand the thing thoroughly.

15364. For the alleviation of what I would venture to call a sensible, reasonable, public opinion, you, having regard to that, do not see any objection to having an additional certificate to authorise the use of curare?—Of course, I would rather that there were not one, because I do not think that curare is abused; but I should offer no strenuous opposition to it if it is considered advisable on what I may call the politic ground. I should only object if I thought that it might be taken to imply that curare was misused at present.

15365. Do all the animals that have curare given them die under the anæsthetic?—Yes.

15366. In no case does the operation admit of recovery?—No. There are instances recorded in the early experiments of Waterton, who discovered the poison. In practice they die under the anæsthetic.

15367. Having regard to the fact that the cases in which curare is used are comparatively few, taking the whole field, and that there is this risk of which you have spoken, do you think it would be any impediment to scientific research if it were enacted that the Inspectors should be present in all cases where curare was used?—I think it is an impracticable thing to have an inspector present when all experiments are going on, because it means that research is so retarded that it becomes practically impossible to carry it out.

15368. You observe that I limit my question to cases where curare is used?—Yes, but one never knows exactly when you want to use it in a particular kind of experiment, and the Inspector is probably not on the spot. You have to arrange weeks beforehand. In the meantime some committee meeting or something happens, so that either you cannot do the experiment, or something happens to the Inspector and he cannot come. I think it would be very difficult. And then, although in a general way I acknowledge the risk, it is on theoretical grounds. I have no reason to suppose that it is a practical thing.

15369. You have suggested as one of the alterations or improvements of the present Act the elimination of non-painful experiments, such as feeding experiments, which do not involve pain, and sub-cutaneous injections, and so forth. Could you give us any kind of definition of those experiments? Have you thought of any expression which would serve to include and express such non-painful experiments as that?—"Experiments not calculated to cause pain" I should have thought would have covered them. The Act is for those calculated to cause pain.

15370. You think that some such general expression as that would be broad enough—I should perhaps say narrow enough?—Yes—not calculated to give more pain than is caused by the prick of a needle.

15371. (*Chairman.*) Supposing that the injection causes fever?—I think I suggested the injection of non-irritating substances.

15372. (*Sir Mackenzie Chalmers.*) That would not cover the case of a substance which afterwards produces a painful disease, as, for instance, plague?—But that requires a special licence for it afterwards. But it would not require Certificate A to perform an

* *By this I mean that morphia has produced complete anæsthesia in the cases and under the conditions in which I have used it and seen it used. Beyond this I can only offer an opinion based on the statements of others; my own experiments do not cover the whole ground.*—J.N.L.

operation on an animal under anæsthetic. The experiment in that case would be one for the investigation of plague. Those experiments for the investigation of a transmissible disease seem to me to require a special certificate. The injection is an incident.

15373. (*Mr. Ram.*) The initial stage in giving an animal a sub-cutaneous injection of plague serum, say, will no doubt cause no pain to the animal, but the subsequent condition produced thereby might be very painful?—If anyone is going to investigate plague or some disease he should have a certificate, I think, that it is for the investigation of the disease, but not for the injection of a substance under the skin. It is rather striking at the wrong point.

15374. I quite follow your point. I was trying to see whether you could help us to formulate anything to carry it out.

15375. (*Sir William Collins.*) You mean a minor operation that is not followed by any morbid sequelæ? —Yes.

15376. (*Mr. Ram.*) Such as injecting a mouse or guinea-pig with milk supposed to be tuberculous?—Yes.

15377. The operation itself would not be painful at all?—No.

15378. I just throw it out in case you might be able to assist us.

15379. (*Chairman.*) One way might be to give power to the Home Office to classify certain operations as not being painful within the meaning of the Act?—Yes.

15380. (*Mr. Ram.*) I dare say if you think hereafter of any definition—you see the difficulty—you will let us know, if you can assist us to formulate anything to meet what you propose?—With pleasure.

15381. There is only one other matter, I think, I need trouble you upon. I think either in your *précis* or in your pamphlet you referred, but only made some allusion, to infantile paralysis. Is it to be hoped that these experiments with regard to nerve regeneration may be of assistance with regard to the discovery of the causes and perhaps finding the cure for infantile paralysis?—The causes are generally of a surgical nature, and are in many cases well known; but the experiments will be of benefit for the cure of the disease, or, at any rate, for the alleviation of the condition. In other words, the paralysis will be diminished or almost abolished.

15382. That is what I wanted to get at. It is pretty widespread, is it not?—There is a good deal of infantile paralysis.

15383. And when it comes in it is apt to maim or perhaps blast the whole life?—Yes.

15384. Do you think that the experiments which have been and may be made in these matters, will be likely to assist in curing or alleviating infantile paralysis?—I think so, certainly.

15385. Has anything been done in that way at present?—It is just beginning. Some experiments have been made, those that I have quoted by Frazier and others.

15386. (*Dr. Gaskell.*) I understood you to say that in demonstrating to your class you made a special point of administering anæsthetics?—Yes, they are generally shown the anæsthesia, the method in which it is produced, and the condition is pointed out.

15387. And stress is laid upon the actual condition that the animal is well under anæsthesia in consequence of the dose that has been given?—Yes. In any particular demonstration the question of the anæsthesia might be entirely left out if it has been done on a previous occasion. There would probably be one demonstration in which attention was specially called to the anæsthesia, and in a subsequent one it would be taken for granted.

15388. Have you read the past evidence before this Commission at all; have you read Mrs. Cook's evidence?—I read it just after it appeared, but I have not looked at it since.

15389. As she has brought in your name, I should just like to have a statement on your part. She says at Question 1785: "In the second volume of the 'Journal of Physiology,' page 267, will be found an account by J. N. Langley, M.A., from the Physiological Laboratory at Cambridge, of an experimental operation on a dog. The dog was narcotised with morphia. I believe I shall have expert support if I say that morphia is not an anæsthetic. The proceedings by which this dog was most cruelly tortured lasted between three and four hours"; she then describes the experiment. Have you anything to say with regard to that statement?—That is an example of the misrepresentation on which so much of the anti-vivisection opposition has been founded. There was absolutely no torture; the animal was perfectly quiet and entirely unconscious throughout. The action of morphia is not understood, of course, completely, and until we completely understand it we shall not be able to avoid this kind of misrepresentation. But there is no kind of doubt that morphia, just as it abolishes the severe pain of neuralgia, peritonitis, and other diseases in man in a small dose, can in a larger dose prevent pain in animals.

15390. There was no evidence whatever of anything in the nature of the animal being cruelly tortured or any sign of pain in that animal?—There was none.

15391. Then there is another statement: "In the 'Journal of Physiology,' volume VI., is an account of how a cat was alternately suffocated, cut, and its nerves exposed and irritated with electric shocks, without intermission, for over two hours and a half. It is stated that chloroform was given at the beginning of the operation; but it is not possible that the animal was kept in a state of insensibility for more than two hours and a half." That statement we have had put before us again and again of the impossibility of keeping an animal anæsthetised for so long a time as two and a half or three hours. Would you just tell us whether there is any truth in that?—It must be the revival of some statement made somewhere by someone who did not know what he was talking about—that animals cannot be anæsthetised and kept under anæsthesia indefinitely. I say undoubtedly that you can do it for many hours. I have seen animals untied and unfastened without producing any kind of movement in deep anæsthesia for hours together.

15392. (*Chairman.*) Was that operation on the cat one of yours?—Yes.

15393. (*Sir Mackenzie Chalmers.*) As regards that particular cat, you have no doubt whatever that during the whole of the operation it was absolutely unconscious?—I have no doubt of it whatever. If you notice the reference to nerves, all the nerves, as I have mentioned before, are those which are stimulating the peripheral end, which could not give pain.

15394. (*Dr. Gaskell.*) You suggested that when a licence has been held for ten years permission to go on experimenting should be endorsed, and no certificates required. I presume that you mean by that that during those 10 years the licence has been used continuously?—Yes, I mean that guarantees have been given of earnest work, and that the work has been conducted under conditions which have satisfied the Home Office.

15395. Do you think that as long as ten years is necessary?—No, that was merely a suggestion. I do not specify any particular term.

15396. I should like to know whether you have seen Dr. Pembrey's evidence, to which Sir William Collins referred when he was questioning you?—I have not.

15397. After the remark which Sir William Collins mentioned at Question 14115, that the Vivisection Act was entirely opposed to the advancement of physiology, the next question is: "Is that opinion held by others engaged in physiological research besides yourself," and he answers "Yes, I think so." You do not agree with that?—I certainly do not agree with that.

15398. You do not consider that Dr. Pembrey would be representative, in that respect at all events, of English physiologists?—No, I think not.

15399. Considering the question of the organisation of an animal in connection with the sensation of pain, do you think that there is any need to include cold blooded animals in the Act as at present?—I think they certainly must suffer very little, but I think again, as a matter of public policy, it is rather difficult to press for their exclusion. I do not think it would be understood. If they were excluded, and people could make experiments on them, I am not at all certain that that would satisfy public opinion. I think it would be better to have a separate certificate for cold-blooded animals, like fish and frogs, which will certainly suffer very little pain rather than to exempt them altogether from the Act. But if by your question you mean whether I think that they suffer much pain, I certainly do not think that they do. The organisation of the part of the brain

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that is connected with pain in man is so small in those animals that it cannot have any very large function.

15400. Do you think that those cold-blooded animals being included in the Act somewhat hampers physiological research in this country?—I think it does, because a good many experiments could be made on frogs, and sometimes on fish, and if they require a certificate it prevents some of them from being undertaken; and, as a side point, I think that rather tends to divert attention to the higher mammals instead of concentrating it on the lower—that there might be fewer experiments on the higher mammals if there were more liberty for experimenting on the cold-blooded animals.

15401. Do you not feel yourself that in the future the study of what I may call comparative physiology is likely to come more and more to the front?—Yes, I think that is going on every day.

15402. And is not the physiology of fishes at present hardly worked at at all?—It has not been much worked at.

15403. Would you therefore recommend this Commission to consider that question of excluding those cold-blooded animals from the Act?—I certainly regard it more as a matter of public policy than of pain inflicted on the animals whethered they are included or not. It is a balance in which the amount of pain inflicted on the animals whether they are included main factors to be considered are the general public opinion upon the matter, and the degree of hindrance to research which is caused by the present Act, and that is a balance which I have not very carefully thought out.

15404. Do you not think that public opinion is mostly confined to dogs and cats, and warm-blooded animals generally?—Yes, largely. I think it is obviously absurd that cold-blooded animals should be classed under the same return of the number of experiments made with cats and dogs.

15405. Even now, at the present moment, I presume that you cannot make experiments upon tadpoles without a licence?—No, you cannot.

15406. (*Mr. Ram.*) In giving demonstrations to your students, do you instruct them as to the method of giving and the duration of anæsthetics?—In the first demonstration generally the method of giving the anæsthetic is described. After that usually the experiment only is mentioned and described.

15407. Do they see the animal put under anæsthetics?—In some cases they see the animal put under anæsthetics in the lecture room. Generally it is a question of time if that is not done, because everyone is very busy, and it takes some time.

15408. And during the demonstration, I take it that it is necessary to keep on the anæsthetic to ensure its being operative, so to speak?—Yes.

15409. And they see that?—Yes.

15410. Do you instruct them as to the necessity of keeping up the anæsthetic, and producing perfect anæsthesia the whole time?—Yes.

15411. On that, may I ask you another question? We have heard the phrase several times, "incomplete anæsthesia." Anæsthesia, according to its proper meaning, means that there is no perception of suffering at all?—No perception of suffering.

15412. Therefore, incomplete anæsthesia is a misnomer?—Yes, it is a contradiction in terms; but the meaning of anæsthesia is very varied.

15413. But anæthesia means no perception of suffering at all—that you have no ἀισθῆσις. In nerve practice, anæsthesia means that there is no sense of touch, but that there is pain. Analgesia means no sense of pain. There are at least half a dozen interpretations, including Claude Bernard's.

15414. Anæsthesia means that which deprives you of all sense of pain?—Yes, that is the popular view.

15415. (*Sir William Collins.*) Is that the sense in which you have used the word in your evidence today?—I used it in the popular sense of absence of pain.

15416. (*Mr. Ram.*) To that extent you think that the term "incomplete anæsthesia" is a misnomer?—It is a contradiction of terms.

15417. (*Sir William Collins.*) You stated, I think, that when an animal was completely under anæsthesia, if you rapped on the table close by you produced convulsive movements?—When it is under morphia alone.

15418. Only under morphia?—The difference with morphia is that it acts very slowly on the lower nervous centres, and some of them it excites and makes more active. It has a more gradual action throughout on the lower centres from the higher to the lower, which is rapidly gone through under chloroform.

15419. That is not the case under chloroform?—No.

15420. Is that the case with man under morphia?—I have never seen an exactly parallel case of man under morphia, so that I am unable to speak from experience.

15421. (*Sir Mackenzie Chalmers.*) There was a statement made by Miss Hageby about morphia, I think, with regard to some of your experiments—it is at Questions 9266 to 9268. Will you read it, and see whether you have any comment to make upon it?—(*After referring.*) The particular experiments which are mentioned here by Wood and Cerna I have not any acquaintance with; but if the statement is intended to imply that a dog is not affected by a moderate amount of morphia infinitely less than that given on that occasion, one can only meet it with a flat contradiction. There is an experiment here by Claude Bernard, which I know, in which it is said that a dose of 30 grains was given, but Claude Bernard expressly states that 50 millegrammes* is sufficient to send an animal into profound stupor, into a condition in which cutting operations on the skin and nearly the whole of ordinary vivisection could be performed with the animal completely untied, and without any movement. That is given in Claude Bernard's "Lessons on Anæsthetics." He also mentions in his experiments the increase of excitability by a non-painful stimulus, such as by rapping on the table and by sounds. The reason why Claude Bernard gives such contradictory accounts of the action of morphia, apart from the reason that he did not know everything about it, was that he relied upon some false experiments of his own. He concluded that the increased excitability from morphia depended upon the brain, and that you did not get this increase of movement when the spinal cord alone was present. That was shown by Kölliker to be erroneous, and it is the easiest thing in the world to show that it is wrong—that this increased excitability comes from the spinal cord and not from the brain. It can be shown in the frog. A frog can be decapitated and morphia injected under the skin; in a short time the same kind of convulsive start that you have in the dog by a slight tap is produced in the frog, and this is of the same kind as is produced in man. But there are many things in the action of morphia that we do not understand; that is a second reason, apart from his erroneous experiments, why Claude Bernard makes so many contradictory statements. A third reason is that in his experiments he is making observations on each experiment as it occurs. It is not a systematic account of the action of anæsthetics; it is a description of an experiment done before an audience, with the experimenter making his deductions as he sees the things done—not in the quiet of a laboratory. Moreover, the account of the experiment was taken down by assistants; he did not write a full account himself, so that the whole form of the account is one that naturally leads to misconceptions and to contradictions.

15422. At any rate, you have no doubt that a sufficient dose of morphia is an absolute anæsthetic?—It is an analgesic.

15423. Are there cases of idiosyncrasies in animals as there are in man?—There are great differences; that is why, in experimenting with morphia, you have to take each animal and watch it.

15424. Morphia ought only to be given, then, by a very skilled physiologist; it is not so safe as chloroform or A.C.E.?—No, it is not so safe. There is a longer interval between the disappearance of pain and the disappearance of consciousness with morphia than there is in chloroform.

15425. (*Dr. Gaskell.*) Still, the disappearance of pain is easily produced; that is the main point. Pain goes quickly with morphia.

* *This is the dose mentioned for a young dog; more is required for an adult dog.*

Mr. G. Sims Woodhead, M.A., M.D., called in; and Examined.

15426. (*Chairman.*) You are a Master of Arts, a Doctor of Medicine, and Professor of Pathology in the University of Cambridge?—Yes.

15427. And you are one of the gentlemen whom the Vice-Chancellor has nominated to give evidence on behalf of the University?—Professor Langley is one, I am the other.

15428. You hold a licence, I believe?—I do.

15429. How long have you had one?—For about 24 or 25 years.

15430. You have performed during that time operations on animals?—A very large number.

15431. And you have had charge, I believe, of the work, or the principal part of the work, in pathological laboratories in the United Kingdom?—Yes; I put that in simply to indicate that I have had a somewhat wide experience.

15432. So that, at various laboratories, you have had responsibility and experience?—Yes.

15433. And for the last four or five years also you have been a member of the Royal Commission on Tuberculosis?—I have.

15434. During the time that you held a licence, have you been in the habit of performing operations in connection with pathological research?—Yes, largely in connection with pathological research.

15435. And also in teaching?—Yes.

15436. And in connection with the diagnosis of disease and the testing of antitoxins?—Yes.

15437. And what has the bulk of your work been in connection with?—In connection with the diagnosis of diseases, especially in connection with diphtheria and tuberculosis; and also in connection with the preparation of antitoxins. In addition to this, I have done work on the healing of wounds, the organisation of blood clots in vessels, the action of variolus vaccine, and the manufacture and testing of toxins and antitoxins, especially in connection with diphtheria.

15438. In connection with all those matters, have you made experiments on animals?—Yes, a very large number.

15439. And you have probably formed a very definite opinion one way or the other as to the value of experiments on animals in such matters?—A very definite opinion.

15440. What is your opinion?—My opinion is that a great deal of the work that has been done recently could not possibly have been done without having recourse to experiments on animals.

15441. Does that apply to all these different inquiries that you have just been mentioning?—Just so —to each one.

15442. Has your work on the Royal Commission on Tuberculosis confirmed you in that view?—Entirely. It would have been impossible even to undertake the work if we had not been able to use experiments on animals.

15443. Have they carried out, or caused to be carried out for them, experiments on animals for their own satisfaction during the last four or five years?—Yes, on a very extensive scale.

15444. Have they reported yet?—They have made two preliminary reports, and have issued four or five volumes of records of experiments.

15445. They have not made a final report yet?—No.

15446. But they have reported upon their experiments to a large extent?—Yes.

15447. And they have reported the evidence?—They have put forward the evidence, and their conclusions so far as that evidence will allow them to go up to the present time.

15448. I gather, then, that they attach importance, as a body, to experiments on animals?—Very great importance.

15449. Have you examined the question of the antitoxin for diphtheria by experiments?—Yes. In fact, I undertook the study of the question for the Metropolitan Asylums Board when they first commenced the work, and I carried it on for some four or five years. I saw the commencement of that work.

15450. About when was that?—1895 and 1896.

15451. Did you report to them upon it?—Yes.

15452. At that time were they using the antitoxin, or were they desiring your report before they used it? —They did begin to use it; but they could not get a sufficient supply, and they could not determine the strength of the antitoxin. So they asked us to determine first of all whether it was possible to measure the strength of the antitoxin; and, secondly, they asked us to prepare the antitoxin for them for the treatment of their patients.

15453. And were you to report upon the value of the antitoxin?—We did report upon its value; we were asked to prepare the antitoxin for them, and then I reported first of all that from what I could learn I considered that it was useful; then we were asked to prepare antitoxin for them, and we undertook its preparation.

15454. Was this at Cambridge?—No; this was at the laboratories of the conjoint Board of the Royal College of Physicians and Surgeons in London, where I was.

15455. Were you at that time in charge of the laboratories of the Royal College of Physicians?—Yes.

15456. Then when they asked you to prepare it, did you do so?—I did.

15457. Did you watch the results of the antitoxin of diphtheria?—Very carefully indeed.

15458. Have you done so since the years from 1895? —Yes, I made reports from 1895 to 1898. The work was then taken over by Cartwright-Wood. I also collected the statistics for 1899; but I then left for Cambridge, and have not had an opportunity of analysing all the statistics since that time. But from the year 1894 to 1898 I kept a careful record, which I now have before me.

15459. You did not keep statistics of that kind afterwards; but I suppose that you kept your eye more or less upon the question?—Yes, pretty carefully.

15460. What is the result of your experience, first up to 1898, when you were keeping this record?—I can give the statistics very shortly. In 1894 very few cases were injected, and the percentage case mortality was 29.6—practically 30 per cent.

15461. In what hospitals?—In the whole of the hospitals under the Metropolitan Asylums Board. I have the figures from the separate hospitals, which I can give you immediately. In the first antitoxin year, 1895, the case mortality had fallen to 21.5, in 1896 to 18.3, in 1897 to 17.7, and in 1898 to 13.1.

15462. Do you attribute that to the antitoxin? It is quite possible that there may have been a decrease in the virulence of diphtheria, as there often is in diseases?—Yes, that is possible. But we have two hospitals which rather took away that ground. In the Northern and Gore Hospital, where they had a large number of cases of scarlet fever—that is to say, cases that suffered from scarlet fever, and afterwards suffered from diphtheria, diphtheria bacilli being demonstrated whilst in the hospital—the case mortality amongst those was very high indeed; but when those cases were treated with the antitoxin—exactly the same set of cases—the mortality fell from something over 45.1 to 1.94 per cent.; or, if we include the similar cases at the Gore farm, 1.19.

15463. Was that between the same years, 1894 to 1898?—Yes, within that period; the fall was so tremendous that it could not be accounted for by anything else.

15464. There was a fall of rather more than 50 per cent. in the other cases?—Yes; but here it falls from over 45 to something lower than 2 per cent. In one set of figures it was between 1 and 2 per cent. In this set the cases were treated at once with the antitoxin. If the same set of cases had been allowed to go for some time untreated with the antitoxin, I have not the faintest hesitation in saying that there would have remained a very high mortality.

15465. You are speaking from those statistics. Have you yourself practised in any diphtheria hospital and watched the cases?—No, I have never watched any

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series of cases in a hospital during the antitoxin period; but, unfortunately, I have had to watch some cases of children of friends. When I was hospital surgeon and hospital physician, of course I had from time to time cases under my charge; but that was before the antitoxin days. But, from seeing cases treated with antitoxin, and comparing them with those that I had seen in the pre-antitoxin period, I am perfectly satisfied, quite apart from statistics, that the antitoxin treatment is of great value. Although I have never practised since I held resident appointments in the Royal Edinburgh Infirmary, I have taken this question up, whenever any of the children of my friends have been attacked by diphtheria.

15466. Then you used the antitoxin in those cases?—Yes, and the results, to my mind, were far more striking than any statistics. The patients in most cases were exceedingly ill, suffering from acute toxic symptoms, which are very marked in diptheria, but in a few hours after the administration of a full dose of antitoxin their condition was quite altered. You see a patient almost livid, breathing heavily, with a very high temperature, in great discomfort, and unable to sleep. You give that patient a dose of antitoxin, and in three to four hours he is breathing easily, the skin is acting, and the patient is comfortable, and he goes to sleep. That, to my mind, is even more striking than any statistics that one can bring forward.

15467. Then you are satisfied of the benefits of the antitoxin in the case of diphtheria?—Yes. I may mention, perhaps, in passing, that in our experiments on animals carried out in connection with the testing of the potency of the antitoxin, an enormous dose of toxin may be neutralised by a dose of antitoxin, and you may see that animal recover without turning a hair, if one may put it in that way, never losing its appetite, never losing in weight, but simply going on as would a normal animal into which no toxin has been introduced.

15468. (*Mr. Ram.*) What would happen if you did not give it the antitoxin?—It would die in from 24 to 48 hours.

15469. (*Chairman.*) I gather that you are thoroughly satisfied of the value of the antitoxin in cases of diphtheria?—Absolutely.

15470. Do you think that there is any possibility of improving the antitoxin and so decreasing the mortality?—I think we have not reached finality in it. I believe, for example, that the antitoxin may be prepared in a slightly different way. We have already results from experiments on animals which indicate that. I believe that in future, instead of using the toxin only, we may use the proteids of which the organisms are made up in order to obtain something in addition to the antitoxin, and that instead of it having an antidote merely to the poison of the diphtheria bacillus, we shall be able to obtain an antidote to the specific action of the diphtheria bacillus itself. This action is local, and not so important as the toxic action; but I believe that a combination of the antidotes to two will probably be more valuable than the single antitoxin; everything is tending to prove that. I might say, in connection with the use of antitoxin in the hospitals, in order to show that it produces not merely a diminution in the virulence of cases, that in the same hospitals the percentage mortality varies very greatly in different years. For example, in the Northern Hospital the percentages were 45.1 in the pre-antitoxin year, in the Western Hospital, 37.1, in the Eastern, 30, in the South-Eastern, 29.7, in the South-Western, 28.5, and in the North-Western, 26.9. Then we find that in the last antitoxin year, 1906, in the Northern Hospital it had gone down from 45.1 to 1.94, in the Western Hospital it had gone down from 37 to 24; in the South-Western it had gone down from 28 to 22; in the South-Eastern from 29 to 22; and in the Eastern from 30 to 19. Although there was a difference in those hospitals in the different years, in all of them there was a downward tendency. This cannot be due merely to the general conditions in the hospitals. Moreover, we found that the more antitoxin was given in any hospital, and the earlier it was given, the greater was the diminution in the mortality; everything else being constant, the greatest fall in the death-rate took place where most antitoxin was given in the earliest stages of the disease, because this latter, of course, is a most important factor.

15471. You said that you kept this question of the diphtheria antitoxin as a remedy under view, although you were not engaged in preparing the statistics, but what do you say as to the use of it and the belief in it by those who attend diphtheria cases now?—All one can say is that our medical men are using more and more antitoxin every year. The worse the cases are the more anxious they are to give the antitoxin; I said before the very best results are always obtained when the antitoxin is given at an early stage, before the poison gets a thorough hold of the patient and damages the tissues. If you can give it early the death rate from diphtheria should be practically nil. The only cases that should be allowed to die are those that do not come under treatment until too late, or the few very virulent cases in which the poison seems to be so plentiful and so active that the patient is, as it were, stunned.

15472. That you believe to be the result of the use of the antitoxin?—Yes.

15473. And the antitoxin, I understand you to say, cannot be successfully tested and applied without experiments on animals?—It cannot be made, of course, without bringing some animal, usually the horse, in, and it cannot be tested without the use of guinea-pigs or some similar animal; we always use guinea-pigs. And here, of course, come in those cases of which Professor Langley has been speaking this morning. In a great many of those cases there is absolutely no suffering of any kind beyond the prick of a needle. The antitoxin neutralises the toxin so completely that you cannot tell that anything has been done to the animal; there is no swelling, no loss of appetite, and no loss of weight; the hair does not fall off, and the animal remains in perfectly good condition, and there is absolutely no pain. It is stated, of course, in our Return to the Home Office that so many thousands of experiments have been done in connection with this work, but probably in nine hundred out of every thousand there has not been the slightest discomfort even, except that produced by the prick of a needle.

15474. (*Mr. Ram.*) What is the poison which is given to the animals that are infected?—The diphtheria poison; it is the diphtheria toxin, the antitoxin being used to neutralse it.

15475. Are all animals subject to diphtheria?—Not all, but you can poison nearly most of them with the diphtheria toxin. You can poison the rat, but you must give it an enormously greater dose than is necessary to kill a guinea-pig. Animals differ greatly in this respect, and you have to determine what the requisite dose is in each case.

15476. (*Sir Mackenzie Chalmers.*) In preparing the antitoxin the horse is used?—Yes.

15477. (*Chairman.*) Have you had any experience in watching the carrying of disease by mosquitoes?—I have seen a limited number of experiments with mosquitoes, but I have followed the question very closely in connection with malaria and yellow fever, but we have of course no opportunity of studying it practically in this country.

15478. We have had many witnesses who have been engaged in it at first hand, so that it will not be necessary to ask you about it unless there is anything that you can shortly state as to your view of the value of those experiments?—I think that many of the experiments recorded are valuable as indicating how much we really owe to the method of experimentation on animals. In the case of yellow fever, although observers were not able to trace the yellow fever infective agent from man to animal, or from man to man directly through experiments, they have been able, by applying the methods which have been used in connection with other diseases, to demonstrate that the yellow fever virus is carried by the mosquito; and so satisfied were they of this, that, as we know, a member of the American Commission submitted himself to infection by that special method. Volunteers offered to live in a house with all the dirty clothes from yellow fever patients and to remain there for a considerable length of time; all came out quite unaffected. It has previously been proved that the carriage of certain organisms from the blood of birds is affected by mosquitoes, and it was suggested that this might be the method by which the infective material of yellow fever is carried from a patient suffering from yellow fever to a healthy individual. As the result of submitting himself to such infection by a mosquito—stegomyia fasciata—Dr. Carroll, a member of the Commission, and several other volunteers, contracted yellow fever. Dr. Lazear died of yellow fever.

15479. In both cases did they take it from mosquitoes?—I am not quite sure that Dr. Lazear contracted the disease as the result of an experiment, but in Dr. Carroll's case it was undoubtedly so, and a number of the volunteer patients were infected in the same way. They simply applied in this case, on themselves, of course, the methods already proved to be of value when animals had been used. In connection with the bird parasites it was proved that they could be carried by mosquitoes, that they passed through certain phases of development in the mosquitoes, and then were transmitted to other birds. It was argued from this that it was possible that the same thing might hold good in malaria and in yellow fever, and it is now proved, I think, as clearly as anything can be proved, that that is the case. That was simply the application of the experimental method to man instead of to animals.

15480. There was no toxin that they had to administer to themselves in that case?—No.

15481. I think that the only other point on which you wish to give evidence as to the carrying of diseases through other animals is that there is still some uncertainty about its effect as it passes through the different animals?—Yes, it has been objected that different animals must be affected in a different fashion by the various diseases, and that undoubtedly is the case. We know, for example, that certain animals are affected by tuberculosis, and that other animals are not affected by it in the same degree. We know, too, that certain pus-producing organisms are very virulent for one species, but that if you pass that pus-producing organism through another species, through a whole series of animals of this second species, it gradually loses its virulence for the first, but gains in virulence for the second. We know that there are very great differences in immunity and susceptibility to various specific infective diseases, but, of course, the organisms producing them can be studied as they pass through the different animals, and I think it is very important that we should know all the phases through which these disease-producing organisms pass outside the human subject. If we cannot trace these organisms, and trace their effects outside the human organism, then we cannot tell from what point we may be attacked by these various disease-producing organisms. That has been our difficulty in connection with the work of the Royal Commission on Tuberculosis. If we can once determine exactly how disease-producing organisms act in different animals, we shall be in a much better position to understand the conditions under which we may eliminate the danger from those diseases.

15482. That, I suppose, would involve a considerable amount of experimentation?—Of course.

15483. Are the Royal Commission on Tuberculosis engaged on such experiments?—Yes, at present.

15484. Who is the Chairman of that Commission?—Sir Michael Foster was the Chairman, but he unfortunately died last year, and now the Chairman is Mr. Henry Power, of the Local Government Board.

15485. (*Sir William Collins.*) Did I rightly understand you to say that we have learnt something with regard to vaccine by experiments on animals?—The only thing, I think, that we have learnt by experiments on animals with regard to vaccine is that in all probability it is an exceedingly minute organism.

15486. Do you think that you have identified the organism?—No, I think it is ultra-microscopic. That is the opinion that I have formed. At one time I performed a large series of experiments on rabbits. Rabbits and monkeys are the only convenient two animals outside the bovine and the human subject on which one is able to raise vesicles.

15487. Was not a prize offered by the Grocers' Company for the identification of the cause of vaccine?—Yes.

15488. Has that prize ever been obtained?—Not as yet.

15489. Is that all that we have learned with regard to vaccine by experiments on animals?—Yes, I think so, except, of course, Jenner's original experiments on cattle, which are so well known to you.

15490. They were rather observations, were they not, than experiments?—That is the case.

15491. Then you dismiss the protozoal bodies which have been alleged to cause vaccine?—I do not say that I dismiss them, but I have seen no fully satisfactory evidence that they cause the vaccinal condition.

15492. At any rate, any micro-organism which may be the cause is at present invisible, and has not been identified?—Yes, I took a large series of vaccines, and I found that I could obtain the best results from those vaccines in which there were no organisms which I could cultivate or see under the microscope, in fact, that the best vaccine was that which contained no organism such as we usually recognise as the cause of one or other of these active processes.

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15493. Not even the small bacillus which has been alleged as the cause?—I could not find that bacillus, and I came to the conclusion that, like the pleuro-pneumonia organism, and perhaps that of yellow fever, it is ultra-microscopic. It will pass through the pores of a very fine filter, but we have no microscopic appliances at present which will enable us to see it.

15494. Is it the same with regard to sheep pox and cattle plague, which were brought before the last Commission by Mr. Simon in 1876?—Did he refer to pleuro-pneumonia of cattle, or foot-and-mouth disease?

15495. The examples on which Mr. Simon thought that he had obtained exact scientific knowledge were sheep-pox and cattle plague?—If he referred to pleuro-pneumonia I do not think we have been able to find any organism yet that we could identify.

15496. If we refer to cattle plague—rinderpest?—There again we are in the same position, I think.

15497. No organism has been identified?—Not that I am aware of.

15498. And with regard to sheep-pox?—I do not know; I have not studied it.

15499. It was claimed, was it not, at one time, that an organism had been discovered, but it was subsequently repudiated?—Yes.

15500. With regard to the Tuberculosis Commission, what was the occasion of the appointment of that Commission; was it not some allegation made by Koch?—Yes; that the tuberculosis of cattle and the tuberculosis of man were different diseases—that they were not transferable from one organism to the other.

15501. Had he based that opinion upon his own investigations?—Yes, we think on an incomplete series of investigations.

15502. But investigations on animals?—Yes.

15503. You think that you have now confuted that opinion?—We have obtained a very large amount of evidence which is in opposition to some of his conclusions.

15504. Also by experiments on animals?—Yes, and we have also come to the conclusion that if we took part of our experiments only we might have come to the same conclusion that Koch came to, but taking the whole of our experiments we are satisfied that the diseases are transmissible; at any rate, that cattle tuberculosis can be transferred to the human subject. We cannot experiment directly in the opposite direction.

15505. Incidentally I understand that you have discovered, or have reason to emphasise, the great importance of soil upon the various seeds in the shape of organisms that you plant in it?—Yes.

15506. Now, as regards the antitoxin, you spoke of diphtheria in animals?—Not of diphtheria; of the toxic condition; it is not quite the same thing. You can produce the toxic symptoms in animals, but it is more difficult to produce the local symptoms.

15507. You do not reproduce the disease in animals that we know in man as diphtheria?—No, because diphtheria is primarily a local disease, and only later becomes a constitutional disease. You have, first of all, the local manifestation, the cause of the inflammation, set up in the windpipe or the upper part of the respiratory tract.

15508. To that extent then one of Koch's postulates of causation fails?—Not necessarily; you do not set up the exact kind of local condition seen in diphtheria of the human subject, but you can set up an inflammation by injecting the poison under the skin of an animal, but it does not fly to the throat; the organism does not move from point to point; it settles down at one point, where it produces its poison, and that poison is carried from that local area to the rest of the body, where it acts upon the various tissue of the body.

15509. Do you produce the false membrane in an animal?—No, because you inject the poison under the skin, and the false membrane is nothing more than the evidence of inflammation on a mucous surface; we have simply fibrin thrown out on to the inflamed surface.

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15510. Can you produce that false membrane in an animal?—You can get the fibrin thrown out from the vessels in very acute cases of inflammation, but as it is not on a free surface it forms no false membrane.

15511. But you do not reproduce the symptoms which are familiar as the diphtheritic symptoms in man?—Only the toxic symptoms. The symptoms are due very largely to the interference with respiration in a child, but most of the toxic symptoms may be produced in an animal, just as they may be produced in a man.

15512. Is the use of the antitoxin based upon the presumption or proof that the Klebs-Loeffler bacillus is the cause of diphtheria?—Yes.

15513. We have been told that in 20 per cent. of the cases of diphtheria that organism is not found?—I think that is quite possible, because in many of those cases you have really the Klebs-Loeffler bacillus crowded out by other organisms, and you get a condition which commences as slight diphtheria, and ends as a septic sore throat. The one organism is practically crowded out by others.

15514. We have also been told that it is often found in healthy throats?—Certainly; I have found it in the throats of healthy nurses who have been attending cases of diphtheria.

15515. And even in cases which have not been in contact with diphtheria?—I have not had the opportunity of examining many such cases.

15516. You have not looked for it there?—No, but I have no doubt that organisms similar to it have been found; these, however, do not give the same toxic reactions. There are certain organisms resembling the diphtheria bacillus, but you can always distinguish between the two by determining whether they produce toxin or not.

15517. You mean the Hoffmann bacillus?—Yes.

15518. Has it not been alleged that in something like 15 per cent. of healthy throats the true Klebs-Loeffler bacillus has been found?—I certainly think it is found far more frequently than is realised; but I also think that it does not attack the throat until there is some slight inflammation, some slight breaking down of the tissues. We have, fortunately, an epithelial surface, which presents a barrier to the invasion of all these organisms, and it is only when there are slight breaches in that barrier that the organism gets in and is able to make further breaches.

15519. Where there is some lesion prior to the entrance of the organism?—Yes. If the organism were not there it would heal, and there would be no further disturbance, but if the organism is there it takes the opportunity of getting in and giving rise to very serious conditions.

15520. You cited both clinical and statistical evidence in favour of the antitoxin. What would be about the total number of the cases of diphtheria that you have yourself watched?—When I was house physician I saw a dozen perhaps.

15521. And since then?—And since then I have seen perhaps a couple of dozen. I have only been called in to special cases.

15522. About 36 cases in all?—Yes.

15523. In regard to the statistical evidence you called attention to the fact that in the pre-antitoxin days, as you call them, there was a variation from 26 per cent., I think I understood you to say, at one hospital, to 45 per cent. at another hospital in the same year of deaths from the disease?—Yes.

15524. You did not tell us whether the total number of cases of diphtheria admitted into the hospitals had increased of late years. Should I be right in thinking that that is so?—I should say that it has in London, but I can also say this—I was looking at the statistics in connection with it a short time ago—that there are now actually fewer cases of diphtheria than there used to be, and that there is not only a much lower death-rate, but a lower actual mortality taking all the large cities of the world.

15525. But taking the London figures that you quoted, should I be wrong in thinking that the numbers were very much larger in the later years than in the earlier years?—No; there are a larger number in the later years during the time that I was examining, because we came to the conclusion that they sent cases in when otherwise they would have kept them out, and also that they sent in the very bad cases in hopes that they might be cured in the hospitals.

15526. Was there not another reason? Was not the area of diagnosis extended by using swab cultures?—Not until they came in, of course. At that time we did not get swabs until the patients had come into the hospital.

15527. Do you say that nowadays cases are not diagnosed by swabs?—No doubt there may be nowadays, but I was referring simply to my own statistics.

15528. I was only asking whether in recent years there has not been an augmentation in the number of cases admitted into the Metropolitan Asylums Board Hospitals, because the area of diagnosis had been enlarged by the bacillary test as against the old clinical test?—I can state that generally, but I do not think that it is carried on on such a scale that it will make a great difference in the number of admissions. In the Cambridge outbreak Dr. Cobbett and Dr. Graham Smith examined bacteriologically practically every case that came in contact with a case of diphtheria; as a result of this method of procedure they were able to stamp out diphtheria in both epidemics. Afterwards Dr. Cobbett did the same thing at Colchester.

15529. *(Mr. Ram.)* Did they use the antitoxin?—Yes, in all cases. They examined every case which had been in contact, and injected the antitoxin wherever they found the bacillus, and so were able to stamp out the epidemic in a very short time in both places.

15530. *(Sir William Collins.)* But in regard to the London cases to which you have called our special attention, should I be wrong in thinking—I want to be quite clear—that the numbers in recent years have been considerably augmented by reason of utilising the diagnosis test of the bacillus?—From the figures to which I have called attention that does not hold. I rely only on those figures which I have had to deal with specially; those are my own work; the others I have not had any opportunity of checking.

15531. Then do you stop short at the year 1898?—Yes.

15532. Then with regard to the years up to 1898, had there been much increase in the proportion of cases treated with the antitoxin between 1895 and 1898?—Yes.

15533. Could you give us the percentage of cases treated with the antitoxin in those years?—I can give you the amount of the antitoxin sent in to the hospitals.

15534. I would rather have the number of cases treated with antitoxin?—I have no doubt that I can give you that also.*

15535. Thank you. What I want to know is whether you attribute the lower case mortality that you mentioned, 13 per cent. as against 21 in the earlier years, to the fact of the larger number of cases treated with antitoxin?—Yes, undoubtedly.

15536. I should have been glad to have the figures to complete your story?—I will let the Commission have them. I have not looked over my report except for a few points, but I may say that in 1895 we only supplied 1,200,000 units of antitoxin to the whole of the hospitals under the Board; in 1896 we sent out 25½ million units, and in 1897 we sent out 60¼ million units. That means, of course, a very largely increased number of patients treated. It ran up from, in 1895, 1,200,000, to, in 1896, 25,600,000, and in 1897 to 60,300,000.

15537. Do you happen to have followed up the case in London; do you know what the percentage case mortality now is in the London fever hospitals?—No, I do not know that it is lower.

15538. Is it much lower than 13 per cent.?—I think it is about 10 per cent.

15539. Am I right in thinking that a still larger percentage of cases is now treated with antitoxin than in 1898?—Yes.

15540. And yet the mortality has been reduced only 3 per cent. further?—It comes to a point, you see, at which it is impossible to reduce it very much. If you could get all your patients in at an early period, if you could treat them within 24 to 48 hours, I believe that you could bring the percentage down to 2 per cent.

* 3042 *cases of diptheria were admitted in* 1894, *of these* 24 *received antitoxic serum. In* 1895 *of* 3824 *cases admitted* 2403 *were injected, and in* 1896, 3758 *cases were injected out of* 5068 *admitted.*—G.S.W.

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15541. Within 48 hours of what?—Of the outbreak of the disease by the appearance of the false membrane.

15542. But you may find the bacillus and there may be no false membrane?—Yes.

15543. Is it not the case now that if the bacillus is found the antitoxin treatment follows?—Yes.

15544. Even without waiting for the false membrane?—Yes, and very wisely so. If you do that, if there should by any slight inflammation, even though the bacillus forms the toxin, and the toxin gets into the tissues and is carried away by the blood and lymph, it is neutralised and the patient is put under the most favourable conditions for recovery at once; But you do not get the patients in as a rule until there is some false membrane. If you could treat them as soon as acute inflammation appears, then I believe you would save almost every case, but if you allow the toxin to act for any length of time on the various organs of the body, you cannot neutralise the harm that has been done, though you may neutralise the poison that has done it, now of course too late.

15545. You think that by using the antitoxin the mortality of diphtheria may be reduced to nil?—Almost, if you could get the cases early enough. Our Northern and Gore Farm Hospital statistics bear this out most fully.

15546. You told us just now about stamping out certain outbreaks. That was using it as prophylactic?—Yes, using it as a prophylactic in cases where there had been contact with diphtheria patients.

15547. Is it your experience that it has been valuable as a prophylactic?—I should say, from our experience of these epidemics, that it is distinctly valuable.

15548. Have you seen a recent paper by Dr. Peters, of Nottingham, rather questioning its value as a prophylactic?—No. But I am afraid that he would have to bring forward very convincing arguments to induce me to alter my opinion under present conditions.

15549. You think that by the prophylactic use of the antitoxin, diphtheria may be abolished in this country?—If you carefully examine all the throats of all "contact" cases I think it would.

15550. The actual preparation of the antitoxin from the horse does not require a vivisection licence?—Not now. We prepared it under a vivisection licence because we did not know the legal position. In fact, it was on our application, I believe, that the matter was placed on its present footing.

15551. The production of vaccine and other commercial bodies does not require a licence under the Act?—I believe not.

15552. (*Mr. Ram.*) The specific organisms of certain diseases have not yet been found?—They have not been demonstrated.

15553. Is it your opinion that in other diseases the specific organism has been discovered?—Yes.

15554. And can be dealt with?—Yes.

15555. And with regard to what you have been telling us as to yellow fever, is it a fact that not only the specific organism can be dealt with as you have indicated, but that it has been made commercially useful as a means of stamping out yellow fever?—Yes, the statistics in Havanna are marvellous. In Havanna, from 1853 to 1900, there were 35,952 deaths. In 1900-01 there were cases varying from 308 in a single month in the seasonal period down to 5; and during the same period there were from 74 deaths in one month down to no deaths. In 1901-2, when these protective measures were put into force, only one year later there were never more than eight cases in any single month, and never more than two deaths. So that from 308 cases, with 74 deaths, there was a fall to 8 cases and 2 deaths. That was entirely the result of putting into force agencies for killing the mosquito. That was the direct outcome of experiments.

15556. That preventive result followed directly from experiments which had been made, without which the origin of the disease in the mosquito would not have been suspected?—That is so, we should not have known anything about the life history and mode of transmission of bird or malarial parasites, and our knowledge of their relation to mosquitoes not being acquired could not have been applied to the infective agents of yellow fever.

15557. One other question with regard to diphtheritic cases. Are you, in your own mind, clear that even discounting other causes which have been suggested as assisting, at any rate, in the diminution of mortality——?—Which I do believe play a part, undoubtedly.

15558. After discounting those other causes, are you still satisfied that the antitoxin treatment is of the greatest benefit in diphtheritic cases?—I am.

15559. Do you find that that is the opinion of the vast bulk of those practitioners who are brought in daily contact with diphtheria?—Yes.

15560. And not only is it their opinion, but is it put by them into practice?—Yes, as I know from the amount of antitoxin that is being used in the country at the present time.

15561. (*Dr. Gaskell.*) Do you use demonstrations at all with your students in your laboratory?—Not before the junior students.

15562. But for the senior students?—Yes.

15563. Do you consider that that is necessary?—I think so. Of course, I am in a specially good position, because I have to do a number of these experiments on animals, and therefore I do not do any special ones. I get the students down to see the experiments that we are carrying out—the senior students, that is to say; so that although I am not performing any special experiments for demonstration purposes, I take the opportunity of letting them see the experiments that we are doing in connection with antitoxin and in similar work.

15564. And those experiments are experiments under licence?—They are all under licence.

15565. Are you able yourself to sign applications for a licence?—No.

15566. Do you not think that you ought to be?—I think that I ought to be.

15567. Would you suggest, then, that in addition to the names of the professors of physiology, medicine, anatomy, medical jurisprudence, materia medica, and surgery, there should also be included in the Act among those people who must sign licences, the professors of pathology, bacteriology, and hygiene?—Yes, I think so, certainly.

15569. All those three ought to be included?—Certainly.

15569. That part of the Act which should be reformed?—Yes, because we really see more of this work than almost any one, except the physiologists.

15570. Is it necessary, do you think, to use the word "professor," or would "teacher" be better?—"Teacher" would be better. A year ago Dr. Nuttall, for example, would not have been able to sign as he was a reader, but he had all the responsibility of a professor as the head of a departmental laboratory.

15571. Perhaps "teacher" and the head of a laboratory would be better?—Yes.

15572. (*Chairman.*) Is there anything else you wish to bring before us?—Professor Langley has suggested that after an operation has been completed we might be allowed to take an animal from the laboratory to some place outside. At present we have to keep all the animals on the premises. Those premises are not always very commodious, and sometimes are not very well suited for animals which must necessarily be carefully looked after, and should be kept under the most hygienic conditions possible, and it would be a very great advantage if we were allowed to take the animals to some place where they may be under better conditions than in the laboratory.

15573. (*Mr. Ram.*) Leaving the address for the Inspector if he wanted to see them?—Yes, I do not wish in any way to limit inspection. I think it is a very good thing that everyone should know that everything must be carried on carefully and without any abuse.

15574. And you have found no trouble from inspection?—None at all.

15575. It has been no nuisance to you?—No.

15576. Might I ask you on that whether your view is the same as that of Professor Langley. I suggested to him, partly to meet public feeling, that in the few cases where curare was used it might be recommended perhaps that the Inspector ought to be present in order to make sure that the anæsthesia was properly complete, and so forth. Would you see any objection to that?—I think it would be a very difficult matter to have the Inspector present. I think if you cannot

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trust a man to do experiments you must not give him a licence.

15577. Including the use of curare?—Including the use of curare.

15578. You will observe that I limited my question to that?—I should not object to a special certificate so that the Inspector might be more vigilant if it were thought necessary in order to allay public fears.

15579. And he would know of the granting of that certificate?—Yes.

15580. Would he know when it was going to be utilised?—Probably not; you could not let him know beforehand the exact hour for the performance of every experiment, but knowing that such experiments were being carried on, he would probably make it convenient to be present at some of them.

15581. (*Chairman.*) Is that all that you wish to bring before us?—There is one other matter, that is, as to the use of dogs. In this matter I should like to support what Professor Langley has put forward. It has been said that they are not useful for pathological work, but I have found on several occasions that we could not have carried out our experiments to a successful issue without recourse to the use of dogs.

15582. I think that the suggestion was that they were not necessary, not that they were not useful, but whether they were necessary?—I should say that they are necessary, even for the very experiment which Professor Langley spoke of, which we have had to perform in connection with the transmission of the tubercle bacillus from the intestine into the body that is along the lacteals. We have had to operate on the thoracic duct; that experiment has been done by a number of people in order to determine the path that the tubercle bacilli traverse in getting to various parts of the body. It is a much more complicated process than we had any idea of. It has been found necessary to trace the tubercle bacillus in the body, not only in the alimentary canal, but after it gets into the respiratory tract and the lymphatics, and so on to its invasion of the blood vessels. It is a much more complicated process than we imagined when we began our research, and one of the experiments which had to be done was the inspection of the thoracic duct for the passage of the tubercle bacillus, and we could not have done that in the same way on any other animal that I know of.

THIRTY-FIFTH DAY.

Wednesday, 13th November, 1907.

PRESENT:

The Right Hon. The Viscount SELBY (*Chairman*).

Sir W. S. CHURCH, Bart., K.C.B., M.D.
Sir W. J. COLLINS, M.P., M.D., F.R.C.S.
Sir M. D. CHALMERS, K.C.B., C.S.I.
Mr. A. J. RAM, K.C.
Mr. W. H. GASKELL, M.D., F.R.S.
Mr. J. TOMKINSON, M.P.
Mr. G. WILSON, M.D., LL.D.

Capt. C. BIGHAM, C.M.G. (*Secretary*).

Sir VICTOR HORSLEY, F.R.S., F.R.C.S., called in; and Examined.

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15583. (*Chairman.*) You are a Fellow of the Royal Society and a Fellow of the Royal College of Surgeons?—Yes.

15584. And I believe you come here to give evidence before us as representing the British Medical Association?—Yes.

15585. Would you just tell us what that body is?—It is a body composed of medical practitioners. It includes about half the British medical profession, and each member subscribes 25s. a year to further the objects of the Association. The objects of the Association very briefly are to further the interests of the medical profession and the scientific knowledge of the medical profession.

15586. And they have acted in various respects in furtherance of those views in various ways?—Yes, they have. They have a Scientific Grants Committee for the express purpose of allotting a certain proportion of the subscriptions of members in the furtherance of scientific work by giving grants of money to young members to pay their expenses for the researches they conduct, and in this manner they have expended upwards of £30,000 since about 1874—since about the beginning of the administration of the Vivisection Act. I need hardly say that medical practitioners have not much money to spend, but they have, I suggest, been led to give this money out of their subscriptions because they see in their daily work the necessity of more scientific knowledge.

15587. You propose to give us your views on what you call the ethical side of vivisection and anti-vivisection?—Very briefly.

15588. Are those the views of the British Medical Association?—Yes. I can put it in almost one sentence, not to waste the time of the Commission. The medical profession consider that every effort ought to be made to increase knowledge for the prevention of suffering and disease, affecting not only man, of course, but also the lower animals; and they consider that anyone who opposes such pursuit of knowledge is acting immorally. To them the moral duty is the pursuit of knowledge, and an immoral act is the obstruction of that pursuit of knowledge. I would compare in this particular the fact that of their own moneys they have subscribed this large sum of £30,000 for the pursuit of knowledge, while of the anti-vivisection party, Mr. Coleridge's Society, to take that alone, have collected from the public £86,000 and have not added one item of knowledge to us for the relief of suffering or the prevention of disease.

15589. I understand that you desire, in reference to this point, to call attention to Mr. Coleridge's evidence?—I do. But before I leave that point may I also draw the attention of the Commissioners to the fact that several witnesses have spoken as though the anti-vivisection party were morally justified in accepting the knowledge gained by vivisection. May I point out to you that it has always been the feeling of the profession that the anti-vivisectionists were not morally justified in accepting knowledge thus gained. The profession would, in fact, regard it thus: that if the anti-vivisection party consider that facts have, as it were, been immorally obtained from Nature, stolen from Nature, the persons who benefit thereby are practically receivers of the stolen facts, and they suggest that the legal position of the moral responsibility of a receiver being almost as bad as that of the thief holds in this particular.

15590. But how would you deal with the question? It is said, and I think generally believed, though it is disputed, that Harvey discovered the circulation of the blood by experiments on animals?—Certainly.

15591. Does that, do you mean, bind everybody in the present day not to be an anti-vivisectionist?—Certainly.

15592. Because it is impossible for him to avoid the conclusions arrived at?—Exactly, and in that connection I have placed upon my *précis* a very brief reference to the Anti-Vivisection Hospital, because the money for that hospital was originally collected many years ago, by Mr. Coleridge's society in part, on the idea that medical practice could be conducted without any reference to vivisection.

15593. Vivisection past or present you mean?—Past or present; whereas we know of course that no one feels the pulse of a patient and derives any information therefrom without being indebted to vivisection. Morally speaking, it seems to me (I am only giving my own personal opinion, of course) that the anti-vivisectionists have no right to derive benefit from experiments upon animals.

15594. Holding the views that they do?—Holding the views that they do. I would add that the Editor

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of the "Zoophilist," one of Mr. Coleridge's employees, Dr. Berdoe, stated in the "Times" that he would rather be treated with a knowledge of Hippocrates than the knowledge of the present day. I can only say as a commentary on that that from my own personal experience he does not rely on the knowledge of Hippocrates, but he sends his own family to receive the opinions of men like myself. However, I pass from that, my lord, and pass now to the single example that I have selected to illustrate the action of the British Medical Association in promoting the pursuit of knowledge, and I have taken the instance of the Pasteur Institute as being also a subject of considerable controversy and object of attack by the anti-vivisectionist party. In the evidence of Mr. Stephen Coleridge, on page 144, line 14, Mr. Coleridge used these words, "The first striking achievement of my society, with which I had the honour to be associated, was the prevention of a public meeting held at the Mansion House" (it should obviously be "to be held') "in 1889, the object of which was to start a fund to erect a Pasteur Institute in England. We prevented that public meeting taking place, and the result was that no Pasteur Institute was set up. England to-day is, I believe, one of the few European countries of importance that is spared a Pasteur Institute, and in England there is no rabies." Now the freedom of England from rabies I take to be one of the great achievements of modern science, and we owe it entirely to M. Pasteur. The position is as follows: In the first place Mr. Coleridge informs the Commission that that meeting was prevented by his Society from taking place. It is not so. The meeting took place at the Mansion House, the Lord Mayor in the chair, on the 1st July, 1889. In the second place, the object was not as he states it; it was not called for the purpose of establishing a Pasteur Institute in England, it was called "for the purpose of hearing statements from Sir James Paget and other representatives of scientific and medical opinion with regard to the recent increase of rabies in this country, and as to the efficiency of the treatment discovered by M. Pasteur for the prevention of hydrophobia." At that meeting speeches were made and resolutions were passed in favour of thanking M. Pasteur for the treatment that he had given to English patients, and then another resolution that I had the honour of moving was as follows: "Professor Horsley proposed, and Dr. Farquharson, M.P., seconded, a resolution to the effect that rabies might be stamped out of these islands, and inviting the Government 'to introduce without delay a Bill for the simultaneous muzzling of all dogs throughout the British Islands as provided in the measure drafted by the Society for the Prevention of Hydrophobia, and for the establishment of quarantine for a reasonable period of all dogs imported.'" I had the honour of acting as secretary of a Committee that was appointed by the Government to inquire into M. Pasteur's treatment, and when the Committee was in Paris M. Pasteur said to us, "Why do you come here to study my method of a curative for inoculation? You do not require it in England at all. I have proved that this in an infectious disease; all you have to do is to establish a brief quarantine covering the incubation period, muzzle all your dogs at the present moment, and in a few years you will be free." When the Committee returned and reported to the Houses of Parliament, this point of course was always before us, and I moved that resolution with the sympathy of the Committee appointed by our Government.

15595. You mean that you had no intention in calling the meeting to set up a Pasteur Institute for the treatment of rabies?—No.

15596. And no such proposal was made?—No such proposal was made at all.

15597. Your resolution I suppose was carried?—Yes, they were all carried. Immediately this point of universal muzzling was raised the anti-vivisection party, Mr. Coleridge's Society in particular, entered upon a virulent campaign against us, and held meetings and endeavoured to show that M. Pasteur's discovery of the real infective nature of the disease was wrong; they revived the old ideas that it was lunacy in dogs, that it was not a disease, and they revived also the particularly dangerous idea (from the public point of view) that it could spontaneously generate in the dog; whereas M. Pasteur had proved that the exact opposite was the case. I need not weary the Commission, but I would point out that we obtained partial muzzling in spite of this opposition, and that that did so much good that then Mr. Walter Long applied the muzzling universally and established quarantine.

15598. First they had the power to muzzle by counties?—Yes; and in the County of London, I showed from the records of the Brown Institution, the laboratory of which I then directed, that the effect was wonderful in immediately cutting down our admissions of rabies, and that it also cut down another contagious disease—distemper, which, you will understand, is due to a respiratory infection. The effect of that was that Mr. Walter Long, when he was at the Board of Agriculture, imposed a universal muzzling and brief quarantine, with the result that, fortunately, now the United Kingdom is free from rabies, and that is the only correct statement in that passage I have read. But it is not due to anything really but the scientific knowledge that we derived on the subject of rabies.

15599. You say that there were two resolutions passed at that meeting—one thanking M. Pasteur for his discovery and labours, and the other in favour of muzzling dogs?—The universal muzzling.

15600. I understand that Mr. Coleridge and his Society attacked the first resolution. Did they also oppose the second?—Yes; they attacked both.

15601. They attacked muzzling as being what—unnecessary?—They attacked muzzling vigorously as being cruelty to the dog and being unkind, because the disease, as they said, could arise spontaneously; therefore, what was the good of muzzling dogs? May I add that subscriptions were collected at that Mansion House meeting, and later on this country sent 40,000 francs to M. Pasteur as a token of the work he had done for British subjects.

15602. Do you say that the desirability and the efficacy of muzzling dogs were proved by Pasteur by experiments on animals?—Yes, certainly.

15603. What sort of experiments?—By his inoculation experiments on animals, which for the first time proved the existence of a distinct virus and of its transmissibility to different animals. Up to that time these really absurd ideas prevailed that it could be spontaneously generated, that it was not a contagious disease at all, that it was really lunacy in the dog and insanity. It required the scientific investigation of M. Pasteur to demolish that. Of course anybody who had had personal experience of rabies in dogs recognised that it was a contagious disease, and that M. Pasteur was quite right. But all the action of the authorities previously had been in ignorance of the proved fact. That is the first point that I wanted to draw the attention of the Commission to, and I should add that the British Medical Association sent a special deputation of their Scientific Grants Committee to that meeting to express the feeling of the profession as to the value of M. Pasteur's work.

15604. As it seems rather important on the question that we have to report upon, could you refer us to any action taken by the National Anti-Vivisection Society against muzzling?—Yes, I could find it, of course.*

* Sir Victor Horsley subsequently forwarded the following note:—

"The responsibility of originating and fomenting an agitation against the use of the muzzle to stamp out hydrophobia rests wholly on the anti-vivisection party as shown by the following facts:—

"After the issue in 1887 of the Report of the Committee appointed by the Local Government Board to enquire into M. Pasteur's treatment of hydrophobia, and in which the general application of the muzzle was strongly advocated, Miss Francis Power Cobbe, the leader and organiser of Mr. Coleridge's Anti-Vivisection Society, began an anti-muzzling agitation.

"One of her pamphlets entitled "The Amendment of the Dog Laws" was published by a small body of antivi-visectionists organised by one of the Executive Committee of Mr. Coleridge's Society, namely, Mr. Pirkis, and though this body was never recognised by the dog breeders and owners of the country and was but an offshoot of Mr. Coleridge's Society, it styled itself "The Dog Owners' Protection Association." Lord Mount Temple introduced into the House of Lords a Bill on their behalf in opposition to a Muzzling Bill introduced by the Society for the Prevention of Hydrophobia. The agitation reached a culminating point on November 25th, 1889, when a public meeting was called by leading members of Mr. Coleridge's Society, Surgeon-General Gordon and others, to protest against the muzzling regulations, at the time when the London County Council was pressing the Privy Council to apply them generally.

"At the meeting, however, a resolution was carried by a large majority in favour of muzzling. After this the agitation gradually died out, although an attempt was made to revive it when Mr. Long subsequently issued his Order."—

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15605. Thank you, if you would?—The action of the anti-vivisection party in that respect we consider to have been immoral. We consider that it was opposing the results gained by the scientific research, that those results were of value to the human community, and also to the lower animals; and that that opposition was immoral. I have also brought the incident before you because there are a great many passages in this evidence by Mr. Coleridge to which I should like to take similar exception; but this was such a striking contradiction of the actual fact that that is why I have taken it as an example of the rest. I propose now to pass to the next part of my *précis*.

15606. If you please?—The second part of my *précis* deals with the question of teaching students by means of experiments on animals. I desire to lay before the Commission the view that such teaching is desirable in the following subjects:—Physiology, pathology, pharmacology, and surgery; because, with the exception of the third I have had experience in teaching each of these subjects. In the first place I would suggest that Sub-section (5) of Section 3 of the Act should be repealed. It is in effect abrogated in the Act by the second part of Section 3, because experiments may be shown to students in illustration of lectures under a special certificate, but as I hope to conclude by persuading the Commission that the certificate system has so many faults that we should get rid of it, may I suggest also that the recommendation should be that this Sub-section (5) of Section 3 should be repealed. That is the first point. The argument in favour of such repealing of Sub-section (5) would be, of course, an argument against the so-called disadvantages of showing experiments to students, and therefore very briefly I would allude to the evidence which has been given by Sir Thornley Stoker, at Questions 761 and 1051, because it is quite evident that that evidence was offered by Sir Thornley Stoker in complete ignorance of the subject. He says at Question 761, page 32, two-thirds of the way down the columns, after reference to the Act: "I am strongly of opinion that . . . experiments on living animals should be forbidden in illustration of lectures, on the ground of their uselessness, and, perhaps, cruelty also." I think that Sir Thornley Stoker's further evidence, in which he informed the Commission that he was cruel in early life shows that his education as a student had been neglected by his not witnessing experiments on animals, because I think it would have assisted him in becoming more humane, and, at any rate, would have certainly prevented him from giving expression to such an opinion as this. He then goes on to say: "Such demonstrations cannot but be demoralising to the young men and women who witness their performance. They seem to be an offence against humanity." Then also Mr. Coleridge referred at length to this in Questions 11372 and 11396. All I can say is that I have shown a very large number of experiments to students, that they have always evinced the utmost interest in them, and they have told me that the facts which the experiment illustrated have been imprinted upon their memory in a way that they could not have obtained by any other means; in fact, they have simply emphasised the statement of Professor Huxley before the 1876 Royal Commission, where he said: "There is no teaching of physical science worth anything as thorough teaching unless it is accompanied by practical instruction." And I unhesitatingly affirm that there is no demoralisation whatever such as has been suggested to this Commission by Sir Thornley Stoker and by Mr. Coleridge. May I also, in speaking of the previous Royal Commission, draw your attention to the fact that in the report of that Commission, on page 17, the Commissioners say: "The evidence we have quoted above seems to show conclusively that at the medical schools where such demonstrations are exhibited under anæsthetics the sense of humanity in the students is not in fact impaired." I have nothing to add to that statement; it is as true now as it was 30 years ago. I suppose, however, that the Commission would expect me to justify my suggestion in a little detail. I therefore would point out that for instance in physiology, I think it is most essential that students should see, and in fact the senior students perform, an experiment on a warm-blooded animal—a mammalian animal—on the heart and the circulation. I choose that as an example. I am sure that no one can acquire a proper knowledge of the action of the heart or of the real phenomena of the pressure of the blood within the vessels unless he has witnessed, and indeed taken part in such an experiment.

15607. Have demonstrations of that description to show the circulation of the blood in living animals been common in past times or are they only of the present time?—I entered as medical student at University College in 1876 when this Act began its operations, and Professor Burdon Sanderson in that year showed us these experiments.

15608. Under the Act?—Yes, he gave us about a dozen, I think, demonstrations on animals during the session.

15609. Supposing that you took 20 or 30 years earlier than that; were demonstrations used then?—I do not know. I should hardly think they were.

15610. Dr. Burdon Sanderson is dead now?—Yes, he is dead.

15611. He was a very strong advocate of demonstrations, was he not?—Yes, he instituted the practical class in microscopic anatomy with Professor Michael Foster, who is now dead; but I think Professor Sharpey also gave demonstrations at University College before Professor Burdon Sanderson.

15612. I am only asking the question with reference to what you would say as to the comparative knowledge of medical men of the present day and of medical men, say, 50 years ago on such a matter as the circulation?—Of course, at the present time we have greatly increased knowledge. Take this question of the blood pressure, for instance the actual pressure of the blood; we now measure it accurately in the human being, and it is one of the essential phenomena which indicate to us the condition of shock after a surgical operation. The question of the pressure of the blood is going to be unquestionably one of the cardinal points in this great question of surgical shock.

15613. You mean that a much more detailed and accurate knowledge of the circulation has been obtained?—Yes.

15614. And therefore is a necessary equipment of the modern doctor?—Absolutely. When I was a student the blood pressure was looked upon as a scientific amusement, as it were—I mean by many practising physicians and surgeons. They took no heed of it; they never attempted to measure it, and I may say they did not attend the demonstrations of Sir John Burdon Sanderson. The next subject is pathology. I wish to point out that having lectured on pathology, and having illustrated my lectures by experiments on animals, the necessity of this method of teaching students has been borne in upon me very closely. As an example I have chosen epilepsy with convulsions of all kinds as being a very common form of disease, and yet one which the student has no means of analysing in ordinary hospital work, and very often hardly sees a patient in a fit at all. And yet epilepsy, for instance, can be reproduced experimentally with absolute fidelity by the simple injection into the veins of a drop of essence of absinthe. In 25 seconds you have a typical epileptic fit produced, and a student who has once seen it, and watched it develop through the body of the animal never could forget, and never has forgotten it.

15615. (*Sir Mackenzie Chalmers.*) Is that the ordinary absinthe of commerce?—It is the pure essence which is diluted for the drink of vermouth. It is extremely toxic.

15616. (*Mr. Ram.*) Would the animal be under anæsthetics?—Entirely.

15617. The anæsthetics would not interfere with the development of the epileptic symptoms?—No.

15618. (*Chairman.*) Nor would the animal be allowed to recover?—No.

15619. (*Dr. Wilson.*) Do you distinguish in that case between epilepsy and convulsions?—No, I am using both the words because I believe myself that practically in all convulsions the cortex of the brain is involved.

15620. But it is the disease in one case and it is the absinthe in the other that is acting. Supposing that the animal were to recover you would not make that animal an epileptic animal?—No, not permanently epileptic, the fit is produced at the time toxically. In the same connection the electrical excitation of the cortex of the brain for the localization of the different centres is also an absolutely essential part, or should be, of a student's physiological education. Not only do you teach him the geography of the brain, but you again show him another kind of epilepsy, that which is called Jacksonian epilepsy in the most perfect degree; in fact there is no other way of showing it.

15621. (*Chairman.*) Are there any of these demonstrations which it is desirable in your view to use before students, in which it is also necessary in order to complete the demonstrations, that the animal should be allowed to survive?—Only under Certificate B.

15622. Are there any which you use by way of demonstration to students?—No.

15623. There are no experiments used by you for demonstrations to students, in which the animal is not put to death before it comes to?—No, unless indeed, for instance, you want to show the students some points in regard to paralysis. Supposing, for instance, you had an experiment in which you divided one half of the spinal cord antiseptically and then dressed the wound, and allowed the animal to recover from the anæsthetic and it healed up, and you showed that animal to the class, say in three week's time, I could conceive that that is a demonstration which would be considered useful to students certainly as demonstrating the symptoms produced by paralysis of one-half the spinal cord.

15624. When it had been shown a second time to the students the animal would be put to death?—Yes, afterwards.

15625. Do you know of that demonstration having been made?—No.

15626. So far as you know, they have all been cases in which the animal has died under anæsthesia?—Yes, every one.

15627. (*Sir Mackenzie Chalmers.*) They must be, under the statute at present?—Yes.

15628. (*Chairman.*) You have already told me that you have never heard of any demonstrations except where the animal had died under the anæsthetic?—That is so, but I am proposing that the whole of that should be repeated.

15629. I am talking rather of the present practice?—Quite so.

15630. Then pharmacology is the next subject?—Yes. Pharmacology is the science of the action of drugs—at the present moment the student is taught *materia medica*, to recognise drugs, but he is only told their action, and if students in Pharmacology were made to perform practical experiments, for instance on an isolated frog's heart, they would receive valuable information. I give one example. I take the two alkaline metals, potassium and sodium; they are almost exactly alike, they are spoken of as simply alkalis, and yet their physiological action on the heart is absolutely different. If a student perform the simple experiment of irrigating a frog's heart with a solution of one and then a solution of the other, he would see that their drug action was totally different. And the same could be said of a great many drugs, and especially of drugs that act on the heart. I think that a student ought to see all those in action on an isolated frog's heart or even on a mammalian heart before he goes into the hospital to give them to human beings.

15631. Would the action be different to such an extent as to make it dangerous to use the one as a remedy precisely as they use the other?—The actual dose depends of course upon many factors.

15632. It would depend on the dose?—Yes, entirely. Then the next subject in which I suggest that students should be taught from experiments upon animals is the one in which I am personally most interested, namely, surgery. The Commission are well aware that at the present moment the only practical teaching in surgery that a student receives, except by what amounts to his experiments on human beings is from operations on a dead body, and those from the anatomical point of view are lamentably few in number. I am referring to the well-known fact also of course that the supply of dead bodies is too scanty in London. But that operating upon a dead body is practically only the revision for the student of his anatomical knowledge. It does not teach him the science of surgery, that is the operating on living tissues, which is a totally different thing. I say the texture and the method of dealing with the live tissue is quite different from that in dealing with the dead tissue; and from the ethical point of view it seems to me that it is not moral for students to gain their knowledge on man when they can perfectly well gain it on a lower animal on an anæsthetised lower animal. In that sense I would bring the use of animals for education in surgery on to exactly the same level as the use of animals for food. What is justifiable for the one is justifiable for the other. Then as regards Section 3 again of the Act, Sub-section (6) relating to manual dexterity I suggest ought to be repealed. It says "experiments shall not be performed for the purpose of obtaining manual skill," that is precisely the reason I think why they should be performed by a student; he ought to obtain his manual skill by operating on the lower animal and not on man.

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15633. To what class of student would you restrict it if you restrict it at all?—This all comes to students in their last two years of the curriculum.

15634. The whole curriculum lasting how long?—The whole curriculum lasting nominally five years; it usually lasts six years. Before a student for instance received a hospital appointment he certainly should do such a course.

15635. And what age would a student be in his last two years; is there any minimum age?—Yes, there is a minimum age, but on the average he would be about 22, from 21 to 23 years of age.

15636. (*Sir Mackenzie Chalmers.*) Before he gets a hospital appointment?—Yes. That means before he is qualified you see, because practically now we do not admit any students to hospital appointments until they are qualified. It is part of their surgical education. It is always very difficult to understand why some of these sections and sub-sections were introduced into this Act, even when you read the Report of the Commission of 1876; and that one in particular I feel sure must have got in somehow under the impression that students would practice on animals without anæsthetics or something of that sort.

15637. (*Chairman.*) Did not the Royal Commission report in favour of that?—I forget. I do not remember the point. But it is in the Act, and I suggest that it should be repealed. Moreover, I am not speaking now without the fact of this method of teaching having been already in operation; it is in operation at the John Hopkins Hospital, Baltimore, perhaps the most advanced school of medicine.

15638. You mean the method that you are advocating?—Yes; it is the most advanced school of medicine and teaching students operations upon animals which they compel them to execute in precisely the same way as they will subsequently operate on a human being. Professor Harvey Cushing, at the John Hopkins Hospital, has now a building for this practical work; perhaps I may show you a photograph of the students equipped as they are (as we all are now for modern operations) and performing the operations on animals (*handing in the same*). The usefulness of this is greatly enhanced by the fact that Dr. Cushing has copied the constitution of our Brown Institution here in London, that he has also instituted hospital wards as part of the building for animals, rooms for the care of the animals, and in that way the poorer classes in Baltimore bring diseased animals to the laboratory to be thus operated upon by the students and cared for in the animals hospital.

15639. (*Mr. Ram.*) In the interests of the animals?—In the interests of the animals.

15640. For curative purposes?—Yes; of course, very often the work is also combined with the requirements of the physiological laboratory. If, for instance, a physiologist wishes to investigate a stomach which has a fistulous opening in it, then it is made under these circumstances in this laboratory, and the animal is transferred to the Physiological Department when the fistula is healed.

15641. (*Chairman.*) In this laboratory of Dr. Cushing's is there a professor or doctor or anyone in charge of the students present in the room?—Yes, a demonstrator.

15642. Some responsible person?—Certainly.

15643. (*Sir Mackenzie Chalmers.*) Who is responsible for seeing that the anæsthetics are properly administered?—Yes, I am coming to the question of anæsthesia directly.

15644. (*Chairman.*) I merely meant in a case like this, where the students work and are allowed to learn their profession upon animals by experiment, do you say that there it is conducted under some responsible head?—Yes, it is conducted most strictly, like any other practical class, and the demonstrator sees to it. The different students are told off to act in different ways; one to act as anæsthetist, another to take the notes, another to do the operation, two or three more to act as dressers, and so forth. They are all told off, and they keep records of all their work. Then, in association with that point, is the

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very important question which has just been raised of anæsthesia. I wish to draw the attention of the Commission to the fact that the risk of death from anæsthesia has always been justly looked upon as a great reproach, and I wish to express my personal opinion that it is purely a matter of knowledge of the dose required, and that as regards the education of students in anæsthetising patients no one ought to be allowed to render a human being unconscious before he has had practice on animals.

15645. What is it that he has to study in it particularly?—The signs of danger, the signs of an overdose with an anæsthetic drug. He can observe all these points on animals exactly as in man. I may say that as regards the investigation of this subject (the actual dose) that is now under the consideration of the British Medical Association, which I represent, and that they have already spent on this subject over £500 to try and determine the exact dose which shall render a person unconscious to pain and at the same time shall not put his life in any danger.

15646. Is there any ascertained scale of the difference between the doses. If you have ascertained it for a man is there any scale that is ascertained to be the proper dose for a rabbit?—It is apparently the same. The percentage of chloroform in the atmosphere breathed that leads the heart to fail or the respirator centre to fail is apparently very much the same in whatever animal. I am speaking now, of course, of warm-blooded animals like man.

15647. Is that a recent discovery?—Yes.

15648. (*Sir Mackenzie Chalmers.*) It is not the same amount but the same proportion you mean?—Yes, the same proportion, not the same amount. The difference of weight, of course, comes in there. You have to poison so much per lb. weight.

15649. (*Chairman.*) That is what I understood from other witnesses. I did not quite follow your answer? —I wish to say that in reading this evidence which has been offered to the Commission I am struck with the extraordinary ignorance that prevails even now on the question of anæsthetising animals. Over and over again witnesses have spoken as though anæsthetising an animal was a totally different thing from anæsthetising a human being. It is nothing of the kind. We have even had revived before this Commission the absurd statement made thirty years ago that you could not keep a dog under chloroform for I forget what the limit was—two hours, or something of that sort. You can keep it under chloroform for a week if you only take the trouble. I have kept an ourang under ether for eight hours, and I would keep a dog or any other animal under an anæsthetic as long as you wish.

15650. By under an anæsthetic, you mean in a condition in which it does not suffer pain?—Absolute unconsciousness to pain. And that, from one's personal experience in the human subject, of course, applies to chloroform as well as to ether, and it applies to morphia—those three narcotics.

15651. You think that that ought to be practised upon animals before anyone practises it on human beings, and that the student's experience gained upon animals is an education, and a complete education, for applying it to man?—I do.

15652. (*Dr. Wilson.*) But the knowledge gained about anæsthesia is never certain—is never complete—because you admit that there are experiments going on now?—But the experiments which are going on now are refinements in order to water down, as it were, the limit. We want to extend the margin of safety without encroaching upon the consciousness to pain. I look, therefore, upon the importance of teaching anæsthesia by experiments upon animals as quite equal to the importance of teaching manipulative dexterity and the recognition and handling of tissues in ordinary surgical operations. Before leaving that point, may I also draw attention to the fact that the anæsthetic substances used in surgery fall into two classes—the respirable, and those which are injected as liquids, such as morphia, for instance, in solution. The action of the respirable gases on the body, and the use of vapours in anæsthesia really date back as long ago as Priestly's experiments on mice with oxygen.

15653. (*Chairman.*) You mean the Dr. Priestly of 100 years ago?—Yes. The idea that invisible gases affected the body was established in that cost by Priestly on mice, and then followed the experiments by Davy on nitrous oxide; those were experiments on men. But again that was a respirable gas. Then we come down to ether and chloroform; but, as regards ether, the real discoverer of the anæsthetic action of ether, Morton, the American, made experiments on dogs. He very quickly, and, I should have said, if I might venture to say so, on slender evidence applied it to man, but he made experiments on dogs, and so did Flourens also. I would accordingly propose that all such experiments with new drugs should always be made on dogs rather than upon man. The next point in my *précis* is the question of new operations in surgery. I shall show directly that many of the operations which are performed now are based entirely on experiments on animals; but I would like to point out that from the ethical and moral point of view, it seems to me that this is an absolutely essential procedure, which ought to be adopted before any new operation is carried out for the reason that if a new operation upon an organ or tissue of the body is performed, no one can foretell what will be the immediate consequences to the animal as a whole. This not only applies to the operation, but it also applies to the actual details of execution. For instance, in operations upon the alimentary canal, the mere handling of the organ to be operated upon, the mere handling of the bowel, for instance, the method of inserting stitches, and so forth —all that comes in as essentials in execution of the operation, and that having to be learnt *de novo*, I suggest ought to be done upon an animal and not upon man. There can be very little doubt that some of the anti-vivisection agitation relating to so-called experiments in hospitals actually bears upon this very point. The anti-vivisection party say that it is immoral to try a new operation, or a new drug, upon a human being in a hospital. They call it an experiment. But, then, they also say it is immoral to do it upon an animal. The only conclusion, of course, is that there should be no more new operations and no more new drugs used in the endeavour to relieve suffering or avert disease. It seems to me an absolute *impasse*, and that if attempts have been made upon patients in hospitals, it has been because a physician or surgeon thought he had a new idea, and wished to put it in practice for the relief of humanity.

15654. I do not think that we have had any witness, so far as I remember, who has come before us from either side who has said that no new drug ought to be employed in the case of a patient.

15655. (*Sir William Collins.*) The distinction was between voluntary and involuntary. I do not know whether you deal with that?—Yes, of course, I do recognise that.

15656. You did not mention it in your *impasse*?—No.

15657. That would be the retort that would be made, I suppose? Probably you will deal with it?—Yes, of course, a very great many investigators have swallowed a great many drugs—Simpson, in inhaling chloroform, for instance.

15658. (*Chairman.*) You were meaning whether the patient's consent was obtained?—Yes, I quite see the point.

15659. That would be the case in an operation; in the case of a drug he could not form any opinion. He might form an opinion whether he would rather die or suffer the risk of pain?—Yes, of course. I have seen a physician propose to a patient, and say, "Here is a remedy that has been suggested; I do not know what its effect is. Will you try it?"

15660. But in the case of an absolutely ignorant person who knew nothing whatever about it?—He probably would not consult him.

15661. He would leave himself in your hands, or you would assume that he had left himself in your hands if you were really doing it not for experiment but with a view of curing him?—Yes, quite so. I gathered that one witness at Question 6077 was rather brought to that conclusion. He said that all experiments were immoral. It was Mr. Graham. He was brought down finally to the point that if there was any serious pain afterwards the remedy might be tried on an animal. It seems to me that is begging the question. The thing is either moral or immoral, and I venture to suggest that any new operation, any new operative method or procedure ought to be tried on an animal before it is tried on man.

15662. That question was with reference to an operation on an animal?—Yes. Then we come to Part III. of my *précis*.

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15663. With reference to this I see the first head you wish to speak of is about antiseptic surgery?—Yes.

15664. I do not know whether you have read the evidence that Dr. Bantock gave the other day?—No, I have not seen it. I should like to read it very much.

15665. I am a little afraid of speaking of these scientific subjects which I do not understand, and of misrepresenting anybody, but he generally seemed to think that surgery as practised now aseptically was nothing but a return to the practice of any really careful surgeon before Lord Lister's views about antiseptic surgery were promulgated. What would you say with regard to that?—The answer to that, of course, is extremely simple; that in the practice of the so-called careful surgeon of 40 or 50 years ago, there occurred numerous deaths from pyemia, septicæmia, erysipelas, hospital gangrene, and so forth, and these do not occur now. Consequently the practice of that careful surgeon was not modern aseptic surgery at all, and for a very good reason—he did not know his danger, he did not know that it was not merely sufficient for him to wash his hands; he did not know that he had to exclude every avenue of entry of microbes into the wound. It is wholly incorrect to make a statement of that sort.

15666. I think Dr. Bantock's view was that microbes did not cause the disease?—Oh, very well.

15667. That microbes were possibly existing in the body, and became active by reason of the disease, but were not the cause of the disease?—That is auto-infection. Of course, we know that is not true. The vast majority of infection in surgical cases comes from outside, that is introduced into the wound.

15668. We have had a large body of skilled evidence against Dr. Bantock's views, but I was not going into the whole of his views—merely that he seemed to think that Lord Lister had discovered and recommended the antiseptic treatment, and that it had proved a complete failure, and that then they had had resort to aseptic surgery, and he said that aseptic surgery in his view was nothing more than what careful surgeons did before Lord Lister touched the matter?—I am sorry to say that, in my opinion, that is absolutely a perversion of the actual position. As I say, I began my surgical work in 1876, just after the introduction of the antiseptic system; consequently I have seen the whole thing develop, and I affirm that our present practice is nothing but the logical outcome of Lister's teaching 35 years ago. That is to say, Lord Lister taught that you had to exclude the microbes from the wound; apparently they were in the air, and were ubiquitous; you had therefore to employ anti septic substances to kill them, as they fall into the wound, and that was the reason why he employed the spray for one thing and antiseptic lotions for another—that you could not trust (because the whole thing is a question of trust now that the whole of modern surgery is a constant anxiety to a surgeon), the condition of your instruments, and your dressings also, unless they were saturated with antiseptics. But Lord Lister him self said over and over again that inasmuch as these precautions were exceedingly irksome to the surgeon, he would be only too delighted to get rid of them, and no one was more delighted than he when it was shown that the risk of infection from the air was so slight that certainly the use of lotions was enough to avert it, and, therefore, away went the spray. But that was always Lister's own position, and always has been.

15669. I gathered from other medical witnesses that that was what you would probably say. I do not think that Dr. Bantock's views were largely supported by any evidence we have had before us, but still, as he is an eminent surgeon, I thought I should like to ask you?—May I add on this very matter of technique in experimenting the investigations which have been made on animals with regard to infection from the peritoneal cavity, because most of the controversy has arisen over the surgery of the peritoneum. Our ideas now about the physiological action of the peritoneum and its resistance to microbes and infection have entirely altered in the last ten years, and consequently our technique has been altered with regard to the use of antiseptics with regard to the peritoneum. I do not think I need labour the point. Of course I have seen, as I said before, the absolute disappearance from my hospital, University College, of certain diseases—they have gone. Anybody who would be asked now to write an article on pyemia or blood-poisoning in a dictionary of surgery, could not do it; the diseases are gone; and that is wholly due to the introduction and the proper execution of antiseptic surgery. Then, in regard to the saving of pain, the invention of antiseptic surgery has saved a degree of pain which is absolutely incalculable. I believe that it has saved more pain than anæsthetics. I suppose that most of us have been operated upon at one time or another, and one knows that the pain of a wound only lasts for about forty hours, and with a wound that has been well made it is a perfectly tolerable pain. After that, in the healing, there is no pain. But when I was a student a simple case like the removal of a breast for cancer used to have painful dressings for six or eight weeks, and amputations still more. If an amputation now is not healed in a week you blame yourself or the house surgeon. Fortunately we very rarely do amputations now owing to Lord Lister again—very rarely indeed; but in the old days amputations used to go on for a couple of months of very painful dressing.

15670. What is it that renders the dressing unnecessary?—The immediate healing; the complete exclusion of microbes from the wound, and therefore immediate healing of the tissues.

15671. The immediate healthy healing?—The immediate healthy healing, so that the saving of pain by the introduction of the antiseptic system, for which, of course, we also have to thank M. Pasteur, as well as Lord Lister, is enormous. Then, as a third point, the antiseptic system has led to the discovery of new operative treatment; it has led to the discovery of absolutely new operations, by which condition are now perfectly easily and safely treated by surgery which before, when I was a student, were looked upon as hopeless.

15672. You connect antiseptic surgery with experiments on animals?—Entirely, from beginning to end. Because, you see, until you could show that a microbe had a toxic or destructive effect on animal tissues there was no basis for antiseptic surgery. It was Pasteur's work by experiments on animals which proved that point. It was the crucial proof that was requisite. And the Anti-Vivisection Society, in their opposition to antiseptic surgery and their repeated quotation of Lawson Tait, for instance, show, I think, a distinct lack of moral feeling. Anybody who could oppose the means of relieving humanity and relieving the lower animals by a means so simple in execution as antiseptic surgery, I think, cannot be actuated by proper moral feeling. I have seen over and over again Mr. Lawson Tait's name mentioned in this connection. I am not here in any way to answer for Mr. Lawson Tait, who is dead now, of course, but I would like to point out his action in regard to the question of the institution of the Pasteur Institute in his country. Mr. Coleridge informed the Commission that no Pasteur Institute was established. If he means that no institute like the Pasteur Institute in Paris was established, then that statement is contrary to the fact, because the British Institute of Preventive Medicine, now called the Lister Institute, is a Pasteur Institute of the same class, established on the Embankment.

15673. When was it founded?—The committee for its founding, I think, was instituted in 1891. At any rate, a meeting was held in 1893 in favour of it at Birmingham, and as I was very keenly interested in the subject from a public point of view, I went to that meeting. Mr. Lawson Tait attended that meeting, and the first resolution was: "That the members of the medical profession in Birmingham and district cordially approve of the objects of the British Institute of Preventive Medicine." "Mr. Lawson Tait said that he fully assented to the resolution, feeling that, while he objected to a certain class of surgical investigations, bacteriological experiments on animals had proved of great value."

15674. That shows that he did not agree with Dr. Bantock's views about microbes?—Evidently not, and yet he is quoted very widely.

15675. (*Sir Mackenzie Chalmers.*) By Dr. Bantock?—Yes, and by Surgeon-General Thornton.

15676. (*Dr. Wilson.*) That is about the surgical point of view of the question, I think—Lawson Tait's surgical point of view, not the bacteriological point of view?—But surgery is applied bacteriology, is it not?

15677. Not altogether?—I should say it was.

15678. (*Mr. Ram.*) You will find at the bottom of page 625, Dr. Bantock was asked, "Was Mr. Lawson Tait practically on the same lines with yourself?—(A.) Almost absolutely"?—That lies between Dr. Bantock and Mr. Lawson Tait.

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15679. (*Chairman.*) And the views of Lord Lister on both the subjects of bacteriology and surgery have been generally adopted, not only in England, but over the whole Continent?—Yes, over the whole civilised world—everywhere.

15680. You now come to the question of the surgery of the nervous system?—That, of course, again is a very typical instance of the influence of experiments on animals. There is no special surgery of the nervous system, it is simply the application of Lord Lister's principles to the treatment of diseases of the nervous system, and consequently the basis of it is a question of diagnosis. I wish to put to the Commission that there is not a single function of the nervous system, the principle of which we know now, which is not derived from experiments on animals; all our fundamental knowledge of the different parts of the nervous system is derived from experiments on animals. I would like also to point out, to begin with (because it is a point that will have to come up several times), that frequently in the history of medicine the greatest mistakes have been made because people have endeavoured to reason from structure to function, and have endeavoured, by mere anatomical investigation, to guess at function. That has led to the worst mistakes. With regard to the nervous system, Aristotle, you remember, said that the function of the brain was to temper and soothe the heart, it was to cool the heart; and it was nothing but the experiments of Galen, simply opening the rigid box of the skull and showing that the brain was as hot as the heart, that got rid of that idea. That mistake, of course, has been repeated again and again. And then again, if you take the experiments of Bell on the nerve roots, the nerve roots, of course, were known to Galen, but their function was not dissociated until Bell did it, and then only in the case of one root. But his language is very striking. What he did was to expose the roots in a rabbit, and as he had no electrical means of stimulation, he touched the two roots, first one with a point of a knife, and then the other, and he found that if he touched one there was no movement as the result—but if he touched the other, immediately there was a movement of the muscles; in other words, that the two things had different functions.

15681. This was, of course, without anæsthetics?—Yes. He says: "I now saw the meaning of the double connection of the nerves with the spinal marrow." Of course he did. The thing had been known to anatomy certainly (Galen lived in the second century, A.D.), for 1,600 years, but it was not until an actual physiological experiment was done that the investigator now saw the difference between the two things. I always think that is a most striking demonstration of the absolute value of experiment. And as regards Bell's position, which has been referred to again and again in the evidence, the facts are extremely simple. Bell did a great many experiments. If you look at his original pamphlet of 1811 (which is in the library of the Royal Society) it is very interesting; it is covered with manuscript notes of his experiments on the brain, the respiratory centres, and a great many other experiments.

15682. On animals?—Yes, he notes over 100 that he did, and as long as he was investigating the physiological side of nature he made discoveries. He gave it up, and he then later become involved in a certain amount of controversy, because Magendie and the French physiologists by their experiments discovered the function of the other root, the sensory root. They proved that it was sensory by stimulating its central end, which is again the only way of discovering its function. And then Bell, unfortunately, entered on a priority discussion, and in a good deal of feeling said he did not owe anything to his experiments. I have forgotten the exact words—at any rate, he repudiated his experiments. As a matter of fact, at that time he was doing nothing, and was adding nothing to science. From the moment that he gave up experiments on animals, he equally gave up his contributions to science. As I am speaking about Bell, may I just allude to a point that I have raised a little lower down about neuralgia, namely, that it was Bell's experiments on the facial nerve, which is a motor nerve, as contrasted with his experiments on the fifth nerve, which also goes to the face, but is a sensory nerve, which saved people from having unnecessary operations performed upon them for neuralgia. But it was not until he made this experiment on a donkey that the difference of function between these two nerves was understood at all. Anatomically they had been known, of course, again for something like 1,600 years. Then I wish to draw the attention of the Commission to the fact that as regards investigation of the anatomy of the nervous system, by far the most illuminating way of doing this now is the method of Marchi. The method is very simple. The animal is anæsthetised; the portion desired to be investigated is excised by a little operation of a minute puncture (or more extensive, if necessary), made in it, the wound is closed, it heals in a few days, and the animal is kept for three weeks alive, apparently in perfect health; it is then killed, the brain is taken out, hardened and stained in a particular way with osmic acid. The result of that is that the nerve fibres degenerate from the injured spot, and when the brain is hardened and stained in this particular method, only those degenerated fibres stain jet black, all the rest form a pale yellowish brown background. The result is now that if we make a small lesion in the front of the brain, and there is a fibre going all the way down the nervous system, we can trace it all the way down to its very end. A single fibre is a thing which, of course, is absolutely microscopic, and until this method was devised our knowledge of the bundles of fibres which connect different parts of the nervous system was to a large extent surmise, because the fibres interlace so that you could follow one fibre—it was absolutely impossible. But now by this method, the degeneration method, as it is called, we have a means of investigating the anatomical structure of the nervous system which is unrivalled and is already adding every day, as researches come in, enormously to our knowledge. So that anatomy, which rather misled us in regard to the functions of the nervous system, is now actually being helped from the physiological side. That is rather interesting in this matter of experiments on animals.

15683. You apply both the experiments on the living animal and anatomy of the dead body afterwards?—Yes.

15684. And its condition?—Yes. Then, of course, the surgical treatment of nervous disease includes operations on the brain, operations on the spinal cord and operations on the nerves. In the case of operations on the brain, for instance, you realise that this is a question of being able to treat disease within a closed cavity; you cannot examine the part; you have to form your opinion as to what is going on entirely from your knowledge of the physiology of the brain, and that we owe, of course, in the greatest measure to the discoveries of the late Professors Hitzig and Fritsch, and in our country Dr. Ferrier. That has all happened since 1870, and we are now able to cure epilepsy, we are able to cure abscess of the brain, and we are able to cure tumours of the brain. These conditions, abscess and tumour of course being invariably fatal and causing blindness, severe pain, vomiting, and so forth. Then in operations on the spinal cord the same thing prevails. In fact, the first operation on the spinal cord I am responsible for, so that I know the history of that subject. The technique of that operation I owe entirely to experiments on animals, and I am very glad of the present opportunity to repeat what I have said on that subject. Then as regards operations on the peripheral nerves, I have already referred to Bell's operative treatment of neuralgia as being guided entirely by his experiments on animals. Then we come to the great subject of nerve suture. Injuries to nerves now of course we deal with successfully by dividing the nerves again and sewing them together in various manners; and the initial work bearing upon that subject was carried out by Flourens, who was the first to my knowledge to make experiment on animals, to suture nerves together, to investigate their function.

15685. About when was that?—I have not got the exact date; I think about 1845, but I can find it out.

15686. Professor Langley was giving us some evidence yesterday about the joining of nerves, and the history of it, and he spoke of that knowledge as having been extended a good deal at the present time?—Yes, enormously. I pass from the nervous system to the alimentary canal, because operations on the stomach and intestines are now done on a very large scale, with great benefit to man, and this is being extended to the lower animals, for obstructions of various kinds, and for cancer and tumours generally. The first direct experiments—this is an instance of direct investigation for a surgical purpose—were made by Barischewsky. That was in 1875, but I would like to point out that Barischewsky himself was already indebted to the experiments of Lembert in 1835, who, by his experiments on dogs, showed what had always

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been the weakness of this department of surgery, the correct manner of applying a suture. I only mention that again to emphasise what I said about the teaching of students, that the actual technique even in such a minutia as the passing of a stitch ought to be done by experiment first. Then our own observer and great surgeon, Travers, in 1821 had made similar experiments, but in 1875 Barischewsky showed a remarkable series of experiments on dogs, in which he had extirpated portions of the bowel, and proved the detailed technique, whereby dogs could be got to survive. Following upon his work, in the next year, Gussenbauer and Winwarter made direct experiments on joining the stomach and the bowel of the same kind; and then Billroth, on the strength of these experiments, performed the first successful excision of cancer of the stomach, and that really inaugurated not only the operations for the extirpation of the disease but also operations for the anastomosis of different parts in the canal, getting round obstructions and removing dilatations, and so forth. While speaking on this question of the surgery of the intestinal canal I should like to make a remark with regard to fistulæ, because here again constantly the anti-vivisection party represent that it is an exceedingly painful proceeding to make a fistula on a dog to connect one of its ducts or its bowel with the external surface, and to let it heal up. The historical case, of course, of Beaumont, of Alexis St. Martin, is always against that. Every case of colotomy, cancer of the bowel, is of course a direct negation of that assertion, and yet it is repeated, and has been repeated before this Commission.

15687. What is repeated?—That a fistulous opening between the bowel and the surface of the skin causes pain, and that the making of a fistula causes pain. I would like to point out that by one method of making operations to relieve the bowel the wall of the bowel is sewn up to the surface—I am speaking now of an operation on man, of course—the bowel is not immediately opened. Three or four days later you slit open the bowel, and the patient is not conscious of it in the least. You can cut open the bowel, you can cut open the stomach, and the patient not be aware that you have touched him.

15688. Without anæsthetics?—Yes. It is an entire misrepresentation to suggest that animals with fistulæ are suffering pain, although they are kept alive for months, of course, very often. But that was rather in passing. I now come to No. 4—surgery of the thyroid gland. I choose that subject also because it is one that I am personally interested in, and therefore, perhaps, know more about; and here again it is most interesting to see how anatomy misled, and, in fact, hindered, investigation, because even the great anatomist, Luschka, as late, I think, as 1880, suggested that the thyroid gland really was of no service, and that it might have been put by the Creator in the neck from cosmetical reasons to pad it out, exactly like a bolster, for support. Of course he was unaware that Schiff, as long ago as 1859, had made experiments on the extirpation of the thyroid gland, and had shown that it was not an accidental structure, but that it was an organ which was necessary to life. Then the rest of the history of the subject also is interesting, because it shows the necessity of experiments on animals for the direct scientific proof of a biological fact. Sir William Gull, and more especially Dr. Ord, had shown that certain patients exhibited symptoms resembling the cretins in Switzerland, and that this could come on them, as it were, as a disease; and Dr. Ord described that the thyroid gland was diseased in these cases. He had one autopsy. Then Professor Kocher had noticed the same thing in his operations on the thyroid gland, but it was suggested universally, and especially in this country, that this was a disease of the sympathetic nervous system, and that if it had occurred in surgery of the neck it was because the sympathetic nerve in the neck had been damaged during the operation. A Committee was appointed, and I was appointed to make the crucial experiment of removing the thyroid gland in a monkey, in order to see whether that was the cause, or whether it was due to damage of the nerves in the neck; and my experiments proved that it was due to the loss of the thyroid gland. The cost of the work was partly defrayed by the Royal Society and by the British Medical Association. This was 22 years ago. I also suggested, in consequence of these results, that we ought to graft a healthy gland into people as a means of cure. I had no opportunity of doing that myself, but it was done abroad with apparent success. The result of these experiments and of his own researches led Dr. George Murray, of Newcastle, to suggest that we could compensate for the loss of the gland by injecting the juice of the gland, the secretion of the gland, and that was the beginning of the successful treatment of myxoedema and sporadic cretinism. In ordinary typical cases of myxoedema the people lose their hair, they become stupid, the subcutaneous tissues are swollen, and so forth. After treatment their hair begins to grow again, and they recover their mental activity and resume their work, as you will see (*handing in Murray's Book with Examples*). The thyroid gland, I should say in parenthesis, has no duct, and that was the reason no doubt why it was looked upon as unimportant; but in those days people did not know what we know now, that there is a whole chapter in physiology on internal secretion which was not dreamt of when I was a student, and that these researches and others of even a more striking character on the suprarenal body by Professor Schafer and Dr. Oliver, as I say, have opened up a whole new field of work, which certainly would occupy several laboratories alone; so that both from the practical point of view in medicine and surgery, and from the physiological point of view, these experiments on animals have been of very considerable service. That is all I propose to lay before you as mere examples of direct advantage accruing from experiments on animals. I hope the Commission recognise that I have only touched on three or four, and that one could spend a great many days on that subject. I now come to a rather more personal point, because I see that a great deal has been made of it in the evidence laid before the Commission, which is also connected with what I have been stating—namely, Part IV., Experiments on Animals in Relation to Surgical Shock and Incomplete Anæsthesia. In the first place, with regard to surgical shock, it is probably not recognised by the public that in getting rid of blood-poisoning Lord Lister had left practically only one cause of death from operations. There is practically only one cause of death from operations, and that is shock, or collapse; it is the one thing we dread. We never expect a patient to die nowadays from the operation unless an accident has occurred, or if they die from this shock. The importance of this was obvious as long ago as 1895, and Dr. Crile, who is Professor of Surgery now in Cleveland, Ohio, came to University College to study British surgery, and, being naturally interested in the question of shock, I suggested to him that we ought to know more precisely what was the condition of the circulation, as the result of an operation performed in the ordinary way. He therefore commenced under my guidance at University College an investigation, which, I understand, also has been put in evidence here, which I notice has been described as "a series of tremendous experiments," as "very horrible experiments"—in fact, all sorts of adjectives—"repulsive," "loathsome," "filling decent people with disgust and horror," and so on.

15689. Are you referring to some evidence given before us?—Yes, all those adjectives have been applied to these experiments of Dr. Crile's.

15690. (*Sir Mackenzie Chalmers.*) By what witness?—Chiefly by Mr. Coleridge, but in some of the questions that I saw they were described as seeming to be very terrible, and so forth. Anyone who is not a surgeon, of course, might naturally apply such expressions to any surgical procedure. In surgery many things are repellent—naturally repellent—but, of course, we do not hesitate from completing an operation because it is unpleasant. If we did surgery would be in a bad way. What I wish to point out is that there is nothing unusual about these experiments of Dr. Crile's. What he describes as manipulations, in ordinary language are exactly what we do upon the human being. I notice that Mr. Coleridge, for example, selected two things. I will select those two also.

15691. (*Sir Mackenzie Chalmers.*) Are you referring to pages 182 and 183?—No, I think it was in his introductory statement—pouring boiling water into the abdomen, on page 146, Question 10265, in answer to Colonel Lockwood. I should like to begin here: "From the dog we can learn courage, constancy," and so on; "it is the weight of its spleen, or the pressure of its blood, which elicits his curiosity, and he digs into its living body in his horrid quest. I desire to bring before the Commissioners the sixteen experiments performed by Dr. Crile in Sir Victor Horsley's laboratory, in one of which the foot of a dog was

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deliberately crushed 'under incomplete anæsthesia.'" Then follow the adjectives to which I have referred; and a little lower down: "The knowledge of what will happen to a dog when its feet are crushed in pincers and boiling water is poured into its inside seems to me as a layman, as remote from any practical service to humanity," and so on. To anyone who is completely ignorant of surgery and the difficulties of surgery these things may, no doubt, seem extraordinary, but I will take those two points—the crushing of a dog's foot with pincers, and the pouring of boiling water into the abdomen. Taking, first, the crushing of the dog's foot with pincers, that is an ordinary description of what we call tarsectomy.

15692. (*Chairman.*) Is tarsectomy an operation performed on man?—Yes, exactly. I was just about to say that a fortnight ago I operated on a young fellow whose leg had been severely smashed in Canada, and the bones of his foot were all welded together, and fixed on to the rest of his leg—absolutely welded on by callus in such a way that he could not walk. It was necessary, therefore, to perform a partial tarsectomy. If I were to describe that operation in this sort of language I should say a large gash was made in the side of the foot, that a stragulus was seized with pincers and was crushed with other cutting pincers, and was dragged out; then other portions of the bone were similarly crushed with pincers, and the wound was closed. In that case the patient survived. In these cases of Dr. Crile's, the foot was similarly gashed open, if you like, the bones were crushed with pincers in a precisely similar way, but the animal did not survive. These experiments were all done under a licence, and the animal was anæsthetised from beginning to end of the experiments, and its death is described by Dr. Crile in his paper in each instance, and he often made a *post-mortem* examination.

15693. What was the object of the experiment on the dog?—In an operation on the periphery of the body (the foot, ends, limbs, and so on), from accidents in manufactories, the crushing of the bone has been known from the time Pirogoff, the great Russian surgeon, to be a fertile source of shock—surgical shock—so that the first thing to do in investigating shock was to devise some operative procedure such as we do upon man, which should involve the cutting of bone. All cutting of bone is crushing or sawing. Therefore you have these descriptions here of Dr. Crile's of repeatedly crushing bones and sawing bones. He sawed one bone six times, I think. That is in order to imitate on the lower animal what we do on man, and yet they are spoken of as something phenomenal—something extraordinary.

15694. (*Mr. Tomkinson.*) With a view of seeing whether that operation would be likely to be fatal?—No; with a view of seeing what effect it produced on the circulation. These tracings give you the state of the circulation as registered by the blood pressure in a mercurial manometer. If you got an indication of fall of blood pressure it would mean that the patient was becoming subject to what we call surgical shock, and, therefore, you must alter your procedure; you must do something to avert it. That was the object of the experiment—to investigate the effect of blood pressure, so that in all these cases he records respiration, effects on respiration, and effects on blood pressure.

15695. (*Chairman.*) But supposing there was a fall of blood pressure, there would be more danger of collapse?—Yes, certainly.

15696. Supposing you do find at a certain point of your operation on a dog that collapse ensues, or threatens to ensue, how do you apply the knowledge that you gain by that experiment to a human being—what is the benefit to mankind?—By trying on the dog further remedies. For instance, Dr. Crile tried an injection of strychnia and an injection of adrenalin to artificially raise the pressure, and all those things we use nowadays invariably in the operating theatre; no operating theatre is complete without them.

15697. I understand that you selected this particular operation with a view to studying shock and how to avoid it?—Yes.

15698. And you selected this particular operation on the dog because the same operation on mankind was found to be one which frequently involved collapse?—Yes, crushing bones and sawing bones.

15699. Finding collapse or symptoms of collapse in a dog, you then tested remedies?—Yes; that is the whole point exactly. Now let me take the other point, because apparently more has been made of that. What are the words? "The knowledge of what will happen to a dog when its feet are crushed in pincers and boiling water is poured into its inside," and so on. As regards the application of boiling water to intestines probably no one is aware of what is done to a human being in many respects. The use of the actual cautery, of the hot iron, or flame, on the effect of which, again Dr. Crile experimented, is very common, that is to the public. But I do not think it is known to the public that a jet of scalding steam is injected into the womb of a woman for certain forms of inflammation.

15700. Under anæsthetics?—Yes, of course, and it is also injected to stop violent hæmorrhage when you are operating on the liver. These procedures are carried out, and what we must know is how far is it safe and justifiable to use such things.

15701. Are all these things under anæsthetics in the case of an animal?—Yes, everything; and to my mind it is perfectly ridiculous to select a set of experiments on an animal which are parallel to what is done on man and hold them up to reprobation. I do not care whether Mr. Coleridge or anybody else reprobates a thing if one is conscious that one is only fulfilling a moral duty—namely, investigating the causes of suffering, and the means of alleviating it and preventing disease.

15702. And without inflicting pain on the animal?—Precisely; without inflicting any pain.

15703. Assuming that reasonable and proper care is used with the anæsthetics?—Certainly, the same moral principle that one applies in this case as, of course, in the case of the education of students. If an animal is anæsthetised, and is kept anæsthetised until it dies, it does not matter morally what you do to it. I repudiate entirely the comments passed upon these researches, and I repudiate entirely the personal allegations of Mr. Coleridge against Dr. Crile's character. At Question 10285 he says: "I want to show that the personal taste of Dr. Crile does not prevent him from torturing animals." Now, in 1899 Dr. Crile proved in the New York press that these statements of Mr. Coleridge regarding him were untrue.

15704. Which statements?—That he had made unnecessary and cruel painful experiments upon animals without anæsthetics, and that the words "incomplete anæsthesia" meant that the animal was conscious of pain.

15705. (*Sir Mackenzie Chalmers.*) If you are dealing with that, I think we ought to call your attention to Mr. Coleridge's evidence at page 182, beginning at Question 10938?—Yes, and 10939: "Would you accept his own explanation?—Certainly not; of course not." I am quite aware that he does not accept it.

15706. May I also call your attention to Question 10947 and Question 10949?—Yes, quite so; I have read it. Of course, Mr. Coleridge accepts nothing.

15707. (*Mr. Ram.*) And Question 10951?—Yes, quite so; he accepts nothing. But my point is that he repeats these charges again and again after they have been publicly proved to be incorrect. I want to point out that his personal allegation against Dr. Crile is wholly unjustifiable in view of Dr. Crile's refutation of the accusation in 1899; that is eight years ago.

15708. (*Chairman.*) To come back to what you did not quite finish, Mr. Coleridge says at Question 10285: "In Experiment CXXXIII. we find these words . . . . 'experiment lasted 2½ hours. Flame was applied to the dog's paw, and we are told in the control experiments, as well as in this, the dog was not under full anæsthesia. In the former the animal struggled on application of the flame'"?—Quite so.

15709. You were going to say something as to whether there was incomplete anæsthesia?—I come to that a few minutes later.

15710. Perhaps, as I have read that statement, you would not mind answering it now?—The statement there that the dog was not under full anæsthesia means that some reflexes were present, and that is certainly probable—the withdrawal of the leg on application of the flame; but that dog was perfectly unconscious to pain.

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15711. Were you present at the operation?—No, but Dr. Crile says so in his introduction to his paper. He says that all these experiments were done on animals anæsthetised.

15712. He says: "We are told in the control experiments, as well as in this, the dog was not under full anæsthesia." Do you know by whom he is told?—No, that is Mr. Coleridge.

15713. I gather that he is told in this very account. Is that the account given in Experiment CXXXIII.?—I will look it up.

15714. (*Mr. Ram.*) In the following answer to Question 10288, you will see: "The animal did not take the anæsthetic well, and the entire experiment was made under complete anæsthesia"?—Here is Experiment CXXXIII. What happened was that the flame was applied before and after. It begins: "Fox-terrier, weight 15 kilos," and so forth, "Chloroform and ether anæsthesia." It was under chloroform and ether. Then "applied Bunsen's flame to the paw" before and after he had injected the sheaves of certain nerves in the leg with cocaine.

15715. (*Sir Mackenzie Chalmers.*) Keeping up the general anæsthesia?—Keeping up the general anæsthesia. And then he says, "in the control experiments as well as in this, the dog was not under full anæsthesia. In the former the animal struggled on application of the flame; after the injection of cocaine he did not," showing that the cocaine paralysed the nerves. That was the point of that experiment.

15716. (*Chairman.*) The nerves which produced what you say were reflex movements?—Yes.

15717. (*Dr. Wilson.*) But "struggled" would imply more than reflex movements?—Oh, no.

15718. Persistent struggling? — It depends upon what you mean. Do you mean simple struggling, or a purposive movement?

15719. Purposive movement—struggle?—Purposive movement and struggle are not the same at all. People struggle violently, and they require several people to hold them down sometimes in an operation, but that is quite a different thing from purposive movement.

15720. That is from the effect of the anæsthesia?—No—during an operation. But to return to my point, my point is that although these experiments have been treated in this way as something particularly outside the ordinary, they are not so—that they are what they were deliberately intended to be in the original purpose of the investigation, a repetition of procedures performed upon the human being which are capable of producing shock; and, if so we ought to know how that shock was produced.

15721. (*Chairman.*) There is another passage which I think it would be well if you would explain in answer to Question 10289 about ten lines down; there is a description given by Dr. Crile himself, I presume, of an operation in your laboratory?—Yes.

15722. Then he says: "Under incomplete anæsthesia crushing the foot caused a very sharp rise, followed by an equally sharp decline of pressure. This was repeated several times." What does "incomplete anæsthesia" mean there?—What it means here is that the anæsthesia was not so deep as to exclude a reflex effect upon the blood pressure. The sharp rise and sharp fall are the rise and fall of blood pressures which are produced when you stimulate the central end of any sensory nerve.

15723. I call your attention to those statements because certainly, to an uneducated mind, as mine is in these matters, reading that a particular operation was performed under incomplete anæsthesia, and that an action likely to cause pain was followed by a sharp rise, would imply that there was not sufficiently complete anæsthesia to prevent pain—that pain was felt?—That is the whole point in regard to the meaning of the word "incomplete."

15724. It is important to explain that as the evidence will be read by many persons, I dare say, who will not understand any more than I do the technical language of surgery?—Quite so. I want to devote now a certain portion of my evidence to that subject alone, namely, incomplete anæsthesia, because here again the amount of ignorance that prevails on this subject is very great. By the term "incomplete anæsthesia" we do not mean that the object of the operation or experiment, as the case may be, is conscious of pain at all. All that we mean is that its nerve centres are not so poisoned that it is not capable of making various demonstrations, even reflex acts. Another expression for the same thing that is very frequently used is "light anæsthesia," or very light anæsthesia.

15725. Are those common and well-understood terms among surgeons?—Very.

15726. Would any surgeon reading this account understand it in the sense that you are giving to us?—Certainly. A surgeon, Mr. Berry, was reading a paper last night on the removal of goitres, and he insisted that anæsthesia should be very light, and that it should be such, that the patient could be roused even to vomit. The real thing is that hitherto owing again I suggest to want of knowledge, the anæsthesia in most operation cases has been far too deep—of that we are quite certain in ordinary surgery; and the introduction of the method of giving chloroform by known doses, to which I referred before, has brought us now to find that during an operation you can very often give no anæsthetic at all for eight or ten minutes, and, in the case of a child, for twenty minutes. The person will remain sound asleep and absolutely indifferent to pain. Then comes the question, when are you to give him any more? In answer just now to a Commissioner, I said that my guide is what I call a purposive movement. If I see a movement which is not a simple reflex withdrawal, or that sort of thing, if I see a movement which seems to have a more purposive character, I always at once order the anæsthetic to be increased. It is perfectly easy to recognise.

15727. Do you wait for a purposive movement, or do you anticipate it in any way?—Frequently we wait for it. If there is any evidence of shock, if there is any reasonable evidence of shock, we wait for it. People admitted into hospitals with a leg completely smashed do not usually suffer pain. A man may have his leg completely crushed by machinery up to the middle of his thigh, and as a rule he is not in pain; he is simply lying there absolutely shocked, perfectly conscious—he will talk to you all about it, but not exhibiting any sign of pain. And, further, there are many factors during anæsthetisation which have yet to be worked out, and this "incomplete anæsthesia" is a degree of anæsthesia under which it is only safe to perform some operations, on the tongue, the throat, and so forth. As a great deal has been made on this point of "incomplete anæsthesia," and an immense amount of evidence has been laid before the Commission, I should like to take the subject up again.

15728. (*Chairman.*) You were going to refer to some instances of error or misstatement about incomplete anæsthesia?—Yes. This is really a much wider question than a mere mistake as to the degree of unconsciousness to pain, because, as a matter of fact, the anti-vivisection party have preserved their existence wholly, I think, on the assumption that cutting operations are performed in England without the employment of anæsthetics, or with a very inadequate employment of anæsthetics. I would like to point out that this subject dates from 1899, when the first attack was made upon Dr. Crile—that in 1899 Mr. Paulton, in the House of Commons, asked a question of Sir Matthew White Ridley, which conveyed the assumption that experiments of this kind were done. In 1899 Mr. Coleridge attacked Dr. Crile at the Home Office, and after he had been fully informed by the Home Office as to the meaning of this term "incomplete anæsthesia," he then wrote to several surgeons, and asked them a question about incomplete anæsthesia, and he informed this Commission that he wrote exactly in the terms in which the Home Office had written to him. As a matter of fact, he did not write exactly in the terms of the letter of the Home Office, because he omitted the all important sentence which defined this term "incomplete anæsthesia." And the result was that he deceived these surgeons into thinking that it was suggested that operations were done upon human beings in hospitals without adequate narcosis. Subsequently they found out (but not until 1902) the manner in which they had been deceived, and the way in which it all came out was so striking that I venture to lay it before you. In 1902 Mr. Coleridge issued to the public a letter, addressed as follows. It was addressed to ladies: "Dear Madam,—This catalogue, which I respectfully ask you to look at, has been translated

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from the German. In reproducing it thus, I have followed the immortal injunction:—

> . . . 'Nothing extenuate,
> Nor set down aught in malice.'

I place it, therefore, in your hands, and leave it to exercise its influence upon your heart. If it lead you to feel that anything is better than that such things should be, if it lead you to know, beyond the reach of gainsay, that pitifulness is a higher thing in the sight of God than knowledge thus obtained, you will send me your help, great or little, according to your means, that I may do what I can as effectively as you make me able, to put an end to these unspeakable deeds, and I shall continue to be your and the poor animals Ever faithful servant,—Stephen Coleridge." That letter is fixed into a catalogue, which is a translation of the German catalogue of an ordinary surgical instrument maker, who makes physiological apparatus for holding animals. In that letter nothing is said about these animals being fixed in this way on tables and boards under anæsthetics. There is not a word about the use of anæsthetics. I have had that catalogue sent to me over and over again by ladies, asking what this means. And it is all part of this constructive misstatement, that experiments are done in England without anæsthetics. This particular instance, I think, is the most immoral really, of any act of the anti-vivisection party, because the "Times," in 1902, published a serious article upon it, drawing attention to it, and subsequently a correspondence arose in which I took part, and in which we appealed to Lord Llangattock, the Duke of Portland, and several bishops who had supported the issue of this misleading document to withdraw it. They have not done so. And it does seem really a terrible condition of things, that persons holding such high authority should lend themselves to the issuing of a statement of that sort.

15729. A statement suggesting that all this is done in England and without anæsthetics?—Yes. Then, further, this misrepresentation is still going on; some 50,000 copies of this apparently have been circulated. It was corrected, as I said before, in 1902, and Mr. Coleridge stated to this Commission, at Question 10559, that where he was shown anything which was wrong he liked to correct it. I may say in parenthesis that he has never corrected anything which he has been shown repeatedly was incorrect, nor has he ever apologised; but that is only in passing. I simply mention this to show that since 1899 there has been this systematic campaign. From Mr. Coleridge's evidence we now know that he bases it all on his reading of Certificate B. His reading of Certificate B is that Certificate B enables workers in this country to do experiments without anæsthetics or to do them incompletely—that is to say, that he was pleased to speak of an initial operation and then of the rest of the experiment. Neither the Home Office nor the scientific workers in this country have recognised such a distinction as regards any cutting operation. And as regards the actual term narcosis, anæsthesia, and so forth, it has always been interpreted by the workers to mean unconsciousness of pain, therefore, whatever phenomena are observed (struggling, the corneal reflex, change in pupils, change in vaso-motor pressure), they are all phenomena which obtain with the animal unconscious of pain. As I said before, this is an important subject, because it has led to the production of this wholesale misrepresentation—to that misrepresentation being still supported by certain persons who would not wittingly, of course, support an immoral Act, who as leaders of the anti-vivisection party (take Bishop Mitchinson, for instance, and the Duke of Portland), were appealed to in 1902, in the public press, to withdraw their support from Mr. Coleridge's letter and catalogue; but they have, nevertheless, continued to give their support. It is not surprising, therefore, that this matter has come before this Royal Commission, and I shall be very glad, in cross-examination, to answer any points in relation to it. I have done my duty, I think, in laying before you a succinct statement of the way in which this particular point has been pressed. And so we come to the last part of my *précis*, the existing Statute of 1876, its administration by the Home Office, and suggestions for its amendment. The Statute itself, of course, naturally, as it is 30 years old, is no longer quite up to date; and it was originally constructed by some one, of course, who was evidently not a scientific physiologist or pathologist, because it might have been very much simplified. In the first place, I take it that the
principles of licensing and registration will both b
preserved; there is no objection to them, and the
certainly must be a satisfaction to the public, ther
fore they will be preserved. The only question com
to be a consideration of how they can best be regulate
In the first place, as regards special conditions in t
licence, assuming that a man has been given a licenc
I notice that Mr. Coleridge made several so-calle
charges against the Home Office. It is not my pu
pose, of course, to go through those; it seemed to m
that they were all baseless; but there was one part
cular charge, in which he mentioned my name fr
quently, and therefore we may as well turn to that.

15730. (*Sir Mackenzie Chalmers.*) I was going to as
you upon the tenth charge, which is the specific one
—Yes, I will now enter upon it, Question 10645.

15731. It begins earlier?—Yes, he begins it in h
opening statement.

15732. He begins in the last words of Question 1026
on page 145, in the first column; he there formulate
the charge: "I am here to charge the Home Offi
officials"?—These are his words: "(10) I am he
to charge the Home Office officials with placing a ce
tain vivisector year after year beyond the reach of t
safeguards erected by the Act' (that is the importa
sentence) "to protect animals from illegal treatme
by licensees, by giving him permission to vivisect
private places, thereby placing him beyond the po
sibility of legal inspection; because by clause 10
the Act of 1876 the Inspector has no right of entry int
or inspection of, any unregistered place." We wi
leave that, because that will come up again afterward
Then the next question is: "What is the name of th
particular person? (A) Victor Horsley." Then, agai
here is further detail of it at Question 10645, on pa
170, in the second column.

15733. And then the next day it comes on again?
Yes, at 11089, page 190. I should like to read t
words in the first part of his answer to Question 11089
"We say that there is no reason in the world why S
Victor Horsley should be placed by the Home Offi
outside the possibility of inspection." That is m
point. I say that that charge against the Home Offi
is wholly and utterly baseless. There is no foundati
for it in real fact. The facts are these: On the 10
November, 1893, I received a letter from the Ho
Office in answer to an application of mine for lea
to do two experiments away from a laboratory, in
private house. These experiments were the inocul
tion of blood from a patient suffering from filar
sanguinis hominis. This is a particular worm whi
discharges its embryos into the blood of a patient
night time. The embryos do not come into the blo
until about 10 o'clock at night, and they flourish
the blood when the patient is lying half unconscio
or unconscious in sleep, and when he wakes in t
morning and begins to move about, these embry
disappear again, hence the only time you can g
the living embryos is in the night, so that it is nec
sary to do this inoculation in the night time. All t
laboratories are closed at night, and Sir Patrick Ma
son, for whom I was going to do the inoculatio
brought the patient to a private house for me to do
As a matter of fact, we examined the man's blo
and we found that he had no embryos in his blood,
hardly any, the reason being that he had injured t
lymph glands in which the parent worm was livin
which had caused inflammation, and had caused t
death of the parent worm, and there was an end
the experiment and there was an end of t
whole matter, because I have never done a
experiment in any unregistered place at any time
my life before or since. As regards the action of t
Home Office, their action has always been perfect
clear. They have never given me any such leave
has been alleged to you. May I read their first lette
"I am directed by the Secretary of State to ackno
ledge the receipt of your letter of the 9th inst. respe
ing permission to inoculate two monkeys at your p
vate house, if possible, in pursuance of the licence he
by you for the performance of experiments under t
Act 39 and 40 Vict., c 77, and, in reply, I am
acquaint you that the Secretary of State will, und
the circumstances, consider the experiments propos
as experiments to be performed under your licen
which will be treated as current." "I am to add th
a special condition will be appended to the licence, e
empting these particular experiments from the cond
tion restricting the performance of experiments to t
specified places." In other words, the Home Offi
knew exactly what I proposed to do to inocula

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nonkeys with this infected blood and nothing more, and they gave me leave to do that and no more, so hat it was an absolutely restricted state of things.

15734. Was a certificate given with it?—No, I only btained that letter.

15735. But there was a condition imposed, was there ot, that you should report?—I am now coming to xplain that. That was on the 10th of November, and n the 21st of November, my licence being due for enewal, I sent it in, and when the Home Secretary enewed the licence he said, "The following condition as been added to your licence. 'And whereas the aid licensee was authorised on the 10th day of Novem- er, 1895, to perform certain experiments at 25, Caven- lish Square, in the County of London, I hereby vary he first condition of the said licence, so that the aid licensee may perform experiments at such place or laces as may be necessary, provided he report to the nspector on each occasion that he performs any xperiment at other than the registered places as uthorised by his licence.'" The reason why that per- nission was given at all was that Sir Patrick Manson nd I had not time between the 10th and the 21st of November to do the experiment. I mentioned that to he Home Office, and they gave me this condition, but ou will observe that when I proposed to do an experi- nent at all I was to report it to the Inspector, and my riginal permission was only to do two experiments, nd finally, no experiments have been done under hese permissions at all.

15736. (*Chairman.*) The operation which you inten- led to do, if the patient had been suitable, would have een taking some blood from the patient?—Yes, and ust injecting it with a hypodermic syringe into a nonkey.

15737. And you would draw it out with the patient's onsent?—Yes, and then just inject it into the monkey ike a vaccination.

15738. That was the whole operation?—That was he whole thing, but what I wanted to do here was to roduce documentary evidence that the Home Office ad not given me, to use Mr. Coleridge's words, un- estricted leave to do experiments when I liked, here I liked, and what I liked. They never id anything of the kind, because, in the first lace, the nature of the experiment was strictly efined, and, in the second place, I was to give notice o the Inspector if I did anything at all.

15739. (*Mr. Ram.*) Before you leave that, on page 70, Mr. Stephen Coleridge refers to this condition, nd quotes a letter that he wrote to the Home Secre- ary in the middle of the second column on that page: I wrote to the Home Secretary on the 25th of July, 905, and I said: 'On page 27 there is a note' (I was hen alluding to the yearly report), 'which runs thus: ir Victor Horsley could also perform experiments t such places as might be necessary for the purpose of is experiments.'" Do you know whether on that age 27 which is there quoted there was the limitation hat you have just told us of that you were to report? -No, there was not. Here is page 27, and there was o such statement of limitation (*handing in the same*).

15740. (*Sir Mackenzie Chalmers.*) That is the return? -Yes, that is a copy of the Inspector's return.

15741. (*Mr. Ram.*) Again on the same page, at Ques- ion 10654, it is suggested that your licence was with- rawn, or your leave was withdrawn in an illuminating ay, as he puts it, just before this Royal Commission as appointed. Can you tell us when it was with- rawn?—No, I have not kept it. The matter was ertainly referred to when the licence was renewed at he beginning of 1906, when the Home Secretary said hat he did not propose to continue the condition. hat is all so far as I remember. I have not kept that etter, but the leave was not withdrawn in any special ay at all. With every letter that we receive the Iome Secretary puts down such-and-such conditions, nd if they are conditions which are no longer neces- ary or are not operative, he just withdraws them hen the licence is sent in for renewal.

15742. But at this time, as I understand, there was o object in your having leave to perform experiments n any private house?—Not in the least.

15743. (*Chairman.*) Mr. Coleridge describes the withdrawal in this way: "It is a very remarkable hing that, contemporaneously with the knowledge in he Home Office that there would be a Royal Com- nission, this leave was withdrawn from Sir Victor Horsley to vivisect where he liked and beyond the reach of inspection"?—That statement, you see, is absolutely untrue. I never had leave to vivisect where I liked, or beyond the reach of inspection. The Home Office never gave me that leave, and that statement is wholly untrue.

15744. The leave that they gave you was for two operations only?—Yes.

15745. And you never used it?—It was for two inoculation experiments only, and I never used it.

15746. (*Mr. Ram.*) In respect of which you were to report?—In respect of which I was to notify the In- spector before I did it.

15747. (*Mr. Tomkinson.*) You never did those two experiments?—No.

15748. They were only inoculations?—Yes.

15749. They were not vivisections at all?—No.

15750. (*Chairman.*) Did the Home Office write to you and say that that condition was no longer in force? —The Secretary of State did not continue it.

15751. How long after it had been given you?—A long time—about seven years.

15752. (*Sir Mackenzie Chalmers.*) Did you come across any other cases of *filaria sanguinis hominis* after- wards?—No, we did not.

15753. I suppose the probability is that the condi- tion was kept open because you might have come across another case?—Yes, quite so; I never took any more notice of it. It was only obtained for a special occa- sion, and I never bothered about it any more.

15754. I suppose these cases are very rare in Eng- land?—Extremely rare.

15755. You may wait some years before you get another suitable case?—Yes.

15756. Is that the disease which produces ele- phantiasis?—Yes, it is. That brings me at once to the certificate system, because, on looking through the so-called charges against the Home Office, and casting my memory back now over the Act—because, as I said before, it really extends over all my professional know- ledge—I can trace any difficulties with the administra- tion, and I can trace special cases in which the anti- vivisection party have made capital, in a large mea- sure to the certificates, and if you please I would like to mention points on each certificate in exemplifica- tion of my meaning, and then I would like to conclude by pointing out why the certificates are not necessary at all, and why they only complicate legislation on the subject.

15757. (*Chairman.*) Will you please do so?—In the first place, Certificate A, the inoculation certificate, says that the experiment is going to be done without anæsthetics, and yet it is an inoculation experiment. When I was Secretary to the Government Committee on Rabies it was my business to inoculate rabbits through a little trephining operation. I gave them an anæsthetic in order to make that trephining operation to do the inoculation, but Mr. Erichsen, Sir John Erichsen, as he was afterwards, told me that was vio- lating the Act, because I was giving these animals an anæsthetic, and I reported them under Certificate A, which said that the experiment was done without anæsthetics; and he said that my conduct under Cer- tificate A ought to be that I should do the trephining operation on the rabbit without anæsthetics. I pointed out to him that it might be a breach of the law, but that it was an absolutely absurd position. I suggest that this is a difficulty that crops up from time to time. I know of other experimenters who have performed rather considerable inoculations, who have always given the animal an anæsthetic, but, owing to the hide-bound wording (unnecessarily hide-bound, of course) of the Act of Parliament, it happens that that action of the experimenter conflicts with the Act.

15758. Is that in the Act or in the form of certifi- cate?—In the form of certificate. I beg your pardon.

15759. (*Sir Mackenzie Chalmers.*) The only thing would be that you would do it under Certificate B. If you had Certificate B there would be no difficulty?— But then it is not classed as an inoculation experiment.

15760. That may be scientifically wrong, but prac- tically for you to do that under the existing Act you would be recommended to take out Certificate B as well?—Yes.

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15761. You would do the operation under anæsthetics, and the animal would be allowed to recover from the anæsthetic for the purpose you wanted?—Yes. I now come to Certificate B, and as regards Certificate B, I once more would draw attention to Question 10319. Mr. Coleridge in his evidence endeavoured to persuade the Commission that the practical administration of Certificate B was (as is shown in Question 10,319, page 150, in the middle of the second column) that the operator would do the cutting operation then the fixing on the apparatus, and then he would go on making his observations, not giving the animal any more anæsthetic. I have seen a great deal of experimental work in this country, and such an idea has never, to my knowledge, entered the head of any experimenter, and I am perfectly certain it was never in the mind of the Home Secretary in issuing the licence and certificates to us. The whole idea of administering of Certificate B was that an animal which was going to have a cutting operation performed upon it should be under an anæsthetic from the beginning of the operation until the dressing was applied and the animal removed to its cage, exactly the same as with a human being; and on that idea alone has Certificate B been worked. Then I will leave Certificate B. We come to Certificate C. Certificate C is, of course, a special certificate that is required for demonstration to students. As I have pointed out, I think it is absolutely unnecessary. A person who is duly licensed and registered is perfectly fit to demonstrate to students; he does not require any guarantee in the shape of Certificate C. Then Certificate D was constructed in consequence, apparently, of the original drafting of the Act, which made this extraordinary statement that "Experiments may be performed not only directly for the advancement by new discovery of physiological knowledge, or of knowledge which will be useful for saving or prolonging life or alleviating suffering, but for the purpose of testing a particular form of discovery alleged to have been made," and so on, "on such certificate being given as in this Act mentioned that such testing is absolutely necessary for the effectual advancement of such knowledge." That presupposes two things: In the first place, that you can repeat a former experiment absolutely; and in the second place, that such repetition ought to have the special sanction of the Home Secretary. Now, that is wholly contrary to science. No man has ever repeated an experiment without learning something additionally new. You cannot perform an experiment absolutely identically like the previous one. You perform it on a different animal; you invariably learn something new. However closely you attempt to approximate the factors of the experiments in the two cases, you do not get an absolutely identical result, and in that way a great many extremely useful discoveries have arisen. Then as to the necessity of having special permission to extend work, it seems to me that that is contrary to the whole spirit of the Act, which, after all, was to promote the further discovery of physiological knowledge. So that this certificate for so-called repetition experiments is unnecessary, and is contrary really to the progress of scientific work.

15762. (*Chairman.*) Would you suggest some amendment to this, or strike it out altogether?—I would strike it out altogether—repeal it altogether. It is not only not necessary, but it is also contrary to the fact. It was evidently inserted in the Act by someone who had no knowledge of physiological experimentation. That is what I mean. I suppose Lord Playfair was responsible in a great measure for the drafting of this Act, and he was a chemist—he was not a physiologist; but whoever was responsible evidently in many respects was not a physiologist—he did not know the actual conditions surrounding experiments on animals. Then we come to Certificate E, and although I have no charge to bring against the Home Office, I did not say that I had no charge to bring against the Home Secretary. Certificate E, I take it, is never applied for now. Certificate E proposed to allow experiments on dogs and cats without anæsthetics—those are the words. Now, when Mr. Matthews was Home Secretary he insisted on Certificate E being filled up by all the experimenters who wished to use dogs and cats, and we protested. We went to him as a deputation—Sir James Paget, Sir Andrew Clarke, and myself. We pointed out to him that this placed experimenters in an entirely false position; that they did not do experiments on dogs and cats without anæsthetics, that if the Home Office wished to know when dogs and cats were used they could be notified, but that to compel us to sign a paper which was not true seemed to us most unjust; and we asked him, therefore, to withdraw Certificate E. I am sorry to say that Mr. Matthews refused, and it was not until we came under the enlightened *régime* of Mr. Asquith that this question was settled; because we made exactly the same representation to Mr. Asquith, and he immediately said, "Certainly, I recognise the injustice," and he constructed the Certificate EE to take the place of Certificate E, which was exactly the same certificate only it left out the words "without anæsthetics." I mention this to the Commission to show that as regards the plenary power of the Home Secretary under the Act—although as a rule, of course, it is worked extremely wisely—there may be examples, like that of Mr. Matthews, in which it may be worked extremely badly. I think the Commission ought to have the fact before them.

15763. (*Mr. Ram.*) Did Mr. Matthews give any reason for adhering to this Certificate E?—No; he simply wrote a letter in which he regretted that he was unable to comply with the wishes of the deputation. We never had any reason from beginning to end. It was evidently simply a matter of internal administration, because Mr. Asquith drafted Certificate EE, and we have worked under it ever since. But I point out that that occurred in 1888.

15764. (*Chairman.*) You mean Mr. Matthews's refusal?—Yes, and that the statistics consequently of the experiments on animals then were not correct. For instance, when I was called upon (and I know many other experimenters when they were called upon) to send in this Certificate E, struck out the words "without anæsthetics," and I certainly had the document returned to me twice, and sent it in again twice. I would not sign the original Certificate E, and ultimately it was allowed with the words struck out by myself.

15765. (*Sir Mackenzie Chalmers.*) So that now in all cases the certificate shows the fact—namely, that experiments on dogs and cats have to be performed under anæsthetics?—Yes; the working now is perfect. There is nothing to complain of at all. But I only mention it to show how the certificate system as a Schedule to the Act of Parliament is liable to misapplication, especially in regard to any physical science, considering that physical science is always altering and advancing. Then the last certificate, F, is another of these special certificates. It is really absurd, of course, that a special certificate should have to be got for experiments on ungulates or special ungulates. It is contrary to science.

15766. (*Chairman.*) What is contrary to science there?—For any one class of animal to be selected in contradistinction to the whole animal world. We ought to have general permission to do experiments on any kind of animal. There is no reason why any one animal should be, as it were, removed from the sphere of the Act.

15767. (*Mr. Ram.*) You may do it on a cow, but not on a horse without getting Certificate F?—Exactly. It is absurd from a scientific point of view—I mean, of course, it was not absurd to those who drew it up. So, summing it all up, I suggest to the Commission that these certificates, which give endless trouble to fill up, are really of no use. Then, of course, the question arises, what use is made of them at the present time, and how are you going to meet it. Very important use is made of them at the present time. The Home Secretary must know, both for his own information what is going to be done in research—he must know what is going on during the research, and he must know what the results of the research are, for his own information, as I said, and also for report to the House of Commons. Now we need not trouble about the middle point, what is going on during the research, because that is provided for by the system of inspection; but as regards the first point, all that is necessary is to do what is actually done at the present time, to insert on the application for the licence that the applicant proposes to do such and such experiments on such and such animals in such and such a way. It covers all that. Then when it comes to the collection of statistics for report to the House of Commons, that depends simply and solely on the form. The present form that we fill up at the end of the year could very easily be expanded in this sort of way: You could have the first column "Class of Experiments—(1) Animals killed before recovery from anæsthetic; (2) Animal allowed to recover from anæsthetic; (3) Animals inoculated." That really covers the whole ground as a matter of fact. Then the next column would be "Number of Experiments Performed"; the next "Number in which Pain was Observed to Occur." Third, "Animal Employed." In a simple form of that kind you have all the facts which are supplied by those certificates, and in a form which would make their being collected and reported upon extremely simple and easy. So that the certificates are wholly unnecessary. They have led to misunderstanding, and they give great trouble to the applicants who have to get them signed and filled in. And, further, Mr. Matthews added another difficulty; that was the restriction as to their numbers or their duration. It is perfectly ridiculous that the Home Secretary, who is very rarely a scientific man, should say whether ten experiments or twenty experiments are necessary for research, and yet that is done every time.

15768. (*Chairman.*) Of course he does not act without some advice?—Well, my lord, when Mr. Matthews put these restrictions of time and number on the certificate form I suppose he was advised to a certain extent by the Inspector, and so on; but, at the same time, the investigator himself does not know how many experiments may be necessary.

15769. I dare say that is so. I was only pointing out that the Home Secretary does not rely solely on his own scientific knowledge?—Yes, I beg your pardon. I see your point.

15770. (*Sir Mackenzie Chalmers.*) You mean that it has this disadvantage: that the person applying has to apply for an extra number of experiments, more than probably would be necessary for him to perform?—Quite so, and not only that, but one other condition that was devised (I have forgotten how long ago it was—it arose out of Mr. Matthews' regulations) was that the investigator might do, say, 20 experiments, but after ten he was to report to the Inspector, but the fact that the Inspector knew that those ten had been done made no difference to the research under the present system of record keeping that we have of experimental work; all this is very much simplified; because some years ago, when Mr. Ritchie was Home Secretary (Lord Ritchie as he was afterwards), the blue form on which the experiments were recorded was given up, really I think in answer to a protest of mine, namely, that the investigator ought not to be put to the trouble and annoyance of having to write all the records of experiments twice over; and Mr. Ritchie then made a standing regulation that the records of all the experiments were to be open for the visit of the Inspector as, of course, they are. Consequently these details, which are inserted on the certificates, and under which the certificates were allowed to be obtained are always available. Therefore that is another reason for regarding the certificates as unnecessary, a waste of time, giving great work in the office, and leading to misunderstanding.

15771. (*Chairman.*) You would have a licence and no certificates?—Yes, a licence drafted in the way I have suggested, and a form of return to be filled up by the investigator at the end of the year would cover the whole ground. Then as regards the plenary power of the Home Secretary, that would remain, of course, and he would insert any special conditions on the licence which he thought necessary.

15772. Permanent conditions, do you mean, or special conditions?—They would last for a year, because the licence always expires at the end of the current year.

15773. Suposing he wanted to give you a licence for inoculating two monkeys, how would he deal with that?—He would endorse it on your licence; he would ask you to return the licence, and endorse it on the licence.

15774. You must have some special application for it?—Yes, that is an ordinary letter.

15775. (*Sir Mackenzie Chalmers.*) Then you would eliminate the advisory bodies in that way. At the present time certificates are given by outside bodies of scientific people?—No, they would have their place in the granting of the original licence; and, of course, they would be the people to appeal to as to whether the investigator was a fit person to conduct the investigation which he proposed to do. Now their advice is sought really three times over for the same thing. If a man gets a licence and Certificates A, B, and E E, which is about the average, they really sign the same thing three times over. It is quite unnecessary.

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15776. (*Chairman.*) Does that exhaust your *précis*?—If you please.

15777. (*Dr. Wilson.*) What body would you recommend to advise the Home Secretary, then, as to the need of new experiments in the field of research, instead of the Association for the Advancement of Medicine by Research?—I really have not thought of it. I do not know of any body at this moment that I could name.

15778. Would you object to the General Medical Council, for example, as the advisory body?—I should object very strongly. I was a member of the General Medical Council myself. It is not at all a body that could do it. You cannot call them together, except at fixed times. Of course, the personnel, as personnel, would be all right naturally; many of them are teachers and know the requirements of education.

15779. But through a Committee of that body could it not be done?—Certainly. The expense question you know in the General Medical Council is always a very difficult one.

15780. Yes, of course, that would have to be met in some way. May I ask whether you are appointed by the Council, or a Committee of the Council of the Association to appear?—The Council.

15781. It may be conceded that the vast majority of the medical profession are in favour of experimentation on animals, but do you think that they take that view just because they thoroughly know the subject, or are they influenced by the experiments they have seen in the class rooms?—The vast majority of them, only from their general knowledge up to about ten years ago. Men qualified during the last ten or fifteen years have seen the experiments, of course, but there are many schools now that do not show experiments at all, and I do not think they teach well either.

15782. I see you allude to the huge total which has been expended by the Association during recent years in the form of grants in aid of research?—Yes, and scholarships.

15783. Do not these grants tempt some of our best students to go in for research, and some of the ablest young men who are just starting in practice?—They enable many who could not otherwise afford it to do so, the scholarships especially.

15784. Then there is the Ernest Hart Fellowship?—Yes, the Ernest Hart Scholarship for Public Health, three scholarships of £150 a year for general science, and then about £400 is expended in grants.

15785. With reference to these grants—I know that all the results of the investigations in the fields of research have appeared in the "British Medical Journal," but has any compendium ever been made to enable one to judge of the advantages that have been gained?—Not that I know of. I remember years ago some considerable number of researches were collected in a little volume, but that has not been done for many years.

15786. Is it not the fact that many of its students who obtain these scholarships go to Continental schools to complete their studies?—They often do.

15787. And there they become imbued, of course, with the doctrines there taught?—Yes.

15788. And is it not also the fact that the majority of the teachers in medical schools are selected from those young men who go to Continental schools?—Certainly.

15789. And should I be right in stating too that almost all the leading articles on questions of research that appear in medical journals, or even in the daily papers, are inspired by these young enthusiasts—because the older men can rarely speak or write on the subject?—I do not know what articles you refer to.

15790. I mean articles on research that you see constantly in medical papers and in the daily papers too—medical research?—Yes, I daresay. I do not know what articles you are referring to at the present moment.

15791. I want to get at this—how medical opinion with regard to these questions is moulded really?—Oh, I see. I cannot tell you. I should have said that the medical opinion was simply the expression of the

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general scientific knowledge of the profession at the time.

15792. I will put it in this way: Though the consensus of medical opinion may be in favour of continuing experimentation on animals, do not you think that the consensus of medical opinion is always a somewhat unreliable court of appeal?—Do you mean, is it possible that the medical profession are mistaken on this point?

15793. Some may be?—That, of course, is very difficult to say. I do not personally think that they can be mistaken on the point, because one sees the present position of scientific research is nothing but the logical outcome of all knowledge that has been going on for centuries. It has not begun recently; it has been going on since the time of Galen; it is all one continuous evolution. I cannot believe that the medical profession are mistaken on a matter like that, at least it does not seem to me quite reasonable.

15794. Talking about this consensus of medical opinion, do you think that if your proposal to operate on dogs to teach students practical surgery, as they do at the John Hopkins Hospital, Baltimore, were submitted to a plebicite of the profession you would obtain a large majority in favour of it?—I do not know. I think probably I should obtain a majority, certainly. And I have not limited the number of animals to be operated on to dogs at all; on the contrary, I have always held the view that the sheep was an animal that particularly lent itself for students' work. The arteries in sheep are the same size as our arteries, the circulation in many respects is the same. The difficulties of operating upon it are the same. The sheep is really a very good animal for research; it is only a question of cost.

15795. Should I be right in saying that beyond setting an occasional fracture or dealing with a rare accident the general practitioner nowadays is seldom called upon to display any surgical skill at all?—Pardon me, the discovery of the antiseptic system has completely revolutionised medical and surgical practice. In old days surgical practice used to be reserved to so-called operating surgeons. General practitioners nowadays do all operations—many of them do very large operations—such as extirpation of the kidney. That has all altered even within my time. General practitioners operate very largely in cottage hospitals all over the country.

15796. Yes, if they are connected with hospitals?—Naturally, if they have hospital practice they continue it in their general practice.

15797. I mean in general practice?—I mean in general practice.

15798. You contend, do you not, that the recognition of tissues, their texture and treatment in practical surgery can only be taught on the living body?—Yes, I say it can only be adequately taught in that way.

15799. And that for the purpose experiments on living animals are essential?—Yes.

15800. You have been talking about Listerism. Surely the revolt against the use of the carbolic acid spray which constituted the leading feature of antiseptic surgery was not brought about by experiments on animals. Because there was a revolt against the use of the carbolic spray?—No, there was no revolt against the use of the carbolic spray at all. The question was whether it was necessary to have this disinfection of the air. It was shown by experiments that air was not toxic to the degree it was believed to be: that organisms present in the air to a large extent were moulds and fungi, which were not particularly poisonous to animals; that the organisms which were dangerous to animals and to man were those which were carried in on instruments, and for that reason people began to risk leaving off the spray, and gradually it was left off altogether. But the spray was not the leading feature of the antiseptic surgery at all.

15801. It was one of the leading features, was it not?—No, it has often been described as part of the ritual, but that is by ritualists. Surgeons are not ritualists—at least, they ought not to be.

15802. Now, as to the value of experiments on animals in teaching or acquiring technique, is it not the fact that some of our leading surgeons, such as the President of the Royal College of Surgeons, who gave evidence before the Commission, and the President of the Royal College of Surgeons, Ireland, have never themselves operated on animals?—Yes, I believe it is. They gained their experience by learning on man.

15803. Then I am going to quote Sir Frederick Treves. Has he not stated that the experiments on animals that he himself made to improve his technique in abdominal surgery rather led him astray?—No; that statement of Sir Frederick Treves has often been quoted and misquoted, and his final correction of it in the "Times" was to the effect that in one particular case, one particular form of experiment, he learnt nothing from it.

15804. (*Sir William Collins.*) Can you refer to the date in the "Times"?—I cannot.

15805. You can put it in?—Yes, I will get it. He has often been twitted with it by the Anti-vivisectionist party.

15806. (*Dr. Wilson.*) Now as to experiments for teaching students physiology, do you agree with the views expressed by Dr. Waller in his address at the meeting of the British Association, not the British Medical Association?—I was there at Leicester.

15807. That "the province of vivisection, essential as it was, was a very narrow and restricted province indeed, in the domain of physiology," and that vivisection was, in fact, an infinitesimal fraction of practical physiology?—That passage from Dr. Waller's address, taken by itself, may be easily misunderstood. What Dr. Waller meant was that in the work of the physiologist in his laboratory and in his class rooms, the actual amount of vivisection he did was extremely small. And so it is; the total net work done by a physiologist in his ordinary routine rarely includes an experiment on an animal of a nature which might be termed vivisection, compared with the mass of his other work. But it all depends upon the investigator. If you are making a research, for instance, in the nervous system in the way I have just indicated, tracing out the track by the Marchi method, you would make, perhaps, ten vivisections; it would take you ten months to work up that material microscopically.

15808. You maintain, I suppose, that experiments on dogs are necessary for teaching?—Absolutely essential. Not necessarily for teaching alone.

15809. I mean for the physiological class-room, of course?—Do you mean for original research or for teaching? They certainly are necessary for demonstrating certain things.

15810. For teaching physiology?—For the students to use them as material, certainly for some experiments they must use dogs; for others, cats and rabbits will do; for others, frogs and so on; it depends upon the experiment.

15811. Because Professor Gotch stated here that for two years he had not used living dogs for teaching purposes, and Dr. Pembrey admitted before the Commission that during all this year, at least, he had not vivisected any animals at all before his class. From that you would rather criticise the efficiency of the teaching?—Certainly I would.

15812. You yourself have, I know, carried out a large number of experiments on animals in connection, first, with Pasteurism, and also as you say, with respect to brain surgery and the physiology of the thyroid gland?—Yes.

15813. Of course, after what you have said, I need hardly ask you whether you have ever had any reason to doubt the efficacy of the Pasteurian treatment for hydrophobia?—Oh, no, not the slightest—not since the Report of the English Committee.

15814. When they have to propagate the virus at the Pasteur Institute; do they propagate it from rabbit to rabbit?—Yes, from rabbit to rabbit.

15815. Do they use dogs at all now?—I do not know whether they occasionally do. But that is not the animal used for propagation of the virus; the animal used is the rabbit.

15816. From rabbit to rabbit?—From rabbit to rabbit entirely.

15817. So that there does not seem to be any need of introducing a new virus, as it were?—No.

15818. But the symptoms of rabies as seen in the rabbit bear no resemblance to rabies in the dog?—It is a very interesting thing—it all depends upon the rabbit. You may take twenty rabbits, as you know if you have kept rabbits, and one or two will be quite playful and even bite, and when you inoculate those rabbits, a rabbit which is naturally excitable will bite as viciously as a rabid dog. And so will a guinea-pig. I have had a guinea-pig inoculated with rabies fly at me exactly the same as a rabid dog will. The majority of rodents simply become paralysed in the hind legs, they have the paralysed form of rabies and not the excitable form. But it depends upon the animal.

15819. Then would you go so far as to say that the Pasturian treatment of hydrophobia has been so firmly established on experimental and logical lines that it can never be successfully discredited; is that your strong feeling?—Certainly.

15820. That was my opinion at one time, too, but I have rather doubted it since?—I have not the slightest doubt. I devoted a very long time to working out the accurate statistics at the time of the Government Commission, and considering that the Pasteur system has reduced the death rate from 15 to ·3 per cent it is such a margin of difference that I never can have doubt.

15821. Statistics can be made to prove or disprove almost anything. Of course, you know a great many patients, over 1,200 cases have been enumerated, have died either during or immediately after treatment—I mean within the time stated by Pasteur?—Who collected those statistics? It was Dr. Luteaud. That evidence was certainly not received by anyone who knew the facts. But there is the evidence which the English Committee collected for themselves, you know. They went over to France, and I myself was over there for two months hunting up the cases which had been treated, and hunting up the authenticity of the rabidity of the dogs, and there is no doubt that the death-rate then that Pasteur obtained was about 1.5, if I recollect right. Since then it has been brought down to 0·3, which is a very usual death-rate of a preventive inoculation.

15822. And you believe that there is very little pain inflicted on the rabbit by it?—Yes. I have seen rabbits completely paralysed in their hind legs, and beginning to be paralysed in their fore legs, eating and looking about absolutely unmoved. It is a kind of painless paraplegia in the rabbit evidently. Human beings only suffer, of course, when they get the spasm and the apprehension of dying from an inevitably fatal disease, you know; that is the suffering of hydrophobia.

15823. A few questions with regard to the functions of the thyroid gland and the diseased conditions connected with it. Of course, you yourself have performed many operations on monkeys and dogs?—Yes.

15824. Am I right in stating that an exceedingly large number of experiments have also been performed by others engaged in this research?—Yes.

51825. Am I also right in assuming that though the initial operation, the removal of the gland or part of the gland, may be carried out painlessly, the conditions induced may be often accompanied with very considerable pain?—Oh, dear, no.

15826. When the animal wastes?—No.

15827. There is no pain at all?—No; but the animals run through the same category of symptoms as the myxœdema and cretinism patients; they become more and more stupid and idiotic; they are not sensitive to their surroundings at all.

15828. Can it be said that after all these experiments on the thyroid and parathyroid glands the functions of these glands are perfectly understood now? —Of course not; they will not be understood in our lifetime. It is impossible.

15829. Has it not been pretty fully established by the experiments of Professor Swale-Vincent and Jolly that the functions of these glands appear to differ very widely in different classes of animals?—I showed that myself a good many years ago. When you say that they differ very widely there must be no misunderstanding on this point. They differ in degree but not in kind. It is only a question of degree.

15830. And myxœdema is believed to be due to a diseased condition of these glands, is it not?—Yes, certainly.

15831. Did not Professors Vincent and Jolly find that none of the monkeys from which they removed the glands showed the slightest symptoms of myxœdema?—In Vincent and Jolly's experiments particularly, many of their animals did not show anything at all.

15832. No signs of myxœdema?—No, I am not sure, when I say no signs that they analysed the connective tissues. I do not think they did; but, at any rate, the animals did not present the very obvious signs. *Sir V. Horsley, F.R.S., F.R.C.S.*

15833. May I ask whether that has been your experience, too?—Yes. I have had monkeys that did not show anything at all. 13 Nov. 1907.

15834. Has there not all through these experiments been a great discrepancy in the results obtained by different observers?—No.

15835. And therefore in the inferences to be drawn from them?—No. There have been discrepancies as in every new subject. Every pioneering investigation is sure to arouse other investigations, and new facts come to light which were not explained by the conclusions of the first investigation, and consequently so-called discrepancies have arisen. That is the ordinary concomitant of the results of scientific work, and it only requires our waiting 20, 30, 40, or 50 years to get the real truth.

15836. Then the only outcome of it all, so far as any remedy is concerned, has been the discovery of the value of the thyroid gland or extract in the treatment of myxœdema and cretinism?—Yes, if you apply the term "only" to it. I should have thought it was a good deal for the sake of humanity to get rid of the symptoms of myxœdema and cretinism, if you have ever seen them.

15837. But are cases of myxœdema and cretinism ever completely cured?—Oh, certainly.

15838. Completely cured?—Certainly, and that is not the end of it. For instance, to cure cancer of the thyroid gland by an operation it is absolutely essential to take out the whole gland. If you take out the whole gland you would make the patient myxœdematous, but if you give them the thyroid they do not become myxœdematous. In other words, the thyroidal treatment under those circumstances has enabled you to remove the cancer, which was a fatal disease.

15939. Then, gain, with respect to demonstrations that sort?—Yes, I have grafted in a case of that sort successfully.

15840. After cancer?—Yes.

15841. After the removal of the cancer?—No, after the total removal of the gland for parenchymatus goitre. I have dealt with cancer cases in the way I suggest by giving thyroid, I have not grafted in cancer cases.

15842. Then in regard to brain surgery, have these numerous experiments on animals assisted largely in that respect?—The successful operative treatment of the brain and spinal cord is wholly due to experiments on animals. As I explained, the technique is simply Listerism, but the application of the technique to a particular spot in the nervous system is entirely the result of physiological research.

15843. But the results of an operation, for example, on a tumour of the brain or abscess of the brain are not very satisfactory still?—There is a great deal to be done in the way of improving them of course.

15844. That is to say the mortality is still very high?—No, but the surgical treatment is imperfect often because the cases come too late. Diagnosis is very imperfect still, and cases are referred for operation which ought to have been operated upon a year before, and that sort of thing.

15845. Did not Professor Oppenheim, of Berlin, state at the meeting of the Medical Congress at Moscow a few years ago that "brain localisation is often most doubtful; the diagnosis of brain abscess from brain tumour is often, if not always, quite impossible?" —Yes he may have made that statement ten years ago. He has recently published with Professor Borchhardt a certain series of cerebral tumours, in which he diagnosed them successfully, and Professor Brochhardt removed them successfully by surgery. I should think Oppenheim would not repeat now what he said at Moscow ten years ago.

15846. You are clearly of opinion, of course, that experiments on animals must still be carried on with regard to assisting in this field of surgery also?—Yes, very much, and assisting also, you understand, in the study of the anatomy of the nervous system, because the Marchi method is revolutionising our method of studying the nervous system and anatomy. We are, of

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course, not only employing experiments on animals for localisation in that way.

15847. Apart from cases of injury, does not the diagnosis of any brain mischief, such as tumour or abscess, rest mainly on the physician?—Yes.

15848. And when a surgeon is called in, such as yourself, of course you yourself diagnose and inquire into the case before deciding on an operation?—Yes.

15849. And share the responsibility in that way?—Yes, most essentially.

15850. Now just a question or two with regard to those experiments of Dr. Crile, and I must confess that they have always been rather terrible reading to me?—When you use the word "terrible," I think that any mutilation of a human being or an animal is conceivably terrible in one sense, but it seems to me unreasonable to apply the term to an operation on an animal, and to deny the term to an operation on a human being. If it is terrible to one, it is terrible to the other.

15851. But you see in the one case the operation is necessary?—I say it is necessary on the animal.

15852. You say it is necessary on the animal to enable you to operate on a human being. That is the position you take up?—Yes, quite, it is necessary in both cases.

15853. Then as some of those experiments were carried out in your laboratory, of course you would be perfectly prepared to carry them out yourself, if necessary?—Certainly.

15854. And, as you have said, you believed they were all painless?—Yes, absolutely.

15855. No matter how ambiguous the language Dr. Crile used?—Quite so, no matter what his language.

15856. And you are of opinion that more experiments even of this kind are still required?—Undoubtedly.

15857. But granting, for example, that a dog is endowed with the same relative amount of vital resistance as a healthy human being, is it logical to infer that a diseased man on whom an operation has to be performed with his vitality lowered often almost to breaking point by suffering, could survive the same degree of surgical shock which is inflicted on a dog which is not diseased at all? I mean that the two subjects on which you operate are in an entirely different condition. I am supposing a diseased human being and a healthy dog?—I do not quite follow you, I am afraid. They are not in an entirely different condition at all, because, for instance, taking the diseased human being, his heart is working along at the same rate as usual. His blood pressure is, as a rule, up to the average, and so forth.

15858. There is not the same amount of vital resistance to death as in the case of a healthy animal?—No, possibly not.

15859. Would not that rather tend to what is called somewhat daring surgery sometimes, just because one sees an animal survive a terrible experiment, would you expect that a human patient may survive the same amount of shock under an operation?—Certainly. Because there is not any operation that I can think of at the present moment that has been performed experimentally on an animal, such as the removal of the kidney, or the removal of a portion of the liver, which has not been equally performed on a human being. In other words, the parallelism between such operations on animals and man is absolutely close. Even through every stage of the operation, and the effects produced, the parallelism is absolutely close.

15860. But as regards shock, you do not know that unknown quantity of vital resistance in the patient and in the dog?—But, pardon me, that does not come in, because you start with a patient with certain vital resistance—a man, for instance—and supposing that during the operation you discovered that his blood pressure was falling, you recognise that, and you treat it.

15861. Is it not the practice to hold an inquest in all cases of death in hospital from anæsthesia?—Yes, it is in England; it is not in Scotland.

15862. They do not hold inquests at all there?—No, they do not hold any public inquiry. They do in England.

15863. Is it not sometimes very difficult to determine whether a patient dying on the operating table dies from the mere shock of the operation or from the anæsthetic?—That, of course, is purely a medical point. Personally, I have never seen anybody die from shock on the operating table. I have seen people die, as I considered, from an overdose of chloroform. That is my personal opinion. If you ask me whether it would be possible to differentiate between the two, that is a very difficult question.

15864. So that, of course, you contend that continuous experimentation is still necessary?—It is bound to go on. It will go on as surely as we go on round the sun.

15865. And also, in respect to drugs and doses?—Still more. There I think we have neglected pharmacology disgracefully. There is hardly a university that has got a pharmacological laboratory. They are beginning to found them now.

15866. Just to take one drug, as an example, about which you have spoken and written very strongly—alcohol—have there not been many experiments performed upon animals, to say nothing of experiments that man has performed upon himself?—Yes, a great many.

15867. And still there is no consensus of opinion in the medical profession about the dietetic or pharmacological value of alcohol?—These are two different points, of course. First, as regard the dietetic value, I should have said that there was a very considerable consensus of opinion in the medical profession that alcohol was not a food—I mean in the proper sense of the word, a food without any adverse effects—I should have said that that was an absolutely accepted position. Then, again, as regards the position of alcohol as a drug, as I have shown, the general consensus of opinion of the medical profession now is, not to use it as a drug, but to use other drugs instead, because they have a less injurious effect, like strychnine, and so forth, and heart tonics, and one thing and another, which can be employed with better advantage to the patient than alcohol. So that the general consensus of opinion of the medical profession at the present time is rather to give up using alcohol as a drug, because they have got other and better means at their disposal.

15868. (*Mr. Tomkinson.*) I think you began by stating that all pursuit of knowledge was moral, and that any obstruction raised to the acquisition of that knowledge was immoral?—I feel that.

15869. Do you carry the former to its logical conclusion, that, irrespective of all results, knowledge must be pressed forward. To give an illustration of what I mean, supposing there were no such things as anæsthetics, do you hold that we should be justified in experimenting in surgical and other operations on the animal world for discovery?—Yes. In old days, before anæsthetics were discovered, I think the old surgeons were quite justified in making the experiments that they did, and they made a great many useful experiments to discover new operations and new facts, in which I think they were quite satisfied.

15870. You think that we are justified in exploiting the animal creation in that respect?—Yes, provided that we treat them as humanely as we treat ourselves.

15871. You mean that, because in old days it was necessary to perform amputations and other terrible operations without anæsthetics, it was, therefore, justifiable to perform the same operations on animals for the pursuit of knowledge?—Yes, in those days, quite.

15872. You said that you considered that anti-vivisectors who accepted and availed themselves of the results of these experiments and of the knowledge gained thereby were themselves *participes criminis*, and at least illogical—in fact, were guilty of complicity with the very thing which they condemn?—I think so.

15873. But I suppose you would admit that probably in the majority of cases the patient is entirely ignorant of how the treatment has been obtained?—Yes.

15874. And moreover the assumption is from your point of view that the same discovery could not have been made otherwise than by experiments on animals?—Certainly.

15875. But I suppose you are aware that it is very much contested in some quarters whether the same results could not have been arrived at, and the same discoveries made by other methods than by experiments on animals?—I know that that has several times been asserted, but the fact remains that the thing was discovered by experiments on an animal, and it is no good saying afterwards: "I could have discovered it in some other way." For instance, it was once said of the circulation by Mr. Tait that anyone with a syringe and a dead body could have discovered the circulation of the blood. A statement like that, of course, is monstrous in itself, but I rather think that is the sort of thing that you refer to. I say that is arguing backwards. We could always get up and say: "Oh, yes, I might have discovered it in some other way." The answer to that is perfectly simple: "Why did you not?"

15876. I do not know that I rightly understood you on the subject of rabies in England. You first of all said that it was an infectious disease?—Yes.

15877. Infectious only in the sense that it is communicable, as we know, by a bite or some contact?—Yes.

15878. But not otherwise than by contact and a puncture?—There is a very interesting thing about rabies. At the Brown Institute we used to have in old days when I was there, rabid dogs brought to us with the saliva dropping out of their mouths on to our pavement and the floor of the place. That saliva, of course, was virulent; but when the saliva of rabies has once dried the virulence is gone. The organism of rabies will not survive drying.

15879. It is innocuous?—It is innocuous. In other words, the only way in which the disease is kept going is by its being absolutely introduced into the living tissues of some other animal.

15880. Then how do you say that the practical extinction of rabies in England, which we now enjoy, was brought about?—By the muzzle entirely, because you can prevent, and it is the only way you can prevent, the one animal introducing the virus into the living tissues of another by preventing its biting.

15881. How does that bear on the subject of vivisection and research through experiments?—I quoted that as an example of the immorality of the anti-vivisectors. I quoted that against the anti-vivisectors because the anti-vivisectors strongly opposed the muzzle. They strongly opposed the extirpation of rabies by the muzzle. I say that was immoral.

15882. I do not know about that, but there was a very strong feeling that the muzzle was not effective, and I may tell you a very strange thing about it. In my county the muzzling order was put in force by the county authorities for a considerable time, and all dogs were muzzled, but after a time it was removed by several boroughs, and after a time our chief constable, who had to report to us every quarter so many dogs seized, suspected or condemned for rabies and destroyed for rabies, advised us (in view both of the fact that the Order was illogical, because there were dogs in the boroughs contained in the county without it, and also expressing the opinion that muzzling had nothing to do with the suppression of it) to repeal the muzzling Order and to go back to the simple Collar Order; and from that very day almost rabies has ceased in the county?—I have no doubt.

15883. And he explained it by saying that hundreds of these dogs he believed were given a bad name, were chased, driven into a great state of terror, and then were destroyed and returned as attacked by rabies, and he thought that it had more to do with hysterical and imaginary rabies than suppressing it?—Fortunately imaginary rabies does not exist, and hysterical rabies was amply met by the collar Order, but the diagnoses must have been remarkable. The Board of Agriculture sent all carcases and heads of dogs to the Brown Institution, until they had their own laboratory, and all the cases were tested experimentally. But as regards the condition of things in Cheshire, I think you forget that Colonel Moorson, the head of the Lancashire Police, was very strongly of exactly the opposite opinion to that of your head constable—that the Lancashire records proved absolutely that though all this isolating and muzzling was done in the counties just as they chose or not, the Lancashire records were very good, and the muzzle had a very marked effect. And so it was in London here.

15884. We may have been just approaching the end of it, but it was perfectly remarkable for years and years we have had a clean sheet now?—May I also point out that in 1889, when that meeting at the Mansion House took place, we arranged with Sir Henry Roscoe that he should introduce a Bill into the House for the universal collaring of dogs, which is the very thing you referred to.

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15885. That was for identification?—Yes, but that was not agreed to.

15886. You repudiate very strongly the charge, or the impression, that the witnessing of these operations on living animals brought about familiarity and, therefore, to some extent a contempt for pain on the part of students or the operator. You repudiate that idea altogether?—Absolutely, because the animals are not suffering any pain; there is no question of pain. On the contrary, it teaches the students how to prevent pain.

15887. I suppose you do not hold that in cases where, as abroad, operations are performed without anæsthetics?—All that idea of experiments abroad, if I may take up that point, dates from the barbarities performed at Alfort thirty years ago in the Veterinary School, but all that is ancient history. In the experiments that one sees abroad now the animals are anæsthetised as they are in our laboratories. Some investigators may not use anæsthetics, but as a rule they all use them abroad.

15888. You spoke of experiments in anæsthetics on animals being necessary before their administration to man?—Yes, I think that students ought to be taught to anæsthetise dogs.

15889. I do not think that even anti-vivisectors would object to that. I do not think that mere experiments in the administration of anæsthetics are looked upon as painful or cruel operations?—Some evidence has been offered to the Commission here—I see it was by Sir Thornley Stoker again—that monkeys when brought into the room exhibited fear and alarm, and so on, and that the giving of anæsthetics to a dog was a very painful proceeding, and Dr. Buxton repudiated it.

15890. I am not here to speak as to that, but that is not an operation?—No, certainly not.

15891. In most cases it is the administration of a drug?—Yes.

15892. Which is quite different from cutting operations?—Yes.

15893. Now, I must ask you a few questions about these operations of Dr. Crile's. They were performed in your laboratory in the College?—The first sixteen were.

15894. Not in your private house?—No, at the College. The rest he performed in America, at Cleveland.

15895. Sixteen healthy dogs were taken and made the subjects of what to the ordinary lay mind were terrible operations. I suppose that you would admit that to an ordinary person like myself they do convey a very terrible impression?—If there was any idea that the animal suffered at all it would be terrible, but surely the use of the word "terrible" is not justifiable in the case of an anæsthetised animal. I am only suggesting that.

15896. On the point of pain and suffering alone, quite apart from the justification of it, would you be quite happy in subjecting a favourite dog of your own to any such operation?—Most certainly. I have got a favourite dog of my own, and if an anæsthetic was given to it I would not care what was done to it—not a bit.

15897. Then you are absolutely satisfied in your mind that complete freedom from any kind of pain was enjoyed if one can use the word, by the animals under it?—Certainly, exactly as I am certain that the patients whom I operate upon in the hospital and in my practice are free from pain.

15898. (*Sir Mackenzie Chalmers.*) Have you ever had anæsthetics yourself?—Yes. I have been operated upon often.

15899. (*Mr. Tomkinson.*) Is it quite clear that human beings do not suffer. Is it not possible when there are those reflex motions that you spoke of that pain is felt, but that there is oblivion in the brain which creates forgetfulness of it afterwards. I ask the question because I have witnessed the case of a person who had a broken leg, and whose muscles round the ankle had stiffened during the process of healing.

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After a time the patient determined to have those fixtures broken down. I saw the operation, and the patient not only struggled, but moaned so as to give me a very painful impression, and I could hardly believe it, although I admit that afterwards he said he felt nothing?—I once removed an infra-orbital nerve, which is an extremely sensitive nerve, from the face of a woman who had had enormous quantities of morphia and drugs for neuralgia before. During the whole of that operation that woman commented on every remark that I made to the house surgeon, and when I tore the nerve out she burst out laughing, yet we went on with the operation, because she did not make the slightest purposive movement to avoid it. She obviously was not feeling it. We finished the operation, and the next day she knew absolutely nothing of what had occurred; she never felt any pain whatever, and she would not believe our statement of what she had done. I saw another case operated on by a colleague of mine, and the patient read my colleague a homily on the future life the whole time. He knew nothing of it. It was a large plastic operation for a burn in the neck—a most sensitive thing—in fact, it was the sensitiveness probably that stimulated the brain. But neither of those patients suffered anything at all. I am quite satisfied on that point. One's own knowledge of anæsthetics is that you go off suddenly and that as a rule you come to gradually, but you have no consciousness of pain during the time the operation is going on.

15900. Is the brain rendered insensible of receiving sensation?—No, the brain can receive sensation. It is only a question of paralysing the higher centres—the highest centres of all. You must only paralyse them. That is the object of all this modern research on anæsthesia. We want to know how just to abolish consciousness, and at the same time to enable all the lower processes to go on unhindered. When we have solved that question we have solved the whole thing of safety to life.

15901. All these experiments of Dr. Crile's were for the same object—to test the amount of shock caused by different operations, and the effect of it?—Yes, the effect of it.

15902. On the circulation of the blood?—Yes.

15903. And that was done sixteen times over?—Yes.

15904. Sixteen healthy, beautiful dogs—fox terriers—were taken. To use an ordinary vulgar expression, do you think the game was worth the candle?—Yes; I am perfectly sure of it.

15905. You admit, do you not, that those operations, published as they have been in that form, have created a tremendous sensation, and probably one of the strongest sources of opposition to the system and attack upon it?—Yes, because of the gross misrepresentations of those operations.

15906. But is not the description of them by the operator?—Yes, certainly; but not the description that has been given of them to the public. On the contrary, several times they have been made the subject of pamphlets which I have been advised legally I could take no action upon, because in an ingenious way the fact of anæsthetics having been given was not introduced; but the whole effect of the pamphlet was to convey to the ordinary man in the street that those operations were done without anæsthesia.

15907. Do you think so?—But the whole strength of the anti-vivisection party is the capacity for writing a thing and giving it a false impression altogether by means of innuendo. That is very glaringly shown in all this evidence which has been laid before the Commission.

15908. I cannot say that they have ever been described to me as having been without anæsthetics, but accompanied with an expression of opinion that complete anæsthesia was hardly possible through such tremendous operations, and was not likely to have been carefully supervised when the animals were kept alive the whole time?—Exactly. And on what authority has that last statement been made, that anæsthesia cannot be kept up under such tremendous operations? Why, operations that we do on human beings are far more tremendous by way of nerve stimulation.

15909. I think there has been more than one witness here who has expressed great doubt whether a dog is easily kept just on the border line?—That is exactly the point that I referred to before. I felt sure that evidence of that kind had been given, and I say that that is simply a matter of knowledge of giving anæsthetics. It is sheer ignorance to say that a dog cannot perfectly well be kept under chloroform, or any other dangerous anæsthetic that you like to mention.

15910. Is that system of introducing chloroform, or whatever the mixture may be, from another room, pumped through a tube, and brought under the mask, I suppose, perfectly satisfactory to you?—Yes, quite.

15911. Quite as much so as in the case of a patient having the chloroform held to his mouth?—Quite; in fact, probably more so. In administering the atmosphere you must give the atmosphere to the individual loaded with a certain amount of the anæsthetic. What we have found now is that during the operation it is quite sufficient to have only 0.5 per cent. of chloroform, an incredibly small amount, which is quite indistinguishable by smell in the room. If you blew that to the patient it would probably arrive at him more regularly than if he drew it in himself.

15912. Because it is properly mixed?—Yes. The only thing is that it involves so much apparatus that you cannot do it in practical working.

15913. Have you had much experience or seen much of the administration of curare?—No. I have used it myself, but I have not used it much, because my lines of research have not been very much on blood-pressure experiments.

15914. Do you approve of it?—It is most useful in blood-pressure experiments.

15915. Do you approve of it?—Of course I do.

15916. Do you not think it may be dangerous, as paralysing all power of motion without rendering the animal insensible?—You mean if it was used alone.

15917. No, I mean used along with anæsthetics?—Oh, no.

15918. Thereby rendering it possible for sensation to be felt, and yet without power on the part of the animal to evince it?—No, I do not think so at all, because, in the first place, taking curare alone, I believe that when we have given curare up to the point of producing paralysis of all the muscles of the body, then the animal or person would be insensible to pain as well. Although all drugs have a more or less selective action upon the different functions of the body, no drug has such a selective action as to be a paralysant of motion only, and the statements about curare which have been made are exceedingly vague, because, fortunately, it has been so very rare that any patient has been poisoned by the poison to the degree of paralysis of the respiratory muscles. The only good case on record is Mr. White's, and in that case the person did not know that the wound was being cut out, and that is exactly what I should have suspected, namely, that when curare by itself is given in such enormous doses as it is in experiments, it acts as a narcotic as well as an ordinary paralysant of voluntary muscles. The evidence so far takes us that way. Of course it is never given alon[e]; it is not allowed by the Act; it is not an anæsthetic under the Act. Consequently it is always given in association with narcotics like morphia, for instance. Then, again, I have read the evidence offered to the Commission as to whether morphia is an anæthetic.

15919. (*Dr. Gaskell.*) Can you tell us where White's case is published?—In the British Medical Journal. I do not know if I have the date here.

15920. Could you quickly tell us just something about that case?—Yes. It was a girl who was dusting a trophy of Indian arrows in the hall of her employer. One of these arrows fell and stuck into her arm, and then fell on to the ground. She took no notice of it, naturally. But in about a few minutes, suddenly, she felt ill, began to lose power, and by the time they called a medical man to her she was already unconscious, passing into the stage of asphyxia after paralysis of the respiratory muscles. The medical men recognised immediately what had happened, and started artificial respiration, while they sent for Mr. White, who came and immediately cut out the wound with the brown poison that came off the arrow still adhering to the flesh. They went on with artificial respiration, and in about another hour or an hour and a half the girl began to breathe spontaneously, and then gradually recovered, and when she recovered consciousness she knew nothing of what had been done, and could not understand why she had the dressing on her arm.

15921. (*Chairman.*) Might it have been from shock?—No.

15922. Or might it have been from some paralysing of the sensory nerves?—It must have been from some paralysing of the nervous system, certainly.

15923. (*Mr. Ram.*) Did White cut out the poison and the surrounding wound without any anæsthetics? —Yes, because the girl was unconscious.

15924. (*Dr. Gaskell.*) At the time the wound was cut out do you think there was a sufficient condition of asphyxia to cause unconsciousness?—No, because they had already been doing artificial respiration for some time, and she could not then have been in an asphyxial state.

15925. What anæsthetics have you had in your own case?—Ether, chloroform, and laughing gas.

15926. Can you tell the Commission whether you have experienced any sensation at all during an operation?—None at all. And on the question of morphia (Mr. Tomkinson, I see, has been obliged to leave), I was about to draw attention to the fact that the question whether morphia is an anæsthetic has been raised here; but, of course, it all depends upon the dose. Morphia given in the dose that is given in the laboratory is an anæsthetic of a most pronounced kind. I could establish that if it was required by an observation on a human being, because a very few years ago, at University College Hospital, a patient of mine, a boy, was given an overdose of morphia by the nurse by mistake. That boy had to be roused, artificial respiration employed, and stimulated in various ways to keep him going and painful impressions made upon him, many of which he resented, but he knew absolutely nothing about it; he was absolutely unconscious to pain, absolutely unconscious of everything for about 12 hours. And in the laboratory the dogs or animals that are given morphia are generally thrown into the same state, that is to say, a poisonous dose is given.

15927. Can you say whether urethane is an anæsthetic?—I cannot say. I have no personal experience of urethane. I have no doubt that it is; it is only a question of dose.

15928. Do you consider that there are idiosyncrasies in the action of morphia. Some people are said to be very receptive of morphia, and it produces excitement; in others you get a much more narcotic effect?—There may be slight differences, but I have never seen a person who did not succumb to morphia, or an animal either.

15929. How would you distinguish whether a man or an animal, when under an anæsthetic, is feeling pain?—That is a very practical point, of course. In the first place, the movements that patients ordinarily make when they are under an operation are simple reflex movements. We endeavour to preserve all those so far as they do not bother the operator. Then anything like a purposive movement, anything having at all a purposive character, I regard as a signal that the return to consciousness is not far off.

15930. Do you not consider that in a brainless frog, for instance, you can get reflex action and purposive movement?—Yes, undoubtedly you can. But you asked me what was my criterion in operations on man. That is my criterion; of course, it is always on the safe side. I mean that my criterion is a safe one. I know perfectly well that persons, however much they may have moved, will not have felt pain, because I have asked them afterwards again and again, especially in this more modern way of leaving off chloroform. With the Vernon-Harcourt apparatus, one simply turns the tap and they breathe air for ten minutes and so on. I have asked them again and again afterwards whether they knew anything of the operation, and they say no.

15931. For instance, would you call a cry a purposive movement?—No.

15932. Can you distinguish between a cry that is a reflex act and a cry that is a cry of pain?—I think I could perfectly well.

15933. In an animal as well as in man?—Yes, most certainly. The groans of a patient during an operation, which one may often hear outside an operating theatre, are distinctly the laryngeal movements accompanying respiration. It is most definite and quite different from anything indicating a return to consciousness.

15934. Have you read through "The Shambles of Science?—Yes.

15935. I suppose you could hardly tell from the statements made there by Miss Lind-af-Hageby, with respect to movements and cries, whether they were reflex or painful?—One could hardly tell also whether they occurred; but, assuming that they occurred, I could not say what they really were.

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15936. With respect to the experiments which you suggest that students should be allowed to perform for the purpose of obtaining manual skill, there are two points, it seems to me. One is manual skill in the operation itself, and the other is seeing the success of the operation afterwards?—I quite agree.

15937. Would you think that the first alone would be of any use if this Commission considered that the animal must be put under anæsthetics and killed before it recovered from the anæsthetic? Would you still recommend that the students should be allowed to experiment on animals for the purpose of acquiring manual skill?—Yes, I think that half a loaf is better than no bread.

15938. But you would prefer the whole system as carried out at the Johns Hopkins Hospital, Baltimore? —Absolutely. May I say on this question of manual skill that one can only speak from personal experience in a matter like that, and I am quite sure that the delicacy of control of dissection, which is necessary in operating on a small animal like a cat or a rabbit, is an invaluable training for the coarser movements which you perform on man.

15939. Then, again, with respect to demonstrations to students, you suggest that demonstrations to show the activities of the cortex of the brain are essential. What animals would you employ for that?—The monkey, but you need not employ a monkey. You can show it all on a cat or a dog.

15940. A cat or dog would suffice?—Yes.

15941. But not any other animal?—No. A rabbit is a very bad animal for localisation experiments.

15942. That would be yet another reason for recommending that dogs should still be under the Act?—Yes.

15943. And in your opinion, rather a strong reason? —A very strong reason. Dogs are essential in so many ways to experimentation.

15944. Monkeys, of course, are not always to be had? —No.

15945. Then with respect to Certificate B, Mr. Stephen Coleridge's objection was to the words "initial operation." Do you think there is the slightest necessity, if this Act was to remain much as it is at present, to alter that certificate? Do you think there is any ambiguity in that term, "initial operation"?—There is not the slightest ambiguity in the certificate as it is worded at present for the purpose for which it is employed; but I should like to see all certificates abolished.

15946. I know; but, supposing that the certificate was to remain there you do not think it would raise any difficulty?—The difficulty has never arisen.

15947. The difficulty is an imaginary one?—Yes.

15948. (*Sir Mackenzie Chalmers.*) I think you have had read to you the answer to Question 10319?—Yes; and I said before that it was a travesty of Certificate B as it is in operation at the present time.

15949. It would be perfectly impossible, you would say, for any operator, after having done the initial operation, to allow the animal to recover and then stimulate by electrodes the cut nerves?—Absolutely impossible.

15950. (*Dr. Gaskell.*) Then there is one other point with respect to the Act that has not been mentioned. I understand that your suggestion is that under your suggested licence you would remove all certificates altogether. Would you be inclined to restrict in any way the animals under the Act? Would you draw any distinction between one set of animals and another?—No. Are you referring now to the whole animal kingdom?

15951. I am coming to that?—No, certainly not. The answer to your question is no.

15952. Would you be inclined to restrict them in the other direction—that is to say, exempting certain animals from the action of the Act?—Yes, up to the present time, as I have always understood by a mistake in an answer across the floor of the House of Commons, this Act has been made to apply to all invertebrate animals. That, of course, lands you in the most ridiculous position at times. Undoubtedly the Act ought

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only to apply to warm-blooded animals. I think that is the proper separation of the animal kingdom.

15953. Was not that what was originally intended?—I have always understood that it was, and I have understood that in a question and answer across the floor of the House, Dr. Playfair misunderstood it, not being a physiologist; he put it down, and now it stands "vertebrate." It is perfectly ridiculous for fish and so forth to be included.

15954. Do not you think that one alteration which might be adopted in the Act would be to restrict it to warm-blooded animals?—Certainly.

15955. You said that you would have a licence to cover everything and abolish the certificates?—Yes.

15956. I do not think it was quite clear whether the form of that licence was put out by you or not?—No, it was not. What I suggested was that all the facts which were detailed on the certificate application form could be, as indeed they are now, detailed on the application form for a licence.

15957. Have you any form of licence which we could get down on our notes so as to let us know what it is you want?—No, but I have a draft form. I am quite satisfied that it would be enough. What is not enough, in my opinion, to give the Home Secretary complete information, is the final form that we have to furnish at the end of the year.

15958. The present form of licence causes the animal to be killed before it recovers from the anæsthetic?—Yes, the present application for a licence.

15959. That is what the licence is?—Yes, but in a new Act, of course, that could be altered. In a new Act that would come to revising Section 3.

15960. I only wanted to know whether for our own information we could have on the notes your idea of what that form of licence ought to be?—I shall be very glad to furnish it in writing to the Commission.*

15961. (*Chairman.*) On the printed form, amended as you would wish it?—Yes, and also with the alteration necessary in the first part of Section 3 of the Act.

15962. (*Dr. Gaskell.*) There is only one other point which I thought I might put to you with respect to Mr. Lawson Tait. You know this little book by Sir James Thornton, "The Principal Claims on behalf of Vivisection"?—Yes, I know it well, it is full of contradictions of fact.

15963. I want to draw your attention to a statement on page 3: "Lawson Tait says, 'This is just one of the cases where vivisection has led us astray. If the carbolic ligature had never been tried on animals, where it seems to answer admirably, it would never have been tried on human patients, where it fails miserably, and has cost many lives. The fact is that the diseases of animals are so different from those of men, wounds of animals act so differently from those of humanity, that the conclusions of vivisectors are absolutely worthless. They have done far more harm than good in surgery.'" Is there the slightest foundation in fact, in your opinion, for the statement that the wounds in animals act so differently from those of humanity?—No, it is absolutely contrary to fact.

15964. Is that true about the carbolic ligature?—It is not.

15965. Did it fail absolutely?—It did not. We use it at the present day.

* See Appendix.

THIRTY-SIXTH DAY.

Tuesday, 19th November 1907.

MEMBERS PRESENT:

The Right Hon. the Viscount SELBY (*Chairman*).

Sir W. S. CHURCH, Bart., K.C.B., M.D.
Sir W. J. COLLINS, M.P., M.D., F.R.C.S.
Sir J. MCFADYEAN, M.B.
Sir M. D. CHALMERS, K.C.B., C.S.I.
Mr. A. J. RAM, K.C.
Mr. W. H. GASKELL, M.D., F.R.S.
Mr. G. WILSON, M.D., LL.D.

Capt. C. BIGHAM, C.M.G. (*Secretary*).

Sir VICTOR HORSLEY, F.R.S., F.R.C.S., recalled; and further Examined.

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(*Witness.*) May I put in an account which Dr. Gaskell asked me for, namely, the description which Mr. White gave of his case of curare poisoning on a human being? The reference to it is the "British Medical Journal"—I could not remember the date—of March 16th, 1889, and I wish to draw attention to the fact that with regard to the very important matter which Dr. Gaskell raised, namely, whether the patient was under the influence of curare or whether she was under the influence of the state produced by asphyxia, as a matter of fact she was still breathing, she had slow, shallowy, jerky respiration, and feeble pulse. The whole track of the wound was covered with a brown apparently vegetable material. It was therefore freely exercised, and stimulants were freely given by the mouth. So that she not only breathed still, but she swallowed; therefore she certainly was only I should have said under a rather moderate dose of curare. But, she was in a very dangerous state from the condition of the pulse, because curare lowers the blood pressure—you see that experimentally, of course, very markedly—and causes a very feeble pulse and ultimately death. Then, as I stated, they kept up artificial respiration until she recovered. The final point that I want to draw attention is "That Mr. White believed that the arrow had been poisoned with curare and cited"—this is an abstract of his paper—"the experiments upon animals made by Claude Bernard, Waterton, and Sibson, of some of which he had been a personal witness, in support of his view." So that he recognised that it was curare poisoning from the effects of curare poisoning that he had seen in experiments on the lower animals. I had forgotten that last point, and I thought the Commission would be interested to hear it. With regard to the other documents I was asked for, I will put them in after my cross-examination to-day, because, of course, although I have got my own views of the working of the Act, it is quite possible that the Home Office have a great many departmental requirements of which I am quite ignorant, and I should like to hear those before I finally formulate my statement.

15966. (*Chairman.*) Sir Mackenzie Chalmers, who is going now to ask you some questions, may touch upon that, and give you an opportunity of saying what you wish?—Thank you very much.

15967. (*Sir Mackenzie Chalmers.*) I do not think we have it in evidence, but you are a practising surgeon?—Yes.

15968. I think you operate at two different hospitals?—Until quite recently I was surgeon to the University Hospital, as well as the Hospital for Paralysis. I have resigned my University College position, I am now Consulting Surgeon to that hospital.

15969. May I take it that for the last twenty years at any rate you have been operating almost daily?—Yes.

15970. And have been continually in contact with the practical administration of anæsthetics?—Yes.

15971. I want to ask you one or two further questions about what we call the Crile case. In the first place, I want to read to you Dr. Crile's application to do the experiments which he did, and to ask you a question upon it: "25th April, 1895.—To the Right Honourable Henry Matthews.—I beg to enclose a form of application for a licence to perform experiments upon the causation and the prevention of shock in surgical operations. I am the Lecturer on Surgery in the University of Worcester, Cleveland, Ohio. My engagements are naturally pressing, and I would be much obliged if you would grant me the licence asked for within a few days. I am also informed as a matter of fact that the experiments I am about to perform do not come within the Act, inasmuch as they do not involve any pain to the animal.—I have the honour to be, Sir, your obedient servant, GEORGE W. CRILE."

Then, accompanying that letter was the formal application in which the experiments were described as "investigation of shock in operations performed under ether. The experiments proposed will be conducted under the licence, *i.e.*, the animals will be anæsthetised with ether and killed before recovery from same." What I wanted to ask you was this: The object of these experiments was to examine the effects of surgical shock, not surgical shock pure and simple, but surgical shock under anæsthetics?—Yes.

15972. Would the supposed object of the experiments have been frustrated if anæsthetics had not been part of the procedure?—Certainly, because we wished in this research to imitate, as far as possible, the operations done on a human being.

15973. Perhaps I may also, to complete the case, read a letter from the Association for the Advancement of Medicine by Research. I do not know whether it is within your knowledge or not that after a report on Dr. Crile's application to Dr. Poore the matter was referred to the Association for the Advancement of Medicine by Research?—I did not know that.

15974. Then perhaps, to complete the matter, I might read their letter. It is addressed to the Under Secretary of State, Sir Kenelm Digby: "Sir,—According to your letter of the 29th ult., I have laid before the Council of this Association Dr. Crile's application for leave to hold a licence under the Act 39 and 40 Vic., c. 77. I beg to return Dr. Crile's papers. The Council wish me to say that they hope this application may be granted. Dr. Crile is Professor of Surgery in one of the American Colleges, and is therefore fully qualified to make the proposed observations with judgment and accuracy. At the Pathological Department of University College he would enjoy every aid for the success of his work, and would have Professor Victor Horsley's constant help. The Council are further of opinion that the proposed observations are calculated to give very valuable results. The occurrence of shock after surgical operations, in spite of anæthesia and of all the precautions that can be taken to avoid it, is a danger that cannot be too carefully studied, and the Council therefore hope that leave may be granted to Dr. Crile to make these observations, which would, moreover, be wholly free from pain, as the animals would be killed while yet under the anæsthetic." After that the licence was granted?—Yes.

15975. Did you see any of the operations yourself?—Yes, I saw about the first six or seven.

15976. In your opinion, were the animals that underwent those operations suffering or not?—No, I think they were not suffering at all. I think they were completely anæsthetised, and unconscious of any pain.

15977. Who is Dr. Goodbody?—Dr. Goodbody is the Assistant Professor of Pathological Chemistry at University College, and when I was not present he was present at these operations.

15978. Have you discussed the matter with him at all?—I have not spoken to him since 1902, when he wrote to the "Times" in the correspondence that ensued upon the exposure of the catalogue of instruments and Mr. Coleridge's action.

15979. Do you know what conclusion he came to from the operations he saw as to whether the animals suffered or not?—I remember that he stated that, in his opinion, the animals were quite unconscious to pain.

15980. Is he an experienced and skilled observer?—Yes; he has been a laboratory worker now for 15 years.

15981. Your physiological experiments have been carried on mainly, if not entirely, at University College?—And at the Brown Institution. I was Director of that Institution for several years.

15982. Has the Home Office Inspector always had free access?—Yes; and he is constantly in and out.

15983. Does he pay pretty constant visits to your laboratory?—Professor Thane does. He visits my laboratory almost every week. Of course his rooms are adjoining mine, you know—his Department, I mean. It is quite convenient for him.

15984. Does he give you notice of his visits?—No, never. The visits have always been surprise visits. It is rather difficult to fix the date; Mr. Erichsen used to give me notice at the Brown Institution; that was before 1890. But I should say that for the last 17 years, about, they have been surprise visits invariably.

15985. Have you not carried on to some extent Professor Ferrier's experiments on brain surgery?—Yes.

15986. These experiments, I suppose, involve the destruction of some portion of the brain?—They are of two kinds. In one set of experiments the animal is experimented on under the licence, and it is killed before recovery from the anæsthetic. Those are stimulation experiments, excitation of the surface of the brain by electric current. And then in order to control the results of excitation you must perform further experiments under Certificates B and E E, in which you destroy, as you say, part of the surface of the brain, and examine the symptoms afterwards.

15987. And then the animal is eventually destroyed?—Yes.

15988. In those operations, on recovery from the anæsthetic, does the animal suffer much?—No, it suffers remarkably little. It is quite a common thing to see a monkey, for instance, from which you have removed a portion of the brain take a banana or grape as soon as it really recovers from the operation. I do not consider that operations on the brain are really painful operations in that sense.

15989. (*Chairman.*) Are you speaking of animals?—I am speaking now of animals.

15990. (*Sir Mackenzie Chalmers.*) You do not think that the animals which are allowed to recover endure acute suffering?—No, I do not.

15991. Is the brain not very sensitive then?—The higher brain itself it wholly insensitive, so far as I know. There are portions at the base of the brain where sensory paths are coming in, which I am sure would be sensitive.

15992. How do you account then, for instance, for the intense pain of headache?—One sees a great deal of that in cerebral tumours; that is due to pressure. The skull is a rigid box, and the fluid within it is at a certain pressure; it requires therefore an exceedingly small increase of tension, a little more fluid injected into the skull to raise that pressure, and the moment that occurs then you have a sensitive membrane, the dura mater which covers the brain, pressed.

15993. It is the compression of the dura mater, is it?—That is a very difficult thing to prove, of course, scientifically, but it certainly seems to be so. The dura mater is supplied by the fifth nerve, the most sensitive nerve of the whole nervous system, and from the fact that the headache varies in cerebral tumour directly with the tension, it seems the most reasonable explanation of headache.

15994. (*Chairman.*) But are the nerves which convey, the sensation of pain nerves of the dura mater, whatever the cause is?—Yes, the dura mater is very richly supplied with nerves.

15995. (*Sir Mackenzie Chalmers.*) But you have to penetrate the dura mater to operate?—Yes, you simply incise it, and turn it aside.

15996. But that does not create pressure and cause intense headache?—The opening of the skull opens this rigid box, and from that moment you have destroyed all the intra-cranial tension.

15997. In the case of ordinary headache you think there is interior pressure?—Yes, interior congestion and pressure.

15998. In consequence of these brain experiments on animals have you been enabled to perform any operations on human beings which you would not have dared to have performed otherwise?—Certainly, both upon the large brain, the cerebrum, and upon the cerebellum the small brain, and upon the spinal cord.

15999. And what of the results?—The results are excellent where the cases have been diagnosed sufficiently early.

16000. With correctness?—With correctness, and sufficiently early. On that point I should like to draw the attention of the Commission to a fact which seems to me always an exemplification of Professor Lankester's statement that scientific investigation does not add to our knowledge by arithmetical progression, but by geometrical progression. Of coure the first operations upon cerebral tumour, now some 20 years ago, revealed to us the fact that this opening of the skull which destroys the intra-cranial tension was

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the mode *par excellence* by which the inflammation of the optic nerves which accompany so many diseases inside the skull could be arrested and the sight preserved, because there are not a few cases in which the patients can be recovered from a cerebral tumour, but remain blind all their lives, and this observation, which was simply made, you might almost say by accident, has been of the utmost benefit to humanity, because now, even in cases where the tumour can probably not be removed, what we call a decompression operation is done to save the sight.

16001. (*Chairman.*) Are you speaking of observations that you made upon a patient?—Yes.

16002. Not an experimental observation?—Not at all. I ought to amplify that, perhaps, by saying that attempts had previously been made to save the sight by opening the sheath of the optic nerve in the orbit, the operation of de Wecker, a great Parisian ophthalmologist; but his operation was not successful. We now know the reason—that the pressure ought to have been relieved inside the skull, not outside.

16003. (*Sir Mackenzie Chalmers.*) Have you discussed with any of your patients afterwards the effects as to whether there was pain consequent upon the operation?—Yes, often. Like all cutting operations, they have a certain amount of pain in the cut itself—a sore pain, which, like ordinary aseptic wounds, disappears in about 48 hours. A certain number of them suffer from headache due to the anæsthetic and to shock.

16004. The anæsthetic produces headache for some time afterwards?—Yes, very often. Not invariably, of course. Many people can have chloroform for an hour and have no headache.

16005. It is almost an irrelevant question, but one witness whom we have had before us objected to physiology on the ground that no advances had been made, and one special point that was taken by that witness was that, although we had been experimenting for a hundred years, nothing has been learned about any of the ductless glands. Is that correct?—No, it is absolutely untrue.

16006. Your own work on the thyroid gland is one example to the contrary?—Yes, it is only one example of a great many researches now on the ductless glands.

16007. Which are adding to our knowledge and to our power in dealing with disease?—Yes, it has all arisen since I was a student. It is an entirely new chapter in physiology.

16008. And a fruitful one?—Immensely, because you see it introduces us to the true chemistry of the living body, bio-chemistry. It is only, of course, by advance in research in bio-chemistry that we can really put internal medicine on a proper scientific basis. The University of Liverpool, of course, which is very advanced in medical matters, has founded a professorship of bio-chemistry.

16009. One other small point. You showed us a catalogue in which certain of the pictures, I must say, look very disagreeable. May I ask, as a matter of fact, are animals which have to be operated upon tied down for the purpose of preventing struggling, or are they tied down simply to keep them in position under the anæsthetic?—They are tied down simply to keep them in position.

16010. Not in any way to mask any purposive movement?—Not at all. In fact, you cannot mask a purposive movement. There was a statement made by Mr. Coleridge in his evidence about an experiment by Professor Starling about fastening down an animal on a board in connection with the Bayliss trial. It is at the top of the second column, page 205, at Question 11569. Mr. Coleridge is asked by Dr. Gaskell, "I want to know whether you do not consider, when a delicate registering apparatus is going on in the animal, which moves perfectly smoothly and gets real good curves, even although the operator says the animal is under slight anæsthesia, that does not show that the animal was not feeling pain?—Not necessarily. I have an answer to that, of Sir Victor Horsley, in the case against me on that very point that an animal can be held tight when it does not move." I suppose he means can be held tight so that it does not move. He is quoting from evidence which was given in Court, but the context of that evidence ought to be given.

16011. Was it your evidence he was quoting from?—It is my evidence he professes to be quoting from, but from my experience of Mr. Coleridge, and especially after what he has admitted to this Commission, I accept nothing that he produces in the way of evidence of what I have said unless I have the context. I will just read it, however, and then explain it to the Commission. "Mr. Rufus Isaacs: Would it be possible to perform the experiment or the demonstration which we know took place if the dog had been conscious? Sir Victor Horsley: No, absolutely impossible, unless the dog had been fixed up with all manner of apparatus, to absolutely fix every bone." I showed the Lord Chief Justice what I meant by fixing every bone with a rod passing through the bones. Of course, it is an absurd position, you observe. It is impossible to fix every bone in an animal. And the Lord Chief Justice asked me whether it could be fixed on the board—they had the board on the witness desk—and I said, "You can fix an animal with apparatus of *that* sort (pointing to the operating board). That is the answer to that—you can so fix an animal on that board, and he pointed to it; they had it in Court." The rest of that is true, but the inference drawn from it is wholly untrue. My point was that it was conceivably possible with an apparatus to fix every bone, but as you must fix every bone to make the animal absolutely immobile, so that it could not move at all, the thing was practically impossible. Mr. Coleridge's was quite a false rendering of my evidence in the way he presented to the Commission.

16012. On that we have been told by some witnesses that there is a sudden transition from pain to absolute anæsthesia. Is that so, or is there a gradual diminution of pain as the anæsthetic is administered?—There is a gradual diminution of sensitiveness to pain undoubtedly. The loss of consciousness is apparently sudden to the individual, but that is quite a different thing.

16013. Pain gradually diminishes as the anæsthetic is applied and pressed?—Yes, exactly like the disappearance of all other kinds of sensation.

16014. So that, assuming for a moment that an animal is incompletely anæsthetised, using it in the English sense of incomplete anæsthesia, there would be diminished pain, although there would be pain?—Yes, if you mean by incompletely anæsthetised, that it was still conscious of pain, it would have less pain in proportion to the amount of anæsthetic which it had received.

16015. I suppose, in certain cases, for instance, in midwifery cases, anæsthetics are given with the view of diminishing pain and not with a view of producing complete anæsthesia?—You want to avoid preventing the contraction of the uterus. If you give a very large, profound dose of chloroform, you would interfere with the normal contractions of the uterus and the expulsion of the fœtus.

16016. I am not quite sure whether there is any point we can discuss further as to certificates, but taking the Act as it stands, the licence and the certificates are framed, not with reference to the scientific quality of the operation, but with reference to the pain, are they not?—Yes, certainly.

16017. Is it not convenient, first of all, to have a licence under which an animal can be anæsthetised and killed without recovery; is it not convenient to have that by itself?—I cannot see that it is convenient except as a means of informing the Home Secretary what experiments are done, and the animal is killed before recovery from the anæsthetic.

16018. Is it not convenient in this way, for the purpose, of course, of assuring the Home Secretary and the public, that under licence alone a man should be only able to operate on an anæsthetised animal which is never allowed to recover, so that if anything further is wanted, a special certificate should be given?—I am sorry that I cannot see the convenience of the proposal, because my proposal embodies exactly the same facts, and it strikes me is simpler. I propose that on the application for a licence the applicant should state the different classes of experiments he is going to do. If he were only going to do experiments on animals killed before they recovered from the anæsthetic, he would simply state so, and there is an end of it. But I do not see why that application should be, in substance, different from any other application so long as he states what he is going to do. The Home Secretary must know, of course, the details of what he is going to do, and then, as I say, the Inspector would see that he was doing it.

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16019. And he would report his experiments?—He would report his experiments on a form which would put these points down in detail.

16020. But you suggest that on his application for a licence he would so far indicate the precise nature of his experiments as to inform the licensing authority what amount of pain would be involved?—Certainly.

16021. That would merely come to this, would it not, that the certificate and licence would be on one document?—That is the whole point.

16022. It is merely saving paper?—No, it is saving more; it is saving office work going about and bothering people to countersign these papers.

16023. (*Mr. Ram.*) Then would you do without the signature in the case of all experiments in which the animal would die under the anæsthetic?—No, I would have every application for a licence countersigned, but at the present moment we have practically to have three documents filled up, and sent and filed.

16024. (*Sir Mackenzie Chalmers.*) By six people?—Yes, for only one thing; and that, I suggest, is unnecessary. One document ought to cover the whole thing.

16025. (*Mr. Ram.*) You would still have a countersignature?—Yes, for the assurance of the public, certainly.

16026. I have only a few points, as a great many of the matters I wanted to ask you have already been put by Sir Mackenzie Chalmers. You recollect that one of the Commissioners put to you, with regard to the suppression of rabies, that in Cheshire they had done away with the muzzling order and got a clean bill of health?—Yes.

16027. Was Mr. Long's order compulsory on the whole country?—Yes.

16028. Do you attribute to the fact that it was compulsory, and compulsory over the whole country, rabies having been practically stamped out here in England?—Yes. I would point out that the north-western district of England (Lancashire and Cheshire), was the focus of rabies, together with the Metropolis; it had been stamped out by muzzling in the Metropolis, and it was stamped out similarly in Lancashire, where the population, of course, is much greater than in Cheshire. Then, as regards Mr. Long's action, as I represent the British Medical Association, may I point out also that so far as I know, the British Medical Association was the only public body in this country that thanked Mr. Long for getting rid of hydrophobia.

16029. A good many individuals have thanked him, I think?—Yes, but I mean the only public body.

16030. Then as soon as Mr. Long's Act passed it became effective in Cheshire as well as in any other part of the country?—Yes.

16031. One or two general points first. There is a quotation of Sir Frederick Treves which you said you thought you could get for us. It is referred to in your evidence on page 719 as follows:—Sir Frederick Treves is reported to have said that he had learnt nothing, as you know, from experiments. Then you stated that he wrote a letter to the "Times" to the effect that in one particular case, one particular form of experiment, he learnt nothing from it, and you stated that you would be able to furnish the reference to the "Times." Have you been able to get it?—I will put it in now.

16032. With regard to allowing students to operate, would you, in any case in which a student was to be allowed to make an experiment for the first place, have the animal under a lethal dose of anæsthesia?—Yes, certainly, if a student was going to do the experiment.

16033. And, secondly, would you have it always under the supervision of some authorised and licensed person?—Certainly.

16034. And would you make that authorised and licensed person responsible for the fact that the anæsthetic was so administered as to be efficient at the time, and certain to be lethal?—Yes.

16035. And only under those conditions would you allow students to experiment at all?—Exactly, just in the same way as any practical class of physiology is conducted now.

16036. With regard to experiments for acquiring manual dexterity, in your opinion would it be distinctly beneficial to the human race, who have to be operated on in many cases by young surgeons, who are learning and in progress of getting perfect, that young students and young surgeons should be allowed to operate on animals under a lethal dose of anæsthesia for the purpose of acquiring manual dexterity?—Certainly. May I point out that in respect of these brain operations that have been mentioned, the appearance of the normal brain ought to be first learnt on an animal. It is extremely difficult very often, when you have exposed a diseased portion of the brain, to tell whether it is actually diseased or not, and the differential diagnosis of the condition of that brain ought, certainly, to be established by experience on animals and not on man.

16037. Is it the fact that by seeing the brain of a living animal, under an anæsthetic, of course, you can learn more than you can learn from seeing the brain of a dead animal, by vivisection?—Yes, the brain has a totally different appearance, and a totally different texture.

16038. Had you only experience of the brains of dead animals, or even dead human subjects, would it be difficult for you, in an operation on a living subject, to decide whether the brain was diseased or not?—Yes.

16039. But by examination of what I may term a living brain under anæsthesia you can differentiate between a diseased and an undiseased brain?—Yes.

16040. With regard to the knowledge of blood pressure, has that been obtained entirely, or if not entirely, largely, through experiments on animals?—Entirely by experiments on animals; the fact that arteries contain blood under pressure was proved by the experiments of Galen, who opened arteries really to see whether they contained air or "spint" as was supposed at that time, and found that they contained blood under pressure. Next, the first definite observations were made by Hales upon animals; and then, from that time onwards, the whole of our real knowledge has been derived by observation on animals. Now, of recent years, the measurement of blood pressure is being introduced clinically, and we are now accumulating records of blood pressure in man under different diseased condition.

16041. And is the knowledge of blood pressure under certain circumstances of vital importance, both clinically and in operations?—Undoubtedly. We have never recognised its importance until recent years. Like every other advance in medicine, it comes very slowly, but we appreciate it at last when it does come.

16042. With regard to inspection, you have answered to Sir Mackenzie Chalmers many of the questions that I was going to ask you; have you ever found it at all hindering to you in your work?—Oh, no.

16043. And if it was thought desirable that the number of Inspectors and the number of their visits should be increased, would it in any way, in your opinion, hinder research or hinder experiments?—Certainly not, if the Inspector was a qualified man.

16044. Do you think that inspection, such as it has been hitherto, has been useful in any way at all?—No, I do not think it has, but there is no objection to it.

16045. Why do you think it has not been useful—for what reason?—Because my experience is that there is really no violation of the Act, and that these iniquitous statements which have been made respecting the indifference of experimenters to pain, and so on, are absolutely untrue; consequently no Inspector has ever, so far as I know, come round on surprise visits and discovered a vivisector callously inflicting pain. The thing is ridiculous.

16046. If it be desirable for any reason that inspection should continue, do you think that the number of inspectors ought to be increased?—I think that the present number of experiments done could be quite well inspected by the present staff, and if the public, the House of Commons, for instance, thought that the inspector ought to be going round more frequently, all they have to do, it seems to me, is to make him a whole time officer. At the present moment he is not.

16047. And to that you, as an experimenter, would have no sort of objection?—No; no experimenter would object in the least to the visits of the inspector; of course, as I say, provided he is a qualified man.

16048. Now, with regard to some of these experiments of Dr. Crile, you are aware that he is reported to have used the words "these animals being under incomplete anæsthesia"?—Yes.

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16049. You have already fully explained that, and stated that in your opinion the animals suffered no sort of pain?—Yes.

16050. It seems to me that the use of those words "incomplete anæsthesia" by him is really the whole cause of the trouble which has been raised with regard to his experiments?—Possibly.

16051. The word as he used it is apparently a misnomer, is it not?—No, we constantly use the terms "incomplete anæsthesia," "light anæsthesia," "very light anæsthesia," and so on, meaning degrees of the depth to which the body is poisoned; and, unfortunately, at the present time there is actually no technical word to describe the condition of a person who is conscious, and at the same time unconscious to pain. I have brought the question myself before anæsthetists, and asked them to draw up a terminology. The word narcosis, for instance, is used by the Germans; they talk about *tiefe narkose*, and *leichte narkose*, in the same way as we do of deep anæsthesia and light anæsthesia; but in both cases the word, narcosis or anæsthesia, we understand to mean unconsciousness to pain.

16052. But, of course, to the ordinary layman, who did not know of that distinction, the term "incomplete anæsthesia" might very well convey the idea that the animal was not wholly insensible to pain?—Yes, and an ordinary honest layman would write to Dr. Crile and ask him what the words meant before he commented on Dr. Crile's action.

16053. We are glad to have your explanation of what the words do mean and what you know them to mean?—Certainly, because those words, if they are translated in any other way, conflict with Dr. Crile's statements in the preface to his paper.

16054. One question about curare, because, although we have had other views on the point, I shall be very glad of yours. The number of cases in which curare is used is comparatively small?—Yes, very small.

16055. In curare, I suppose, one or two indications of anæsthesia are absent?—Yes.

16056. I do not know if you follow my question?—Yes, quite.

16057. Therefore, whether the anæsthesia is perfect or not, depends upon the skill of the person giving the anæsthetic, and his assurance that it is a sufficient and complete dose?—Yes, his observation of what is going on, with the changes, for instance, in the blood pressure.

16058. If it were thought desirable, in order, perhaps, to allay reasonable public feeling with regard to curare, that the inspector should be present in all cases when curare was administered, would that, in your opinion, be an impediment to research?—Not in the least if the convenience of the investigator is safeguarded.

16059. Provided that the inspector was present, he could satisfy himself about the depth of the anæsthesia?—Yes, and the correctness of its administration.

16060. (*Sir William Collins.*) I do not think you have told us how long you have held a licence under the Act?—I think my first licence was obtained in 1883.

16061. Have you held it continuously since then?—Yes, continuously.

16062. And I do not think you told us what certificates you hold now and have held in past times?—I have held, but I could not tell without actual reference, Certificates A B C and E E. I also held Certificate F at one time, when I was at the Brown Institution.

16063. That was for operations on ungulates?—Yes, special ungulates.

16064. You mentioned incidentally some of the animals that you employed for your experiments; could you name them all?—Yes. I have experimented on the rodents, on rabbits, and guinea-pigs. I have experimented on carnivora, the cat, and the dog. I have experimented on ungulates, as I have mentioned, on the horse, and on the donkey, and on sheep, and of higher animals, on the monkey.

16065. Could you give us any idea of the number of animals that you have employed in the course of your experiments?—No, I am afraid I could not do that. Experimental work varies very much. I daresay that I do, on an average, about 110 or 120 experiments a year.

16066. And that has been continuing over a period of 24 years, has it not?—Yes.

16067. The present Inspector, you have told us, is Professor Thane, who is on the staff of University College?—Yes.

16068. He was preceded by Dr. Poore?—Yes.

16069. Who also was connected with University College?—Yes.

16070. And he was preceded by Sir John Erichsen?—Yes.

16071. Who also was connected with University College?—Yes.

16072. Do you think that University College has had an excessive amount of attention by way of inspection in consequence?—Oh, no, not a excessive amount, because, as I say, I think that inspection does no harm.

16073. I mean by reason of the facility afforded to the inspector by his connection with University College, do you think that there has been a greater opportunity, and that that opportunity has been used more by the Inspector?—No, I do not think so. Dr. Poore and Mr. Erichsen (as he was then) both came down pretty frequently to the Brown Institution when I was there. I should say that Dr. Poore inspected the Brown Institution as often as he did University College. I was only quoting Professor Thane's actual rooms—being next to mine.

16074. I suppose that University College does not have less attention by way of inspection than other places?—I do not think so at all.

16075. How many do you think of the 110 experiments that you perform in a year would be witnessed by the Inspector?—That is very difficult to say. Professor Thane has witnessed a great many of my experiments.

16076. Half of them, do you think?—No.

16077. Ten per cent?—Yes, certainly 10 per cent.

16078. Do you think more than that?—Yes, I should think even more.

16079. How much more?—You see he knows very accurately when I am likely to be doing an experiment. I have fixed days in the week which I give up to scientific work, and he knows those days, so that he very often comes in then, and finds me in the middle of an experiment as a rule. He has certainly witnessed quite 10 or even 15 experiments of mine in the year, personally.

16080. Do you think the Inspector witnesses as many as 15 per cent. of the experiments performed by other experimenters throughout the country?—I should think not. I was asking Professor Gotch how often they were inspected. I am sorry to say that I have forgotten at the moment how often he said that it was, but certainly not as much as that.

16081. That would rather suggest that University College does have more inspection than other places?—But then he is at Oxford you know, and Oxford is some distance from London. University College ought to be compared with some of the other medical schools in London.

16082. But the Inspector's duties extend over England, do they not?—Yes, they do, but you see he is a part time officer.

16083. I rather gathered from the replies that you gave to Mr. Ram that you looked upon inspection as superfluous, is that so?—Yes, I do, judging inspection from the popular meaning of the office of Inspector.

16084. Might it be regarded as harmless trifling?—I think it is a harmless non-necessity, certainly. I think the idea that the Inspector prevents pain to animals is trifling; it is perfectly ridiculous.

16085. The evidence which you have been so good as to give to the Commission has been largely concerned with physiology and surgery?—Yes.

16086. Physiology being the science of living things you would, *a priori*, suggest that it might be advanced by experiments upon the living?—Yes; I think it can only be advanced by experiments on living tissues.

16087. And surgery dealing largely with the making and healing of wounds you might also *a priori* suggest would be likely to be advanced by experiments on the living?—Yes, and inasmuch as the diagnosis part of surgery is quite as important as the operative technique, *a fortiori* it must be advanced by experiments on the living.

16088. I think you told us in your examination-in-chief that there is not a single function of the nervous system, the principle of which we now know, that is not derived from experiments on animals?—Yes, not a single fundamental principle in the functions of the nervous system.

16089. The function of the sensory and motor nerves must, in your opinion, be determined by experiments on living animals?—Yes.

16090. You suggest that there should be no certificates required at all?—Yes, I do. I think they are unnecessary documents, and consequently give unnecessary work in the Home Office, and they give unnecessary trouble to investigators who have to obtain them.

16091. Are you opposed to the Act altogether?—Oh, no. I look upon the Act as necessary in view of public opinion. Public opinion has decided now for 30 years that experiments on animals should be conducted by certain persons under certain restrictions, and, of course, one quite accepts that. It is obviously a matter of satisfaction to the public, and there can be no objection to it.

16092. But from the point of view of physiology do you regard the Act as superfluous?—Quite, if you mean that the Act is really an Act to prevent cruelty to animals as it is entitled, I do object to it. I object to any Act appearing on the Statute Book as a "cruelty to animals" Act when it refers to scientific experiments. It is an unjustifiable use of the word cruelty, and it ought never to have been accepted by the Government of the day.

16093. You object to the title of the Act?—I object to the title of the Act *in toto*, but to the purpose of the Act, namely, that experiments should only be done in registered places, and only by persons who hold a licence from the Home Secretary there can be no objection whatever, at least, I cannot see any.

16094. But the justification which I understood you to suggest was the gratification of the public?—I did not say the "gratification" of the public. If the public is uneasy on a public question I think it has a right through the House of Commons to be satisfied, and this Act is the satisfaction to the public.

16095. Would you agree with a physiologist who was in the witness chair some days ago, who told us that the Act was entirely opposed to the advancement of physiology?—If he meant that the Act hindered the progress of physiology, I am not prepared to contest it at all, because I can give you an example at the present time. There is an American physician who has come here purposely to study the surgery of the nervous system. I advised him to apply for a licence to do some experimental work in my laboratory, and it has taken him six weeks to obtain that licence. Now here is a man whose time is limited, and all these modern processes of investigation take endless time; the anatomical investigation of the material, that you prepare in the experiments takes months, and so to cut off six weeks of that man's working time I think was unjustifiable. One, of course, does not expect a Department to go on without some official routine, it must follow a certain routine, but if that routine can be diminished by an amendment of the present Act, then most certainly it ought to be.

16096. You referred us to the Report of the previous Royal Commission of 1876. I see that in that Report it is stated: "In considering the question of legislative interference, we have found in some minds a decided prepossession against it. This appears to be connected, as in the case of Mr. Lister, with a notion that such interference implies an imputation of cruelty upon those who are engaged in those investigations—an imputation they are conscious they have not deserved." Do you agree with that?—Certainly. I have already protested against the title of the Act on that very ground.

16097. But Mr. Lister regarded any legislative interference as superfluous?—So do I. I think it is perfectly superfluous for the object for which it was designed. The object of the Act was to prevent the infliction of unnecessary pain. At the time when it was passed there was no infliction of unnecessary pain. I quite agree that such legislation then was superfluous.

16098. Then you do think that the Act of 1876 was superfluous legislation?—Certainly; but if the public demand it, I do not object to it.

16099. Now, Mr. Pembrey, in answer to Question 14090, said: "I think we ought to be given a licence to cover all experiments. I think that the Act is entirely antagonistic to the advancement of physiology. If we were given a licence for all experiments, there would be no more cruelty. There would be a great saving of time and a great limitation of work, and there would actually be in the long run a saving. Q. What do you mean by a licence for all experiments? A. I mean without any conditions. Q. With or without anæsthesia? A. Yes, without any limitation at all, and without certificates. Q. That is to say, you are to put yourselves in the condition in this country which, I understand, physiologists are in in some parts of Germany, where there is no limit? A. I think there should be no limit—that is to say, that a recognised physiologist should be given a licence to cover all experiments. Q. Without anæsthetics? A. Without anæsthetics, or with anæsthetics without certificates." Does that state your view?—Undoubtedly; in this way: I propose that an applicant for a licence should state on that application exactly what he is going to do, whether he is going to do experiments with anæsthetics or without anæsthetics, although no such application has ever been made since 1876; or whether he is simply going to do inoculation experiments. In fact, he should put upon his application the details of his proposed experiments, so that the Home Secretary should know exactly what he is going to do; but the Home Secretary, being thus informed of what is going to be done, would still, of course, under the Act, have the plenary power which he possesses at the present time, of deciding whether a licence should be granted or not.

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16100. And should the Home Secretary, in arriving at his decision, be advised, in your opinion, by the Association for the Advancement of Medicine by Research?—Certainly.

16101. Are you a member of that Society?—I am. The Home Secretary cannot possibly, I think, be guided correctly in the administration of the Act except by experts on the subject, who alone know what is the probable value of the proposed experiment, and who alone know the qualifications of the investigator who is making the application.

16102. I rather gathered from the Report of the previous Royal Commission that they contemplated the Home Secretary calling to his aid persons skilled, no doubt, but not necessarily belonging to any Association?—I suppose that the paragraph relating to the countersigning of certificates includes that; that is to say, that they thought it would be quite sufficient to have the names of two individuals as guarantees. The Association for the Advancement of Medicine by Research was not in existence at that time, and consequently the Commissioners, I take it, did not think of an Association doing the work. As a matter of fact, both parties do the work now; the work is done doubly—it is done, as contemplated by the original Commissioners, by two individuals who countersign the certificates; but, in addition to that, all the papers go to the Association for the Advancement of Medicine by Research for further investigation.

16103. Is there anyone who plays the part of *advocatus diaboli*?—In the Association, do you mean?

16104. No, in the advice obtained by the Home Secretary?—Not that I know of. I take it that the Home Secretary himself is both judge and advocate in that matter. He certainly is the judge.

16105. You have laid great stress upon the importance of vivisection as necessary for the teaching of students?—I have.

16106. Does that apply to all medical students?—To every one.

16107. In physiology, first of all, you mentioned?—Yes.

16108. Then in pathology?—Yes.

16109. In both those cases would it mean practice by students as well as demonstrations?—Certainly in physiology. As regards pathology, I think it ought also to be so; but, then, I am a pathologist, and perhaps I think that pathology ought to take a larger place in the curriculum than it does.

16110. And is the same thing true in pharmacology?—Yes.

16111. And in forensic medicine?—The tendency now, you know, is for special classes of forensic medicine to be given up. There is much less time spent in forensic medicine than there was when I was a student, because pathology is taking its place; so that prac-

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tically forensic medicine you might group under the head of pathology.

16112. Then you also suggested that experiments on animals should be employed in a course of anæsthetics?—That is most essential.

16113. By every student?—Yes; I had proof of that this very morning in an operation.

16114. Do you care to amplify that?—No; it might be painful. It was a question of sheer ignorance from want of having seen the condition in an animal first, and inability consequently to diagnose movements which were asphyxial in character, as contrasted with movements which were purposive in character; and, as you can see, that might be a very fatal mistake.

16115. Then, again, in surgery you advocated that all students should be instructed by vivisection experiments, with a view to obtain manual dexterity?—Yes; first with a view of obtaining a knowledge of technique, and secondly with a view of obtaining manual dexterity —the words in the Act.

16116. And you have no reason, from your own experience, to think that the effect of witnessing experiments on living animals is demoralising to students?—I know that it is not. It is a sheer invention on the part of those who offer such evidence.

16117. You quoted a portion of the Report of the Royal Commission of 1876 on that subject?—I did.

16118. There is a preceding portion of the sentence which I do not think you quoted; may I call your attention to it: "But the tendency to demoralisation is connected, as the shadow with the substance, with the rightness or the wrongness of the thing itself"; and then follow the words you quoted: "And the evidence we have quoted above seems to show conclusively that at the medical schools where such demonstrations are exhibited under anæsthetics the sense of humanity in the students is not in fact impaired"?—Quite so. I do not see how the previous words in any way alter the sense of the part that I quoted.

16119. In an earlier paragraph of the same Report they say: "It is manifest that the practice is, from its very nature, liable to great abuse; and that since it is impossible for society to entertain the idea of putting an end to it it ought to be subjected to due regulation and control." And, further: "It is not to be doubted that inhumanity may be found in persons of very high position, as physiologists. We have seen that it was so in Majendie." Apparently the Commissioners had it in their mind that there was peril in the permission of these experiments, and need for circumscribing them and watching them?—Undoubtedly they had it in their mind, although they had no absolute evidence before them that such a thing as inhumanity actually existed in the schools in England.

16120. They called attention to Majendie?—Yes; but Majendie was in 1820; that was 50 years before that Commission.

16121. (*Chairman.*) In France?—Yes, and in France; he was not in England at all. The English medical profession had always discountenanced anything like the giving of unnecessary pain in experimental work, and certainly at that time, as I have already stated in my evidence, demonstrations had been given at University College for some years, and there was no idea of demoralisation of students or anything of the kind. There had been, on the contrary, a great accession of interest and of educational value in the course of physiology.

16122. (*Sir William Collins.*) Do you think that there is such a distinction to be drawn between the practice of vivisection abroad and its practice in England?—At the present time?

16123. Either at the present time or formerly?—Formerly, no doubt, the English medical profession, as a profession, like the public, were more humane than their continental brethren, but at the present time if you read any scientific record of experimental work, you will find that the animals are all anæsthetised.

16124. Then do I rightly understand that you do not draw any distinction between vivisection, as now practised on the continent and in America, and as practised in this country?—I cannot say that I draw no distinction, because we know perfectly well that the Romance Nations, for instance, have not the same natural regard for animals that we have. They do not value animal life or regard animals in the same way that we do.

16125. (*Mr. Ram.*) An English crowd would not stand a Spanish bullfight?—Certainly not.

16126. (*Sir William Collins.*) Do not the Spanish retaliate upon the English foxhunt?—Yes, they may. It is a question of degree. That is not a question really which has ever appeared to me to have any bearing on this subject. We are concerned here with our work in the English medical schools, and it does not matter what goes on on the continent; we have nothing to do with it from a legislative point of view, and therefore it always seems to me utterly irrelevant to the subject, so far as we are concerned as a profession.

16127. You referred us to the Report of 1876, and I was anxious to know whether these paragraphs were present to your mind?—Yes, that paragraph relating to Majendie was.

16128. And his "infamous" experiment, as Sharpey call them, according to that Report?—Yes.

16129. You spoke of there having been no evidence before the last Commission of "inhumanity' in this country; they had the evidence of Dr. Klein, had they not?—Yes.

16130. You said that you were unable, or you omitted, to give us that quotation which you kindly promised about Sir Frederick Treves. It was, I understood you to say, a contradiction of something which is frequently quoted from his works?—The original quotation, I think, was in a lecture.

16131. Is this the original quotation, that we may have it clear? This is from the "British Medical Journal," of November 5th, 1898, in the report of an address by Sir Frederick Treves on "Some Rudiments of Intestinal Surgery," in which he stated: "Dr. Halsted, by the way, expresses his conviction 'that there should be a law compelling all surgeons to practise on animals the operations for circular suture of the intestines and for intestinal anastomosis.' I hope this view will not commend itself to the legislators of this country. Many years ago I carried out on the continent sundry operations on the intestines of dogs, but such are the differences between the human and the canine bowel, that when I came to operate upon man I found I was much hampered by new experience —that I had everything to unlearn, and that my experiments had done little but unfit me to deal with the human intestine"?—Yes, that is the reference. I have obtained for you Sir Frederick Treves' answer to it.*

16132. I am much obliged to you?—Of course, Sir Frederick Treves could answer for himself, if the Commission desired it, naturally, but may I point out also that he did those experiments many years ago, and that is why I should not accept his experience any more than the personal experience of a man who had done experiments which were no use to him. I may suggest, with all respect to Sir Frederick Treves, that we have, many of us, done experiments which have been failures.

* The witness subsequently forwarded the following letter:—

To the Editor of the Times.

Sir,—My attention has been drawn to a letter in the "Times" of April 9th, signed by Mr. Trist as Secretary of the London Anti-Vivisection Society. In this letter it is stated that I have testified as to the fallacy of vivisection.

My solitary utterance on the subject of vivisection is contained in an address delivered at Birmingham, in October, 1898 (Lancet, November 5th, 1898). Speaking of the suturing of intestine, I said that I had found out that operations upon the intestines of dogs were useless as a means of fitting the surgeon for operations upon the human bowel.

Those who are familiar with the controversial methods of the anti-vivisection party will not be surprised that certain of my remarks have been cunningly isolated from the context, and have been used (in advertisements, pamphlets, and speeches) to condemn all vivisection experiments as useless. The fallacy of vivisection can hardly be said to be established by the failure of a solitary series of operations dealing with one small branch of practical surgery.

No one is more keenly aware than I am of the great benefits conferred upon suffering humanity by certain researches carried out by means of vivisection.

I am, Sir, your obedient servant,

FREDERICK TREVES.

6, Wimpole Street, W.
April 14th.

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16133. We have also had our attention called to a passage from Koch's work on the cure of consumption, in which he said: "Here, again, is a fresh and conclusive proof of that most important rule for all experimentalists, that an experiment on an animal gives no certain indication of the result of the same experiment upon a human being." That, I gather, you would not agree with?—I do not agree to that at all.

16134. Now, in your *précis* you refer to the morals of the question of vivisection and anti-vivisection?—Yes.

16135. You gave us the views of the British Medical Association on the morality of vivisection and the immorality of anti-vivisection?—Yes.

16136. I understand from you that, in your opinion, to experiment upon man is immoral?—Certainly. A modification in surgical procedure might be a trivial matter, in that it did not involve any risk to life, and that, if tried on a man first, would by many people be called an experiment. I should not object to that if there was really no risk to the individual on whom it was tried, and, of course, that has been the practice of all surgeons who have not first tried their methods on animals; it has been a common practice, I mean, up to the present time. But I suggest that as a general principle, it is undoubtedly immoral. If a new idea or a new method is to be tried, it certainly ought to be tried on an animal first, because, as I have pointed out in my previous evidence, it is not only that you do not know the result until you have made the experiment, but you actually do not know that your method of performing the procedure is going to be the best.

16137. Then you speak of the morality of vivisection as against the immorality of the anti-vivisectionists?—Yes.

16138. And you suggested that the anti-vivisectionists were immoral in utilising knowledge which had been obtained by means of experiments on living animals?—Yes; I suggest that they are immoral from two points of view. I suggest that they are immoral, in the first place, because they oppose means whereby suffering and disease may be prevented, and I think it is immoral to do that—to oppose means whereby we can obtain new procedures against disease and suffering; that is the first thing. Then, inasmuch as they hold that vivisection is immoral—that it is immoral to use animals for these purposes—I say that it is immoral of them to make use of knowledge gained by vivisection. And I think that is the general public view also. I repeat what I said before—that the money for a so-called anti-vivisection hospital in Battersea was collected on that very ground, and yet now, at the present time, evidence has been laid before the Commission by anti-vivisectionists—Mr. Coleridge, Dr. Stephen Smith, and others, claiming that they are justified in making use of the knowledge derived by vivisection.

16139. Mr. Tomkinson put it to you, when he said that, in your opinion, all pursuit of knowledge was moral, and any obstruction to the pursuit of knowledge was immoral?—Certainly.

16140. But human vivisection, in your opinion, would be immoral, I understand?—Yes.

16141. And if knowledge could only be obtained by experiments on man there would be a conflict between your morals and your pursuit of knowledge, would there not?—No. The moral question would then come in in the sacrifice of the man; and if a man was a scientific observer, who chose to sacrifice himself, I should say that he was not doing an immoral act, unless he caused some serious disability to himself, as some men have done.

16142. But take the case, not of experiments upon the man himself, but upon other persons—involuntary experiments?—I do not approve of them.

16143. For instance, when Lady Mary Wortley Montagu introduced inoculation into this country, the practice was started upon some criminals in Newgate, I think. Do you think that was moral or immoral?—I think it was immoral.

16144. Recently I think Dr. Garnault, in Paris, has inoculated himself with tuberculosis?—Yes.

16145. Do you think that is moral or immoral?—I think it is decidedly immoral, because practically it was one form of committing suicide, or it might be.

16146. That is to say, it would be immoral to expose a human being to peril or disease?—Yes, to disable him.

16147. The knowledge obtained from experiments on human beings in that way would, in your opinion, be immoral knowledge?—I think it would be immorally gained.

16148. And you ought not to use it?—Oh, no.

16149. I understood you to say that the knowledge obtained by an immoral procedure ought not to be used; that it was immoral to do so?—What I said was that persons who held that vivisection was immoral were not entitled to use the knowledge thus gained.

16150. But you hold that human vivisection is immoral?—Certainly.

16151. Would you not be similarly barred from using knowledge obtained by human vivisection?—Certainly.

16152. That is what I put to you?—I beg your pardon. I did not understand your question. I certainly think so.

16153. Was not some knowledge obtained by the experiments of Herophilus upon human beings?—We are told so, but it is very uncertain really whether he actually did vivisect human beings. It is said that he anatomised them.

16154. Is it not said that he dissected alive some 600 persons?—The allegation is made.

16155. He was so charged by Tertullian, was he not?—Yes, I believe so, but we do not actually know what knowledge he did gain by those alleged vivisections of human beings.

16156. Have you not called attention to the work of Herophilus in your book on "The Brain and Spinal Cord"?—Yes. I have quoted him in my historical narrative.

16157. Do you suggest that he, by his investigations, added knowledge with regard to the nervous system?—He is alleged to have done so.

16158. Was he "the first to discover the peripheral nervous system or nerves, that these latter were connected with the brain and spinal cord, and that they conveyed sensory impressions"?—He was so alleged.

16159. Do you not allege it yourself?—No, I quote it, but I should be very sorry to vouch for its accuracy, because, as a matter of fact, the most detailed experiments on conduction are those of Galen, which I also quote.

16160. Was Galen not charged with experiments on human beings?—He was accused of it.

16161. He did use a good many animals for experiments, did he not?—Yes, the animal apparently that he used was the pig, and in the mediæval period they also used the pig.

16162. I gather from your book that you set a good deal of store upon the experiments made by Galen?—Yes.

16163. Have the results which he obtained remained absolutely correct until the present day?—Yes, they are the foundation of our knowledge.

16164. There was a piece of evidence given by the late Sir James Paget before the last Royal Commission, upon which I shall be glad to have your view. He said: "There must be reason and moderation in the use of these experiments"—that is, vivisection experiments. "I can quite believe that ardent physiologists put more trust in the experiments on living animals than I should; and certainly those studying therapeutics and diseases think more of them than I should. I think more of the advantage of clinical inquiry. But I am very anxious that there should not be a general condemnation of experiments on animals, since it seems to me that there are a number of things absolutely essential for the life of man that cannot be ascertained by other means." Do you agree with that?—Certainly. What Sir James Paget is saying there is that clinical observation is very useful and that experiments are useful.

16165. (*Chairman.*) You say that you agree with that, but I rather gathered from the evidence which you have given before that you place a good deal more faith on experiments on animals than Sir James Paget wished to express in that answer?—I cannot extract that from Sir James Paget's statement. Sir James Paget is saying there what everyone must agree with.

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16166. He says: "Some things," "in some instances." That was what I was referring to.

16167. (*Sir William Collins.*) It was rather a question of the relative importance of clinical inquiry and experimental inquiry?—I do not agree that there is any relative importance in any method of acquiring knowledge in our work. It has often been alleged, for instance, that anatomy is more important than physiology. It has been alleged by anti-vivisectors that experimenters, as they call them, have neglected anatomy for physiology, and in the same way it might be alleged, or it might be deduced from Sir James Paget's remarks, that persons like myself would neglect clinical observation for experiments in the laboratory. It is a monstrous suggestion altogether; gether; it is perfectly monstrous.

16168. (*Chairman.*) I do not understand Sir James Paget suggesting that; it may be true in some cases, but I do not think he is suggesting it. I think that what he suggests is that, out of the sum of human knowledge obtained, he inclined to the view that more has been gained from clinical observation and less from animal experiments than you would put it. That is all I understand him to say?—I suggest that an attempt to make a comparison of that sort is absurd from the scientific standpoint. Both avenues of knowledge, if properly pursued, give us new ideas.

16169. I think you hardly follow me. I am not suggesting that Sir James Paget says the reverse of that in the least; he says that both avenues will bring results, and they are not put in conflict at all?—No, I am sorry that I did not understand you. I know the point that has been raised again and again; it is suggested that clinical observation is better than experimenting in a laboratory. All I maintain is that such a statement as that is absurd from the scientific standpoint. There are some things that you can gain from clinical observation which you could not see in a laboratory. Of course, that goes without saying, and *vice versâ*. You must do both things; you must have all these avenues of work open.

16170. (*Sir William Collins.*) The President of the Royal College of Surgeons, when he was here, told us, I think, that he had never himself practised vivisection, and he did not advise the practice of vivisection for the purpose of acquiring manual dexterity. I gather from what you have told us that you disagree with that?—I totally disagree with that, of course.

16171. I gathered from some of his replies that he relied upon his experience in operations on the human subject?—He is obviously one of those who have learnt on man. He has gained his experience by operating on man.

16172. That you regard as immoral?—Certainly. I think the more moral proceeding is for a man to have learnt by experiments on animals first.

16173. (*Chairman.*) Do you mean by experimenting or by operating on patients?—By operating on patients.

16174. What you said was immoral was not operating on patients, but experimenting upon them?—That is what I meant.

16175. (*Sir William Collins.*) I understood you to say that knowledge obtained as the result of experiments upon human beings in the course of surgical operations was, in your opinion, immoral?—The use of the terms "moral" and "immoral" are relative; but what I say is, that I think it is, if you like to put it so, a more moral proceeding for a man to gain that knowledge of technique, and so forth, by trying first on an animal the experiment, or by being educated by operating on an animal, before he operates on a man.

16176. That introduces us into the sphere of relative morals?—Well, it is the basis of the whole of education. Are you going to gain your experience on the bodies of persons that come into your hands, or are you going to gain it in the laboratory by experiments on animals? That is what it comes to; you cannot shirk the question.

16177. I took the notion of relative morality from yourself?—Yes, certainly.

16178. We have many times had the question of reflex action and purposive movement before us, and I shall be glad to get from you, as a great authority on neurology, what you really mean by reflex action?—By a reflex action I mean an automatic, which may be, and usually is, an unconscious response to a stimulus.

16179. Is reflex action ever conscious?—That depends upon the sense in which you use the term. A perfectly conscious being may execute a reflex action. For instance, if somebody suddenly stuck a pin into my hand I should certainly draw my hand back, and of course I would be perfectly conscious; but there would be nothing purposive in that act of mine; it is purely reflex.

16180. Would it not be a purposive act?—Not at all; the time is too short. In my opinion, a purposive act is a conscious act.

16181. Is a purposive act always a conscious act?—In my opinion, a purposive act is a conscious act.

16182. (*Mr. Ram.*) What is a sneeze?—It is purely reflex.

16183. I am conscious when I sneeze?—Exactly; that is what I say. You must not introduce the connotation of consciousness in the discussion of reflex action, because the latter only means that the response in the nervous system is executed within an extremely short period as contrasted with a response which involves the higher centres, and therefore takes a longer time to execute. But if you introduce the question of consciousness or unconsciousness, then you must add a great deal of analysis of higher functions, especially in regard to this question of anæsthesia.

16184. (*Sir William Collins.*) You have called attention to that, I think, in your book on the "Brain and Spinal Cord," where you say: "A vast array of writers have discussed the question as to whether we are to regard the lower animals as possessing, as they say, consciousness, and from this have even proceeded to formulate ideas as to whether they have a soul; while owing to the remarkable discoveries of the pathologists about 40 years ago as to the functional activity exhibited by the spinal cord of the frog, when separated entirely from the brain, an animated debate was actually excited as to whether the spinal cord possessed the attributes of the mind"?—Exactly, that was so.

16185. G. H. Lewes, I think, took part in that discussion?—I had forgotten that.

16186. Is a frog deprived of its cerebral hemispheres capable of purposive acts?—It is capable of most complex reflex acts which might be regarded by an ordinary observer as purposive, but to argue from that to purposive acts such as are performed by warm-blooded animals, with an intact nervous system and fully conscious, is quite another thing.

16187. Are those acts of a brainless frog conscious acts?—No; in my opinion, there is no connotation of consciousness unless you have an activity of the highest centres present. The cortex of the human brain is undoubtedly necessary for the purpose of consciousness.

16188. In your *précis* you state that "no experiments on unanæsthetised animals of a painful character have been performed in the United Kingdom during the administration of the Act"? So far as I know, there never has been a cutting operation performed on animals without anæsthetics.

16189. I could not quite harmonise that with what Dr. Thane told us in answer to Question 457, where he is asked, "Are there any experiments that you can describe as being really painful"?—That is another thing. He means painful after an inoculation. I am not aware of any application ever having been made to the Home Office for a cutting operation to be performed without anæsthetics.

16190. Then, when you say that no "experiments of a painful character have been performed on unanæsthetised animals in the United Kingdom during the administration of the Act" you are prepared to allow that there were these painful experiments which were referred to by Dr. Thane?—Not at all. I do not know what Dr. Thane is alluding to except inoculations. I am sure, at least from my own personal knowledge, that he was not alluding to a cutting operation performed without anæsthetics. The question has been raised again and again.

16191. I was about to read his answer. I thought you knew what it referred to?—I beg your pardon.

16192. His reply was, "On the other hand, it is certain that in some cases of this group"—that is, experiments performed under Certificate A——"—Yes, those are inoculations.

16193. "The infection or injection is followed by great pain and suffering. I may mention the injection of tetanus toxin and the infection with plague; also the insertion of certain drugs"?—Yes, none of those are what I stated. I was referring to cutting operations.

16194. Are they not performed on non-anæsthetised animals, and under the administration of the Act?—Yes, but they are not cutting operations.

16195. But I did not gather that the passage which I have read to you contained that proviso?—What I referred to in my *précis* was a statement which I have made again and again in the public Press, and which has never been challenged, that no cutting operation on an animal has ever been performed in this country without anæsthetics under the administration of the Act.

16196. I have the words of your *précis* before me, and they are: "No experiments on non-anæsthetised animals of a painful character have been performed in the United Kingdom during the administration of the Act"?—Exactly, and I have explained to you what that sentence refers to, namely, cutting operations.

16197. Without that elucidation it was not perfectly clear, to my mind at any rate?—Because you hardly realise that a *précis* does not necessarily convey everything that is in a man's mind.

16198. It shows the advantage of a little examination on the *précis*?—Quite so. But the anti-vivisection party have repeatedly alleged—in fact, it is their whole stock-in-trade—that cutting operations are done in this country on animals without anæsthetics; it is part of their business to show that; and, as I have stated, under the Act it is impossible, because no application has ever been made to the Home Secretary to perform such operations.

16199. Now, with respect to the experiments of Dr. Crile's to which you have called our attention, and which have been so often before us, they were done, I understand, under your guidance and at your suggestion?—Yes.

16200. And they were continued in America, I think, subsequently?—Yes.

16201. Did the series in America differ from those which were performed in this country?—I do not know.

16202. What was the conclusion at which Dr. Crile arrived as the result of his experiments?—That there were changes in the blood pressure of a marked character—changes which we ought to contend against in operations on man.

16203. I see that the conclusion of his paper is set out thus. Is this correct. "Surgical shock, then, is due mainly to a vasomotor impairment or breakdown. The cardiac and respiratory factors may be of considerable importance. However, the main effect is on the vasomotor mechanism. If the foregoing be true, it will be seen how much more important is prevention than treatment. Prevention of shock may best be accomplished by taking into account all the known physiologic functions of every tissue and organ of the body in a way that would suggest itself to any practical surgeon. While the cause may be local, the treatment must be general. It would seem to be desirable to direct special attention to the distinction made between collapse and shock; the result of action is reaction; of rest is restoration." That was the conclusion of the whole matter, was it?—Yes.

16204. And I understood you to say that these experiments of Dr. Crile exactly reproduced what was done on a human being?—Yes.

16205. Among those experiments was one—I think the first experiment—of cutting out both cerebral hemispheres?—Yes.

16206. Have we an analogue to that on a human being?—No, I did not say that all the experiments of Dr. Crile exactly reproduced what was done on a human being.

16207. That description would not apply to them all?—Certainly not. If you wish to analyse any physiological function you must introduce into the experiment various operative procedures to exclude the functions of different parts, which, of course, is not necessarily a part of the procedure of an operation on a human being.

16208. Tearing out the brachial plaxus was another?—Yes, that was to imitate a well-known fly-wheel accident which is not uncommon in factories.

16209. Crushing the foot extremely before the corneal reflex was abolished was another?—Yes.

16210. That, you say, tarsectomy represents?—I said that tarsectomy reproduced the crushing of the bones of the feet. I should say that a railway accident also correctly represented the experiment you are describing now.

16211. Tarsectomy had been performed long before Dr. Crile's experiment, had it not?—Yes, all the operations for divisions of bones had been performed, of course, long before anæsthetics even were given.

16212. Then crushing the testes and other parts?—Yes, that is a most important question, because the operation of castration has frequently been followed by very grave shock—fatal shock.

16213. I did not quite understand you. I thought you suggested that there was some analogous operation on the human body to the experiment of Dr. Crile of pouring boiling water on to the intestine?—Pouring boiling water into the abdominal cavity. It does not matter whether it is on the intestine or the abdominal wall.

16214. Any procedure in which boiling water was used?—Yes. I quoted the injection of scalding steam into wounds of the liver.

16215. Would the temperature at which that steam touches the liver be the same as that of boiling water applied to the intestine?—Certainly.

16216. Really?—Practically. You get the nozzle of the injector close to the liver; that is the whole object of the procedure—namely, to produce instant coagulation at a scalding temperature.

16217. How is the jet of steam produced?—By a boiler under pressure.

16218. Do you think that the temperature applied to the organ by that means would be that of 212 deg. Fahrenheit?—No, not absolutely, but practically the same thing. You must produce a scalding effect, and the question is whether it will produce special shock.

16219. Had that method not been employed before Dr. Crile's experiment?—Oh, yes.

16220. The President of the Royal College of Surgeons told us, I think, that he did not like many of these experiments of Dr. Crile. Tastes differ?—I do not know what anybody means by liking or not liking an experiment. To my mind, as I said the other day, in answer to Mr. Tomkinson, about the use of the word terrible, or disagreeable, and so forth, if an animal is anæsthetised all those expressions are absurd. If the animal were not anæsthetised, then, of course, it would be quite another question.

16221. Do you mean that if these experiments of Dr. Crile's had not been performed under anæsthetics they might be described as outrageous?—Certainly, because here we are armed with anæsthetics, and it would have been the infliction of terrible pain—well, wickedly.

16222. The introduction of anæsthetics into these experiments, in your opinion, made all the difference between their being outrageous and, shall I say unfairly, moral?—Certainly, at the present day, because the whole idea was to investigate conditions of shock as they arise in operations on the human being, and no one would dream of applying such procedures to a human being in a conscious and sensitive state.

16223. Did I understand you rightly to say that you would have no objection to your own pet dog, if it were under anæsthetics, being submitted to those same experiments?—Certainly, if my own pet dog was to be killed and it was put under anæsthetics, I should not care what was done to it as long as it did not recover consciousness to pain. Why should I?

16224. I am afraid that I am not in a position to make replies?—But you asked me a question, and I think there are deductions from questions which one is sometimes entitled to comment upon.

16225. Have you heard of some experiments which Dr. Watson performed in America, of dropping dogs from a height?—Yes. I know of those experiments.

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16226. Were they, in your opinion, valuable experiments?—I cannot at the moment call to mind whether they revealed any new conditions; I should have to look them up again to see whether, in my opinion, they really did add to our knowledge. I cannot at the moment express an opinion.

16227. Were they justifiable, in your opinion?—Certainly.

16228. Were they performed under anæsthetics?—I cannot tell you that, but I should say that the mere dropping of an anæsthetised dog from a height in order to obtain concussion of the spinal cord, would be a very useful experiment, if it was followed up by an anatomical investigation of the spinal cord, and I can see no objection to it whatever, if the dog was anæsthetised.

16229. Were those experiments of Dr. Watson's severely condemned by the "British Medical Journal"? —Quite possibly, if the dogs were not anæsthetised.

16230. That journal is the organ of the Association on whose behalf you are giving evidence?—Yes.

16231. With regard to the thyroid and myxœdema we have had a great deal of evidence on that subject. Should I be right in thinking that clinical investigation had, prior to your own experiments, may I say, suggested a close relationship between the diseases and defects of the thyroid gland and the cretinoid condition of a human being?—No. Sir William Gull, in speaking of those patients as cretinoid, had no pathological investigation of the case before him. The only pathological investigation that was made at all was Dr. Ord's. The word cretinoid was used by Sir William Gull simply from the appearance of the patients, but he, of course, had no idea whatever that it had anything to do with the thyroid gland. Dr. Ord's case was the first case in which the thyroid gland was shown to be degenerated, as well as other organs in the body.

16232. In Dr. Ord's case he showed that the diminution of the thyroid gland was associated with an almost complete annihilation of the proper gland structure by the degenerative material, did he not?—Yes, but all the clinical work, as I have pointed out, was subsequent to Schiff's experiments in 1859. Mine were not the first experiments on the subject.

16233. Had not Hirsch pointed out that goitre and cretinism were a pair of diseased types closely related in their pathology and etiology?—Yes.

16234. That would be prior to 1885?—Yes, certainly.

16235. And had not Hilton-Fagge contributed some cases on sporadic cretinism occurring in England as long ago as 1871?—Yes.

16236. Had he not pointed out there that there was no thyroid gland to be detected?—I do not remember that, but, of course, the connection between sporadic cretinism and goitre had been pointed out a great many years before that.

16237. Do you remember Curling's work in 1850, in which he published two cases of absence of the thyroid body connected with defective cerebral development, where he showed that there was not the slightest trace of the thyroid body in those cretins?—No, but it is quite possible. Many cretins, on the contrary, have a very large thyroid.

16238. Had not Fagge suggested that the presence of goitre was protective against cretinism?—I dare say, but the clinical knowledge on the whole subject was in a complete fog, of course.

16239. But through the fog was there not emerging a notion that cretinism was combined with absence of the thyroid gland?—No, I do not think so at all, because many cretins have enlarged thyroids.

16240. Did not Curling himself in 1850 call attention to the fact that cretinism was combined with absence of the thyroid gland?—I do not know. I do not know the case that you refer to—the paper of Curling's.

16241. When was the first successful operation in this country of transplanting the thyroid gland of sheep into a human subject?—That I do not know. I do not know who did the first transplanting of sheep's thyroid at all.

16242. Have you never heard of a case?—Oh, yes, I have a sort of idea, but I really cannot answer that question.

16243. You were, I think, on some Commission or committee that inquired into tuberculosis in animals? —Yes, it was a Departmental Committee.

16244. It was a Committee of the Privy Council in those days?—Yes.

16245. You are not, I think, a member of the present Tuberculosis Commission?—No, I am not.

16246. Do you accept the view that the so-called tubercle bacillus is the cause of tuberculosis?—Of course.

16247. And is it capable of being transmitted hereditarily?—Do you mean by the spermotozoa to the ovum, or in the ovum itself?

16248. The reason I ask is because of your own special report upon that former Committee?—My minority report of one, you mean?

16249. May I read the words, to remind you of what you wrote: "Tuberculosis is notorious, even among the laity, as a disease which is transmitted from parent to offspring. This is a fact with which cattle breeders are specially familiar, and which finds strong expression in the evidence attached to this Report. Further, this generally received truth has been completely confirmed by the results of scientific investigation, as is also duly set forth in this Report." That is to say the result of scientific investigation, prior to the report of that Departmental Committee, led to the conclusion that tuberculosis was hereditarily transmissible?—Yes, that was because Baumgarten had found the bacillus in the ovum in a rabbit, and Johns in a new-born calf. Of course, if that occurred, clearly you could say then that that individual if the embryo developed, in spite of the bacillus, would be an instance of hereditary transmission.

16250. Is the hereditary transmission of tuberculosis accepted by pathologists to-day?—Not in man, so far as I know.

16251. In animals?—That I do not know. But the question then before the Departmental Committee was the question whether certain infected prize bulls communicated the disease, and the evidence before the Committee was to the effect that they did.

16252. And you suggested that there should be legislation to prevent breeding from those tuberculous animals?—My Minority Report went a great deal further than that. The reason why I was in a minority of one was because I proposed the extirpation of the disease by stamping out, and by the compensation of the owners; but none of my fellow Committee-men would report in favour of compensating the owners.

16253. But there was also this point of legislation with regard to preventing breeding?—Naturally, but that was only a side issue compared with the enormous question of compensation.

16254. I should like to ask you a question in regard to the evidence given by Sir William Fergusson, who was a distinguished surgeon, of course, before the last Royal Commission. In their Report the Commission say: "Sir William Fergusson thinks that experiments which involve suffering are carried to a greater extent than they need be, and that there is continued and useless repetition. His own opinion is much less favourable to these experiments than it was when he was young, because he had much less grasp of the subject at that time. The more mature judgment of his later years has led him to say to himself that he would not perform some of the operations now that he performed in his earlier years. He thinks if the public really knew what was actually going on in this country at this time they would expect an interference on the part of the Crown and Parliament"?—I am perfectly well aware of that statement of Sir William Fergusson's. I always thought that it was a most unwarrantable statement for any man to make, from my knowledge, at any rate, of what went on in a medical school. I do not in the least degree know to what he was referring in the matter of detail, and I am not sure that the Commission pressed him on that point at all.

16255. (*Chairman.*) The operations in his early days of which he speaks were performed before anæsthetics were used?—Certainly. Sir William Fergusson was a student in Edinburgh.

16256. About what time?—I suppose about 1830.

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and the description of his time, and the share that he took in resurrecting bodies, and so on, is what is called a romantic story. I cannot see any romance in that kind of thing, but I see a great deal of romance in that answer of his, so far as medical school work was concerned in 1876. I do not believe for a single moment that there is an atom of truth in his innuendo, at least, of that statement, that the physiological medical schools of this kingdom were committing cruelties by unnecessary experiments on animals.

16257. (*Sir William Collins.*) 1876 was, of course, long after the introduction of anæsthetics?—Long after; but you have the evidence before you; I do not remember his particularising in his evidence an instance of such cruelties. I do not believe that there is such an instance in the evidence you hold in your hand.

16258. Chloroform was discovered by experiments on a human subject, was it not?—The actual use of chloroform as an anæsthetic agent was practically discovered by Simpson inhaling it himself, and giving it to his friends.

16259. (*Sir William Church.*) It has been put before us several times in this room that the essential feature of Listerism was the use of the spray. Is that a total misconception?—Absolutely.

16260. Do you remember in what sort of cases Lord Lister first made use of his antiseptic treatment?—I think it was in regard to compound fractures, in which his celebrated putty was used, carbolic putty, in order to exclude organisms.

16261. Was it not his principle to try and turn a compound fracture into a simple one; that is to say, where the skin was not broken?—Exactly.

16262. And to cleanse any protruding part first with carbolic acid in order to destroy any organisms which had got into it?—Precisely.

16263. And then to protect the wound and make it into practically the same condition as if the skin had not been broken?—Yes, he covered it over with this antiseptic putty.

16264. And the spray was not used?—It was not used at all in those days.

16265. In your answer to Question 15811, you are speaking of the necessity for demonstration in teaching physiology, and you are asked: "Professor Gotch here stated that for two years he had not used living dogs for teaching purposes, and Dr. Pembrey admitted before the Commission that during all this year, at least, he had not vivisected any animals at all before his class. From that you would rather criticise the efficiency of the teaching?" and you say, "Certainly, I would." I want to be quite clear. Does that answer apply to Professor Gotch and Dr. Pembrey?—No, it applies only to the second part, because Professor Gotch gives physiological demonstrations.

16266. You were not criticising Professor Gotch's teaching, because he had only used rabbits instead of dogs in illustration of his lectures?—Not in the least. I was criticising Dr. Pembrey's statement that during all this year he had not vivisected any animals at all before his class. I think he ought to have done so.

16267. Then, in answer to Question 15952, you say: "Undoubtedly the Act ought only to apply to warm-blooded animals"—would you leave cold-blooded animals entirely outside the Act?—Yes, practically it come to this, that the frog, which, of course, is a cold-blooded animal, is used enormously in teaching physiology, yet everything can be shown on a frog after its decapitation.

16268. You say in the next answer: "It is perfectly ridiculous for fish, and so forth, to be included"?—Yes, if you take fish, for instance, I think that it is unnecessary to anæsthetise fish.

16269. I suppose you think that they suffer pain?—I am not at all sure of that. Now, I come back to an answer which I gave earlier this morning there is no pain without consciousness—that consciousness in man is undoubtedly associated only with the complete function of the cerebral cortex. As you descend the animal kingdom you at last arrive at the fish, and you find that the fish has no cerebral cortex at all. I do not, therefore, regard fish as possessing either consciousness in any way analogous to what we call consciousness or sensitiveness to pain.

16270. Shall we take the snake?—The snake has a very thin semblance of a cortex, but a fish has no cortical grey substance at all.

16271. Do you really think that the invertebrate kingdom suffer no pain?—Certainly. I do not believe that the invertebrate kingdom are at all conscious of pain. If you apply a powerful stimulus to an invertebrate, it shrinks up undoubtedly by a reflex or defensive act, but I do not think that that act is accompanied by any pain—certainly nothing in the least degree approaching what we have any idea of.

16272. Then you would not even apply an Act of Parliament dealing with cruelty to animals to cold-blooded animals and invertebrate animals?—To take a case in point, you mean that if some boys were found torturing a frog they ought not to be punished?

16273. Yes?—I do not agree to that. That point has already been frequently discussed, of course, in connection with the moral right of wild animals; I mean as regards the statutes. I do not think that point really bears upon my position at all. I am referring now to legislation affecting the performance of an experiment for the sake of gaining knowledge. I do not think it is in any way analogous.

16274. You would leave, then, cold-blooded animals, that is to say the animal kingdom, from birds downwards, outside an Act dealing with experimentation on animals?—Certainly.

16275. And you would not agree with Professor Langley, who thought it was desirable that they should be under a separate clause?—What is Professor Langley's statement?

16276. He said, in answer to a statement at Question 15399: "I think it would be better to have a separate certificate for cold-blooded animals, like fish, frogs, etc."?—I cannot see the advantage of it at all, because, of course, here again we have the question of the certificates, and may I point out once more that my proposed amendment of the Act would provide the Home Secretary with absolute knowledge of whether it was going to be done on a warm-blooded animal or a cold-blooded animal, and, if belonging to either class, which animal? All those details would be furnished to the Home Secretary, and consequently there is no reason to construct a special certificate for cold-blooded animals; it would be unnecessary. And, if Professor Langley's point was that a fish, for instance, was conscious of pain, as I have said before, I have given my reasons to the contrary.

16277. I do not wish to pursue it. Professor Langley's words were: "I think they certainly must suffer very little"—that is pain—"but I think, again, as a matter of public policy, it is difficult to press for their exclusion"?—I did not know that I was here to answer on questions of public policy.

16278. But those are the words that Professor Langley used in reply to the question, whether there was any need to include cold-blooded animals in the Act?—As I have stated, I do not think that there is any need, and in connection with fish, I have given reasons for supposing that they do not suffer pain. I see that Professor Langley says here: "The organisation of the part of the brain that is connected with pain in man is so small in those animals that it cannot have any very large function"—certainly as regards teleostean fish it has no function at all; consequently, I do not believe that fish suffer pain. There is plenty of direct evidence, of course, on that point, of fish that have taken a fly a second time, and so forth, when the hook is sticking in the side of their cheek. However, I need not waste the time of the Commission on that.

16279. You would agree with Professor Langley, but you would go a little further than he does; you say that you think that they should be excluded?—Yes, you must draw the line somewhere for the purposes of a statute, of course, and therefore I think the best line would be between the warm-blooded animals and the cold-blooded animals.

16280. (*Chairman.*) Are anæsthetics sometimes administered to animals through a tracheal tube?—Yes.

16281. And for that purpose it is necessary first to perform the operation of tracheotomy?—Yes.

16282. In those cases are the animals anæsthetised when you open the throat?—Yes, of course.

16283. They are previously anæsthetised?—Yes.

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16284. (*Chairman.*) You are a Fellow of the Royal College of Veterinary Surgeons?—Yes.

16285. And also a Fellow of the Royal Society of Edinburgh?—Yes.

16286. Have you been Professor of Therapeutics in the Royal Veterinary College, London?—Yes.

16287. And for six years, I believe, you were Examiner in Therapeutics at the Royal College of Veterinary Surgeons?—I was.

16288. Since you were qualified have you practised generally as a veterinary surgeon?—Yes.

16289. And you are now in consulting practice as well as in general practice?—Yes.

16290. And you have treated and operated on, as patients, a very large number of animals in your time? —A very large numbei.

16291. Have you experimented on them, too?—I have experimented on them, but not since I left the College. Practically I have done very little absolute experimentation.

16292. Your time has been taken up with practice? —Yes, absolutely.

16293. I am going to ask Sir John McFadyean, whom you know very well, to ask you questions about the matters on which you come to give us evidence?—Very good.

16294. (*Sir John McFadyean.*) You have for a number of years made rather a special study, have you not, of the administration of anæsthetics to domesticated animals?—I have.

16295. Did you investigate this by way of experiment, as well as by observation on animals that were to be subjected to surgical procedures?—I did.

16296. What was the motive for your undertaking the investigation when you were at the Royal Veterinary College?—When I was a student I was always taught, and I think it was generally understood, too, by the majority of practitioners, not only the veterinary practitioners, but also by the general public, that the dog in particular was a subject which could not be chloroformed with safety, and it was entirely with a view to attempting to do a little towards undermining that idea, or else seeing if the idea was absolutely a true one, that I undertook for many years a number of experiments for which I obtained the necessary license, and with various patterns of apparatus, to see whether the dog was, or was not, a good subject for anæsthesia, chloroform anæsthesia mainly.

16297. You had charge of the Out-Patients' Department at the Royal Veterinary College?—I had.

16298. I suppose there are some thousands of animals treated there annually?—Many thousands every year.

16299. Can you tell me what anæsthetics were in common use in connection with that and other veterinary colleges before you began your investigations?—If ever we had, on the insistence of the owner, to chloroform a dog, either my teacher, when I was a student, or myself, when I had the Professorship, always told the owner that the result might be fatal, and it frequently was.

16300. To what do you attribute those unfortunate results prior to your investigations?—Partly to a want of skill in administration, but mainly to a wrong method of administration.

16301. Your experiments were carried out on dogs and cats principally, were they not?—The horse is generally understood by the veterinary practitioner to be a good subject for chloroform, therefore I confined my attention to the dog and cat, because the horse was said to be a very good subject for anæsthesia, therefore I had no need to investigate it; in fact, I always used chloroform freely for the horse.

16302. Was there any sufficient experience to show that horses might with safety be anæsthetised?—Yes.

16303. Did you use any other farm animals for experiment—the pig, for instance?—I did use some pigs, but not any great number.

16304. Or sheep?—Yes, I have chloroformed lambs, but not any great number.

16305. And cattle?—Cattle I have practically no experience of. I have done a few odd ones. I have experimented mainly upon the dog and the cat.

16306. What may be said to be the general outcome of your experiments and your experience with regard to the safety of administering anæsthetics to dogs and cats?—I think I can claim that it has given a great impetus to the anæsthetisation of animals for surgical operations among veterinary practitioners; for I know that a very large proportion of the modern men, practically all the men of recent years, whenever they have any canine practice have continually used chloroform now for their surgical operations.

16307. Have you kept any record of the number of dogs that you have yourself anæsthetised?—I kept records of my first 1,200 consecutive cases, after once I had got my apparatus in working order. Since then I know that I have done many thousands, but I am in busy practice, and I thought the thing was proved sufficiently up to the hilt with 1,200 consecutive cases, and I am afraid that I have not kept definite records since.

16308. Can you give us any particulars with regard to the 1,200 consecutive cases with chloroform. Was it with pure chloroform?—Yes, or rather methylated chloroform.

16309. Not the A.C.E. mixture?—No, chloroform.

16310. What was the date of the 1,200 consecutive cases?—They have nearly all been published in the medical papers; 850 have been published. It was during the period of my Professorship at the College.

16311. Which terminated about five years ago?—Six years ago. It would be, I should think, between 1894 and 1896 roughly. It is about ten or twelve years ago.

16312. Do you remember in how many of these cases death occurred as the result of the anæsthetic?—Yes, I attributed the death to the use of the anæsthetic in five instances. But I want particularly to say here that these patients were taken as they came. By far the greater proportion of them were poor people's animals which came to the Out-patients' Clinique of the Royal Veterinary College, and we had not an opportunity to prepare them as one would do in ordinary practice with a patient brought to a veterinary surgeon for anæsthesia, mainly because we had not accommodation for taking in a number of animals to prepare them previously, partly because the owners themselves are usually very fond of their animals and will not allow them away from home longer than they can help, and partly because with a busy clinique such as we have at the College we have not time to do anything more than one considers to be absolutely necessary in the way of preparation.

16313. By preparing them you mean seeing that their stomachs were not overloaded?—Yes, I mean that they had not had any solid food that day, and were not anæsthetised with a full stomach. Observation proved on several occasions that if the animal was anæsthetised with a full stomach (like a puppy which had just gorged itself) it was excessively dangerous: it would often die.

16314. These 1,200 animals would be mainly diseased animals?—Most of them were diseased animals sent for some surgical procedure.

16315. Subjected to anæsthesia without any preparation?—Most of them without any preparation. Some of the 1,200 cases included patients either brought into the College for an operation, or which were operated on outside, in which case they were prepared, but by far the majority were absolutely unprepared. I mean that some of them were in a diseased condition in which from my experience now as a general practitioner I should have hesitated to give any anæsthetic; and the *post-mortems* were made by yourself.

16316. Could you say that your later experience, which I gather includes more than 1,200 cases, would be more favourable to the safe use of anæsthesia than in the first 1,200 cases— take your last 1,000 cases in general practice?—Yes, it would be much more favourable. I am perfectly satisfied, and I still use chloroform for any operation which I think necessitates chloroform, without hesitation.

16317. You performed very extensive operations on dogs, I suppose?—I did, and minor ones, too, for the same reason, because I was told particularly that in dental operations, for instance, on a human being, it was considered very dangerous to extract a tooth under chloroform, so that whenever opportunities occurred for that in a dog, whether they were loose or fixed I gave chloroform on every possible occasion.

16318. Have any of your operations extended over a period of an hour or two?—Yes, a number of them. The longest was five hours. That was not an operation; it was a case really of chorea. I wished to see whether chloroform anæsthesia would benefit chorea or not.

16319. Did the dog die?—No; the chloroform was given by students; they were men who had seen me give chloroform, but it was given by relays of students, because five hours is a long time for one man to be at it.

16320. I now want to read to you some evidence which has been given to us by previous witnesses bearing on this point. For instance, one witness told us, in answer to Question 812, that he thought it was "generally impossible to keep a dog alive for two hours under full anæsthesia, or for one hour." Do you agree with that?—Absolutely no.

16321. You think that is certainly erroneous?—I do not think, I know it is erroneous. It was the idea years ago, but it is erroneous. I will prove that to anybody.

16322. You still use chloroform and not the A.C.E. mixture?—Absolutely, except just in certain cases.

16323. Have you used the A.C.E. mixture pretty extensively, too?—Yes, I have used it extensively.

16324. Did you find that it was particularly dangerous?—No.

16325. But why have you abandoned the A.C.E. mixture?—I use the A.C.E. mixture still at times.

16326. But you also use chloroform?—Yes, I prefer chloroform, especially for abdominal or any serious operations, because it is so much easier for my assistant to keep the patient still.

16327. So that you are perfectly satisfied, as the result of your own extensive experience, that it is possible to keep a dog under the influence of chloroform to such a degree that it is quite unconscious for a period of two hours?—I am so satisfied of it, that I cannot understand any one who has had any experience at all making the opposite assertion.

16328. I am not sure that we have had any evidence to the contrary from anybody who has said that he had extensive experience, but it seemed to be a pretty widely-felt opinion, especially among laymen, that it was practically impossible to keep a dog under an anæsthetic for a long period without a very great risk of the animal being killed by the anæsthetic?—I know that it has been a common impression because I have many thousands of people to deal with in the course of a year, and a very great number of them are ladies, who have pet dogs, who have this impression, or who have had this impression.

16329. The next point I would like to ask you is, have you experienced any great difficulty, when anæsthetising animals, in satisfying yourself as to when they had become unconscious to pain?—No, I have had no difficulty.

16330. You do not think that there is any great difficulty in that?—I do not think that there is any great difficulty. I have purposely experimented in order to test that particular point by starting my operations soon, at a certain stage, and then, of course, leaving it until the animal is quite deeply anæsthetised, and in my own mind I have no hesitation in saying that I think I can tell when an animal feels pain and when it does not feel pain. I tell by the response to my stimulus.

16331. And in practice at the present time you are never in doubt?—I am never in doubt.

16332. (*Chairman.*) When you perform an operation do you administer the anæsthetic yourself?—I cannot do that. I must do the operation antiseptically. I cannot touch anything that is not surgically clean.

16333. It is administered under your supervision?—Yes, by my nurse generally. We have nurses for it, or it is done by an assistant, but mainly a nurse has done it lately; a lady especially trained to it.

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16334. (*Sir John McFadyean.*) Do you think that the administration of chloroform to a dog involves any cruelty to the dog itself?—No.

16335. We have been told so here. I will read the answer to Question 808 in the evidence of Sir Thornley Stoker, and I think we have had other evidence to the same effect. The answer was: "The amount of terror that a dog feels, even in being put under chloroform, is rather painful to witness." Does your experience bear that out?—No, it does not. A dog will struggle against anything being put on its nose, but it is only struggling against a foreign body. If I put this mask which I have here on a dog's nose, or if I put it on the operating table it will struggle, naturally, because it is something foreign to it, but not from fear. It struggles to get away, as every dog will.

16336. You do not think that dogs exhibit signs of terror in anticipation of being chloroformed?—Not if they are gently handled. I purposely did something to test that question a little while ago in front of several gentlemen who had an idea that a dog would show a great deal of terror. They were gentlemen connected with a certain Society. I asked two of them to come one afternoon to see that what they had thought was not really the case. We chloroformed a couple of animals; we did one abdominal operation, and then, in front of them, we took a number of dogs purposely out of their kennels. I should think there were 12 or 15 dogs, one after another put on the table; we put them down on the table with the operating hobbles.

16337. Did you convince the persons for whose benefit it was done?—I think they were convinced; I believe so.

16338. (*Chairman.*) You mean that you merely put them in position without operating?—I may say that there were 10 or 12. I did two operations under chloroform. Those were all that I had that afternoon, and I took a number of dogs out of the hospital kennels and put them on the operating table with the hobbles. They were put there gently, as we always do put them, and when a dog is put on the operating table with the hobbles, if it is soothed and talked to gently, it does not show excessive fear.

16339. (*Mr. Ram.*) Those would be curative operations?—Those dogs were patients in the hospital.

16340. (*Sir John McFadyean.*) May I ask whether in operating upon dogs you invariably secure them to the table?—Not always, because occasionally, for perhaps some minor operation, a lady will insist on having the dog chloroformed in her arms, and that we do.

16341. But as a rule they are fastened to the table?—For our own convenience they are. I have some hobbles here.

16342. Have you ever seen an illustration of the way in which so-called vivisectors fasten dogs to a table?—I think I have seen a catalogue, but I forget what it was like. I think the position is similar to my own, but there are certain alterations which I think are safer for chloroformisation. I think that the table to which you refer is curved a little on the top. Mine is a flat table.

16343. In your case the dog is secured by the four legs?—By the fore legs and the hind legs, and it is stretched out in such a position that its chest gets free play.

16344. Is that in case it might be incompletely anæsthetised and be in a dangerous state when you begin to operate?—No, it is done because I think that is the safest position; it gives its chest the freest play. If its legs are extended it has the freest play for the lungs.

16345. You think that is the safest position for anæsthetising an animal?—I do.

16346. But I suppose it is also the most convenient position for the operator?—Certainly.

16347. Then I want next to elicit your opinion with regard to the possibility of inducing wounds inflicted on animals to heal directly, that is, by first intention. You succeed frequently, do you, in inducing even large wounds in the lower animals to heal without suppuration?—I do. I have a horse at the present

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moment with a wound aoout 11 inches long. Nine inches of it has healed without suppuration, and in the dog I should never bandage or dream of touching the wound again for from five to ten day. I never bandage wounds. Many thousands of dogs have had operative wounds made on them and have never been bandaged, and I have never bandaged an antiseptic surgical wound on a dog but once during the last six years, and that time I regretted it. I did it at the owner's request, and I regretted it. I do not always get a healing by first intention, but in by far the majority of cases I do. That means that I have no irritation there, and the dog has not licked his sutures out. If irritation is present, or if the wound is painful, there is no animal which is so eager as the dog to get at it with its tongue and spoil all my work.

16348. We have been told here that it is scarcely possible to apply bandages to one of the lower animals in connection with a surgical wound with any chance of the bandage being kept on; that the pain in the wound will usually induce the animal to tear the bandage off. What do you say in regard to that?—In reply to that I say that if my operation is done antiseptically, then, if the dog tears the bandage off it will not be pain in the wound that makes it tear it off, but the restraint of the bandage to which it is unaccustomed, and that is one of the reasons why I prefer not to bandage. I do bandage afterwards if the wound suppurates; but I always know that the dog has to be watched to see that it does not get the bandage off, and it is a continual nuisance to me.

16349. But do you ever leave an extensive surgical wound in a dog unbandaged?—Always.

16350. That is your rule?—That is my absolute rule.

16351. (*Mr. Ram.*) You put a dressing on?—Yes; the operations are done antiseptically, and the wound afterwards is dried properly with ether and various things. Time is taken over all this, and the wound is afterwards dressed with iodoform colloid, or some such preparation (we have several preparations of that kind), in order to hermetically seal it from contamination from the exterior.

16352. (*Sir John McFadyean.*) It has also been represented to us that extensive surgical wounds inflicted in physiological experiments on the lower animals must necessarily be painful when the animal comes out of the anæsthesia. Have you any evidence bearing on that question?—I have no experience of a physiological laboratory; I never worked in one; but my own experience is that the statement is absolutely untrue. I argue in this way: the wounds are the same whether the human surgeon makes them with the scalpel or I make them; if they are done antiseptically they are the same in principle. If my wound was painful I should expect, from my knowledge of the dog, or even of the monkey (for I have also had experience of monkeys), that the animal would immediately go for those sutures and pick them out, or, at all events, lick the wound continually. That is evidence of irritation in the dog, but I do not find that as a rule. I am annoyed when I find that a wound has got contaminated and my dog has licked the sutures out. By far the greater majority of my wounds heal by primary union.

16353. And apparently without pain?—Apparently without pain. I have done this repeatedly to prove it. I have done abdominal section, removed the ovaries, and perhaps the uterus of a bitch, and sent it away from the hospital repeatedly within 10 days, and frequently on the fourth, fifth or sixth day.

16354. Do you employ any restraint to prevent the dog licking the wound?—Absolutely none. The dog is simply put into a cage and given over to the nurse until it is out of the chloroform, and when once it is out of the chloroform it is simply put into a kennel like other dogs.

16355. What do you say with regard to the popular belief that licking is good for a dog's wound?—It is not good. I want wounds left alone. If a dog licks the wound it infects it.

16356. It is an indication that it is experiencing irritation?—Yes. That, again, I have gone into, because when the wounds have been irritable the application of antiseptics has caused that irritation to cease quickly.

16357. There is just one other question that I should like to ask you, and that is, what would be your opinion as to the wisdom and advisability of making it illegal to use dogs for experiments. I want, in the first place, to ask whether you think it would be in the interest of the extension of knowledge with regard to disease of the dog itself?—Do you mean to use diseased dogs for experiments?

16358. No. I mean, would you approve of its being declared illegal to make an experiment on a dog, even for the purpose of extending knowledge with regard to diseases of the dog?—It was particularly understood that I was not to be asked questions of that kind. You will quite understand me. For certain reasons, I was particularly asked that I should come in the character of an independent witness, neither on the vivisection side nor the anti-vivisection side. I came on the anæsthetic question, and for other reasons, too. I was given to understand that I should not be asked questions of that kind.

16359. I have no desire to press the question, but I ask you what your own inclination may be?—I will tell you this much, that if I had a diseased dog, and I thought that some experiment upon it whilst it is in a state of disease would help other diseased dogs (and I knew it was a hopeless case with our present knowledge), I should not hesitate to try that experiment, whether surgical or medicinal, upon that diseased animal.

16360. That is hardly what I wanted. I suppose you admit that there is a good deal of ignorance with regard to diseases of the dog itself?—Yes.

16361. Is an extension of knowledge with regard to such a common disease as distemper desirable?—It is very desirable.

16362. Supposing that you were commissioned to try to extend that knowledge, how would you set about it now?—I would be only too glad to act on a Commission which involved the use of a certain number of dogs in order to save the majority of dogs.

16363. Then, that comes to an expression of opinion that to declare experimentation on dogs illegal would tend to keep up ignorance with regard to diseases of dogs?—It is impossible to make progress without utilising some healthy dogs. Take your own example of distemper; it would be impossible to make progress in the knowledge of the disease without utilising healthy dogs.

16364. Without experiments?—Without experiments.

16365. You do not think it would be possible to investigate the question of distemper at the present time without experiments?—I do not, and, moreover, I am quite prepared to go to this extent, that to let a few dogs comparatively have distemper, whether given artificially or whether given naturally, would be a much better thing than to let hundreds of thousands die every year from this scourge, as they do at the present time.

16366. (*Sir William Collins.*) Do I rightly understand that you were Professor of Therapeutics at the Royal Veterinary College for seven years?—Yes.

13667. Did you illustrate your lectures by experiments on living animals?—To some extent. I got a certificate to permit me to use anæsthesia to demonstrate the methods of giving chloroform anæsthesia. I had to get a certificate to do that before my students.

16368. You held a licence?—Yes.

16369. Which certificate did you hold?—I think it was C; it is some years ago.

16370. Was it limited to the question of anæsthetics?—No; I wanted to demonstrate poisons, too. When an animal was to be destroyed I wished to do it before a class of students, explaining the different symptoms as one went on. But another object that I had, which I worked at when I was at the College, was to try to find out the most humane method and quickest method of destroying a dog.

16371. Then, you demonstrated the poisoning of dogs by various poisons?—The killing of dogs by various things; and as the result of that I had a little apparatus made which, I think, is very humane.

16372. Did you administer anæsthetics as well as the poison in those cases?—Not always.

16373. So that they were experiments without anæsthetics?—Yes; the dog had to be destroyed, and it was destroyed.

16374. (*Chairman.*) What was the poison that you generally used?—Prussic acid.

16375. That has always been the drug used, has it not?—Yes; but the reason of my experiments was that when dogs have prussic acid given to them, it is very rapid in its action and it kills within 30 to 45 seconds; but with a large proportion of them, and with cats too, you get a most horrid yell or scream, which is very objectionable and sounds as if it might be painful. It may be involuntary, but it sounds as if it might be painful during those few fractions of seconds.

16376. (*Sir William Collins.*) Was prussic acid the only poison that you employed?—No, I used chloroform a great deal.

16377. (*Sir Mackenzie Chalmers.*) And morphia?—I did not use morphia then.

16378. (*Sir William Collins.*) You spoke of certain ill-results which had occurred in the administration of anæsthetics to dogs. What did you mean by that?—I was always taught that if you gave a dog chloroform it never woke up again, it was killed. Those are the ill-results. Frequently one used to be giving a dog chloroform, or a student would give it chloroform, and suddenly respiration would cease, and it would be impossible or extremely difficult to bring the dog back to life again.

16379. Why was that?—Because it got an overdose of chloroform.

16380. Then what you have learnt of recent years has been to measure and graduate the dose?—Yes.

16381. Do I rightly understand that you regard chloroform as better than the A.C.E. mixture for animals that you have to deal with?—For the majority of dogs I do.

16382. Do you always administer it by a mask?—Always.

16383. Not by a tracheotomy tube?—Never. You cannot with a patient.

16384. Have you any experience of hypnotism as a means of anæsthetising animals?—I have none. I have tried it and failed.

16385. Why do you not employ a tracheotomy tube in cases that you have to deal with?—Because naturally my clients would not like me to make a wound in their dog's throat when I can give chloroform so readily without it. They would not like them to go home with a wound in the throat, and the trachea itself would be injured.

16386. Have you ever given an anæsthetic by means of a tracheotomy tube?—I do not think I have. If I have it is only once or twice.

16387. Would it introduce any complication, in your opinion?—I have had no experience of it. I should not dream of doing it on a patient. The complications would be the unfortunate necessity of making a wound and having to heal it afterwards again.

16388. I see that before the last Royal Commission on Vivisection a memorial was put in on behalf of the veterinary surgeons of Scotland to this effect: "We, the Court of Examiners for Scotland of the Royal College of Veterinary Surgeons, desire to express our opinion that the performance of operations on living animals is altogether unnecessary and useless for the purpose of causation." That is signed by "James Syme; James Dunsmure, M.D., President of the Royal College of Surgeons of Edinburgh; J. Warburton Begbie, M.D.; John Lawson, President of the Royal College of Veterinary Surgeons; B. Cartledge, M.R.C.V.S., Member of Council of the Royal College of Veterinary Surgeons; William Cockburn, M.R.C.V.S.; Charles Secker, M.R.C.V.S.; and James Cowie, M.R.C.V.S. I fully concur in the above, John Wilkinson, principal veterinary surgeon to the Forces." That was put in in 1876, but the document was earlier than that. Do you wish to say anything with regard to it?—No, it is so long ago, and medical science in five years is completely revolutionised. I know nothing of their opinions then.

16389. Do you wish to give any opinion now as to whether experiments on living animals are useful for the purposes of veterinary science?—I should say the same to you as I said to Sir John McFadyean. Take the case that he instanced. If you mention any specific instance, take distemper.

16390. Have we discovered the cause of distemper?—We want to do so.

16391. Then that would hardly be an instance of definite value that had accrued?—No, but if you want an instance of definite value having accrued, so far as I personally am concerned, it was the knowledge that the abdomen was opened continually in a physiological laboratory which encouraged me personally to open the abdomen in a dog for removing certain things from it. I mean the knowledge that it was done with safety. When I was a student in the pre-antiseptic days, as the thing was practised when I was a student, I dare not open the abdomen of a dog without warning the owner that the patient would very probably succumb, not from the operation itself, but from peritonitis or something of that kind.

16392. The abdomen was opened, was it not, in pre-antiseptic days in veterinary surgery?—It was opened, but the veterinary surgeon never failed to warn his client that there was a prospect of grave trouble afterwards. Take, for example, the operation of spaying a mare, ovariotomy of a mare, which I think now can be claimed to have been revived again in England. It was done seventy or eighty years ago continually, and spaying a cow, too. Charlier, at the beginning of last century, proved that the abdomen of a cow could be opened with safety and the ovaries removed.

16393. That was in the pre-antiseptic days?—Yes, but Charlier, looking back over his methods, was antiseptic without knowing it. He boiled everything, and cleaned his hands and cleaned the cow's passage out before doing the operation; and that was the reason of his success.

16394. Did it answer to the antiseptic method?—He did not call it the antiseptic method, but he operated with great success certainly, because he boiled his instruments, and washed his hands clean and washed his cow. As I say, during Charllier's time, in his hands the operation was very successful. After his time it fell into disuse again, because other people tried it and they were not successful, and the same with the mare. In the present day I think that any modern veterinary surgeon will go forward and do these operations practically stating that his patient will live afterwards, but the primary ones must have been experimental.

16395. In that case the animals have benefited by improvements in surgery in the case of man?—Done by human surgeons on man.

16396. Done by surgeons rather on human patients?—Yes.

16397. (*Sir Mackenzie Chalmers.*) As regards these operations that you have told us of on dogs, taking out the uterus and the ovaries, is that done on account of disease, or is it done to prevent the animal breeding?—Sometimes for one thing, sometimes for the other.

16398. Does your practice extend to horses as well?—Yes, I have a great number of horses to treat.

16399. Can you give a horse anæsthetics?—Yes.

16400. Can you give chloroform safely to a horse?—Yes.

16401. When a horse is castrated what do you use, the knife or the ecraseur?—I do not use either. I use a special instrument generally. Sometimes, if I have a pupil who wants to see an ecraseur used, I have one, and I use it.

16402. What anæsthetic do you give to a horse?—Chloroform always, or, if it is local, cocaine perhaps, but choloroform always for castration.

16403. In the case of ordinary farmyard animals are anæsthetics ever used?—They are used.

16404. When a veterinary surgeon does it?—Yes.

16405. Not when the farmer does it, I suppose?—I should think that the farmers would hardly use it. Some farmers are very clever at it, but the majority would not dare to use it. It requires special skill.

16406. In the case of a horse, I suppose it is always done by a veterinary surgeon?—No, many horses are castrated all over the country by the castrators. Thousands of horses in England even.

16407. By an ordinary man—by a gelder?—By a castrator, whose father and grandfather probably have been castrators before him.

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16408. In that case are anæsthetics used or not?—Now and again we come across a man who perhaps himself started his career at the College—a man, I mean, of some education—who will dare to use chloroform, but it is very rare to find that. By far the majority use no anæsthetics at all. To begin with, they would have a difficulty in getting a poisonous agent like chloroform; they would have to get it through a friend who held a medical qualification.

16409. Is castration a very severe operation; is there great pain?—There is great pain for an instant, but it is very rapidly done, and the horse will go straight to its manger, in the majority of cases, afterwards. In fact, I have seen them repeatedly pick up grass off the ground before the hobbles have been removed, within a few seconds of the operation done without anæsthetics.

16410. They are cast, of course?—Some are cast, some are done standing. A very large proportion of horses are done standing up. Some men simply go in without even any method of restraint at all, or without a companion. Some men will go into a loose box and operate and come out again within four minutes, with the two testes in their hands, but in the majority of instances two men are used, one at the head and one at the hind quarters, and the operator himself will do the whole thing in a few moments. Personally I have done it in two and a half minutes, so that I know it can be done, and I am not an expert at it.

16411. (*Chairman.*) At what age is the horse operated on?—Some are done as yearlings, but it is usually done at two, three, or four years old. Some horses are done when they are very old, ten and twelve years old, but the majority are two, three, and four years old.

16412. (*Mr. Ram.*) Is the horse restrained in any way when it is done standing?—Sometimes a twitch is put on to its nose, and it is pushed into a corner. Sometimes, if the horse is troublesome, the twitch is taken off and a man just holds his head. It is a very quick operation.

16413. Does the operator get underneath the horse or between its hind legs?—No, he gets just in front of the hind legs, and takes hold of the testes; the instrument is used very quickly, and the testicles are removed in a few minutes, as I say. I think there is a man who does it between the hind legs, but the majority do not. As a rule the horse does not kick.

16414. (*Sir Mackenzie Chalmers.*) Is there more danger in giving a horse chloroform than there is in giving a dog chloroform?—I do not think so. In both cases it has to be done, of course, with a great amount of skill and care.

16415. You would not expect to have a larger percentage of deaths among horses than you would among dogs?—Not if I had the supervision of it myself.

16416. You told us that you had experimented as to the most humane method of destroying a pet animal?—Yes.

16417. Is prussic acid, in your opinion, still the most humane method?—No, it is the quickest, but it is not the most humane, in my opinion.

16418. You mean that the struggle may possibly be a painful struggle?—I can hear it now. I can hear it for so long afterwards, if the animal does scream. Some do not, some will tumble dead at once, but a few do scream, and I certainly should not care to do it in front of the owner; I should prefer to get the owner out of earshot. Whether it is a cry of pain, or whether it is purely involuntary, I am not prepared to say. I do not know.

16419. (*Chairman.*) Both with cats and dogs?—Both cats and dogs will sometimes give either a scream or a yell just when they appear to be going over the border.

16420. (*Sir Mackenzie Chalmers.*) What, in your opinion, is the most humane method of destroying an animal?—I think the lethal chamber, with one or other of the present-day gases.

16421. Carbon monoxide?—I myself use a mixture of coal gas and chloroform.

16422. That would be carbon dioxide and chloroform?—That is what I use myself, and that is what I think is very humane.

16423. Does it require long immersion?—No, as a rule the patient will become unconscious in four minutes; then you leave it for a few minutes, perhaps another five minutes, to be sure; because if you take them out into the air, then they may come round again.

16424. With the carbon monoxide there is no recovery, is there?—It depends upon the quantity given with carbon monoxide.

16425. When once you have stopped the breathing there is no chance of resuscitation?—When once you have stopped the breathing, with either of them, it would be highly improbable. I have never succeeded in resuscitating an animal when once breathing has stopped.

16426. (*Chairman.*) But suffocation that takes four minutes is rather painful, is it not?—If the anæsthetic is allowed to go in gradually into the box where the patient is I think that the animal will pass away without pain, and I say so from having watched many dogs and cats put into the lethal chamber, or, rather, into a lethal box with a glass top on it.

16427. (*Sir Mackenzie Chalmers.*) There is no struggle?—Now and again you get a struggle; but any dog will struggle against inhaling anything that is foreign to it, but I do not think that is a struggle of pain. It will try to get out of the box, of course. If it is left there in the box without anything put in, it will try to get out of it quickly—that is a necessitous thing.

16428. (*Chairman.*) You do not think that it suffers pain?—I do not think so, and I speak from a large experience now.

16429. (*Sir Mackenzie Chalmers.*) You think that is the most merciful and painless death which you can inflict on animals?—Yes.

16430. For instance, would morphia poisoning be painless?—I think poisoning with morphia would be painless, but I cannot say. I have never managed to poison more than one dog with morphia in my whole life, and I have tried with a large number.

16431. Does it require a very large dose to be fatal?—A very large dose.

16432. More so with a dog than with other animals?—I believe so. A dog is extraordinarily insusceptible to the toxic effects of morphia.

16433. Does it make any difference whether the morphia is injected or given by the mouth?—I have never tried by the mouth. I have always injected it hypodermically. It would make a difference, because everything given under the skin acts more quickly and is more drastic in its action than when it is given by the mouth; it is absorbed so much more quickly.

16434. (*Mr. Ram.*) In those cases in which you experimented yourself, and of which you took notes of consecutive cases of dogs under chloroform, did you say that there were only five deaths in 1,200 cases?—That is so.

16435. And did you in that series of experiments derive any knowledge as to the length of time for which it is possible to keep dogs under anæsthetics?—Yes.

16436. You have tried that?—Yes. Most ordinary operations can be done within half an hour quite readily, but a number of operations took me over an hour.

16437. And you had no difficulty in anæsthetising the animals?—I had no difficulty in maintaining the anæsthesia at all. I did keep several dogs under anæsthesia for over two hours, and one for five hours.

16438. You have known a case of a dog being under anæsthesia for five hours?—As a matter of fact, relays of students kept it under for me, but I was about, and knew that the animal was anæsthetised the whole time.

16439. During the whole of that time was the dog insensible to pain?—It was.

16440. We have been told by one of our earlier witnesses that the horse is not a good subject for anæsthetics, and therefore that many people are afraid to have valuable horses anæsthetised when they are castrated, or otherwise dealt with. Has that been your experience?—That has not been my experience.

16441. With regard to either a horse or a dog, do you find after ill effects in consequence of the anæsthetic—sickness or trouble of any sort?—It is most rare to get sickness. The animals are stupid and dull for perhaps an hour, or it may be a couple of hours. A sensitive dog will be stupid for a couple of hours.

16442. Seedy?—Yes, it will be seedy because it will not feed. Supposing I operated this afternoon on a little dog, I should not think anything if my nurse told me to-morrow morning that the patient was not taking its breakfast, but at the same time I should know that it was rather against the rule. The next day I should expect it to take its food.

16443. Is a dog sick after anæsthetics?—It is most rare.

16444. Is it possible for a horse to be sick?—Only under certain diseased conditions, for instance, when it has a ruptured stomach, or when it swallows hawthorn berries. You cannot make a horse sick if you try.

16445. It is not subject to any emetic?—No, you cannot make a horse sick.

16446. Have you ever seen any animal under curare?—No.

16447. (*Dr. Gaskell.*) I should like to know, please, how you know when your animal has had sufficient anæsthetic not to feel any pain?—By its irresponsiveness to a severe stimulus.

16448. Responsiveness of what kind?—Either by a cry, by an expression of pain in that way, or by shrinking away from the stimulus.

16449. But might not movement take place?—When an animal is under anæsthesia movement is always taking place; the mere fact of the respiratory muscles working draws the legs backwards and forwards, and frequently the legs are quivering a little.

16450. You very frequently find in animals that you do get movements of the legs more or less synchronous with respiratory movements?—Yes.

16451. That is common under anæsthetics?—That is common under anæsthetics if the animals are not strapped down tightly.

16452. But that is no sign of pain?—That is no sign of pain.

16453. And if you have a dog chloroformed sufficiently to remove pain, can you tell us at all how far that dose is removed from a dose which would kill?—In figures do you mean?

16454. No, I mean in your general impression is there a large margin between the two things, or is it near?—There is a margin; when a dog has been under proper chloroform anæsthesia for some few minutes it is often very difficult with the apparatus which we use to give it a lethal dose. When once the system has become tolerant to chloroform, and it is still given in a rational manner with the proper percentage of 9 to 2 per cent., it is extremely difficult. It will often take half an hour to finish it off.

16455. The danger then is at the beginning of the administration?—The danger is at the beginning. I am quite satisfied about that, and if the anæsthetic is given in an excessive dose at the beginning, your dog goes under quickly, literally like the snuff of a candle, but when once I get the animal tolerant to it I can let my kennelman or groom come and give the chloroform, and I can attend to the operation. I know I am all right.

16456. Are you right in thinking that the danger at the beginning is due to an overdose?—I think I am. I think it is due to asphyxia.

16457. Have you read a paper by Martin and Embrey in the "Physiological Journal"?—I have not seen that paper. I would like to read it through.

16458. It is to the effect that the first administration of chloroform, especially to an animal like a dog, causes a slow stoppage of the heart through the action of the vagus nerves. That is well known, of course?—Yes.

16459. And that then if the animal is apparently dying, if you cut the two vagi nerves it will recover, and you can go on with the administration of chloroform quite happily?—That is interesting. I have not seen the paper, but I will read it. The other is only my own idea, of course. I am not a skilled physiologist.

16460. The danger is at the beginning, not necessarily because you have given too much chloroform, is it, but because of the action of the chloroform in the medulla oblongata at first?—I am only speaking as a clinician, and my experience is that the danger is only at the beginning.

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16461. That is also the physiologists' experience, I may say. When you say that it takes a large dose of morphia to kill a dog, you mean a dog as compared with other animals?—Yes.

16462. Would you consider that morphia is a narcotic to a dog?—Yes, I do consider it a narcotic.

16463. Do you use it in your practice in the same way as medical men do in their practice with human beings for allaying pain?—I use it largely for stopping pain.

16464. And you find no difficulty?—I find no difficulty in the dosage.

16465. Do you find any difficulty in stopping pain?—Sometimes a dog does not respond as I should expect it to do.

16466. Is that a question of dosage, or is it a question of idiosyncracy?—I think it is a question of idiosyncracy, but I am not prepared to give a definite opinion upon that point. I begin to think it must be an idiosyncracy.

16467. (*Chairman.*) Of all dogs, or of certain dogs?—Of certain dogs. I am speaking of dogs alone.

16468. (*Dr. Gaskell.*) I think you may take it as a rule that morphia, except in very large doses, will act in much the same way on a dog as it does on a human being; it will cause absence of painful sensation, I mean?—I presume so; I have that impresion, but I have no experience of it in human beings, except that people have told me that they feel comfortable after it, and dogs certainly give me the impression that they are comfortable after it in the majority of instances.

16469. Have you any experience of such an anæsthetic as chloral with dogs?—I cannot say that I have used it sufficiently to form an opinion.

16470. Or urethane?—I have not used it at all.

16471. Practically you administer chloroform?—Chloroform, the A.C.E. mixture, cocaine and the different local anæsthetics.

16472. (*Dr. Wilson.*) Do you ever use morphia before administering chloroform in an operation?—I have sometimes done so. It is not my general practice to do so, but I have done so at times.

16473. I think you said in answer to a question which was put to you, that you prefer to use chloroform because animals keep far more still during the operation than when you use the A.C.E. mixture?—They keep far more still for a longer period than when I use the A.C.E. mixture. When once an animal is anæsthetised by chloroform I know that my anæsthetist need not keep pressing the bellows so frequently as with the A.C.E. mixture. The animals waken up much more quickly from the stage of unconsciousness when the A.C.E. is used than they do with chloroform; that is to say, the stage of anæsthesia with the A.C.E. by some few seconds, or minutes probably.

16474. You have no experience yourself, of course, but would you conclude that if the A.C.E. mixture was used for keeping up artificial respiration, through a tracheal tube, for example, in preference to chloroform, there would be more risk of the animal becoming conscious to pain during a prolonged operation?—Not at all if the anæsthesia is kept up. It requires more pumping of the apparatus to keep the animal deeply anæsthetised with A.C.E. than it does with chloroform alone, and in the same way with ether, it requires more pumping than with chloroform alone. The stage of anæsthesia with chloroform is longer, that is to say, you can cease pumping chloroform for a longer interval than you can with either of the other two.

16475. Do you think it requires more careful supervision when using A.C.E.?—I think it requires less supervision than using A.C.E. or ether, but A.C.E. particularly. I do not use ether much because it sickens the dog so afterwards.

16476. Are your dogs so completely muzzled that they cannot cry out during the operation?—If they are under chloroform they cannot cry out. Do you mean when they are going under?

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16477. Yes, you say that you use a stimulus to test whether they are sufficiently under chloroform?—I do not say that I do now, but I have done so. They would have no muzzle on at all.

16478. The response to the stimulus would be a cry?—Probably, or the retraction of a limb.

16479. Then when you are operating on them there is nothing to prevent their crying?—Absolutely nothing. If they could not cry they could moan. They just have their heads in the mask and there is nothing to stop the jaws from opening, or they might have a piece of tape round the nose. We always put a piece of tape round the nose when we are handling dogs as a matter of precaution, because however fond a dog is of you, if you do something to it that it does not quite understand it may take hold of you, and frequently will have hold of you.

16480. But supposing you used a wire muzzle instead of tape?—You could not get a wire muzzle under the mask. Besides, I do not like a wire muzzle, it is not a safe thing to use.

16481. But you would not use morphia alone without either the A.C.E. mixture or chloroform for an operation, would you?—I should for certain minor operations. For example, the operation of removing a very small tumour or removing a small elliptical piece of skin from the eyes, which is very common in certain breed of dogs with certain diseases.

16482. I was referring to a rather serious operation?—No, not for serious operations. I should give either the two or chloroform itself.

16483. (*Sir William Collins.*) Did I rightly understand you to say that dogs are insusceptible to morphia?—They are very insusceptible to a toxic dose of morphia.

16484. Did I rightly understand you to say that you had not succeeded in poisoning a dog with morphia?—I have only succeeded in poisoning one.

16485. Have you made many trials?—I use it largely. I have injected it largely either before chloroforming or I have tried on several occasions, when the owners have thought morphia a humane method of putting a dog away. I have given large doses; I have given as much as 20 grains, and to my astonishment, because I did not know that the dog was so insusceptible as that to a toxic dose, the dog has been alive the next night, 24 hours afterwards.

16486. Is it an easy thing to give a lethal dose of morphia to a dog?—Personally I do not think it is. Some dogs will become perfectly comatose with a grain, with a small amount. The other day I had a small animal; I gave it, I think, two grains, but it acted on that animal just as much as I should have expected four or five grains to have acted on an animal of the same size. The idiosyncrasy of that particular animal was very great towards it. Frequently half a grain will produce a stupor in one animal when it will take a full grain or a grain and a half to produce it in another animal of the same size.

16487. Then morphia affects different dogs differently?—It does.

16488. Can you not regulate the necessary dose by reference to the weight of the dog?—Generally you can, but you never can be quite certain.

16489. I thought you suggested just now that the weight was not a guide?—Not to the toxicity.

16490. Or to the effect?—I say it is not a certain guide.

16491. You told us that you have given chloroform to dogs some 1,200 times?—Literally thousands of times. I kept notes of my first 1,200 cases.

16492. As an anæsthetic?—Yes, as an anæsthetic.

16493. How many times have you administered morphia to dogs as an anæsthetic?—I should say pretty nearly a hundred times.

16494. For considerable operations?—Not for major operations.

16495. Why not?—I have done so and given chloroform afterwards.

16496. But have you used morphia alone as an anæsthetic for major operations?—No.

16497. Why not?—I do not use it. I prefer chloroform. I always use chloroform.

16498. Why do you prefer chloroform?—Because, for one thing, I am in the habit of using chloroform, and have been for many years, and I like chloroform.

16499. Is it because chloroform is a better anæsthetic?—Undoubtedly, for major operations.

16500. (*Dr. Gaskell.*) In those cases where you have used morphia, do you consider that the animal has not felt pain during the operation?—I do not say that. I mean that I should not use morphia for any major operation.

16501. But in cases where you have used morphia, have you felt satisfied that the animal has not suffered pain?—No, I cannot say that. I mean just at the very instant. If you take that eye operation that I was speaking of, the snip is done instantaneously, but my animal will flinch, although it passes immediately into a state of dreaminess again, and does not appear to suffer, but just at the instant of the snip, the eye being, of course, a very tender part, it will just flinch, perhaps.

16502. (*Sir John McFadyean.*) What was the condition of that dog which survived a dose of 20 grains of morphia?—Carcinoma.

16503. But what was the condition of the dog two hours afterwards?—Absolutely dead to the world, literally.

16504. Do you think it was conscious to pain?—No; I could make it respond by perhaps lifting the foot up and dropping it down again; I forget what I actually did, but I know when the nurse went to it she patted it or did something, and it just lifted its head for a mere fraction of a second, showing that it did know her for a second, but it could not recollect.

16505. Was it susceptible to such stimuli as you have mentioned?—Yes.

16506. Have you had any other case approaching it in which a dog would stand 20 grains?—No, not when I have given as much as that. I have given 16 grains, though, once before, and nine and ten grains several times, and the animals did not succumb.

16507. So that you think it might be pushed so as to obtain its full effects as an anæsthetic without much risk of killing the dog?—That is my experience. I have been astonished at it.

16508. (*Sir William Church.*) Have you experience of laudanum in dogs?—I give it sometimes.

16509. Does it fail to act in the same way; are dogs tolerant of a large dose of laudanum?—My experience is that they are very tolerant of a large dose of laudanum.

16510. Have you any idea what would be a lethal dose for a moderate-sized dog?—No, I have not any idea whatever.

16511. Is it the same with cats?—I have no experience of its administration to cats, and I have not been able to find any experience.

16512. (*Chairman.*) You hold a licence, I suppose?—I have not had a licence for seven or eight years now. I just held one whilst I was at the College. I got it for the purpose of doing this work with anæsthetics.

16513. You have held a licence?—Yes.

16514. And you know that if you hold a licence, and if you perform a cutting operation on an animal, it comes under the Act?—Yes.

16515. Are there many operations which you perform on a cat, or a dog, or a horse—I mean which would be a patient—without an anæsthetic, for which you would be obliged to use an anæsthetic in case of an experiment?—I do not quite understand the point.

16516. Are there any cases in which you operate on an animal as a patient without anæsthetics?—Yes, plenty of cases. I never give an anæsthetic for a mere lancing of an abscess. There are plenty of instances in which you cannot give an anæsthetic.

16517. I was asking you about tracheotomy. If you were going to cut an animal's throat for the purpose of putting in a tube, would you for that preliminary operation, in the case of an ordinary animal patient, use anæsthetics?—Yes, unless the patient was at the point of death. Tracheotomy is an operation that we have to do in a hurry sometimes. If I was doing tracheotomy on a hunter that is a roarer and must be made useful, I should give cocaine. I should not give chloroform, because cocaine answers the purpose.

It is a mere prick of a needle to begin with, that is the only pain that the animal feels.

16518. If it was at all a serious operation you would give chloroform?—Yes.

16519. But there are a good many cutting operations which you would require leave under the Act to perform, and would only obtain leave to perform with anæsthetics, which you would perform on a patient without anæsthetics?—Yes, I expect that I should. I do not use anæsthetics for every little thing. For one thing, there is not time, and for another thing, I do not consider that the inconvenience which the patient is put to for the time being in suffering afterwards from the anæsthetic is worth it, because the operation is done so quickly; but for any major operation I should always use anæsthetics.

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16520. (*Sir John McFadyean.*) Have you ever fired a horse in full consciousness?—Yes.

16521. Have you done it a thousand times?—Not quite that, but I have done it.

16522. Do you do it as a rule without anæsthetics? —Frequently.

16523. Have you any doubt as to its being an enormously painful thing?—It is a painful thing, but I generally use an anæsthetic, I generally use some cocaine. At the same time, I do not always do so.

THIRTY-SEVENTH DAY.

Wednesday, 20th November 1907.

PRESENT:

The Right Hon. The Viscount SELBY (*Chairman*).

Sir W. S. CHURCH, Bart., K.C.B., M.D.	Mr. W. H. GASKELL, M.D., F.R.S.
Sir W. J. COLLINS, M.P., M.D., F.R.C.S.	Mr. J. TOMKINSON, M.P.
Sir M. D. CHALMERS, K.C.B., C.S.I.	Mr. G. WILSON, M.D., LL.D.
Mr. A. J. RAM, K.C.	

Capt. C. BIGHAM, C.M.G. (*Secretary*).

In the temporary absence of Viscount Selby, Sir William Church took the Chair.

Mr. W. OSLER, M.D., F.R.S., F.R.C.P., called in; and Examined.

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16524. (*Sir William Church.*) You, I think, are Regius Professor of Medicine at the University of Oxford?—Yes.

16525. You are a Fellow of the Royal Society, and a Fellow of the Royal College of Physicians, London, and you have been, I think, Professor at the Johns Hopkins University, Baltimore?—Yes.

16526. You wish to make a general statement showing that medicine offers no exception to the other sciences?—I put down very briefly these statements in my *précis*, particularly with reference to one or two points that perhaps have not been dealt with by other men who have already given evidence.

16527. You have read the evidence before the Commission?—Not all of it, but that of Professor Gotch and one or two others.

16528. Have you read Professor Starling's evidence? —Yes, Professor Starling indicated to me particularly the lines on which I, as a practical physician, could perhaps best give evidence, and I put down here these one or two points which perhaps have not been, as was thought, fully dealt with by others.

16529. You agree with the general line of the medical and physiological evidence, I suppose?—Fully, particularly with that of Professor Starling, the President of the Royal College of Physicians, and the President of the Royal College of Surgeons, whose evidence I have read.

16530. You would like to add a few points?—Yes, particularly these points that I have referred to in my *précis*. I do not think it is worth while to speak in regard to the influence of experiments on animals or in regard to the way in which we have learnt the etiology of acute infections through those means. But I think that the story of yellow fever illustrates, perhaps, more satisfactorily than any other, the remarkable way in which experiments, carefully devised and carried out, may influence not only our knowledge of the etiology of a disease, but may influence extensively the commercial relations of nations, and save not only thousands of lives but millions of pounds annually. Would you like me to speak of that particularly.

16531. If you please?—Yellow fever has been the great scourge of the regions round the Carribbean Sea, more particularly Mexico, the West Indian Islands, Brazil, and every few years it has spread into the Southern States of America, and occasionally has reached Philadelphia, and even as far north as Boston. In the early part of the last century on several occasions it reached Europe, and there were extensive outbreaks in Spain, costing some thousands of lives. Many attempts had been made to find out the cause of the disease, but all had failed up to the year 1900, when a Commission was sent to Havannah by the United States Government especially to investigate the cause of yellow fever. That Commission, composed of Drs. Watten Read, Carroll, Lazlear, and Agramonte, recognised particularly the relations of the mosquito to the disease, and they went out with the specific object of determining, if possible, to discover the germs of the disease. The experiments which they devised were carried out in a United States Army Camp in Havannah, and they are among the most remarkable that have ever been made. The camp was entirely isolated, so that there could be no possibility of communication with the outside. It was composed of a certain number of immunes, that is to say, persons who were no longer susceptible to yellow fever.

16532. (*Mr. Ram.*) In consequence of having had it, do you mean?—In consequence of having had it—and of non-immunes. That is a common division of the population in Brazil and Havannah—namely, into immunes and non-immunes. A man is asked whether he is immune or not.

16533. (*Sir Mackenzie Chalmers.*) Does one attack confer immunity as a rule?—One attack confers immunity. In this camp a house was constructed with two compartments, divided from each other by a wire mosquito-proof screen. There were two sets of experiments made in connection with this little house. In the first place, into one side of this hut 15 infected mosquitoes were placed. Those were mosquitoes that had bitten a yellow fever patient within the first three days of the illness. Men were selected, partly from the Army and partly from civil life, who had expressed and signed their willingness to submit themselves to experiments. I may state that one or two of the medical men also volunteered. Into the compartment with the 15 mosquitoes a non-immune went in the morning, in the afternoon, and on the following morning and submitted himself to the bite.

16534. For an hour, or something of that kind?—I do not know how long, but long enough to get a number of bites. Within five days he had the disease. At the same time in the adjacent compartment, which was simply screened from these mosquitoes by a wire

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netting for 21 consecutive nights two non-immunes slept. They did not get the disease.

16535. Were any precautions taken to keep the ordinary mosquitoes from them?—Yes, of course; the compartment was double-doored and very carefully screened so that there was no possibility of that. I may say that experiments of that nature were repeated on several occasions, demonstrating quite conclusively that, so long as these infected mosquitoes were kept from biting, though there was only a screen between them, the individuals did not get yellow fever. Then experiments were made on a very extensive scale to determine whether the disease was conveyed by means of fomites, that is to say, whether, as was usually supposed, the disease was carried by infected clothing and by the excreta of the patients, and by the vomit. For that purpose the clothing and material soiled by the vomit and by the blood, and by the stools of the patients were placed in one of these rooms, and a group of non-immunes slept in contact with this clothing, in some cases between the actual sheets of the beds in which these patients had died, for 21 consecutive nights. That experiment was repeated with a second set of non-immunes sleeping, as I say, with the bed linen and with the soiled materials of patients who had died of the disease. Not one of them took yellow fever.

16536. (*Sir William Church.*) What were the numbers of the non-immunes that slept in connection with the fomites?—I think two or three, two soldiers and one surgeon, for 21 consecutive nights; and a second party for the same period. Then these men were subsequently experimented upon by placing them in the section of the house with the infected mosquitoes, and in each instance they took the disease. Altogether 22 soldiers subjected themselves to the experiments and 22 took the disease; fortunately none of those cases proved fatal. One fatal case was a former assistant, Dr. Lazear, who had been for several years in charge of my clinical laboratory. He submitted himself to the bite of an infected mosquito, and three days subsequently developed the disease and died.

16537. (*Dr. Wilson.*) May I ask what interval there was between the exposure to the fomites and individuals being put into the place with the infected mosquitoes?—Some days, possibly weeks' intervals; I cannot say exactly.

16538. (*Sir Mackenzie Chalmers.*) What is the incubation period of yellow fever?—From three to five days. The mosquito to become infective must bite a patient within the first three days of the disease.

16539. (*Mr. Ram.*) Of its having been in contact with the disease, do you mean?—I mean that the mosquito must bite the yellow fever patient within the first three days of the patient having the disease. The mosquito itself is not infective under a period of 12 days; the mosquito may bite an individual anywhere up to the twelfth day after receiving the infection without being infective; then it remains infective all through the rest of its life. Of course the interesting practical point comes out, that this series of experiments has already revolutionised life in those regions. Havannah within the next two years was cleared of yellow fever, the first time in the 300 years of its existence. The French Academy sent a Commission to Brazil to study the disease, and they have reported in harmony with the American Commission—namely, that the disease is transmitted by the mosquito, and by the mosquito alone, and only by a mosquito that had bitten yellow fever patients within the first three days, and that the mosquito did not become infective until after 10 or 12 days subsequently.

16540. (*Sir William Church.*) I think in yellow fever the transmission of any organism from the mosquito to man has not yet been followed out?—No, we do not know the organism; but it must be a protozoon, possibly a spirochoete, which undergoes a slow evolution in the body of the mosquito.

16541. But nothing has hitherto been discovered comparable with the plasmodium of malaria?—No, but it is possible that it is a very minute spirochoete, which passes through an ordinary filter. This is the kind of discovery that will revolutionise conditions of life in the tropics. The discovery of the malarial parasite and the discovery of relations of yellow fever with the mosquito will enable the Panama Canal to be built. Without those two investigations the probability is that it could not be built.

16542. (*Mr. Ram.*) Or if built, would cost a tremendous sacrifice of human life?—It would cost an enormous sacrifice of human life, just as happened with the French. Now there are 20,000 whites on the Isthmus at work; of course, nearly all of those are non-immune. There has been practically no yellow fever, and what is much more important, because it was not the yellow fever that killed the French to the same extent, there is no malaria.

16543. (*Sir William Church.*) In these experiments that you have been detailing to us, animals do not come in; the animal experimented upon was man?—That is so, only as man is an animal. I am referring to those experiments only as an illustration that it is through the experimental side of medicine, the experimental spirit in medicine, that these great revolutions have been effected, revolutions with which there is nothing else in human endeavour to compare from the standpoint of humanity. There is not anything else in the whole development of the British nation that is going to have so much importance as the discovery of the mode of transmission of malaria. It is going to make the tropics habitable. And all this has come about through the experimental method and the experimental spirit. Without these such investigations could not have been made, and these perfectly phenomenal results could not have been achieved. It was the same spirit that gave us anæsthesia, and the same spirit that has given up antiseptic surgery, and the same spirit which has given us preventive medicine—three things which stand out in the record of human achievement, with which nothing else may be compared—I mean from the standpoint of everyday, common humanity.

16544. Then your contention would be that this experimental investigation into the interaction between the mosquito and man producing yellow fever would never have been thought of if it had not been for previous experiments on animals?—Never. The men who made these investigations spent their lives in laboratories, and their whole work has been based on experimentation on animals. They could not otherwise, of course, have ventured to devise a series of experiments of this sort.

16545. (*Dr. Gaskell.*) Could you experiment with yellow fever on animals?—Yes, recently Wolferston Thomas, from the Liverpool School, working in Demerara, I think, has shown that it is capable of transmission to one of the higher anthropoid apes.

16546. Do the natives take yellow fever?—Yes, everywhere; but they often have it in such a mild form, and have it as children.

16547. Still, even in that mild form, the mosquito would transmit it?—Yes.

16548. (*Dr. Wilson.*) I understand that when the mosquito has once bitten a patient suffering from yellow fever, after the first 10 or 12 days it remains capable of transmitting the disease during its short life?—Sometimes it is a long life comparatively—many months.

16549. Has that been determined?—Yes, it will live through many months; it will live through the winter.

16550. As the fever has disappeared in Havanna may I ask whether the special stegomyia mosquito has also disappeared?—No, they have made an active warfare against it, but I think the mosquito itself has not disappeared.

16551. But if the mosquito is capable of transmitting the disease during its existence, after it has bitten a patient suffering from the disease, how can you get rid of the disease, so long as the mosquito lives?—That is only a matter, I think, of 8 or 9 months. I do not know definitely what the life of a mosquito is. I do not think it is more than over the succeeding winter. What do you say, Dr. Gaskell?

16552. (*Dr. Gaskell.*) I should think not.

16553. (*Sir William Church.*) Is there anything further you wish to add about yellow fever?—No, I think not. The work in Brazil lately has shown that even in large centres, like Rio, by the mosquito brigades thoroughly carrying out measures such as were introduced in Havannah by Gorgas, the reduction in mortality and in the number of cases has been very striking.

16554. By taking steps to prevent the breeding and multiplication of mosquitoes?—Yes, and by carefully reporting and carefully isolating the very earliest cases I look forward to the total abolition of yellow fever within five years, and that, from the commercial standpoint alone, will revolutionise the trade of those districts. Every few years that a terrible epidemic arrives and spreads New Orleans and the Southern

States, and the commerce of the whole country has been paralysed for long periods.

16555. (*Sir Mackenzie Chalmers.*) How do you account for the epidemic's rise and fall, because the mosquitoes were always there, and the yellow fever on which they feed is always there?—That is the difficult point; it is not easily explained. It depends very much upon how large the immune population is. The greater the proportion of non-immunes, the heavier will the epidemic be, because in a city like Havanna every non-immune within a year of his arrival there took the disease—he could not escape.

16556. (*Mr. Ram.*) Now that you have discovered that the mosquito is the cause, how do you ensure protection against it?—By screening the houses properly and scouring out all the pools and protecting the water tanks.

16557. By destroying the habitat of the insect?—Yes, and, particularly in Havannah, by covering up and protecting the watertanks, and by the active energetic fight against the mosquito which has been made so successfully.

16558. Then the object is the extirpation of the mosquito?—Yes.

16559. (*Sir Mackenzie Chalmers.*) The mosquito does not fly very far, I suppose?—Some of them go long distances, particularly when carried by the wind. Then, of course, they are carried by ships, and get into the holds, and get from one country to another in this way.

16560. (*Sir William Church.*) Is it not the case that in the mosquito which is connected with yellow fever the changes from the egg to the complete insect occur very rapidly?—Very rapidly.

16561. And also that its breeding places specially are not running waters or large pieces of water, but small pools, and even the water left in hollow utensils, which are not thrown away, around private houses?—Yes. I have in my pocket a recent little monograph on yellow fever by Goldberger, issued this year by the United States Yellow Fever Institute, giving a very good account of the mosquito (*handing in the same*).

16562. (*Mr. Ram.*) Might I ask has there been at present any distinct diminution in the multiplicity or numbers of mosquitoes?—Yes, in all these regions, particularly where an active crusade has been waged, as in Havannah, in cleaning out these places where they have been breeding.

16563. They have become diminished in number?—Yes.

16564. (*Mr. Tomkinson.*) I suppose they never swarm as the midge does in this country?—Oh, yes, they do, and even in the cities, in countless myriads.

16565. Is it held to be possible to exterminate them?—Yes, I do not think there is any question about it. It has already been done in many places. Ross worked at that in Ismailia; and even in the West of India under the most unfavourable conditions, the reduction in the numbers of mosquitoes has been very remarkable.

16566. Do you suppose that if it were a fact that disease of a serious nature could be communicated by the midge in England or Scotland, their numbers could be seriously attacked and diminished upon the moors and bogs, and places where they swarm?—In the immediate neighbourhood of houses it could be done. A thing which has been demonstrated, parallel with that, is the reduction in the number of cases of malaria. Take, for instance, a place called Sparrow's Point, in Chesapeke Bay; within the last five or six years, since we have known how malaria was transmitted by the mosquito, the number of cases has been diminished almost to vanishing point there, entirely by clearing up the pools and waging war against the mosquito. There has been a remarkable reduction in the cases of malaria there.

16567. (*Mr. Ram.*) The mosquitoes, of course, have been diminished too?—Yes.

16568. (*Mr. Tomkinson.*) Are those pools the breeding places?—Yes. This discovery is making Africa, of course, habitable for the white man. Many of the young men who have been out on the recent scientific expeditions have by carefully protecting themselves lived for years there without having an attack of malaria.

16569. (*Sir William Church.*) You desire also, I think, to give some of your personal experiences in the treatment of cretinism and myxœdema?—I thought it would perhaps interest the Commission (as there are diseases in which I have been specially interested, and have had an unusually large experience), if I were to speak of them, because our knowledge of the methods of cure has come so directly from experiments on animals. We have learnt through experiments upon animals that the total removal of the thyroid gland was followed by a group of symptoms resembling cretinism and myxœdema. Gull showed us and Ord showed us that a spontaneous malady occurred in man of the same character. The surgeons taught us that occasionally following the total extirpation of the gland for goitre, the same symptoms occurred. Then for very many years—centuries, indeed—this condition of cretinism has been known in certain regions in which goitre is endemic, as in Switzerland. All of these conditions, particularly the sporadic cretinism and myxœdema, may be cured completely and permanently relieved by the administration of a powder of the thyroid gland. I do not know that there is a single step in our knowledge of this method of treatment (which in some ways is the most phenomenal that has ever been introduced) which has not been the result of experimentation on animals. If you will allow me just to show you here an illustration, striking as this is, it is even more striking in a woman. Take a woman, for instance, who, with advanced myxœdema, may be reduced to a condition of imbecility, dementia, to a simply frog-like or a toad-like caricature of her former self, and in a hopeless, helpless condition. Within six months that woman may be perfectly well. And she stays well. Here, for instance, is an illustration (*producing a set of photographs*) of what may be done in eleven months. *Here* is a typical little cretin child, between two and nearly three years of age, unable to walk, unable to talk, with a pot-belly and prominent navel, with no sign whatever of intelligence about its face—in fact, showing the typical apathetic, dull condition that characterises these cases. And *here* you see the successive changes in that child in the course of eleven months. You see I have marked it *here* after two months' treatment, after three months, after five months, after seven months, and at eleven months (*handing round the photographs*). That is practically a miracle. That child was doomed to hopeless imbecility. And I have had a whole series of such cases; eighteen or twenty in all.

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16570. (*Mr. Ram.*) Is it the diseased thyroid gland which produces myxœdema?—Failure of the development of the thyroid gland in a child, or disease of the thyroid gland, sometimes following measles, for instance. The evidence has, I think, already been presented about the steps by which our knowledge has been gained in that way, I suppose by Sir Victor Horsley.

16571. (*Sir William Church.*) It has been stated that this knowledge could have been arrived at by mere observation of persons who were suffering from disease of the thyroid gland. What is your opinion of that?—I do not think so. We were struggling helpless until Sir Victor Horsley's experimental work, by which was demonstrated the identity of the condition produced by experimenting upon animals with the condition which Gull and Ord had described told us of.

16572. (*Mr. Ram.*) When the thyroid gland in animals was extracted the animals became like that?—The animal became exactly like that. I do not think there is any question about it.

16573. (*Sir William Church.*) Could you have found out in man what was the function of the thyroid gland. Has a normal thyroid gland ever been excised in man?—I do not think so. I do not know of any condition in which actually the normal gland would be excised.

16574. Excising a diseased gland is not the same thing, of course, as excising a normal gland?—Not at all.

16575. Therefore, until a normal gland was excised it would have been impossible to determine what influence the thyroid gland had upon the economy?—Quite impossible. What I feel, of course, is that such a thing as that, such a transformation as that which I have shown to you, is such a tremendous gain to humanity (just think what it must be to an individual who had a child in that condition), that it is impossible to put against it lives of a certain number of dogs sacrificed. I do not think that the two can be weighed together. And when you think that that has been done in hundreds and hundreds of cases, and will be done, and that these people remain well, I think it

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is one of the strongest evidences in favour of the benefits which have been derived from experiments on animals that we can possibly have.

16576. (*Mr. Ram.*) I do not think we have had explained to us exactly how the treatment is applied?—It may be applied in several different ways. Usually it is applied by simply feeding with thyroid gland, a certain amount of powdered gland of a sheep or pig. But these children must take it continuously. I have had one or two instances in which the parents have said, "We do not want to keep up this treatment. I do not want to send every three months to the chemist and get this stuff; it is expensive," and the children just begin to lapse into this same condition of stupor and imbecility.

16577. (*Sir Mackenzie Chalmers.*) After the eleven months?—Yes, in one case after five or six years.

16578. You still have to continue the treatment?—Yes, you have to continue the treatment practically for life.

16579. (*Mr. Ram.*) There is something absent in the child which has to be supplied?—Yes, it is that internal secretion of this gland.

16580. And you must permanently supply it?—Yes, by this method.

16581. (*Mr. Tomkinson.*) How was it discovered—through experimentation on animals?—Chiefly through Sir Victor Horsley's experiments.

16582. But how was it discovered?—By removing the gland, in monkeys particularly, and finding that this condition was produced. I have forgotten exactly who it was that first transplanted the gland into an animal from which the gland had been removed, and then these symptoms all disappeared, showing that the function of the gland could be substituted either by a portion of the gland put underneath the skin or if the powdered gland was fed to an animal.

16583. (*Sir William Church.*) Will you now go on to the third portion of your *précis*?—That is a very important one—namely, the question of how far animals should be used to train young men in the technique of surgery. Of course, we know nowadays that surgical operations, particularly on the abdomen, have increased in a remarkable way. If you take any one of the smaller provincial hospitals, for example, the number of abdominal operations to-day is perhaps three times as great as it was 25 years ago. It is a very difficult thing to teach young men operative surgery. The usual way is to operate on the dead body. That is all very well in certain directions; it teaches the man perhaps how to make a nice stump of a limb, but he misses a great deal. He is not taught technique. It is impossible almost to teach properly the finer operations in the abdomen; I should say that it is quite impossible to do it systematically and thoroughly. A few years ago, when I was connected with the Johns Hopkins Medical School, we instituted there a course of operative surgery on animals only for a select group of senior students who wished subsequently to practice surgery; that is to say, a man had to state that he had a special interest in it, or he was not admitted to the course. A laboratory has been built, called the Hunterian Laboratory, which is in its equipment an animal hospital—that is to say, there are special rooms, and every precaution is taken to make it just as clean and as sweet and as aseptic as a hospital ward. It was very soon found—in fact, in the second course—that the veterinary surgeons and the citizens brought their animals there to be operated upon.

16584. (*Mr. Ram.*) Their diseased animals?—Their diseased animals; and this gave the senior medical students an opportunity of performing special operations under the skilled direction of one of the junior surgeons. I have brought here a paper which shows the sort of work which is done there. Here are a series of cases in dogs with tumours and herniæ, as you will see if you turn over the pages (*handing in a book*), which have all been operated upon by the senior students, and who have reported the cases. You see a whole series. These cases of hernia particularly are most interesting. I would like just to say a few words as to how the class is conducted.

16585. (*Mr. Ram.*) Is this a curative operation?—Yes, a curative operation. Two of the men who are to operate, for instance, in a case of one of these big herniæ of the dog prepare all the dressings beforehand; they prepare everything just as if it was in a hospital, and as if they were the nurses in the hospital. They prepare the animal. The animal's belly is carefully shaved, and may perhaps undergo a preliminary day or two of treatment in a case of big hernia, just as a patient would. Then the teacher gives a brief lecture, with black board illustrations, or sometimes the young man himself who is to operate has to give to his fellow-students in the class a little clinique on the hernia, what it is, and what he is going to do. Then the men proceed under the direction of the teacher to operate on the animal themselves. Of course, that is a totally different matter in the way of teaching from operating on a dead body. They have to know how to control hæmorrhage. And every single point in the technique is all done by those men themselves; and you see in those pictures what excellent results have been obtained. These are by senior students.

16586. Is the animal always anæsthetised?—Yes.

16587. (*Dr. Wilson.*) Do they operate on healthy dogs when they cannot get diseased animals?—I was just going to speak of that. A course also consists of all the important abdominal operations—short circuiting for example the intestine, or operating for the removal of the spleen or operating for anastomosis of the bowel. Those are operations done under anæsthesia on healthy animals. Then, in this Hunterian Laboratory, all experiments for the physiological laboratory or for the pharmacological laboratory are done, so that in that way the animals are there, and the results are always satisfactory, because the things are done with the greatest caution and care, and the greatest care is taken of the animals. I would like to read to the Commission one paragraph by the man who has introduced this, Dr. Cushing, because I think we have all felt there that it would be very much better if we could get animals other than the dog to operate on. If pigs were cheaper, for instance, I think they would be much better, because the intestines of the pig are more allied to those of man. But this paragraph expresses very well the feeling which I think actuates the man in charge: "There is naturally a feeling of regret in the minds of many—of none greater than our own—that animals, particularly dogs, should thus be subjected to operations, even though the object be a most desirable one and accomplished without the infliction of pain, and did expense permit, we would gladly have used animals with which there an association of less acute sentiment on the part of all." Then he states: "This feeling in the past few months has been somewhat mitigated by the fact that, learning of our work, the owners" of diseased dogs "have begun to bring these animals to us for treatment." But I do not think anyone could visit the place and see the way in which the class is conducted there without feeling that there is just the same care taken of the animal really as of a human being, both prior and subsequent to the operation.

16588. (*Mr. Ram.*) Is there any statutory regulation of experiments in America?—I do not think there is any.

16589. (*Sir William Church.*) Can you tell the Commission at all what proportion the animals that are brought to the Hunterian Laboratory for operation bear to healthy animals which are got for demonstration?—I could not say that. There is a very large number given here for this year, but how far that has grown or not I cannot say.

16590. I should be glad if you could tell us a little more as to the courses of instruction that these men receive. Do they first of all see a number of demonstrations upon healthy animals, and then, when they have received that instruction, operate themselves upon diseased animals?—An important operation, for instance, like this of hernia, would not be given to a man unless he had already a certain measure of technique, and till he had had some training. Does that answer the question?

16591. Not quite fully. I did not know whether, for instance, in training these men, they all had to perform some operation upon a normal animal in the first instance?—I do not think necessarily. What we felt was that if possible it would be very much better to send out a group of young men who wished to practise surgery, knowing all this modern technique, and knowing it thoroughly, having learnt it upon animals, than that they should go and learn that technique in the first place on their patients.

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16592. Technique can be taught on human patients in hospitals, and is now so taught, is it not?—No, not the technique, for instance, of stopping hæmorrhage. Let me read to you a statement that is made here by Dr. Cushing with reference to this point, because I think it will state quite clearly these four points: "The paramount call upon the instructors, in this Listerian era, is that he shall emphasise and drill into his students, not as mere onlookers or hearers, but as actual performers, the significance of that much-abused term, surgical 'technique,' of which to-day the all-important element is asepsis—the first and the everlasting thing to be indelibly stamped on the make-up of everyone who proposes to undertake operative work, whether as a surgeon or investigator. Surgical cleanliness, which must become a reflex matter—an operator's second nature—and which, like all other reflexes, must be learned early, is necessarily disregarded in the time-honoured methods of teaching this branch." That is the ordinary dissecting room course.

16593. I was not alluding to the dissecting room course, but rather to the practice in the wards and in the operating theatre?—If you will excuse me, I think that is met by these other points: "Next in importance to the acquirement of this reflex habit of cleanliness is the ability to dissect, and to gently manipulate living tissues without damaging them so as to interfere with perfect reactionless healing."

16594. That is what I am coming to next. That appears to me to be a point in which operation upon animals is of great assistance to a surgeon—that you are dealing with living tissues, and not with dead ones. And here——?—Yes. And, again: "A third great requisite, which cannot be learned upon the cadaver, is an acquirement of skill in the proper control of hæmorrhage, from the large as well as small vessels, for the old-fashioned rough methods of hæmostasis happily are still followed by few. Nor, finally, can facility in the particular technique of visceral surgery" (surgery of the abdomen and chest) "be properly obtained through practice on the lifeless body, a fact which almost all of those who have been pioneers in these fields of work have emphasised." I may state, too, in addition, that this laboratory has been used extensively for trying new methods of surgery. For instance, quite extensive researches have been carried on to test these new methods of surgical treatment of arterial disease, particularly aneurism, and these new methods of ligaturing arteries, and of suturing arteries, which possibly may have a very important influence upon the treatment of arterial disease.

16595. I suppose in your mind the great advantage is that the student in these courses of operative surgery on animals is handling and dealing with living tissues?—Yes.

16596. I do not see much in what you have said about the technique which I should have thought a student could learn in his clinical work, and in the operating theatre on man?—Possibly; in some places where, perhaps, he is allowed to operate; but it is a very different thing to do an operation yourself and to sit in the amphitheatre, and even to be close to the surgeon and pass sponges and pass lint to him.

16597. The circumstances would be very different, would they not, in many cases in which you operate on man and on animals—for instance, the length of the mesentery and all that sort of thing in abdominal operations would be very different?—Yes, and yet, if you take one of these animals that was operated upon—I happened myself to see one of those operations on the hernia—they are very similar in the dog and in man.

16598. Could you say from your own knowledge that what is now suggested to be a regular course of instruction for those who should become surgeons has been the practice for many years among surgeons themselves in their operations?—Nearly all great surgeons, of course, have operated on animals from John Hunter's day. I should think that nearly all the great surgical procedures have been tried on animals first, just as medicines are tried on animals before they are tried on man.

16599. Should you say that the Act which was passed in this country in 1876 had any effect in preventing surgeons acting as efficiently as they otherwise might do?—I think it has hampered a number of surgeons; I think it must hamper them. I do not know how far a surgeon would be allowed to perform operations on animals for a definite purpose.

16600. He cannot without a licence?—But even with a licence, with the definite purpose of testing some surgical procedure which he desired to do. Do you say he cannot?

16601. (*Sir William Collins.*) For manual dexterity the Act forbids it?—Yes, but not simply for manual dexterity, but if, for instance, he had some new device like Hunter's device for curing aneurism, coud he do so?

16602. It would be practically part of a scientific investigation, I apprehend?

16602A. (*Sir William Collins.*) For manual dexterity —Sir Mackenzie Chalmers will correct me if I am wrong—that you cannot get a licence for acquiring manual dexterity, but if the experiment is for a scientific purpose, like some new method of tying an artery, you can.

(*Sir Mackenzie Chalmers.*) Wherever there is an increase of knowledge to be gained by experiments you can get a licence.

(*Witness.*) Then that would cover it.

16603. (*Sir William Collins.*) There is no Federal law on the subject of vivisection in any of the States, is there?—I do not think there is any Federal law.

16604. In none of the States, either, is there such a law?—No.

16605. Do you think that experimental investigation prospers more under that system than it does under ours?—I do not know that it prospers more, but it is hampered less, I think; there are fewer vexatious restrictions.

16606. Having had experience of both systems, would you suggest that we should follow the method of the States?—Personally, I feel that the matter could be left safely in the hands of the men who are in charge of the physiological laboratories and the scientific men of this country. I feel, of course, rather strongly; I would not like, perhaps, to express my feelings as strongly as they exist on what I regard has been in some ways a standing insult to the humanity of these men. They have been hounded by a great section of the public in a way that I think is disgraceful to the English people. These are men who have lived lives of devotion and self-sacrifice, and belonging to a group of men whose service to humanity has been so incalculable that they ought not to be treated in the way they have been.

16607. You think that we might trust implicitly to the humanity of the physiologists?—Absolutely. I know these men; they are just as humane as any other men; and to place these vexatious restrictions upon them is an insult, I think.

16608. Referring to the special cases which you have brought under our knowledge, I understood that in the case of yellow fever the recent experiments have been on man?—Yes, definitely, with the specific consent of these individuals, who went into this camp voluntarily. They were volunteers, just like "forlorn hope" volunteers.

16609. We were told by a witness yesterday that, in his opinion, to experiment upon man with possible ill-result was immoral. Woud that be your view?—It always is immoral, without a definite, specific statement from the individual himself, with a full knowledge of the circumstances. Under those circumstances any man, I think, is at liberty to submit himself to experiments.

16610. Given a voluntary consent, you think that entirely changes the question of morality or otherwise?—Entirely.

16611. There were a good many experiments upon animals in regard to yellow fever prior to these experiments that you have told us of, were there not?—Very many; there were, of course, the experiments by Sternberg and Sanarelli.

16612. Did Sanarelli claim to have discovered the cause of yellow fever?—Yes, a special organism.

16613. I see in Mr. Paget's book he states: "In 1896, at Flores, Sanarelli discovered the baccillus icteroides; and by October, 1897, he had prepared an immunising serum, which was able to give a considerable amount of protection to animals." "Furthermore, Sanarelli was able to show the preventive value of the serum" in a case, I think, at San Carlos, and Paget says: "Every

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prisoner, except one who had already had the fever, was therefore given the preventive treatment. At once the outbreak stopped; no more cases occurred, though only a weak serum was used, though the state of the prison and its occupants was unhealthy, though the fever, two months later, was still raging round the prison in the town"?—I do not think anybody places much reliance on that. I think Sanarelli's work has not been substantiated.

16614. I see in the paper by Joseph Goldberger that you have just handed in to the Commission, on page 7 there is a heading: "Experiments to show that the Bacillus icteroides Sanarelli stands in no causative relation to yellow fever"?—That was the first work that the American Commission did.

16615. Then if the bacillus of Sanarelli stood in no relation to the cause of yellow fever, I apprehend that this argument of a protective serum falls to the ground?—Entirely. I think it is not used at all.

16616. Did not F —— also describe an organism as the cause?—No, Sternberg.

16617. And did not Durham and Myers also describe another bacillus as the cause?—I do not know that they described it as actually the cause. Sternberg's organism was a mere suggestion. I do not think it was definite.

16618. I see on page 236 of Mr. Paget's book he refers to the work of Myers and Durham, and states that "The Report gives evidence that the disease is due to a bacillus, which is not the bacillus icteroides"?—Yes.

16619. Is this bacillus of Myers and Durham to be dismissed as also non-causative?—I think it is probably a spirochoete like the protozoal parasite—not an ordinary bacterium.

16620. Then I see "A later Commission," according to Mr. Paget, "to New Orleans, September, 1901, to January, 1902, reported an extensive series of investigations, which seem rather to support the belief that the bacillus icteroides is the cause of the disease." Were those investigations on animals?—I do not know, but I think these more recent observations have shown that we do not know the nature of the parasite.

16621. Then all theory and practice based upon a bacillus as the cause of yellow fever we may now dismiss as erroneous?—It is, in all probability, a spirochoete, not an ordinary bacterium. That that seems most likely is shown by the similarity to the cycle of development of the mosquito in malaria taking a specific period of time. It takes 12 days in the body of the mosquito before the poison becomes active. It is very unlikely when you judge from analogy of the filaria of the mosquito parasite, that that is anything but a living organism that passes through a definite cycle, just as the malaria parasite does.

16622. But morphologically are we able to find the cause of yellow fever?—No, but that is not surprising, when you remember that only quite recently the spirochoete of syphilis has been discovered. Probably it is an exceedingly minute organism.

16623. Is the method whereby you think that yellow fever may now be abolished that of immunising the community or destroying the mosquito?—Destroying the mosquito and isolating all early cases.

16624. By isolation and by the destruction of the mosquito?—Yes.

16625. I rather gathered that you considered that the case of the use of the thyroid gland for the purpose of the cure of myxœdema and cretinism was one of the strongest examples to be cited in favour of vivisection experiments on animals?—Certainly.

16626. Those of us who have witnessed the changes resulting in cases of myxœdema and cretinism as the result of feeding, or in the early days grafting the thyroid, cannot fail to be struck with the enormous advance that has been made?—Just so; and the cases were recognised as not uncommon. There were two patients, for instance, who had become hopeless dements following total removal of the thyroid gland, and who had been in that condition for years. At once, following Sir Victor Horsley's work and the discovery of the fact that the thyroid extract was potent, these patients were given it, and after many years of almost complete imbecility they were cured.

16627. Should I be wrong in thinking that prior to Sir Victor Horsley's work there had been a good deal of clinical work?—There had been a good deal of clinical work by Gull and Ord, and the men of Clinical Society's Committee here in this country. The disease of myxœdema was well known, and cretinism was well known; but there was no suggestion that it could be treated until the results of Sir Victor Horsley's work were known.

16628. Are you familiar with a paper by Curling upon the subject, published in 1850 or thereabouts?—Familiar at long range; I have seen it.

16629. Did he suggest an association between the absence of the thyroid gland and the presence of cretinism?—I think he did. I have referred to the paper several times, but it is some years ago.

16630. Do you remember one by Hilton Fagge in the seventies, I think?—Yes; but, of course, Gull's paper and Ord's work, and particularly the demonstration here in 1881 at the Congress of that remarkable group of cases, more than anything else stirred up the profession to the recognition of myxœdema as a special disease. On the Continent, before that, it was not recognised at all.

16631. Does not Hirsch in his "Historical and Geographical Pathology" cite a good deal of literature pointing to an association between cretinism, goitre and disease of the throat?—Between cretinism and goitre, that was long recognised, and was of course obvious, as so many of the cretins had a great big goitre. But the connection between myxœdema and goitre, and the connection between myxœdema and cretinism was not recognised until after Sir Victor Horsley's investigations. It was not really until Sir Felix Simon made the suggestion that these three groups of cases—cretinism, both endemic and sporadic, the myxœdema cases that came on spontaneously, and the operative cases that succeeded extirpation of the gland by surgeons were really one and the same disease, and all that came from loss of function of this thyroid gland.

16632. I ask you whether Hilton Fagge and Curling, prior to Ord and Gull, did not associate the absence or disease or defect of the thyroid with the cretinoid condition?—Yes, but it was long before then. I think that it was known that cretinism and thyroid diseases were associated.

16633. That they were a pair of allied diseases?—Yes, the cretinism and goitre had a definite association. That was manifest, of course, in Switzerland and in Savoy. I think all of those French and Italian Commissions lay great stress upon it. Whether Hilton Fagge suggested the connection with myxœdema I do not know.

16634. The name myxœdema was first given by Ord?—Yes.

16635. Did not Fagge suggest that the presence of goitre might be protective against cretinism?—I do not know. I would not be certain.

16636. In regard to the Johns Hopkins' Hospital at Baltimore, you would advocate the introduction of that system, I gather, into this country?—Under certain conditions. I think, for instance, that if certain of our senior students who were to work at surgery could be taught it on diseased animals at the Brown Institution or at the veterinary college, it would be of great advantage.

16637. Is there any proposal on foot at Oxford to introduce the Johns Hopkins' system there?—There is no practical medical school at Oxford. The men are only taught the scientific branches.

16638. You have called attention to an article by Professor Harvey Cushing in which he expresses regret that it has been necessary to employ the dog, and even though anæsthetics were used he still apparently regrets the use of the dog?—I think we all do.

16639. On grounds of sentiment?—Yes.

16640. Not sentiment in any disparaging sense, but in a lofty sense?—Yes, I think we all do.

16641. He puts in the words "did expense permit." Apparently it is a question between the sentiment and the expense?—I do not know that it is altogether. I suppose the pig would be a more suitable animal in some ways.

16642. Then apparently he suggests that the difficulty has to some extent been got over by the fact that the public bring diseased animals to the Johns Hopkins Institution?—Yes.

16643. Do you think that it would be possible to limit the system to the use of animals that are diseased and not employ those which are healthy?—Yes, in a large city like this, when you think of the number of animals that are diseased, and that really require operation, I do not think there would be a serious difficulty.

16644. Would it necessitate keeping the animals alive after an operation?—They would be operated on to keep them alive—animals with hernia, for instance.

16645. I am afraid I have not made myself clear. I was suggesting would the scheme be practicable if animals which are suffering from disease, and possibly a disease which would require the destruction of the animal, could be employed for the purpose of operation under anaesthetics?—No. I think such animals should be destroyed without being operated on. I think it would be much more humane to destroy those animals, especially when you consider the list that is given in that article of all sorts of diseases that are curable by operation, and with the greatest relief to the poor animals, and the greatest relief also mentally to the owners.

16646. But I understand that one of the chief reasons why you advocated this system was in order to familiarise those intended to become surgeons with such operative processes as anamostasis, short circuiting of the intestine, and so forth?—Yes.

16647. Do you think that opportunities arising for performing those operations only on diseased animals would be sufficient for your purpose?—I think not, except in cases perhaps of hernia, where very often there has to be a resection of the section of the intestine that is being tied in the hernia.

16648. Do you think that those comparatively rare operations would be adequate for your purpose?—I do not know that.

16649. So that it would necessitate the utilisation of normal animals for the purpose?—Yes, if it was carried to any great extent. With a very large number of students it would, of course, be impossible.

16650. Would you advocate it for all medical students?—I would not advocate it for all medical students, but for the special group of men who wish to become practical surgeons. I think it would be very much better for these men to learn a special technique on animals, than that they should learn that technique at the risk of their fellow creatures subsequently. Take, for instance, the case of appendicitis; there is a great deal of very delicate technique often required in the manipulation of the stump of the appendix, and if that is not attended to properly the risks are considerable; and there is all that finer manipulation of such a delicate structure as the bowel.

16651. Would you advocate it in the case of all those medical students who are going to take up residence on medical appointments?—I think it very important, particularly for the resident surgeon of a hospital.

16652. For all intending to become house surgeons? —For all definitely purposing to become practitioners of surgery.

16653. (*Sir Mackenzie Chalmers.*) I want to ask you a few questions—first as regards the Act in England. The Johns Hopkins University is in Baltimore?—Yes.

16654. At any rate in that State there is no Vivisection Act?—I do not think there is such an Act in any State.

16655. At any rate there is not in that State?—I do not know of any.

16656. From your experience in the States, do you think that the consequence of there being no such Act there is a somewhat lower standard than there is in England of care for animal suffering in experiments?—I should not think so; not that I have ever seen.

16657. If you have no Act is there not the danger that while you might perfectly well be able to trust experienced medical men and experienced physiological teachers there is nothing to prevent the youngest and wildest student undertaking painful operations?—Oh yes there is. In any well-managed laboratory no student could do it.

16658. But outside the well-managed laboratories, there is nothing to prevent it?—No, but there is nothing now in this country. No Act is going to prevent a group of boys from torturing animals; they have been able to do that at any time.

16659. Except that they are punishable?—A man is punishable in any case under the Act for the Prevention of Cruelty to Animals which deals with it. There is a very active society in Baltimore, and as a rule in the United States animals are treated with just as much humantiy as they are in this country, particularly in the cities. I think in the large cities it is surprising how little ill-treatment of animals you see. I should say animals are just as well looked after there as in this country.

16660. You do not think that there is a risk of painful experiments being done without proper care and by people who are not properly qualified to perform them? —Not in well-organised laboratories.

16661. Take Baltimore for instance; would a student performing a painful operation *bonâ-fide* for some purpose of increasing his knowledge or improving his technique, be liable to prosecution under the ordinary law of cruelty?—I do not think so. I do not see under what circumstances he would do it except in a laboratory.

16662. To give a possible instance, he might try some of the, I was going to say, feeding or rather starving experiments in his own rooms?—I do not know how far he would be liable; it is quite possible he might be liable. He probably would be.

16663. Do not you think it is better that in order to perform experiments which may be painful, there should be some formality gone through to impress upon people the need of care and the need of humanity?—Yes, if you wish to act at the dictation of a group of people who distrust the greatest benefactors humanity has had.

16664. Even apart from that?—I do not know apart from that that it would be done.

16665. Now, then, coming to the use of animals to acquire technique; I suppose that a great deal of good could be done if animals were dealt with by students only for curative purposes; they would learn a good deal?—Yes.

16666. If students before obtaining hospital appointments operated upon animals merely for curative purposes, treating the diseased animal exactly as a diseased patient, there is nothing to prevent it in the existing law of England?—No, I do not suppose there is.

16667-8. Then as regards what you were saying about yellow fever, these particular experiments that you referred to were animal experiments, but the particular animals happened to be volunteer men?—Volunteer soldiers.

16669. If other animals had been available you would have preferred, of course, that the experiments should have been performed upon them?—Naturally.

16670. But as a fact, no other animals except volunteer men were available at the time?—That is so; I do not suppose the higher anthropoid apes could be had there, and it was not known that they are susceptible.

16671. And their susceptibility probably is not so great as that of man?—No; and there has only been just this one observation that Dr. Thomas has recently made.

16672. Still, there is no essential difference between that experiment and other animal experiments you would say in principle?—Certainly, given the definite consent of the individual.

16673. But I mean, putting aside all other questions, for scientific purposes this was an animal experiment?—Of course it was.

16674. How long have you been in practice? You have been in large practice as a physician, not only as a teacher, but as a practising physician?—I have been in practice for thirty-five years.

16675. You saw the introduction of what is popularly called Listerism?—Yes.

16676. You have watched the effects of it?—Yes. I started in pre-Listerian days in the old Montreal general hospital, where amputations nearly always suppurated, and a great many cases, of course, died.

16677. A great many gangrened and died?—Yes.

16678. After a severe operation in those days did you rather expect bad symptoms?—We always expected trouble after nearly every operation. A case

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that healed by first intention, we all looked at with great surprise.

16679. And now?— Now a case that does not heal by first intention we all look at with great surprise—exactly the reverse condition.

16680. Has the introduction of what is called Listerism had any effect on another troublesome thing, hospital sore throat?—It has removed all that condition which was known as hospitalism previously—a whole group of infections to which surgical cases were liable.

16681. We were told by a witness the other day who has had considerable surgical experience, that Listerism is now broken down and discredited?—Where did you produce that gentleman from—Hanwell?

16682. I will not mention his hospital, but he said that the antiseptic treatment had been absolutely discarded, and that the aseptic treatment was a reversion to the old pre-Listerian days?—It is the difference between tweedledum and tweedledee. The antiseptic surgeons practised aseptic surgery, and it is aseptic surgery.

16683. They are both applications of the same principle?—Yes.

16684. There being a slight difference in the one, due to increased knowledge?—Yes, it does not make any real difference.

16685. The witness, Dr. Bantock, also denied that the bacilli or protozoa which are found in inseparable connection with certain diseases have any causative effect, and he put his view rather strikingly. He said you might as well say that trout or salmon in a river were the cause of the river as that the bacilli or protozoa were the cause of, say, Malta fever, or malaria, or sleeping sickness. Perhaps I might read you some words of his?—I think you might spare me. I decline to listen to twaddle of that sort. I would not answer a question of that kind.

16686. I will put it in this way. In America, as well as in England, are these minute entities now universally recognised as causes and not merely concomitants?—Oh, yes, by everybody who has paid any attention to the subject.

16687. I should like to read you one or two words in which he puts his theory: "The diphtheria bacillus, again, is associated with the septic or membranous throat, the typhoid bacillus with ulcerated intestine, the cholera vibrio with the inflamed intestinal canal, and the plague bacillus with the inflamed bubo or pneumonic lung. In all these instances it is not logical to conclude that these micro-organisms, instead of being pathogenic, are playing the part of Nature's scavengers"?—He is quite entitled to his opinion.

16688. But it is not the opinion of the medical profession?—It is not the opinion of any man whose opinion is worth listening to, who has paid any attention to the subject, who has studied the subject, and who knows the conditions as they exist.

16689. Does his opinion represent anything of what might be called a school of medical thought?—Not at all.

16690. Or is it a merely individual eccentricity?—It represents the school of the back numbers.

16691. Just one casual point with regard to myxœdema and cretinism. You mentioned Switzerland. In consequence of the recent discoveries is cretinism being reduced in Savoy?—I think they have been making endeavours. There are great difficulties about it, though. They have not, I think, done as much as, perhaps, we could have expected. I cannot speak positively about it, because it is some years since I looked up the question of endemic cretinism—in fact, I know nothing at first hand of endemic cretinism.

16692. You have never seen a case of endemic cretinism treated?—No, but I think cases have been treated, and some treated successfully.

16693. Is anything known of a local cause in Savoy which predisposes to cretinism?—I do not know definitely. I think a good deal of work has been done recently.

16694. (*Mr. Ram.*) What was it that first led to the suspicion of the mosquito as the cause of propagating yellow fever?—It was recognised in the Southern States very many years ago that yellow fever was worse in the seasons when the mosquitos were abounding. A very remarkable man named Nott was one of the first to suggest that the insects acted as carriers. Then I may state also that Mr. Finlay, a Scot, who has been practising for many years in Havannah—for 25 years—has been an ardent advocate of the mosquito theory, and he deserves a great deal of credit really for having insisted that the mosquito was the carrier; and it was largely on his suggestion or based on his strong belief, that the United States Government Commission carried out this series of investigations.

16695. Are animals in the infected districts liable to attacks of yellow fever?—No.

16696. Is any animal other than the anthropoid ape, so far as you know?—We only know it of the anthropoid apes experimentally; it is known that they suffer. They do not exist in the regions where yellow fever is.

16697. I take it that this great discovery of the mosquito as the author of yellow fever, and of malaria, too, is not due directly to experiments on animals?—It is not absolutely, unless you speak of man as an animal.

16698. I meant animals other than men?—No, not the domestic animals.

16699. You told us in your evidence, and I think we quite appreciated it, that it was the research spirit to which we owe this most important discovery?—Yes, these things would not otherwise have been attempted and done. These are the descendants of men who have made modern medicine what it is to-day, and that has been altogether through the experimental spirit.

16700. So I gathered. But otherwise than that, the mosquito theory for malaria and for yellow fever is not directly due to experiments on animals?—No, not directly.

16701. With regard to malaria being carried by mosquitoes, have you been able to ascertain in that case, as in the case of yellow fever, that the mosquito must have bitten an infected patient within a certain time?—No, I do not think there is any day limit, as there is in yellow fever. The probability is that at any period in the course of malaria—that is to say, when the parasites are circulating in the blood, the mosquito may become infective.

16702. But is it your opinion that in that case also in some period, perhaps undefined as to malaria, the mosquito must have bitten an infected patient?—I think that is definite, and a special variety of mosquito; it is not any ordinary mosquito.

16703. And ultimately it is to the destruction of the mosquito that you look for immunity from these two diseases?—Yes, the destruction of the mosquito and isolation of the cases.

16704. Either destroying them or keeping them away from the human subject?—Yes, but you must have an infected patient and the mosquito to keep up the chain.

16705. I think other witnesses have told us with regard to malaria that in their opinion (and I think they said it was based on experiments), given a district in which no mosquito could get near the infected patients, given a certain time over that district, the disease might be wholly stamped out in that district even though mosquitoes subsequently existed there. Have you any evidence as to that? You follow my question?—No, I did not quite catch it.

16706. It was this: Taking a certain area at present infested by mosquitoes whose bite gives malaria, if you were to clear that district for, say six months, of all human creatures—at any rate of all human creatures that had any malarious poison in them at all—even at the end of that period, though the district might be full of mosquitoes, those mosquitoes, having lost the source of contamination, would not infect other human creatures?—No, they would not.

16707. You think that is so?—I do not think; there is not any question about it.

16708. With regard to experiments by pupils, would it be possible in the case of those experiments which you have advocated should be allowed to senior students, as to abdominal sections, and so forth (I am not speaking of curative operations, but experiments), to enact that in every case the animals shall be under a lethal dose of anæsthetic?—Yes.

16709. And that in no case need the animal for the

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purpose of the experiment be allowed to live?—Exactly.

16710. Therefore would you go to the length of saying that it would not be unreasonable to enact that in every case the animal so experimented upon should be under a lethal dose?—Yes, there is no reason why that should not be so.

16711. And that would not put any fetter upon scientific experiment?—No, you do not mean animals operated upon for curative purposes.

16712. No, for experimental purposes only.

16713. (*Dr. Gaskell.*) Do you not want to keep the animal alive afterwards?—Not always. There is no reason for it except for physiological reasons.

16714. I thought for the whole purpose of your surgery it was necessary to see what happens afterwards?—It would not be absolutely necessary, to obtain the technique for instance. In doing a gastric fistula for a physiologist the animal would have to remain alive; but for securing the technique itself it would not be necessary that the animal should come out of the anæsthesia.

16715. I was thinking that it was your usual custom at the Johns Hopkins Institution to keep animals alive?—Yes, but it was not absolutely necessary.

16716. (*Mr. Ram.*) I limited my question, as you know, to experiments by students for the purpose of gaining either technical knowledge or manual dexterity?—Yes.

16717. And in neither of those cases would it, in your opinion, be a hindrance to scientific research to enact that the animals must be under a lethal dose?—No, it would be no hindrance to obtaining actual technique in the performance of the operation.

16718. You have had experience both in American laboratories, where there is absolutely no statutory enactment controlling you, and you have also had experience here in England under the statute which exists to-day Putting out of question what you were saying as to the want of trust in men of noble life, have you found that the English Act has embarrassed you at all in any of your operations?—I can only speak personally upon that. I am not a physiologist. I am a practising physician.

16719. But from your knowledge of others do you consider that the English Act, as applied to-day, is embarrassing to legitimate experiments in authorised and well-regulated laboratories?—I do not think that I know sufficiently the conditions to give an opinion about that which is worth expressing; it is only just a sort of general impression one has that a number of one's friends complain of vexatious restrictions.

16720. As to experiments by students on living animals under a lethal dose, you instance that they would be able to learn control of hæmorrhage, for example?—Yes.

16721. At present, I suppose, the control of hæmorrhage can only be practically learned by a man, whether a student or in a more advanced stage, who first operates upon a living man?—Yes.

16722. Does this control of hæmorrhage require actual technical and manual dexterity?—Yes, it very often requires a great deal of skill. But it is not so much in the control of the hæmorrhage that I should say it is necessary; but within the past 20 years you see entirely new surgical conditions have come up, particularly in operations on the stomach and intestines, and for a man who has not had a good training in technique it is a very difficult thing at first to get it. I have seen it over and over again. A man who has had technical facility and practice handles his needles and everything in a totally different way. My contention is that it would be a great deal better for the community to have a group of highly-trained young men who could have at any rate opportunities to learn that technique on animals instead of having to learn and to perfect that technique on their fellow creatures.

16723. Of course, in the absence of such technical experiments on animals, the best operator has to begin in what I may term a fumbling way?—Yes, they all begin in the same way.

16724. On the human subject?—On the human subject. It is not as if they began without seeing many men who operates. A man has seen usually scores of operations before, and has helped at them; so that if he has nimble fingers he can usually imitate the man he has seen operate before, and he gets a good deal of preliminary knowledge. He begins with opening abscesses.

16725. There must be a first time for him?—Yes.

16726. Could not this technical skill and manual skill, which you say is so desirable, be obtained by experiments on animals, the animal in each case being wholly insensible to pain under a lethal dose of anæsthetics from which it can never recover?—Yes, in the majority of instances I think that it could. These, however, are surgical questions, and my surgical brethren will smile at my evidence perhaps. I am not a surgeon, only I have been in the habit of watching a great many surgical operations.

16727. On that, may I ask you, have you ever practised vivisection yourself?—Yes, to a slight extent years ago, when I was teaching physiology for a few years.

16728. Not since then?—No.

16729. Was that in this country?—No, it was in Canada.

16730. With respect to the evidence which has been given to us by some witnesses, and some witnesses of great distinction, as to the experience gained by operating on animals for experimental purposes, is the difference between animals and man so great that such experience is useless or practically even misleading when applied to the human subject?—I should say decidedly not. For instance, an operation on the arteries in animals and all the delicate operations on the blood vessels are practically the same.

16731. You would not agree with that view then?—No, but there again that is a surgical point on which my opinion is not really worth anything.

16732. You said that in many cases you thought that pigs were much more instructive animals to operate upon than dogs, apart even from the natural and wise sentiment with regard to dogs?—Yes.

16733. Is it then merely a matter of money with regard to the pig, that it would cost more than a dog?—The pig is a very difficult animal to handle, of course. It has got stomach and intestines much more like those of man.

16734. To that extent it would be a more useful animal?—Yes, it would be better. The pig was used, of course, in old days. I suppose that Galen's experiments were largely upon pigs.

16735. So we have been told. Is there any difficulty in anæsthetising a pig?—I do not know. I have anæsthetised a great many animals, but I never tried a pig.

16736. At any rate, given a pig under an anæsthetic it would be more nearly allied to man in its viscera than any other animal?—Yes.

16737. And to that extent it would be a more useful animal to operate upon?—Yes, I should say very useful.

16738. You have told us that in your opinion it might be perfectly safe to trust to the humanity of the operators, and that no legislative enactment is necessary with regard to them; but do you not think that there is an advantage at any rate in an Act, which limits and makes criminal the performance of any scientific operation otherwise than in a licensed place, with a view to restraining students and others from operating, say, in their own rooms, and in places with no restriction?—There is no objection to having certain places licensed.

16739. And with regard to the other restrictions such as are at present in the Act you have said that you regard them as unnecessary or even injurious to the reputation of men of noble lives, who have been great public benefactors; but do you not recognise that there is such a thing as a sane and reasonable public opinion of persons less informed than yourself and those associated with you, and less informed perhaps than this Commission are, who still have to be regarded with a view to the restrictions which may be put on possible cruelty to animals?—But why should they not act as they act in other cases through the Society for the Prevention of Cruelty to Animals? I see no reason why that sentiment should not receive its expression through the ordinary channels.

16740. But apart from that, unless there were certain enactments as to the performance of operations, as to the place and as to the method and as to the sur-

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rounding circumstances and limitations, it would not be possible, would it, to apply a general act of cruelty to animals?—I do not know enough really about the conditions here to speak definitely about that.

16741. At present the Act for the Prevention of Cruelty to Animals is directed to wanton infliction of pain, without any ulterior cause or any good ulterior motive, and if you relied only on that, would it not be certain that there would be a great number of vexatious prosecutions of people who, from the purest motives and in the most humane way, carried on experiments which might be thought to be painful?—I suppose there would be, unless you had certain places, as you suggest, definitely licensed.

16742. Just one question as to the difference between what has been called here antiseptic surgery and clean surgery. Is there a difference? Is aseptic surgery, that is to say the abandonment of the spray treatment during an operation, tantamount simply to a reversion to the practice of cleanly surgery before the days of Lister?—In all ages there have been a few men who have had clean hands and have appreciated clean surgery; but the difference between the ante- and the post-Listerian days is the difference between chaos and order, and between a colossal surgical mortality then and nowadays, there is a progressively diminishing mortality. There has also been a great diminution in actual human suffering by the introduction of the Listerian methods and this has perhaps been the greatest of its benefits. When one thinks that before the days of Lister every operation case was dressed every day, and the poor children particularly were howling and weeping, and now a case is operated on and there is but a single dressing as a rule.

16743. (*Sir Mackenzie Chalmers.*) May I put it in this way. Is the foundation of Listerism (which is now applied universally), that contamination of a wound is contamination from without, and not from within. Is that in popular language correct?—Yes.

16744. (*Mr. Ram.*) Do you agree with this evidence, which one witness gave to us. I will read you the questions and answers: "Now with regard to this aseptic or antiseptic treatment, you have said that you were one of the pioneers of that which is practised so much now, namely, the protective measures taken by being cleanly?—(A) Yes. (Q) By being cleanly do you mean anything more than merely the use of water? (A) And soap." Is that the way in which in any great operation, abdominal section and so forth, safety is procured? Is the Listerian system sufficiently carried out if merely soap and water are used for the instruments and hands of the operator?—Not for the instruments, but often for the hands of the operator. But it varies in different cases; it is not simply one thing. Aseptic surgery includes a whole group of things—besides that the cleansing of patients, cleansing the doctors, cleansing the nurses, and the absolute protection of instruments and gauze from any contamination.

16745. And in the treatment of the wound afterwards no dressing?—The only difference is that the wound is not treated afterwards; there is no subsequent treatment. A good clean incision does not require any treatment.

16746. A good clean incision keeps clean; it cleans itself?—Yes

16747. (*Dr. Gaskell.*) When you spoke just now about malaria and Ross, would you not say that Ross's work was largely done by experimentation on birds?—Entirely; it was entirely experimentation.

16748. It would be a little false to say that the cause of malaria was discovered without experiments on animals?—Certainly, because there were experiments on birds.

16749. It was the life history of the organisms found out by Ross by experimentation on birds which really settled the question, was it not?—Yes.

16750. Could you tell me when those surgical experiments upon animals began at the Johns Hopkins Hospital; do you remember the year?—I do not remember the year definitely. I should think about 1904—the session of 1904-5.

16751. But they were going on when I was there with the Mosley Commission in 1904; was that the first year?—Yes.

16752. I was wondering whether any of those students who had that course were now in practice?—Yes, a number of them.

16753. Could you tell us at all whether there has been any noted difference in those students, when they first began operating upon man as compared with others who have not had that training?—I could not tell you any specific instances, but there have been two or three men who have gone out from the school extremely dexterous operators; I mean men who have had sufficient operative experience to tackle almost anything in the way of an operation, experience gained altogether on animals in comparative surgery.

16754. What I was rather wanting to know was whether the older people belonging to the Johns Hopkins Medical School, like yourself and others, have come to the conclusion that there is a distinct advantage in this training?—I think so. I must say that I was very greatly impressed with what I saw when I was out there last year, and saw the course.

16755. Not theoretically at all you understand, but actually whether the men turned out have shown themselves better men?—Yes, I think so. It is a course only given to a group of men specially interested in surgery.

16756. Quite so, the best men?—Yes, the best men, and the men who wish to practise subsequently.

16757. I was wondering rather whether any comparison could be made between the men who have been sent out since that school has been started, and previously to that. It is very difficult, of course, to make any statement of that kind?—Yes.

16758. Can you tell me at all about Professor Howell's Physiological Laboratory? Are you acquainted with the work going on there?—Yes.

16759. Is there not something of the same sort there also in physiology? Do not the students do physiological experiments for themselves very largely?—Yes, but they are done now largely in the Hunterian laboratory.

16760. Was it not the habit there rather (I remember when I was there Professor Howell showing it me) that a group of students would perform an operation for physiological purposes under the supervision of the head of the laboratory?—Yes.

16761. And that still continues?—That still continues, but I think now that all the important operations are done in the Hunterian laboratory, because the animals are better looked after, as it is just like a hospital.

16762. In your opinion that is a good method of teaching?—I think it is impossible for certain aspects of physiology.

16763. It is not done, of course, in England?—I think very greatly to the detriment of the teaching of physiology.

16764. So far as I understand there is this sort of difference: that in England the teacher demonstrates an experiment before a class of, say, 20 to 30 students; in the Johns Hopkins Hospital that same experiment is demonstrated by the students to the teacher, the students being only, say, three or four?—I think only in suitable cases; I do not think all experiments are so performed.

16765. I mean in suitable cases. And you would rather think that that is a better method of education than the mere demonstration that we have?—Yes, in a certain limited number of cases; I think particularly demonstrations of blood pressure and demonstrations of the effect of anæsthesia are practical experiments which it is very difficult to get students taught properly, unless they are either shown or do these experiments themselves.

16766. Have you any knowledge at all with respect to those experiments which are done in that way in Professor Howell's laboratory of any inadequacy in the method of experimentation, in the anæsthesia or anything of that kind?—I think not. I think they have been very careful. Dr. Howell is a most humane man in every way. So far as I have ever seen things there—and I have been in the laboratory at intervals—I have never seen anything that was in the slightest degree indicative of cruelty to animals.

16767. But I mean carelessness; whether the students have in these operations evidently been extremely careful?—Yes, I should say so.

16768. That was my experience when I was there; they seemed to me extraordinarily careful?—Certainly.

16769. (*Mr. Tomkinson.*) Do I correctly understand that it is believed now that malaria and such complaints are only communicated by infection carried by insects?—Yes.

16770. And that the extirpation of the insects would mean the extirpation of the disease practically?—Yes, and the isolation of the patient in the case of malaria and treatment with quinine, not allowing the mosquitoes to get to him.

16771. But it must have originated some time or other from climatic conditions?—That is the old story, whether the owl came from the egg or the egg from the owl. I do not know that we have settled that yet.

16772. Does myxœdema exist in animals?—I do not think it does. I would not be certain. I have never read of an instance.

16773. Do I correctly understand that the thyroid gland in animals has been utilised for the purpose of injecting the fluid into the gland of a person in whom the function was wanting?—No, not injected into the gland; it is just simply eaten; it is taken as a powder.

16774. If, as I understand, there is no Act in America, and yet there is still a stringent law for preventing cruelty to animals, how is the profession protected against prosecution?—I think public sentiment trusts the men who are in charge of laboratories.

16775. (*Sir Mackenzie Chalmers.*) Is there no active anti-vivisection society in Baltimore?—I think they are active in many places. I do not know that there is one in Baltimore.

16776. They do not take any action in Baltimore, at any rate?—No.

16777. (*Mr. Tomkinson.*) There have been no prosecutions for cruelty that you know of?—I do not know of any.

16778. Although there is no exemption from the provisions of that Act given to the faculty?—No.

16779. (*Dr. Wilson.*) Was it not really through the strong representation on the part of the medical profession that the Vivisection Act was passed in this country?—I could not say. I do not know about it. I should not have thought so.

16780. Are you familiar with Dr. Leffingwell's writings?—Yes.

16781. I think he points out that it was through the strong attacks that appeared in the "Lancet" and the "British Medical Journal" that the Vivisection Act was passed?—That is news to me.

16782. You do not know that?—No.

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16783. The Hunterian laboratory is an endowed laboratory, is it not?—It is supported by the University.

16784. There is no special endowment?—No, I do not think so.

16785. Do you not attach the same importance in teaching surgical students to what has been called living pathology?—I am not a teacher of surgery.

16786. But you do recognise that the question of sentiment, the ethical side of the question, is a question that has to be faced. With regard to experimentation on dogs, for example, you yourself have said that it is rather regrettable?—Yes.

16787. Then with regard both to the extinction of both yellow fever and malaria, is not the final recourse had to sanitation, drainage, and so forth?—Yes, but sanitation which has been taught, remember, by these very things. What sanitation would there have been in the Isthmus to-day if it had not been for the work of Manson and Ross in malaria, and of those men who worked at yellow fever? Sanitation would not exist if it had not been for those men. Those are the essentials which precede it.

16788. But still those essential sanitary conditions were believed to contribute largely to the extermination of the disease before the mosquito theory was started?—But there was no knowledge how to carry out that sanitation effectively.

16789. Is there not very little analogy between yellow fever and malaria, as regards what you call the immunes and non-immunes?—I do not know about that, because in every malarial district a large proportion of the population, particularly children, really have malaria without having any symptoms of it.

16790. That is to say they get the plasmodium in the blood?—They get the plasmodium in the blood, so that they practically are immune and yet their danger is because they are capable of infecting mosquitoes and in that way conveying the disease to others.

16791. I am referring to Europeans suffering from the disease. Do they often have second attacks?—Yes.

16792. But with yellow fever you do not have second attacks?—No.

Lieut.-Colonel EDWARD LAWRIE, M.B., I.M.S., called in; and Examined.

16793. (*Sir William Church.*) You are a Lieutenant-Colonel in the Indian Medical Service?—Retired.

16794. You are a member of the Royal College of Surgeons?—Yes.

16795. Have you any other degree that you wish to have mentioned?—I am an M.B. of Edinburgh University.

16796. I hope you understand that this Commission is only concerned, in so far as anæsthetics go, in the giving of anæsthetics to animals which are under experimentation, and that we have nothing to do with the question as to how anæsthetics are best administered except to animals under experimentation?—Yes.

16797. I think you have certain points that you wish to bring before the Commission on that head?—Yes.

16798. The first is that anæsthesia and vivisection are, or ought to be, inseparably allied, and until the administration of chloroform is established on a uniformly sound basis the question of vivisection cannot be set at rest. Would you wish to make any remark upon that? I hardly follow it. I should like to know quite what you wish us to gather from it?—That until chloroform is given in the way that has been proved to be correct, vivisection cannot be carried on without pain; that the present position of chloroform leads to its administration in the wrong manner, and that leads to the administration of other anæsthetics which are thought to be better than chloroform.

16799. And which you consider are not anæsthetics at all?—No, I do not say that at all. I say they are not so safe as chloroform.

16800. Still, I do not see what the immediate connection between the two is?—What I mean is that vivisection ought not to be practised unless it is invariably done under anæsthesia.

16801. That is the law?—But the anæsthetic is now given through a tracheal tube, which necessitates a painful operation before anæsthesia is commenced.

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16802. On the contrary, the animal is anæsthetised before the wound is made?—In experiments I saw, the operation of tracheotomy was done first.

16803. (*Sir William Collins.*) Without anæsthetics?—Yes.

16804. (*Sir William Church.*) Where was that?—In Edinburgh, in Professor Rutherford's laboratory. I have narrated them in my *précis*.

16805. What date?—1890.

16806. I suppose there was no necessity for it?—No necessity whatever that I know of.

16807. And you wish the Commission to infer that that is ordinarily what is done in physiological laboratories?—I can only speak from my own observation. I understand that that is the way that anæsthetics are usually given in physiological laboratories.

(*Dr. Wilson.*) It would be a violation of the Act.

(*Sir Mackenzie Chalmers.*) It is a violation of the Act certainly; an offence is committed if that is done.

(*Witness.*) I have seen it done.

(*Dr. Wilson.*) Then it was a violation of the Act.

16808. (*Sir William Church.*) Then you go on to say that in consequence of the erroneous principles which govern the present teaching of anæsthetics, deaths occur, and the employment of such principles in vivisection experiments involves cruelty. But as the animal has to die before it recovers from the anæsthesia I do not see that it matters to the animal?—But if the tracheotomy is done first?

16809. That you have told us; we can pass on from that. It is certainly news to the Commission and contrary to what we have heard is the practice.

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16810. (*Sir Mackenzie Chalmers.*) Have you ever seen it done in any other laboratory except by Professor Rutherford in 1890?—I have seen it done without any chloroform at all. I did not see it done, but I know it is done.

16811. (*Mr. Ram.*) Where?—I would rather not say where if I am not obliged.

16812. When?—In 1894.

16813. In this country?—Yes.

16714. (*Sir Mackenzie Chalmers.*) In Scotland?—No, in England.

16815. (*Dr. Gaskell.*) What was done?—Cross circulation experiments.

16816. But what was done; was tracheotomy performed without chloroform?—The whole dissection was performed without chloroform.

16817. Do you say that it was performed without anything in the nature of an anæsthetic?—It was performed with a small dose of morphia.

16818. (*Mr. Tomkinson.*) What was the operation?—What is called a cross circulation experiment. The necks of two animals are dissected and the large blood-vessels cross-connected.

16819. (*Sir William Collins.*) What animals were they?—Dogs.

16820. (*Sir William Church.*) But it was under an anæsthetic?—I do not call morphia an anæsthetic.

16821. (*Mr. Ram.*) In no case?—Not unless it is given in a poisonous dose.

16822. (*Sir William Church.*) Then you go on to say that the chief fallacy which throttles anæsthetics at present, in the operating theatre as well as in the laboratory, is the unfounded doctrine that one of the dangers of chloroform anæsthesia is death from heart failure?—Yes.

16823. You deny, then, the fact of there being such a thing as heart failure in chloroform?—From the direct action of chloroform I do.

16824. I hardly know what the direct action of chloroform means?—Chloroform has no direct action on the heart. That is what I mean.

16825. I think you agree that chloroform lowers the blood pressure?—Most certainly it does. It is the first effect of chloroform.

16826. And how is the blood pressure lowered?—The blood pressure is lowered by narcosis of the vasomotor centre of the brain.

16827. What condition of the heart does that lead to?—It rather eases the work of the heart.

16828. Does nothing else happen to the heart at all?—Nothing else whatever until the respiration stops.

16829. But when respiration is impeded?—When respiration stops, then the heart runs down and fails.

16830. In what way?—From want of nutrition; its nutrition is cut off and it runs down and fails.

16831. Is there no other cause?—In the complicated action of the drug there is no other effect whatsoever.

16832. Its contraction is not interfered with?—Not a bit.

16833. It does not become dilated?—No.

16834. Then you go on to say that the Hyderabad Commission on Chloroform proved that the uncomplicated action of chloroform, which may be called its normal action, is to cause (1) the normal fall of the blood pressure, with (2) unconsciousness, (3) then anæsthesia, (4) then stoppage of the respiration, (5) then failure and stoppage of the heart, and (6) then death. I suppose that is agreed to by all anæsthetists, is it not, in physiology?—They do not teach it. They teach that it has a direct action on the heart.

16835. Is not that the order in which the symptoms appear?—That is the exact order in which they occur.

16836. You also on your *précis* state that, owing to the rejection by physiologists of the results of the work of the Hyderabad Commission on Chloroform, vivisection experiments have been brought into disrepute in Great Britain. I fail quite to see the bearing of that?—Because they are not done painlessly, as I take it they ought to be. Vivisection experiments are not done painlessly.

16837. (*Mr. Ram.*) Is that your opinion, or your knowledge from facts?—It is from what I have seen myself.

16838. (*Sir William Church.*) Am I to conclude from that that you think that an animal which is receiving an anæsthetic through the trachea does not become unconscious to pain?—It does, of course.

16839. Then in what way is the animal not unconscious?—In that it has had tracheotomy first, which is a painful operation. To get the tube into the trachea you must perform tracheotomy.

16840. But so far as our information goes that is not so. You have given us information which may be very valuable, that you yourself saw it done once, or heard of it; I think you said that you did not see it done?—I saw it done a great many times, as it happens, in Rutherford's laboratory in Edinburgh.

16841. In 1890?—In 1890.

16842. And you inferred from that that when an animal is kept under an anæsthetic with a tube in its trachea the initial operation is never done under anæsthesia?—I do not say it is never done, but I say that I know from my own observation that it is done without an anæsthetic.

16843. (*Dr. Wilson.*) When you say "is done" you mean that it was done when you saw it. You could not say that it is done now?—No, I cannot say that it is always done from my own observation, but I understand, of course, that it is done constantly in that way.

16844. Now?—Yes, I understand so; but I cannot say from my own observation.

16845. (*Mr. Tomkinson.*) In the case that you saw, how was the animal restrained?—It was fastened down on what was called the rabbit board.

16846. Did it cry?—No, I do not think it made any particular noise.

16847. Was it muzzled?—It was tied up.

16848. Was it muzzled?—Yes.

16849. Was it prevented from crying?—It was absolutely tied down so that it could not move.

16850. And prevented from making a cry?—I do not remember about crying at all.

16851. Did you say that it had morphia?—No, it had nothing at all before this.

16852. (*Sir Mackenzie Chalmers.*) Did you know that it had no anæsthetic of any sort or kind?—Yes, I saw it done without any anæsthetic of any sort many times.

16853. (*Dr. Wilson.*) The skin, I think you say in your *précis*, was snipped with scissors?—Yes; I took it, of course, from seeing that—I was fresh home from India—that it was the usual way of doing vivisection operations.

16854. (*Sir William Church.*) But you have had plenty of opportunities since 1890 of becoming acquainted with what is the habit in physiological and other laboratories?—No, I have been in India since then.

16855. Is that the way you do it in India?—Never.

16856. (*Dr. Gaskell.*) Who was present in Professor Rutherford's laboratory at the time?—His laboratory assistant.

16857. And himself?—Yes, and myself.

16858. Unfortunately he is dead?—Yes.

16859. Are you quite sure that nothing was given to the animal beforehand?—Yes, quite certain.

16860. (*Sir William Church.*) You go on to say that with stoppage of the respiration the intake and action of chloroform cease, and that if the animal does not then recover spontaneously the circulatory mechanism shuts down and it dies from failure of the heart?—Yes.

16861. Therefore you agree with other anæsthetists that death occurs in one of two ways: either from failure of respiration or from failure of the heart?—No, only in one way; I say from failure of respiration, and that leads to failure of the heart. The heart must fail if the respiration stops.

16862. Sir Victor Horsley (you say) has shown that death occurs in the same way by hanging when the neck is broken, and by bullet wound of the brain; and you say that in all three cases the respiration stops, more or less rapidly in the case of chloroform, instantaneously in the other two, and the heart afterwards runs down and fails. That you think is only possible when respiration has failed first?—Yes.

16863. In all three cases you say cardiac failure is attributable to the cessation of respiration, and chloroform has no more direct effect on the heart than the bullet or the hangman's rope. Then you have been good enough to furnish some photographs; but really that is a scientific question that we have not to go into at this Commission here?—Of course; but I have seen the same effect myself in a bullet wound of the brain, and I know of its happening in hanging.

16864. (*Sir Mackenzie Chalmers.*) What happens in hanging?—I know of two cases of hanging this year, in one of which the heart did not stop for 15 minutes after the hanging, and in the other for five minutes.

16865. Although the neck was broken?—Yes.

16866. Would sensation be gone?—I think so, certainly.

16867. (*Sir William Church.*) Then pages 2 and 3 of your *précis*, unless there is something you wish to draw our attention to, have not much bearing on experiments on animals until you come to the point on page 3 where you are describing the difficulty of making an incision into the trachea because of the fur?—Yes.

16868. That we have gone into. You say that that is what you saw done once?—Yes, many times.

16869. And you also say that you saw another physiologist perform prolonged vivisection experiments on dogs under a ten-drop dose of morphin solution, this dose being equal to one-twelfth of a grain of solid morphin. Had they no other anæsthetic of any sort or kind?—I believe not until the observations began on the action of chloroform.

16870. (*Sir William Collins.*) Were those dogs insensitive to pain when they had the morphia only?—They could not have been.

16871. (*Dr. Gaskell.*) Why not?—It is quite impossible that a ten-drop dose of morphin could make a dog insensible to pain.

16872. What is the usual dose given to man?—As a sedative to lull certain kinds of pain, 30 to 60 drops.

16873. One-twelfth of a grain?—No, that is much too small a dose.

16874. That very frequently is the dose?—I have never seen it given in that dose to relieve pain.

16875. One-eighth of a grain is frequently given?—That is very insufficient.

16876. Is it not sufficient in renal colic to stop pain?—Morphin has no effect whatever on renal colic. I have had experience of it myself.

16877. (*Sir William Collins.*) Would it make a man insensitive to a cutting operation?—No, not in the least; it would have no effect on the sensibility.

16878. (*Mr. Ram.*) You mean in the dose that you have just named?—In any dose I believe, short of a poisonous dose, if you take renal colic as an example. It has no effect whatsoever on renal colic.

16879. (*Sir Mackenzie Chalmers.*) You mean that with a lethal dose pain is not stopped until death occurs?—A lethal dose is a different matter. I am talking of a medicinal dose.

16880. In the case of a lethal dose, when does insensibility to pain come in?—When the animal is narcotised.

16881. (*Dr. Gaskell.*) Is it a mistake then for the medical profession to suppose that morphia is a good thing to stop pain?—It does not have much effect if there is much pain. It does lull pain in a big dose.

16882. (*Sir William Collins.*) Does the conclusion in the question just put to you arise out of anything that you have stated to the Commission?—I do not think so, but I can tell you my own experience about renal colic and morphia. I passed a kidney stone soon after coming home from India, and I was in such fearful pain that I had to send for a medical man. I sent for a great surgeon who lives close to me, Mr. Cheatle, whose name everybody knows, who gave me morphin, but it had absolutely no effect. In about 20 minutes he proposed to give me another dose. I said I was very much afraid of morphin, as it appeared to increase the collapse, but not at all afraid of chloroform. He then gave me chloroform for five and a half hours until the stone passed. But the morphin had no effect whatsoever, and it was given in a full dose.

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16883. (*Mr. Tomkinson.*) But I suppose that even if morphin can be said to lull pain it need not be an anæsthetic for cutting operations?—It is not an anæsthetic in any sense of the word.

16884. (*Mr. Ram.*) Not even in a lethal dose?—In a lethal dose it makes a man comatose. It takes an enormous dose to have any effect on a dog.

16885. (*Dr. Gaskell.*) Have you tried it on dogs?—Yes, we have. I have the notes of it here if you like to have them.

16886. (*Sir William Church.*) I think we have had a great deal of evidence that it requires a large dose of morphia to affect a dog. I think page 4 of your *précis* is chiefly what I might call matter which does not concern us; it is chiefly a scientific question. Then on page 5 you say that in the vivisection experiments which have been conducted by physiologists for the last 15 or more years, either to uphold the heart theory of chloroform or for scientific purposes, the anæsthetic has been pumped into the lungs with a motor through a tracheal tube; and that the meaning of this is:—(a) that no physiological experiments on the action of chloroform during this period possess any true scientific value, and (b) that owing to the anomalous position of anæsthetics, all vivisection experiments have been accompanied by unnecessary pain. Would you please explain a little what you mean there?—The essential point in giving chloroform is never under any circumstances to interfere with natural respiration; but if you give chloroform through a tracheal tube you must do that—you must interfere with the natural respiration, and you must vitiate the experiment so far as the action of the chloroform is concerned.

16887. I do not follow you there. It is received by all physiologists that it interferes with natural respiration when the animal is anæsthetised through the intentional opening of the trachea?—It is a matter of ordinary common sense that if you pump the chloroform in through the trachea it must interfere with the natural breathing.

16888. (*Sir Mackenzie Chalmers.*) But it produces anæsthesia?—It produces anæsthesia, and it produces other effects as well caused by interference with the natural respiration.

16889. (*Mr. Ram.*) The animal does not suffer any pain?—No.

16890. (*Dr. Gaskell.*) If it is not pumped is it not natural respiration if the animal is breathing the air mixed with chloroform through the tracheal tube?—That is a different thing. I have seen that done, but this remark does not apply to that. It applies to pumping.

16891. Do you think that pumping in the way you mention is the usual method?—It is a very usual method, I take it; I understand that it is.

16892. If artificial respiration is not required. You mean artificial respiration, I presume?—No, I mean the way of giving chloroform pumped in by a motor.

16893. I have never seen it done?—It was done in those experiments described in the trial, Bayliss *v.* Coleridge.

16894. It was passed through a Woulff's bottle?—No, it was done by a motor in the next room in those experiments.

16895. (*Sir William Church.*) Do you know of any physiologists of repute who take the same view with regard to that, that if air charged with chloroform is administered through the trachea to a dog it interferes with the correctness of their experiment?—I cannot tell you what other people think about it. I can only tell you my own view of it.

16896. Then can you tell me of anybody else's views that coincides with yours?—Everybody's view must coincide with mine. It is actual fact; it is not a matter of opinion. If you pump air into the trachea it must interfere with natural respiration.

16897. (*Sir Mackenzie Chalmers.*) You are only speaking of the effect of chloroform, not other anæsthetics?—Yes.

16898. And experiments under the action of chloroform?—Yes.

16899. (*Sir William Church.*) But it does not cause

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animals to have unnecessary pain?—Not if they are tracheotomised under an anæsthetic.

16900. You would withdraw, I suppose, your statement No. 2, on page 5, that it necessitates a painful operation before the administration of the anæsthetic begins?—If the tracheotomy is not done first it does not apply to those cases, of course.

16901. (*Mr. Ram.*) Or if it is done under anæsthetics?—If the tracheotomy is done under anæsthetics. But if it is done under anæsthetics, why should they change the mode of administration? And of course nothing can be worse for students than to see chloroform given in that way.

16902. (*Sir William Church.*) I suppose you are aware that chloroform is given in that way in a considerable number of operations on man?—Through the trachea?

16903. Yes?—No, I am not. I have never seen it given in that way. It may have to be done as a very, very rare and exceptional thing.

16904. And you consider that it teaches students wrong methods of administering anæsthetics which must tend to vitiate the whole of their careers?—Yes.

16905. Do you really mean the Commission to infer that the teaching of the administration of anæsthetics is seeing the physiological experiments in the laboratory?—I do not quite understand what you mean.

16906. You say, "It teaches students wrong methods of administering anæsthetics, and this must tend to vitiate the whole of their careers"?—Yes.

16907. I asked whether you think that teaching the administration of anæsthetics consists in seeing the physiological experiments on animals in laboratories? —They ought to be taught the right way of giving chloroform in those experiments, which ought to be utilised for that purpose. It is just as important as showing them other physiological effects.

16908. You do not mean the Commission to infer that that is the way students are taught to give anæsthetics?—So far as the laboratory is concerned it is.

16909. The physiological laboratory?—Yes, the physiological laboratory.

16910. But what proportion of the experiments which are done in physiological laboratories are done in this method of having the trachea opened and a tube inserted?—That I cannot tell you.

16911. Do you wish the Commission to infer from this evidence that you have given that it is the usual method?—I understand that it is the usual one; but of course the Commission can find out quite easily whether it is or not.

16912. You would not like to express an opinion yourself as to whether it is the usual way or not?—I imagine that it is the usual way, most certainly.

16913. You have not made any inquiries before you made this statement?—It is from what I have seen myself and from the manner in which all the experiments on chloroform have been described that I say chloroform is always given in that way through the tracheal tube.

16914. In cases of prolonged demonstrations before students, you mean?—Yes.

16915. You do not think it is the usual way in ordinary experimentation in a laboratory?—I understand that it is, but I may be wrong about that, of course.

16916. (*Dr. Gaskell.*) The point is the pumping. Is that what you imagine is the usual way, pumping the air and chloroform?—Yes.

16917. That is not true?—That was described as having been done in the Bayliss trial, and it was done in Embley's experiments.

16918. (*Dr. Wilson.*) That is counteracted by artificial respiration, is it not?

16919. (*Dr. Gaskell.*) It would be necessary for artificial respiration—not natural respiration?—But it does both.

16920. It is for artificial respiration that pumping is done?—And for the giving of the chloroform.

16921. In artificial respiration. In natural respiration the animal breathes air saturated with the right amount of chloroform?—This remark does not apply to natural respiration.

16922. (*Sir William Church.*) Then you go on to state that to make sure of painless vivisection, two things appear to be necessary; first, that it should be made illegal to give chloroform to animals in any other way than by the mouth, on the principles which have been proved to be sound by the Hyderabad Commission. That is what you would recommend to this Commission?—I think that certainly ought to be done in order to put a stop to giving chloroform in this way.

16923. Has nobody else worked at the administration of chloroform except the Hyderabad Commission?—Hundreds of people have, so far as I know.

16924. Do they all agree with the conclusions of the Hyderabad Commission?—The conclusions of the Hyderabad Commission have been absolutely ignored.

16925. It appears in other words that the majority of those who have studied the subject do not agree with them?—Certainly they do not.

16926. Would it not be putting a considerable difficulty in the way of experimentation if animals could only be kept under anæsthesia by what you call the natural way?—No, I do not think it would at all.

16927. In what you call the natural way, is there any method by which you can actually measure the quantity of chloroform that the animal receives?—No, there is not.

16928. I suppose you know that great attention has been paid to the actual amount of chloroform that the patient receives now?—It has been said that the chloroform has been measured, but no one can possibly take measure of the amount that is respired—the amount that is taken in by the patient.

16929. Are you acquainted with Mr. Vernon Harcourt's method of measuring chloroform?—Yes, I am. I have not seen it done.

16930. You do not think it reliable?—It is very reliable in this way, that it dilutes the chloroform; but it cannot measure the amount that the patient takes in.

16931. Then you recommend, secondly, that the General Medical Council should have the power to treat the infliction of unnecessary pain by experimenters on animals as a disgraceful act. I do not quite understand what you mean by that?—Some such power should be given to the General Medical Council in order to put a stop to unnecessary pain in vivisection.

16932. But the General Medical Council only have power over the medical profession?—Exactly.

16933. You would leave the physiologists, who appear to be in your mind the chief offenders, untouched? —But they are part of the medical profession, I take it.

16934. (*Chairman.*) Not necessarily, unless they have a medical degree?—Of course, some such power ought to be given. Someone ought to have the power of putting a stop to unnecessary pain. And, of course, there is the important point, the teaching of anæsthetics.

16935. (*Sir William Church.*) You say that chloroform given on the principles advocated by the Hyderabad Commission is a totally different anæsthetic from chloroform given on the heart principle; and you say that the former is always safe, the latter never. That is your own opinion?—Well, it is proved by statistics, and anybody can be taught to give chloroform safely on the Hyderabad principle, while no one can on the heart theory.

16936. You think that the giving of chloroform to dogs would be a reliable method of instruction in the use of anæsthetics for medical students?—Most valuable.

16937. You consider, then, that anæsthetics now are not given as skilfully as they might be by the medical profession?—I do not say anything about the skill. It is not a question of skill; it is a question entirely of principle.

16938. Then you go on to say that it would be a great boon to our sailors and soldiers in war time if all nurses and hospital orderlies were taught to give chloroform—at any rate, up to the stage of unconsciousness?—Yes.

16939. Then you would advocate the use of chloroform by others than those who hold a medical diploma? —I would advocate the use of chloroform by nurses,

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and by hospital orderlies, and by medical students under proper supervision.

16940. Then you go on to say that the specialist in anæsthesia was called into existence by the belief, originated by physiologists in 1864, that the fall of the blood pressure under chloroform is synonymous with heart failure. Surely that is rather a bold statement, is it not?—I think that is really what led to the employment of anæsthesia specialists.

16941. Were there not men specially skilled in anæsthetics before 1864?—There were, but a special class of specialist was called into being by the experiments of 1864, and those of 1879, which followed.

16942. You finish your *précis* by saying that chloroform was given by students in Edinburgh for more than twenty years in the middle of the last century in all surgical operations without a death. Is that quite the case?—It is absolutely true so far as the clinical wards were concerned.

16943-4. There have been deaths from chloroform in Edinburgh; I cannot say whether in the clinical wards or not?—At that time, I believe that in that period there may have been a death, there was a death, but it was not in Syme's wards. I am talking now of the wards of the professor of clinical surgery during that period.

16945. But there were accidents from chloroform, were there not?—There were no accidents in his ward.

16946. But Simpson had a fatal case?—Yes, he had, I believe.

16947. That is one. Are you aware of the difference in the way inquests are held in Scotland and England?—There are no inquests that I know of in Scotland.

16948. So that a death from chloroform does not get into the papers?—There is an inquiry by the Procurator Fiscal.

16949. But you feel quite sure that during twenty years—I do not know what twenty years you take—there were no deaths?—No death in Syme's wards from 1847 to 1870—until he died.

16950. But you do not say in Mr. Syme's wards?—My remarks apply to Syme's wards.

16951. But it makes a considerable difference?—It really does not make much difference, because I know of no death in the whole of the infirmary during that time except one rather doubtful one. That applies especially to Syme's wards. The chloroform in those days was invariably given by students. During the whole time that I was in India in charge of many different hospitals the chloroform was given by students also in precisely the same way, and there was one death.

16952. The whole bearing of your evidence before us to-day is that chloroform can be given with perfect safety, so long as the respiration is attended to?—So long as the regular respiration is maintained.

16953. And you would always be able to see whether respiration is embarrassed and you are getting into danger?—You have no business to allow the respiration to become embarrassed at all under chloroform. You must maintain regular respiration. If the respiration is embarrassed you must take away the chloroform.

16954. (*Sir Mackenzie Chalmers.*) And stop the operation?—No, on no account, stop the administration until the respiration is regular again. That is a matter of simple management.

16955. Is there any danger of pain supervening when you stop giving the chloroform?—No, not a bit, you only stop it for the temporary embarrassment.

16956. You can go on with the operation, the patient being perfectly free from pain?—You just stop the chloroform, you take away the cap while the embarrassment lasts, and then go on with it again. You do not allow the respiration to be embarrassed while chloroform is given.

16957. (*Sir William Collins.*) How long have you been engaged in studying anæsthetics?—I began to give them as clinical clerk in Edinburgh in 1865.

16958. How long have you made them a special study?—Ever since then.

16959. A special study?—Yes, a very special study.

16960. About how many cases have you administered chloroform to?—I published some statistics in 1901 for 10 years. That included 17,000 cases, and I have not got the record for previous years, because I was moved about in India. I cannot quite tell how many, but the number was enormous.

16961. We have been often told that chloroform was discovered by experiments on man, and not on animals?—Yes, but its present position depends on laboratory experiments.

16962. Was it discovered by experiments on man?—I believe so.

16963. I understand that you think Syme's method of administration satisfactory?—It was satisfactory in his day.

16964. Very satisfactory?—Yes, perfectly.

16965. Was it based upon clinical experience?—It was based entirely on clinical experience.

16966. Did Syme rather deprecate experiments on animals?—I cannot say anything about that. I do not know what his opinion was.

16967. I think before the last Royal Commission of 1876 it was stated that he rather opposed, at any rate, in his later years, experiments on animals?—From my recollection of him I should think he did, but I cannot say positively.

16968. You are a strong advocate for watching the respiration, and you think the suggestion that chloroform kills by action on the heart is erroneous, I understand?—We have proved it; the Hyderabad Commission have proved it.

16969. Did the Cambridge experiments to which you have alluded tend to show that it acted upon the heart?—The Cambridge experiments were a failure; they were allowed to be wrongly performed.

16970. Were they vitiated?—In the experiments that were done at Cambridge the chloroform had access to the brain when it was supposed to be having access to the heart alone.

16971. Was it thought as the result of those experiments that chloroform acted upon the heart?—Yes, it was allowed.

16972. Is that what you consider a wrong principle?—Yes, it is a wrong principle, from our point of view.

16973. Was the vitiation of those Cambridge experiments admitted?—It was admitted by Dr. Gaskell himself.

(*Dr. Gaskell.*) Oh, no.

(*Witness.*) I have your letter.

(*Dr. Gaskell.*) The vitiation of the experiments was not admitted by me.

16974. (*Sir William Collins.*) Are you referring to the letter on page 17 of your book on chloroform?—Yes.

(*Sir William Collins.*) I do not know whether Dr. Gaskell wishes it read.

(*Dr. Gaskell.*) You may read it if you like.

16975. (*Sir William Collins.*) What is the clinical practice at the present time of those who administer anæsthetics; is it to watch the heart or watch the respiration?—All the teaching at present in the textbooks is that chloroform affects the heart, but I have seen many of the best anæsthetists in London give chloroform, and I have never seen any of them watch the pulse.

16976. On page 20 of your work on chloroform I see you quote Sir Lauder Brunton's statement that "the fear of chloroform paralysing the heart is based on laboratory experiments rather than a clinical experience." Is that the point that you wish to bring out?—Yes, that is a point that I wish to bring out.

19677. That clinical experience rather than laboratory experiment has been the truer and safer guide in the administration of chloroform?—The point is this, that the laboratory experiments were supposed to afford scientific proof that chloroform affects the heart.

16978. I understand you to suggest that they do not?—Our experiments proved where those scientific experiments were mistaken. The Hyderabad Commission experiments traversed those experiments, and we showed where the mistake arose in the physiologists' experiments.

16979. So that the laboratory experiments I understand you give contradictory results?—Yes.

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16980. Whereas the clinical experience you regard as safe?—And there are no contradictory results in this way. All our own experiments were done by chloroform administered in the ordinary way by inhalation. All the laboratory experiments on which this theory is based, that chloroform affects the heart, were performed, so far as I know, with chloroform given through a tracheal tube.

16981. But there were results which were opposed to yours?—They are opposed to observations we made when chloroform is given by the mouth.

16982. You quote with approval the statement of Sir Lauder Brunton: "The fear of chloroform paralysing the heart is based on laboratory experiments"?—Yes.

16983. So that apparently there were laboratory experiments which suggested that chloroform paralyses the heart?—Yes, those are the experiments of the 1864 Commission.

16984. Was that conclusion in your opinion right or wrong?—That conclusion was really wrong, because it was inferred that the fall of blood pressure was synonymous with heart failure.

16985. Then should I not be right in saying that the conclusions arrived at from the laboratory experiments were contradictory?—Yes, they were in that way, but we brought them all into harmony.

16986. Was the harmony supported from the first by the clinical experience?—It was.

16986a. (*Sir Mackenzie Chalmers.*) I do not want to go into technical matters, but the Hyderabad Commission experimented with animals, did they not?—Yes.

16987. Do you agree that animal experimentation is necessary for the purpose of determining the action of chloroform and other substances?—It was necessary then to refute other experiments by laboratory experiments. There was no other way of refuting them.

16988. Do you agree that animal experimentation generally is necessary for advance in medical science?—Yes.

16989. And in physiology?—Yes.

16990. And in pharmacology?—Yes.

16991. Has medical science received great benefit from it in the past?—Yes, enormous benefit, but it would receive the greatest benefit of all if the Hyderabad experiments were not ignored.

16992. Apart from these tracheotomies that you saw in Professor Rutherford's laboratory, have you any knowledge yourself of any painful experiments being performed without anæsthetics in England?—I have seen two sets of experiments.

16993. In one of which I understand the animal had a dose of morphia?—They all had a dose of morphia.

16994. In one set of experiments nothing was done?—No.

16995. And in the other set of experiments, the cross circulation experiments, morphia was given?—Yes.

16996. Apart from that, do you know anything about the ordinary practice in laboratories in England?—No, except what I heard upon that trial, the Bayliss trial.

16997. The evidence in that trial?—Yes.

16998. It did not appear at that trial, did it, that tracheotomy was performed before the animal was anæsthetised?—I do not remember that anything was said about it.

16999. So far as we have had evidence of the Commission here, the evidence has been that an anæsthetic is administered first. Assuming that to be so, all that is done afterwards would be painless, would it not?—Quite.

17000. You have no doubt whatever that in whatever way chloroform is administered, it is an absolute anæsthetic, absolutely freeing the animal from pain?—Absolutely; from the time of unconsciousness there can be no pain.

17001. Your point is that it is improper and wrong to perform tracheotomy before the chloroform is administered?—Yes.

17002. Is tracheotomy a very painful operation?—It is a very painful operation, I should think.

17003. It is done continually on children, is it not?—It is done for croup and diphtheria.

17004. Pretty continually?—Yes.

17005. It is done on children, of course, without anæsthetics?—Not always.

17006. Could you give anæsthetics to a child with diphtheria?—Yes, you can give a small dose, but a child as a rule by the time it gets to the stage where tracheotomy is required is almost unconscious. It would practically not need an anæsthetic, or at any rate only just enough to divert its attention from the incision. You would not need more than that.

17007. In the case of a child it is not what you call a painful or severe operation?—It would be if the child was not suffocated.

17008. We have had evidence which does not always coincide on this point: That when chloroform is administered some witnesses have said that there is a sudden passage from feeling pain to insensibility, and others have said that there is a gradual diminution of pain. What is your view on that point?—Supposing that you are giving chloroform, you can keep on asking the patient questions, and suddenly the patient will stop answering, and has become unconscious.

17009. Do you think unconsciousness to pain goes before or after the general consciousness?—It goes after.

17010. For instance, if chloroform is administered not in a sufficient quantity to produce unconsciousness, is there any diminution of pain?—The patient would remember it.

17011. But do you think the pain would be equally acute?—No, not nearly so acute.

17012. You think, in fact, that as chloroform is administered, there is a progressive diminution of pain?—A progressive diminution of sensibility to pain.

17013. And that diminution of insensibility precedes unconsciousness?—Yes, and directly unconsciousness supervenes then there can be no pain; from that moment the patient will never remember anything you do afterwards.

17014. He will never remember it, and so far as you can tell will not feel it?—He will not feel it at all.

17015. You connect the feeling of pain entirely with the higher centres?—With consciousness. The patient must be conscious in order to have pain.

17016. That is to say, conscious to outward surroundings?—He must not be unconscious.

17017. But pain may be diminished before that stage is reached?—Yes.

17018. Have you performed other vivisection experiments besides those with reference to chloroform?—No, except with ether.

17019. Your whole study has been the action of anæsthetics?—Yes, and, of course, I taught students to operate by vivisection.

17020. In India?—Yes.

17021. You practised the student on an animal?—Yes.

17022. Which, of course, you anæsthetised?—Always.

17023. And then the student learnt what we may call manual dexterity?—Yes, he learnt how to become accustomed to do operations on living tissues, and also how to give chloroform at the same time.

17024. Do you attach great importance to students doing operations on living animals before they practice on living human beings?—It is of tremendous importance in India, because it is the only chance that a student ever has of doing an operation at all.

17025. But in England what opportunity has he?—In England a man goes through successive stages in hospitals of every kind. He may be house surgeon and hold all sorts of appointments, and becomes assistant surgeon and then surgeon to a hospital. But these men in India are frequently sent straight out from the schools into a district of which they have complete charge, hospital and all.

17026. They may have to do important operations which perhaps they may have only seen once in their student career?—Yes, they have to do everything from the time they get charge of a district.

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17027. You are speaking of natives, of course?—Yes.

17028. Is there any difficulty with the native population in regard to teaching students on animals; is there any very strong feeling against it?—No, there is no feeling whatever as long as you do not give the animals pain.

17029. As long as the natives are satisfied that the animal is anæsthetised properly there is no feeling whatever?—No.

17030. You have no protests?—No, never.

17031. Is it since your day that what is called the Pasteur Institute has been established?—It has been established since I left India.

17032. You cannot tell us anything about it?—No.

17033. Can you tell us anything about the feeling of the natives with regard to that?—I think the natives would place the most implicit confidence in it, from what I know of them.

17034. Have you ever discussed with natives in India the question of animal experimentation?—Yes, often.

17035. Their objection is to the causing of anything like unnecessary pain?—Yes.

17036. As regards vivisection generally, do you agree to this extent, that it is not only lawful to experiment on animals, under anæsthesia of course, but that it is lawful to allow the animal to recover for the purpose of watching the effects of the operation?—Yes, I agree in that.

17037. And do you think it is necessary to inoculate animals for the purpose of watching the progress and spread of the disease?—I cannot give an opinion of any value about that, but I should think it is.

17038. You agree, I suppose, that for very slight operations more discomfort and pain would be caused by giving an anæsthetic than, say, the mere prick of a hypodermic needle?—I think it should be entirely left to the patient, however slight the operation is.

17039. Taking the case of an animal, supposing an animal is to have a hypodermic injection, it is more humane, is it not, to use the hypodermic needle without an anæsthetic than to give the animal an anæsthetic?—I do not see how it could be, although I should think it is unnecessary to give an anæsthetic. You could give an anæsthetic in the box without the animal being upset by it or terrorised.

17040. You do not think that the administration of anæsthetics to animals causes any great discomfort or apprehension?—None whatever, unless you fasten them down or hold them down.

17041. How do they do it in India?—They put them into a box with a glass cover, and directly the animal falls down on the floor of the box you take it out and put it on the table.

17042. You think that given in that way, the animal does not suffer?—It does not mind it or make any objection to it.

17043. What animals do you use in India?—Dogs, monkeys, rabbits and cats.

17044. But you would not yourself use an anæsthetic to an animal in case of an injection with a hypodermic needle or inoculation?—I should think it hardly necessary; it is so momentary.

17045. (*Mr. Ram.*) As I gather, your evidence comes merely to this, does it not? You hold a certain theory as to the action of chloroform, as to the way in which it acts?—Yes.

17046. Other people hold that chloroform acts in a way other than that which you believe?—Yes.

17047. It is admitted, is it not, on all hands that whichever theory is correct, chloroform rightly administered is a safe and sure anæsthetic, and absolutely frees the animal from pain?—Yes, but everybody does not agree that it is a safe anæsthetic at present.

17048. I leave out the word "safe." I meant by that that it is an anesthetic which absolutely frees the animal from pain?—Yes.

17049. I see in your *précis* you use this phrase, "but if it" (that is, chloroform) "is given on the theory that it may cause death by heart failure, sometimes suddenly it is the most dangerous of all anæsthetics"?—Yes, it is.

17050. Dangerous, I suppose, in that connection means likely to cause death?—Likely to cause a certain percentage of deaths.

17051. So that it would not matter to the animal which is under operation, whether the chloroform caused death or whether it did not; in neither case would the animal feel?—That is so.

17052. So that whichever theory is right (yours or the other people's), so far as the animal is concerned the animal is equally safe if it is under a proper dose of chloroform?—So far as an anæsthesia is concerned.

17053. This Commission, as I daresay you know, has to report upon the practice of subjecting animals to experiment, whether by vivisection or otherwise, and then to consider whether the law wants amendment. Will you just tell me, please, how your evidence bears upon that, from the point we have to report upon?—My point is that all vivisection experiments can be performed painlessly if chloroform is given in the right way.

17054. Then it comes down to this, does it not, that your evidence is valuable to us according as it bears upon the method of administering chloroform?—Yes, in two ways, as regards the animal and as regards the student.

17055. You have seen it administered years ago *viâ* the trachea?—Yes, but it is on those very experiments performed in 1894 that the present teaching of anæsthetic is based.

17056. And in the instance which you gave you said that there was no previous anæsthetisation of the animal?—That is so.

17057. If an animal to which chloroform is given through the trachea is previously properly anæsthetised, your theory may be invalidated, but the animal does not suffer any more?—No, if the tracheotomy is done under anæsthesia

17058. Then so far as regards the point that we have to inquire into here, it comes down to this, does it not, that you in 1890 and 1894 saw what according to your belief was, I will take it if you like that it was an actual fact that you saw the administration of chloroform *viâ* the trachea, the animal not having been previously anæsthetised?—Yes.

17059. That was an illegal operation?—That I do not know anything about.

17060. But perhaps you will take it from us as being really contrary to the very words of the Act?—Certainly.

17061. If that be so the Act wants no amendment in that respect?—No.

17062. Because the Act already makes it illegal?—Yes.

17063. Were you aware when you saw it that it was contrary to the Act?—I was told so afterwards.

17064. You were not aware of it at the time?—I did not know it at the time.

17065. How soon afterwards did you know?—Immediately afterwards.

17066. Did you report the case to anyone as being an illegal act?—No.

17067. (*Dr. Gaskell.*) I am just going to ask you one question. Can you give me the name of any person who has been in the habit of administering anæsthetics or has studied the action of chloroform who believes that chloroform has no effect upon the heart except yourself?—Surgeon-General Bomford.

17068. You mean in India; he was with you in India, was he not?—Yes.

17069. Is there anyone in this country?—I understand that all Syme's pupils have the same view as I have, but still I cannot speak for anybody else.

17070. I meant people who have been in the habit of giving anæsthetics. I mean professional anæsthetisers and that kind of person or people engaged in the investigation of anæsthetics in physiological laboratories?—In physiological laboratories, no, I cannot give you any name, but I can give you names of many surgeons and practitioners.

17071. They have hardly studied the question, have they?—They have been accustomed to doing operations, and have studied it, I should think, very closely.

17072. (*Mr. Tomkinson.*) You give a description, I see, in your *précis* of a case in which tracheotomy was performed without anæsthetics upon a rabbit, and in

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view of the questions which you have been asked as to the painfulness of it, I want to ask you one question. I see that you describe it as having been necessary or convenient to cut away the skin—to flay the skin with scissors?—Yes.

17073. That, of course, would be exceedingly painful?—I thought so, frightfully painful.

17074. There would be no comparison between the pain caused by a mere incision in the skin of a child's flesh with that inflicted upon that rabbit?—No comparison whatever.

17075. In which the skin has to undergo the most painful operation that can be imagined almost?—Yes, a very painful operation.

17076. The skin, of course, is excessively sensitive?—Yes.

17077. The cutting away of that skin, the flaying of it back would be an exceedingly cruel thing without an anæsthetic?—Yes.

17078. And that was done in that case?—Yes, in several cases.

17079. And in no case was an anæsthetic administered before the tracheotomy?—No, in no case.

17080. What is the supposed advantage of introducing the chloroform in that manner instead of through the mouth, as I see you say? That means the mouth and nostrils, I take it? The inhalation is through the nostrils?—Through the mouth and nose.

17081. The mouth has to be covered as well?—Yes.

17082. What is the supposed advantage of administering it direct into the trachea instead of through the nostrils and mouth?—I cannot imagine any advantage whatever.

17083. You hold that there is none?—Yes.

17084. And it is a wholly unnecessary operation?—Yes, and it teaches the students a wrong way of administration; they learn nothing from it.

17085. Of course it would never be so administered to a human patient except in case of obstruction?—Except in exceptional operations round the mouth or jaws which would interfere with natural breathing.

17086. You hold strongly that morphia and morphin are not anæsthetics?—I am certain that they are not.

17087. Until the point is approached of almost stupefaction approaching death?—Until their poisonous effect is produced.

17088. I asked the question of a previous witness, whether the condition of a subject under the strong influence of morphia was not akin to that of a man stupefied with alcohol. You, I suppose, would hold that in neither of those cases are they incapable of feeling pain, although apparently stupefied?—I think you might do a great deal more to a man stupefied by alcohol without his feeling it, than to a man under the influence of a medicinal dose of morphin. We did a great many experiments with morphin in the Hyderabad Commission, and our conclusion was that dogs are very indifferent to the action of morphin, so that if you want to produce any effect upon them at all you have to give an enormous dose.

17089. Have you any experience of the action of curare?—No.

17090. Or the use of it?—No.

17091. Have you an opinion about it?—I really have not, because I have no experience of it at all.

17092. But you know, I suppose, generally, what its effect is?—Yes.

17093. Do you hold any strong view as to the necessity or the advantage or the propriety of administering curare in company with other anæsthetics?—No, I cannot see what is to be gained by it myself.

17094. It is rather a leading question, but should you have any fear that its paralysing effect would prevent the animal from showing signs of pain, although it might feel it?—I should think it might, but I really do not know.

17095. (*Dr. Wilson.*) Were these morphia experiments carried out at Hyderabad on dogs to render them, or to attempt to render them, completely insensible to pain?—We carried them out really to see whether they deepened the anæsthesia or not.

17096. With chloroform?—Yes.

17097. Have you operated on dogs with morphia alone as an anæsthetic?—No, we never used it as an anæsthetic; but it had no effect in deepening anæsthesia.

17098. Can you give me an idea of the dose that would prove fatal to an ordinary sized dog?—I should think you would have to give it at least 5 or 10 grains.

17099. But it would vary very considerably, I suppose, according to the idiosyncracy of the animal, and possibly the breed of the animal?—Yes.

17100. But you could not be guided by the body-weight?—No.

17101. Did you experiment with any other narcotics besides morphia?—We experimented with various drugs administered in order to ascertain whether they had any effect in modifying the action of chloroform; such as atropin, morphin, nicotin, and so on.

17102. But with your knowledge of morphia or morphin, would you say that it was an unreliable anæsthetic for experiments on animals?—It is not an anæsthetic at all.

17103. I mean an abolisher of pain, then?—It would not abolish the pain of a cutting unless you poisoned the animal with it.

17104. But supposing that an animal gets what is called a lethal dose, how long would it live do you think under an operation?—That I cannot tell you. It might live some hours.

17105. But it would be completely insensible to pain all the time?—Yes, quite, if it were comatose.

17106. Have you seen much of plague out in India? You do not come here to give evidence on it?—No; but I have seen a great deal of plague. I was Plague Commissioner for Hyderabad for five years.

17107. Do you believe in Haffkine's prophylactic?—At the time when I saw it used, it was always putrid.

17108. Have you studied the literature of the subject since?—I cannot say that I have followed it very closely since.

THIRTY-EIGHTH DAY.

Tuesday, 26th November 1907.

PRESENT:

The Right Hon. The Viscount SELBY (*Chairman*).
Sir W. S. CHURCH, Bart., K.C.B., M.D.
Sir W. J. COLLINS, M.P., M.D., F.R.C.S.
Sir J. McFADYEAN, M.B.
Sir M. D. CHALMERS, K.C.B., C.S.I.
Mr. A. J. RAM, K.C.
Mr. W. H. GASKELL, M.D., F.R.S.

Captain C. BIGHAM, C.M.G. (*Secretary*).

Mr. JOHN HUGHES, F.R.G.S., called in; and Examined.

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17109. (*Chairman.*) You, I believe, are Secretary of the National Canine Defence League?—Yes.

17110. How long have you been so?—Three years.

17111. You have not, I understand, had any special education in surgery or physiology?—No.

17112. You speak principally to what we have been calling the ethical side of the question?—Yes. I wish to lay before the Commission the work that my society has done without entering into the scientific aspect of the question at all, that I propose to leave to the scientific witnesses whom we hope to produce.

17113. We should like shortly to hear what the number of members of your society is, and what their object is, but perhaps you will take it very shortly, because our object is not to inquire into the protection of dogs generally, but only with regard to surgical experiments?—I quite understand.

17114. Perhaps you will tell us, to begin with, what the number of the members of your society is?—Our membership at the present moment is 4,421.

17115. That is to say, I suppose, they subscribe?—They subscribe.

17116. One member of the Commission asks what is the subscription?—I am giving you a list of the members up to the present day. I can only give you a list of the subscriptions up to last year.

17117. (*Sir Mackenzie Chalmers.*) Is there any minimum subscription?—2s. 6d.; that is the minimum subscription.

17118. (*Sir John McFadyean.*) Annually?—Yes.

17119. (*Chairman.*) I suppose we may shortly take it that the object is that indicated by the title—namely, to defend dogs from anything that you think cruel or unjust, or wrong treatment in any direction?—Yes; to protect dogs from ill-treatment or ill-usage of any kind.

17120. How long has your society been in existence?—It was founded in 1891, but it was founded, not as a Canine Defence League, but as an Anti-Muzzling League. It was called into existence at the time of the rabies scare in 1891, when the Muzzling Orders were made by the Board of Agriculture.

17121. Do I rightly understand that the League was founded with the view of opposing what I may call the muzzling legislation of Mr. Long?—Yes, quite so.

17122. Have they continued always of that view?—They have retained that part of the work, of course.

17123. Of opposing muzzling?—Yes, that is a part of their work, but the work has developed in other directions since then. As the society has grown in strength they have seen other objects which they thought it advisable to take up, so that now the work of the League covers the whole of the question of the protection of dogs from ill-usage.

17124. Do I rightly gather that they always have been, and are opposed to, the principle of muzzling dogs, and of excluding dogs coming from abroad in order to prevent the disease of rabies being spread?—If you refer to the quarantine regulations of the Board of Agriculture, I cannot say that the society as such is opposed to quarantine regulations. We certainly think they are too stringent, and would like to have them altered in that respect.

17125. Does your society think they are useful in preventing the spread of rabies?—Yes, as long as they are not made too stringent.

17126. Do they object to muzzling on the ground that it is a cruel, though effective measure, or that it is a cruel and ineffective measure?—Cruel and ineffective.

17127. Quarantine they think is effective, but muzzling they think is not?—Quite so. We believe that quarantine ought to be in existence because it is a measure of prevention, to prevent the possibility of the spread of rabies. We believe, on the other hand, that muzzling really has been the means of causing rabies in dogs, rather than of preventing it.

17128. I daresay some other members of the Commission who deal with these matters will ask you some questions about that; I will not pursue it myself. As regards vivisection, that, I understand, you are altogether opposed to as regards dogs?—Quite, so far as dogs are concerned.

17129. Not merely to vivisection, but to all experiments on dogs either by way of inoculation or testing for drugs or for the causes of diseases?—Yes, we oppose every kind of experiments on dogs.

17130. And that is whether the dogs are anæsthetised or not?—Yes.

17131. Do you draw any distinction between the doing of it under anæsthetics and without anæsthetics?—No, we do not believe that any experiments either with or without anæsthetics should be permitted.

17132. You think that they are equally unjustifiable?—Yes.

17133. When I say you, of course I am speaking of the views that you represent?—Yes.

17134. Do you consider that experiments on animals generally are unjustifiable?—I can, of course, only give my own personal opinion on that question. I have every reason to believe that a large number of the members of my society are opposed to the vivisection of any animal, but I know on the other hand

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that there are some who, whilst they believe that the vivisection of some animals may do good, are equally strong against vivisection of dogs.

17135. That is not that they think that useful results might not be obtained from experiments on dogs, but that they think dogs ought to be exempt?—They think dogs ought to be exempt on moral grounds, and also so far as I can understand their views, because they believe that other animals could be experimented upon with greater effect for purposes of investigation.

17136. In all cases?—In all cases.

17137. However, that is a matter as to the distinction between the formation of the dog and the formation of man or a monkey, which is not a matter that you profess to speak about yourself?—No, not on my own authority. I have, of course, the testimonies of others; but I hope to bring forward a witness who will speak specially on that point.

17138. I think we had better, perhaps, keep clear of those questions in examining you, because we have had many gentlemen on both sides who speak from personal experience and physiological knowledge?—Yes.

17139. I understand that your main reason for claiming exemption for the dog is on moral grounds?—That is the first reason. I have put down the reasons as they occurred to me in my *précis* of evidence.

17140. That, I think, is a matter that does not require much elaboration, because while people may differ as to the weight of the reasons, the nature of them is quite obvious. Then you speak of their docility, faithfulness and companionship?—Yes.

17141. And evolution. What do you mean by that? —What we mean is that generations of training and being brought into contact with human beings have rendered the dog so much more highly developed than other animals that it is not a fit subject for experiment.

17142. But the word evolution is often used in another sense. Do you mean by it, that they are so far on their way to becoming men?—That they are nearer human beings. I do not say that they will ever become men.

17143. Not that they are on their way to that as an end?—They are on the road, but I do not know that they will ever reach it.

17144. Then you also say that they were never intended for experimental purposes. I do not quite understand what you mean by their never being intended. Do you mean never intended by Nature or Providence? —Never intended by Nature or Providence. What we know of dogs is of such a character that we cannot conceive of the possibility of their ever having been intended for the purpose of being put to pain, or for experimentation for the purpose of finding out cures for disease.

17145. When you come to say that they were never intended by Providence for that purpose, is not that entering into a very large subject, perhaps a little outside our scope. Would you say that man was never intended to be eaten by a tiger?—Certainly.

17146. Or that a cat was never intended to torture a mouse?—I cannot say that I can enter into that, but I certainly agree that man was never intended to be eaten by a tiger.

17147. I am not so sure about it. What is the distinction between a tiger eating a man and a cat eating a mouse, from the tiger's and cat's point of view?—I cannot conceive of the possibility of a beneficent Ruler and Creator creating man for that purpose.

17148. (*Mr. Ram.*) Or a mouse?—Or a mouse.

17149. (*Chairman.*) Perhaps that is pursuing, as I say, questions that we cannot follow out to the end; but it is rather difficult to assume that Providence intended a cat to torture a mouse, but did not intend a mouse to be tortured by a cat?—I can only say that it is tortured by the cat. Dogs, I am afraid, are tortured by men, too, but I do not think they were intended for that purpose.

17150. (*Sir Mackenzie Chalmers.*) I am afraid that cats are sometimes tortured by dogs?—Yes, and dogs by cats.

17151. (*Chairman.*) But when you say that dogs were never intended for experimental purposes, it opens a question which is rather difficult to decide, if you follow it out thoughout the animal life?—I quite agree, but I only put that down as the view of my society; they certainly believe that, on the same ground that you put just now, that a man was never intended to be eaten by a tiger.

17152. That is very much the same question?—Quite so.

17153. Then you give as another reason the futility of experiments. As to that again, you are not speaking from your own personal experience?—No; I have simply put those down as the reasons held by my society.

17154. We may take it, shortly, from you that that is one of their reasons?—Yes.

17155. And another is the difficulty of anæsthetisation?—Yes.

17156. That stands on the same ground?—Yes, it does, but I might say that we have had special evidence on that point, some of which I hope to be able to lay before the Commission to the effect that a dog cannot be anæsthetised in such a way as not to suffer pain; that if you give it an anæsthetic strong enough to do that you kill it.

17157. We have had before us some very eminent anæsthetists as well as eminent physiologists, and we have also had some gentlemen with knowledge and experience who have spoken on the other side of the question, but having stated that as the ground you act on, we need not follow it further with you?—No.

17158. And in the same way, with regard to the difference between human and canine organs, we have had physiologists speaking about the differences. There is no dispute that there is a difference; whether it absolves physiologists from the necessity of using dogs is another question?—Our point is that the differences are so great that all experiments on dogs are useless for the purpose of understanding the human organism.

17159. You have endeavoured to obtain legislation in favour of your views?—Yes, we have had a Bill before the House of Commons during the last three Sessions, but, unfortunately, we have not been able to get a good place in the ballot, with the result that the Bill has had to be put down under the 11 o'clock Rule, and as it was blocked every time, it has had no opportunity of being discussed in the House. I enclosed with my *précis* of evidence a copy of this Bill for each member of the Commission. I do not know whether you wish me to go into it.

17160. I think we have all read it—I have. It is a very short Bill?—Yes.

17161-2. You need not read it through. Practically it is to say that a dog shall not be operated upon; that whatever permission may be given by Parliament for operating on other animals, dogs shall be excepted? —Yes, that it shall be unlawful to perform any experiment of a nature causing, or likely to cause, pain or disease to any dog for any purpose whatsoever, either with or without anæsthetics. That is really the purport of the Bill.

17163. And that nobody, of course, shall be licensed for the purpose?—Yes.

17164. And then the rest is penalties?—Yes.

17165. That Bill, I think, has never been debated in the House of Commons?—No, it has never been debated in the House. I might say, in this connection—although I put it down in another connection—that we canvassed all the candidates at the last election on the question, and we have written pledges from considerably more than half the Members of the present House of Commons that if an opportunity occurs they are quite prepared—and they are pledged, in fact—to support the Bill.

17166. You issued a petition, I think, to the effect that the use of dogs in vivisection experiments is in no way necessary for the advancement of medical science or for instruction?—That is the medical petition. The general petition comes first, which is in these words: "That your Petitioners are entirely opposed to the vivisection of dogs, who, on account of the generations of training to which they have been subjected, and their domestication as household companions and pets, have been rendered exceptionally intelligent, peculiarly sensitive to suffering and to terror, and are docile and obedient even under torture. Your Petitioners are furthermore of opinion that, on account of this docility and obedience to the word of command, the dog has of late years been specially selected by vivisectors for extensive and peculiarly revolting and painful experimentation, and also for demonstrations of a prolonged and agonising nature before classes of students. The Home Secretary stated on May 10th, 1903, that at one laboratory alone—that of University College, London—no fewer than 232 dogs were used for experimental purposes during the year 1902. Your Petitioners therefore pray that your Honourable House will be pleased at an early date to pass the Bill now before Parliament to exempt dogs of every kind and breed from being made the subject of vivisectional or other experiments of any kind whatsoever." That is the general petition.

17167. I see that you do not refer to anæsthetics. Is that because you think it is immaterial whether anæsthetics are administered or not?—Yes, we oppose experimentation of every kind on dogs, with or without anæsthetics.

17168. But it is very material, is it not, on the question whether the operations on them are of an agonising nature?—Yes, we do not allege that all the operations are of an agonising nature.

17169. But when you allege that all of them were of a prolonged and agonising nature, and do not say anything about anæsthetics, is that not a statement that no anæsthetics are used?—I do not think we say that they all are. We say "the dog has of late years been specially selected by vivisectors for extensive and peculiarly revolting and painful experimentation." It does not say every one of those experiments.

17170. It does not say "some of which are," but it says were "specially selected by vivisectors for extensive and peculiarly revolting and painful experimentation." However, you say that what you mean by that is "some of which were"?—Yes, we are simply giving that as a reason for doing away with it. The possibility of that being done in any case is a reason why it should be done away with altogether.

17171. Still the people who sign this petition are not people who have any apportunity of examining statistics, at least not people likely, most of them, to examine statistics, to check statements of this kind. I am only pointing out that what we have learnt from a great deal of the evidence before us is that these operations are in almost all cases done under anæsthetics—in all cases where they are not privileged by the Act?—Quite so, but a great number are privileged by the Act.

17172. Not a great number.

(*Mr. Ram.*) A very small number, indeed.

17173. (*Chairman.*) What do you say?—I think it is a large proportion—too large a proportion, I say.

17174. How large a proportion do you say? Perhaps you have some information which we have not got?—I am sure you have the information that I have, but I think the proportion is fairly large. I find that last year, according to the Home Secretary's Returns, there were no less than 281 certificates issued permitting experiments on dogs and cats and recovery from the anæsthesia; those must necessarily be of a painful character.

17175. (*Mr. Ram.*) What are you reading from?—The Home Secretary's Return of last year.

17176. Does that distinguish between cases allowed to recover and cases not allowed to recover?—Yes.

17177. (*Sir Mackenzie Chalmers.*) Certificate B you mean?—Certificate B plus E E.

17178. (*Mr. Ram.*) Would you kindly read it again?—"Permitting experiments on cats and dogs, and recovery from anæsthesia, 281."

17179. (*Chairman.*) But how many of those 281 were permitting operations on cats and dogs, and how many recovery from anæsthesia?—The whole 281 were recovery from anæsthesia.

17180. For all animals?—No, cats and dogs only. Under Certificate B, allowing for recovery of any animal, 917. I am only going into the question of dogs.

17181. There are gentlemen here more familiar with these statistics than I am; I will not ask you any more about it. You say that 281 is the number?—Yes. I have only the Home Secretary's word for it.

17182. (*Mr. Ram.*) What are you reading from?—This is an extract from the Return of the Home Secretary.

17183. May I see it?—Certainly. (*Handing in the same.*)

17184. (*Chairman.*) How many signatures were there to your petition?—We have received up to date 754,584. Of that number we have already presented to Parliament 673,294.

17185. You mean that you have gone on receiving signatures since the House rose?—Yes, we are receiving them continually.

17186. Then you also have a medical petition?—Our medical petition is a short petition. It is as follows: "That we, the undersigned medical practitioners of the United Kingdom, are of opinion that the use of dogs for vivisectional experiment is in no way necessary for the advancement of medical science, or for the adequate instruction of medical students. We therefore most earnestly and respectfully pray that your Honourable House will be pleased at an early date to facilitate the passing of a measure prohibiting the vivisection of dogs." We have already received 1,261 signatures to this petition, and a large number of those have been sent in voluntarily. We sent a number of letters out, asking medical men to sign, but we made no attempt whatever to canvass the whole of the medical profession. I may say that fully two-thirds of the medical profession have not been approached on this matter.

17187. Of the one-third who have been approached, how many have signed?—1,261.

17188. I see that you wish to give an explanation of your reasons for withholding the names of the signatories from the public?—Yes.

17189. I did not know that they were withheld, but in point of fact you mean that you do not make the names public?—We do not make the names public, except with the express consent of the medical men who sign. We find that some of them object to it, and the reasons they give are such that we must respect them; they say that it places them at a disadvantage with the profession, and that since the heads of the profession are very much in favour of vivisection, they find they are both boycotted and persecuted if they are known to be anti-vivisectionists.

17190. What percentage of the 1,261 write in that strain?—A very small percentage. I could not tell you the exact number, but it is a small number comparatively. A large number have no objection whatever to their names being published.

17191. Do you mean that you have had 12, 20, or 30 letters of that kind?—I should say under 100. We put in our covering letter, a copy of which I enclosed for the Commission: "I may add that no signatures received will be published in any way, or made use of for any purpose other than for presentation to Parliament." I thought it was only fair that the Commission should know exactly how we get these signatures.

17192. You have letters, you say, from medical men?—Yes, I have a very large number of letters from medical men, all of which were unsolicited; we have never asked for them.

17193. It is rather difficult to take the opinions in letters of medical men with regard to questions of medical knowledge. We might dispense with witnesses altogether in the Commission if we did that?—Yes; I quite understand that; only I thought the Commission would like to know on what we, as a society, base our views. We get these letters from all over the country from scientific men, and these come to us quite unsolicited. Our view, is, of course, that there is a very strong consensus of medical opinion—I mean a very large minority of medical men—who are opposed to the vivisection of dogs.

17194. I think we have given an opportunity to you, and I believe you are going to avail yourself of it, to call a couple of medical men?—Yes, only two, my Lord, out of a very large number.

17195. There is no objection to your stating that you have received a number of letters from medical men and the general effect of them; but if we were to have a number of letters read from different medical men who give their names, and stating what their views were, it seems to me that we should get into considerable difficulty because any member of the Commission might say that he desired to ask them questions and we should be led into a number of cross-examinations of witnesses?—As you wish about

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it, of course, my Lord. I have a selection of the letters here. I have not got them all.

17196. I think we may take it generally from you?—I should very much like the Commission to allow me to read these letters; it will not detain you a long time.

17197. I do not think the Commission object to your reading two or three for samples, but you produce a very formidable bundle there?—I do not intend reading all those, but I hope to be allowed to read about a hundred of them.

17198. I think you are a little *exigeant*?—I do not want to withhold anything from the Commission.

17199. You might give us two or three of the strongest.

(*Mr. Ram.*) And the most eminent leaders.

17200 (*Sir Mackenzie Chalmers.*) What we should like is really a man with scientific qualifications entitled to speak in the name of science?—May I point out before I begin that I have already printed some expressions of medical opinions, a copy of which I enclosed for members of the Commission; it is a leaflet that we have issued. I may say that those expressions of opinion were taken from letters which were sent to us in reply to our letters asking for signatures to our petition; but since then I have received in connection with my attempts to get witnesses before this Commission, 700 or 800 letters, quite unsolicited, from people, in a great number of which very strong expressions of opinion are given on this question of the exemption of dogs from vivisection.

17201. (*Chairman.*) Do you say 700 or 800 from people or from medical men?—From medical men. In addition to that I ought to say that I advertised offering a prize for an essay, during the summer, on the moral and scientific reasons for exemption of dogs from vivisection, and a large number of medical men, seeing this advertisement, wrote to me immediately for a copy of our petition, as they wished to sign it. Those, of course, came quite unsolicited, simply by seeing the advertisement.

17202. Your pamphlet was addressed, I understand, to the question of the exemption of dogs?—Quite so.

17203. And that is what their letters are on?—Yes.

17204. They are not letters on vivisection generally?—There are several of them that go into the question generally.

17205. But when you said about 100 of them, you were not referring to anything more than the exemption of dogs from vivisection?—Yes, that is our position. We do not profess at all that 1,260 doctors are in favour of total exemption from vivisection of all animals. I have a letter here, for instance, from Professor Woodroffe Hill, a very well known veterinary surgeon. This happens to be the first that comes to my hand now, if you will allow me to read an extract from it. I had hoped to have Professor Woodroffe Hill before you; but, unfortunately, his health is such that he cannot attend just now.

17206. (*Sir John McFadyean.*) Where is he Professor now?—I cannot say; but he is known as Professor Woodroffe Hill. He is a Fellow of the Royal College of Veterinary Surgeons. He says:—"As an earnest student of canine pathology for over 40 years, and having closely observed the various traits of canine character, I consider the vivisectors' evidence before the Commission concerning dogs is perfectly monstrous, notably with regard to anæsthetics." That is an extract from his letter.

17207. (*Dr. Gaskell.*) Does he say why he considers it monstrous?—That is all he says. Of course, I have not asked him any questions.

17208. I was wondering whether he gave any definite statement as to the meaning of the word "monstrous"?—No. Then I have a letter here from Dr. Frederick Page, of Newcastle-on-Tyne:—"Dear Sir,—My views about vivisection are that no vivisection for purposes of demonstrating to students facts should be allowed. I do not think we should interfere with injections."

17209. (*Chairman.*) He does not say of dogs there?—No; but he signed our petition for the exemption of dogs. And he goes on later:—"I would absolutely forbid dogs to be employed for vivisection purposes." This is one of the men, of course, who do not believe in total anti-vivisection for all animals; but he is very strongly in favour of the non-vivisection of dogs. Then Dr. MacCarthy says:—"I am quite prepared to give evidence in support of my position—that is, total abolition of vivisection on dogs, abolition of vivisection on all other animals, except by expert original researchers." That is Dr. MacCarthy, of Abbeyfeale, County Limerick. I have also a letter from Dr. Hogg, the coroner for Middlesex, in which he says (I cannot put my hand upon it at the moment) that he is prepared to give evidence in favour of the total exemption of dogs from vivisection, in the same way as the others do. I find it rather a difficult matter at a moment's notice to select the strongest letters, as your Lordship asked. Dr. Kellard, of Southsea, writes:—"I may further say that I have the greatest horror of vivisection. I cannot understand how anyone can tolerate it. There is enough misery in the world without going out of our way to make animals suffer in this cold-blooded, fiendish way." I would like to point out that these are the doctor's words, not mine:—"This earth is almost a hell for many poor animals. It is an excuse for a morbid curiosity in many selfish wretches."

17210. Do these letters which you have read to us give us a general idea of the views which have been expressed to your society?—Yes, I think they do. Some, of course, are stronger than others.

17211. It would be impossible to take the evidence, you know, of 100 doctors in this sort of way. I think we cannot go on reading any more. We may take your word for it that those are fair samples of a number of letters that you have received?—Very well.

17212. You have told us about the Members of the House of Commons who are pledged to support your Bill?—Yes. Of course, as public opinion is shown in elections, I should like to say a word about it. As I said before, we approached every candidate during the last General Election with a view of obtaining their views on this subject, and we received a reply of some kind or another from most of them; and from the replies that we received we found that 380 Members of the present House of Commons are pledged to support the Bill. These candidates were approached not officially from the head office in every case; in some cases they were, but in most cases they were approached by some of the members of the League who happened to live in their constituency. In one or two cases we made a special effort either to return men whom we were anxious to return, or to oppose and upset the election of a man where we considered it was necessary to do so and in two cases I may say that two men who had opposed our Bill in the House in the previous year were, mainly through our instrumentality, defeated.

17213. Can you tell us who they were?—One was Dr. Hutchinson, the Member for Rye, and the other was Sir Mark Stewart, Member for Kirkcudbright in Scotland. They both opposed our Bill; I mean they blocked it in the previous Session, and our members felt that a determined effort ought to be made in those cases. A very determined effort indeed was made in Rye, and in spite of the fact that the party to which Dr. Hutchinson belonged was in the ascendancy at the moment, he was very badly beaten. That, I think, is only one example of the public opinion that we have behind us on this question.

17214. I am afraid that most Members of the House would think that it is rather a strong thing to say that Sir Mark Stewart was thrown out on the question of exemption or dogs from vivisection?—It may be so, but I am quite sure that we exerted great influence at the election.

17215. It may have had its effect, no doubt?—Perhaps I ought not to say that we threw him out. All I know is that he was thrown out.

(*Mr. Ram.*) It may have been *post hoc*, not *propter hoc*.

17216. (*Sir Mackenzie Chalmers.*) You opposed him and he fell?—Quite. It may have been a coincidence, but it is a fact for all that.

17217. (*Chairman.*) Then you wish to refer to Professor Schäfer's experiments?—I put that down in my *précis* simply because I think he has appeared before the Commission.

17218. It is not a subject that you have personal knowledge about?—No, only so far as regards the society opposing the experiments at the time. They got up a petition, it was before my time, to the Home Secretary, and also a petition to Her Majesty the Queen, on this subject, to which they received a reply expressing her disapprobation of the experiments. They also obtained the opinion of 109 medical men, and among them were very strong letters in opposition to these experiments. This is the petition that we sent to the Home Secretary: "We, the undersigned medical practitioners of the United Kingdom, beg respectfully and earnestly to protest against the licence which has been recently granted by the Home Office to Professor Schäfer to submit unanæsthetised dogs to the process of drowning with a view to studying various means of resuscitation therefrom for the benefit of human beings. We would point out that previous well-known experiments of a similar nature have proved futile of result, and we are of opinion that the present experiments must be equally unsatisfactory and disappointing, by reason of the difficulties and fallacies associated with the anatomical and physiological differences between the animals operated upon and the higher creatures whom these procedures are supposed to benefit. But even if some slight gain in knowledge above and beyond that with which we are already acquainted were possible by such means (which we do not admit), we should, nevertheless, emphatically protest against such inhuman cruelty, perpetrated in the name of science, upon the helpless, sentient, and sensitive creatures. We trust, Sir, that you will yet use your great power and influence to put a stop to any further experiments of this nature, which can but bring discredit upon a Government Department, which possesses the power of control in questions where the exercise of justice and humanity are concerned."

17219. You tell us that that came into the hands of Her present Majesty the Queen?—Yes.

17220. And she sent you a letter?—Yes, a letter from the Honourable Charlotte Knollys to Lady Paget, who sent the petition to the Queen. I find that it is marked "private."

17221. Then perhaps we had better not have it. I should not like to admit a letter from the Queen marked "private" without Her Majesty's permission. I will take your word for it that Her Majesty disapproved of the experiments?—If you please.

17222. (*Sir John McFadyean.*) Has the letter been made public?—No, of course not; it is marked "private"——

17223. (*Chairman.*) What is the next point you wish to mention?—There is a reference in my *précis* to the "British Medical Journal," and to a letter by Dr. Silvester, which appeared in that journal at that time. I am sorry that in my hurry to come away I forgot to bring it with me; I daresay it will be known to the Commission. If the Commission will permit me to send it on again, I shall be glad to do so.

17224. We have had Professor Schäfer here, and I think that some of the witnesses who were objecting to his experiments read from the "British Medical Journal"?—Anyhow, may I send it on again.

17225. Is Dr. Silvester alive?—I really do not know. I think so. I am not sure.

17226. You have told us about your membership? —I may say that in addition to our membership, we have now a large number of centres all over the country. We have over 200 centres, and there is an honorary secretary in each place who is responsible for carrying on the work of the League, so that the influence of the League is really very far reaching. I should like to emphasise one fact, if I might. We believe, of course, feeling that we have a majority of the House of Commons in favour of our Bill, that the Bill has been already passed by the nation, and that it only awaits an opportunity to be passed by the House of Commons.

17227. I understand that one of the objects of your League is the total suppression of the support of vivisecting institutions by public money?—Yes.

17228. You mean hospitals that have laboratories? —We mean that; but what I had particularly in view in that was the support of institutions from the rates and taxes which are licensed for vivisection. We do not believe that Government Departments, such as the Board of Agriculture, the Local Government Board, and others, should have any laboratories licensed for this purpose. We do not believe either that any local institutions supported by county councils or local authorities should be used for vivisection.

17229. Would it not be more accurate then, to say the total suppression of the support of laboratories for vivisectional purposes by public bodies?—Yes.

17230. That is what you mean to convey?—That is what I mean exactly. We very strongly feel that the using of public money for this purpose is not only unfair and injust, but immoral.

17231. And you object to the proceedings of the Local Government Board and the Colonial Office for the purpose of ascertaining the causes of disease and the remedies?—Yes, quite so. We object to vivisectional laboratories for those purposes because we do not believe that they are needed.

17232. Then you desire, what I hope we all wish to do, to raise the standard of humane feeling in the country?—Yes, that is really one of the great objects that we have in view.

17233. And the view of your League, I understand, is that vivisection does not do any good, which is a matter on which you do not profess to give expert evidence, but also that even if it did, dogs ought not to be experimented upon?—Yes.

17234. That that would not be sufficient cause?—Quite so.

17235-6. Then you have some notes about the Buisson bath propaganda. Would you explain what that means?—That arose at the very commencement of the League's operations in connection with the rabies scare. The views of the originators of the League were that the rabies scares were really got up largely in the interests of Monsieur Pasteur's Institutions, and they adopted the Buisson bath as an antidote to hydrophobia and found that in all cases where it was tried it was a very great success. I may say that this also was before my time, and I am only speaking from hearsay, not actual experience. These baths are, I understand, designed to bring about a free action of the skin.

17237. Have you not some witnesses coming who understand this from the medical point of view?—I am afraid I cannot do that, because I am only allowed to bring two witnesses, and I cannot very well bring witnesses who are experts on all points.

17238. We were obliged to select from each society a limited number. We have a very long list?—I quite understand that, but we rather feel that you have limited us rather more than other societies.

17239. I do not think so?—Well that is my view.

17240. Would you tell us without professing to argue for the medical merits of this system, what it is?—It is a vapour bath, the object of it is to take out of the system any virus that may have been injected into it by the bite of a rabid dog, and my society believes that this is really a more commonsense method of dealing with the question than by injecting a virus of the same character as that which has been already injected.

17241. Your society, I suppose, is governed by a committee?—Yes.

17242. Who express the views of the Society?—Yes.

17243. Are there any medical men upon it?—Yes, there are three.

17244. Will you tell us who they are?—Sir James Thornton, Dr. Hogg, and Dr. Bayfield. In addition to that we have a number of medical men who are vice-presidents, who also, of course, are of the same opinion as the committee. I could give you the names of those if you wish it.

17245. I daresay that some of your papers have their names on them?—Our letter paper has them.

17246. That will be quite sufficient?—Dr. Charles Bell Taylor is one of our vice-presidents, Dr. Carpenter is another, Sir Francis Cruise, Honorary Physician in Ordinary to His Majesty the King in Ireland, and Sir Robert Jackson.

17247. (*Sir John McFadyean.*) Might I ask whether the personal opinion of each of these gentlemen has been obtained on this particular point, that is to say, as to the failure of the Pasteurian method and the success of these baths in every case?—No, we have not canvassed them on those points.

17248. Are you sure that they approve of what you

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are telling us?—I am sure that they do not disapprove, or else they would not associate themselves with the society.

17249. It is not in the constitution of the society is it, that it must believe that the Pasteurian treatment is inefficacious, and not in keeping with common-sense?—Well, they have a copy of our Report every year, in which we refer most pointedly to this question, and the gentlemen I named last have only recently become vice-presidents, and they have not done so without knowing exactly what they were doing. Of course, they must generally agree with everything the society does or else they would not associate themselves with the Society.

17250. (*Chairman.*) I presume that all your members are in favour of the exemption of dogs from vivisection experiments?—Yes, and all the other objects that we have in view. We have a large number of objects besides those.

17251. Yes, I mean for the protection of dogs generally from cruelty?—Yes.

17252. But I was coming to the point about the Buisson bath?—That is certainly one of the objects of the society, and one of the forms of the work that we have been prosecuting.

17253. (*Mr. Ram.*) Is that stated publicly as being one of the objects of the society?—Yes.

17254. In what publication?—In the Annual Report.

17255. Can you give me one?—I have only one that is marked. I can send you one.

17256. Can you read me the words that would show to any intending vice-president that he was committing himself to the refutation of the Pasteurian theory and the adoption of the bath?—We have here in one Appendix particulars of how this thing is to be done.

17257. How what is to be done?—How we are prepared to assist people who want to use the Buisson bath.

17258. But I want to see whether a vice-president would necessarily know that on becoming a vice-president he was committing himself to disbelieving in the Pasteurian theory and adopting the Buisson bath. If you will show me that I shall be obliged?—There is a marked reference in the Report itself, both of last year and this year, in fact every year, to this work, which cannot fail to be brought to their notice.

17259. (*Chairman.*) I think that one of your medical witnesses who is coming might very well qualify himself to give us a really medical opinion upon this subject?—Yes.

17260. That would be better than taking it from one who does not profess to be a medical expert?—I will do my best to get a witness to do it. I cannot undertake definitely to do so, but I will do my best.

17261. You see there are gentlemen here who are familiar with medical questions?—Yes, I quite understand that.

17262. (*Sir William Collins.*) Do you state anywhere what are the precise objects of your association?—Yes, in our Report.

17263. Will you read what are the objects for which the association is formed?—"Organised opposition to the muzzle. This includes the repression of dog scares, opposition to Pasteurism, and a Buisson bath propaganda." That is what I was asked for just now. That is the first object that we put down.

17264. (*Chairman.*) Which Report are you reading from?—This is in every one. This is our last published Report.

17265. Is that passage that you have just read in your first Report of 1891, because you said it began with muzzling?—I cannot say; I do not know what the Report of 1891 was; but this is really an object of the society at the present moment. The second object we put down is "quarantine business of every kind"; that is to say, we assist people who want to bring their dogs over from the Continent by giving all information and assistance in the way of quarantining dogs.

17266. (*Sir William Collins.*) Is not the object defined more definitely than stating "quarantine business of every kind"?—That includes everything connected with quarantine.

17267. That might mean either that you were in favour of it or opposed to it. Is your society incorporated?—No, it is not.

17268. Is it registered?—No.

17269. Amongst the objects of the society is there anything stating a general opposition to the practice of vivisection?—No, not general opposition to the practice of vivisection, opposition to the practice of vivisection of dogs.

17270. Then so far as vivisection is concerned, is the sole object of your association to free dogs from the practice?—Yes, so far as the object of our society is concerned.

17271. Then your society, as such, would not oppose the vivisection of guinea-pigs or frogs?—I am not prepared to go into that at the present moment. We are a dog society.

17272. But if the society does not go into that, should I be wrong in concluding that it is not an object of your society to oppose vivisection of guinea-pigs or frogs?—It is not an object of the society to do that.

17273. Or higher mammals, such as the ape?—We cannot go beyond the dog. We are a dog society. I have every reason to believe that practically the whole of the members are against vivisection, as such; but, as a society, we cannot do that.

17274. Then your society, as such, does not oppose the vivisection of the ape?—Not as such. We have taken no measures to do that.

17275. Does your society oppose the vivisection of dogs, if the dogs are rendered anæsthetic throughout the operation?—Yes.

17276. Then if the dog were excluded from the operation of vivisection your society would, so far as vivisection is concerned, have exhausted its object?—Well, I suppose so. I cannot say what action they would take if they succeeded in excluding dogs from vivisection—whether they would go on to some other animal. But at present we are certainly confined to dogs.

17277. But your society, as such, *quâ* vivisection, limits its operation to dogs alone?—At present.

17278. You make no claim to speak, I understand, on the scientific aspect?—No, I do not.

17279. (*Sir John McFadyean.*) Your society at present numbers 4,421 members?—Yes.

17280. Can you tell me how the sexes are distributed?—I could not.

17281. Are the majority of the male or female sex?—I should say the majority are ladies.

17282. What is the income of the society?—Last year our income was £1,698.

17283. Can you tell us how it is mainly expended?—Yes.

17284. (*Sir Mackenzie Chalmers.*) You publish a balance-sheet, I suppose?—Yes. Our printing last year cost us £664; elections, lectures, dog shows, memorials, travelling expenses, £133; advertisements, £116; office rent, £80; salaries and wages, and extra clerical assistance, £487.

17285. (*Sir John McFadyean.*) I think that is enough, thank you. Can you tell us what your clerical expenses would be at the last election?—The amount I have given, £133, covers that.

17286. It is mixed up with these items?—Yes.

17287. In what sense did you oppose Sir Mark Stewart when he was a candidate?—We sent people up there to speak, and we also issued leaflets calling special attention to what he had done on this question.

17288. You sent people who addressed public meetings?—Yes.

17289. Were these largely attended?—Yes.

17290. Can you tell me where any one was held?—I could not at the moment; I can give you the particulars later on.

17291. Do you know whether the successful candidate thought that you had lent him any assistance or not?—I really could not say. I fancy he did.

17292. Have you had any number of letters or statements from voters to say that your views had influenced them?—In that constituency, yes.

17293. How many?—I could not say off-hand.

17294. Five, twenty, one hundred?—I really could not say.

17295. You are sure that you had five letters?—Oh! yes, a good many more than that.

17296. What do you mean when you use the expression "the rabies scare"?—What we mean is that orders were given by the Board of Agriculture to muzzle dogs, on the ground of rabies being in certain districts. We deny that rabies was there.

17297. Is this a point on which you are prepared to offer direct evidence, and not hearsay evidence?—No, it is not.

17298. Then who is going to come? Who are "we," when you say: "We maintain that these were not cases of rabies"?—The society.

17299. Do you mean to say that the whole 4,421 members are entitled to hold an opinion on that point?—I think I am quite entitled to say that, since they are members of the society and connected with it.

17300. Then, on the average, they are better instructed on that point than you are? They are better entitled to form an opinion on that question that you are? I put it to you. It may appear an affirmation, but it is a question?—No, I do not for a moment profess that every one of the 4,421 members would be prepared to come here and give evidence on the subject; but they have their opinions on the subject. So have I. You asked me was I prepared to give expert evidence.

17301-2. I did not use the word "expert"?—I beg your pardon; I understood you to say so.

17303. No; I said "direct"—direct evidence as opposed to mere opinion?—Yes.

17304. You say that you do not desire to offer evidence on that, but that it is the opinion of your society?—Yes; I am quite prepared to bring you cases.

17305. Cases which were not cases of rabies?—Yes.

17306. You are prepared to give evidence on that?—Yes, I am quite prepared to bring forward cases which were not cases of rabies, but were returned as cases of rabies at that time.

17307. Do you know anybody who ever held that the statistics with regard to rabies were accurate in every case?—No; everybody, I think, held that they were absolutely inaccurate.

17308. Everybody did?—Everybody who knew anything about it, because the data from which the statistics were obtained were inaccurate.

17309. Just a moment, please. Everybody, you say, who knew anything about it. Then the expert officers who were employed by the Board of Agriculture were among those who knew nothing about it?—No, I do not think that they believed in the Rabies Returns.

17310. Will you give us some evidence to show that they disbelieved them?—It is very evident, from the fact that they changed the data upon which this evidence was taken so frequently, that they had no confidence whatever in the statistics given.

17311. Please give us particulars as to the way in which they changed the data?—They first of all based it on autopsy. In the next year they decided on another form—I can give it to you in a moment—the inoculation tests were brought forward. I have reasons for believing that in the first year, when the test method was adopted, the number of rabies cases returned was very much greater, simply because it was an improper way of doing it.

17312 What was there improper about it?—In this way, every man, almost, even the policeman, in many cases, I understand, was allowed to declare a dog rabid, without any evidence at all, except his own opinion.

17313. Where do you find it laid down that the policeman might make the diagnosis?—I am told so.

17314. Who told you?—I have been told so several times. I could not quote any authority, but that is the impression on my mind.

17315-6. That is not evidence before this Commission. But did you approve or did you disapprove of the attempt of the Board of Agriculture to make the statistics more accurate by introducing the inoculation test?—We approve, of course, not of the inoculation test as such, but of every effort that is made by the Board to be accurate.

17317. Then I will put it the other way: Did your society disapprove of the attempt of the Board to make the statistics more accurate by resorting to the inoculation test?—I do not quite understand your question.

17318. I will spend a little time in making it plainer, if I can. You complain that the statistics were very inaccurate and misleading when the diagnosis was based on something else than the inoculation test?—Yes.

17319. The inoculation test was introduced in order to make the statistics more accurate, and I understood from you just now that you are prepared to admit that they were more accurate afterwards?—They were more accurate afterwards, but I do not admit that they were accurate.

17320. Did your society approve of this procedure in order to make the statistics more accurate?—They certainly approve of every method being adopted, if it is a proper method.

17321. Then they approved of this method?—I cannot say whether they did, or did not, approve of the inoculation test as such. They certainly are very desirous that the figures should be accurate, but whether the mode adopted for that purpose received the approval of my society, I am not in a position to say.

17322. This is not a scientific question; it is a moral question and an ethical question, and I will put it direct to yourself. Do you approve of that method of endeavouring to obtain accurate information regarding the occurrence of rabies in this country?—You say that it is not a scientific question. I must reserve to myself the right to an opinion on that question; inoculation is certainly a scientific question, but I say that I approve of every proper method which may be used for the purpose of accuracy.

17323. That, of course, is not answering my question, because you introduced the word "proper"?—But you say that it is not a scientific question; I say that inoculation certainly is a scientific question, and if you ask me whether I approve of inoculation you enter on a province of science.

17324. I am afraid that you have misunderstood me. I did not say that this was a scientific question?—I understood you to say that it is not a scientific question.

17325. The question I put to you was not a scientific question, it was a moral question. You admit that these scientific questions have a moral and ethical side, because you have come here specially to speak upon it?—Quite so.

17326. Very well, then; has this method of inoculation, which is a scientific method, got an ethical side?—I disbelieve in inoculations entirely.

17327. Then you disapprove of this particular method of endeavouring to make the statistics accurate?—That is a scientific question on which I am not prepared to give an opinion.

17328. Do you mean that the question, whether you disapprove or not, is a scientific question?—The question whether inoculation is the right way of finding out the correct figures is, I say, a scientific question.

17329. But I have not put that question. I will ask you, please, to assume that it is a good method, and that it actually does enable you to obtain accurate information?—If that is so on those grounds, I have already said that I approve of every proper method of finding out correct figures.

17330. But do you approve of this method?—I cannot say whether it is a proper method.

17330A. But I ask you to assume that it is what it professes to be, an accurate method. Do you approve of it?—If it is what it professes to be (I cannot have an opinion upon that), I certainly approve of any proper method for the purpose of finding out correct figures.

17331. That may be held to mean that you approve of this particular method if it is accurate?—If it is accurate and proper.

17332. What do you mean by the word "proper"?—Right, morally right.

17333. And what I am asking you is whether you

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think it is morally right?—I did not understand you to ask me that. You asked me whether I approved of it.

17334. But, of course, I tried to make it plain that that was whether you approved of it from the moral or ethical standpoint?—I have already said that I totally disapprove of every process of inoculation, because I do not think it is a proper one.

17335. May I take that as meaning that you disapprove of this particular method?—From that point of view I do.

17336. So that from the point of view of your society they would rather have had the statistics inaccurate than have had them made accurate in this way?—We do not for one moment say that this is the most accurate way of finding out statistics. What we do say is that they certainly were more accurate when they took this method of finding them out, but we say that even then they were inaccurate, because we have cases of dogs having been put to death for rabies during that period which on *post-mortem* examination were proved not to be suffering from rabies at all.

17337. (*Chairman.*) Put to death by whom?—By the authorities.

17338. (*Sir John McFadyean.*) Did these animals which were killed, as being the subject of rabies, and which were afterwards found to be not suffering from rabies, figure in the statistics as rabies?—So I understand. For example, the chairman of my society, Mr. Pirkis, had a dog which was condemned as being a rabid dog, and was put to death. They insisted on a *post mortem* examination, and when the *post-mortem* took place it was found to be that what the dog was suffering from was that it had swallowed a piece of leather, which stuck in its throat, and, of course, made it very ill, and caused the people to think it had rabies.

17339. Then it was proved that it was not rabid simply by the *post-mortem* examination?—Yes.

17340. Do you think that a reliable method of telling whether a dog is rabid or not?—I am told so. I am not in a position to give an opinion myself.

17341. But I thought that a minute ago you endeavoured to discredit the statistics of the Board, by pointing out that they were based on *post-mortem* examinations?—No, I did not; inoculations, I think I said.

17342. You think that *post-mortem* examination is more valuable than inoculation?—I should think so. Of course, as I said before, I am not giving a scientific opinion; I cannot do that, but I should imagine so.

17343. You are aware of the extraordinary reduction in the cases of rabies in this country which followed the introduction of the muzzling order?—I am aware of the reduction in the figures, but, as I said before, my society did not believe in the existence of rabies to the extent that they were returned by the Board of Agriculture.

17344. Does your society believe that there is such a thing as rabies at all?—Yes, but they believe that it is a very rare thing.

17345. How many of these cases, say in the year 1895, when there were 672 cases, do you think were cases of rabies?—Very few.

17346. How many is very few?—I really could not say.

17347. One, two, or three?—It is quite impossible to say.

17348. Do you think it is possible that there might be none of them cases of rabies?—Quite possible.

17349. So that all those who have given special attention to the subject, including veterinary surgeons and medical men, are entirely mistaken?—Probably.

17350. Why probably? Does it not seem probable that they would be right rather than wrong?—Perhaps I ought to have said possibly instead of probably.

17351. So that you do not attach even any importance to the fact that no cases of rabies have been returned during the last few years at all?—We do not attach the same importance as the Board of Agriculture do, because we do not believe that rabies existed to the extent that they said it did in previous years.

17352. Do you think it exists at all now?—Not in this country.

17353. Do you think it existed at all before the Muzzling Order was introduced?—I am not in a position to say that.

17354. So that it is actually debatable whether there has been any rabies in this country?—Yes.

17355. Do you wish to give evidence before the Commission based on your own reading and observation to bear out that view?—I am not in a position to do so.

17356. Whose opinions are you quoting?—I am quoting the opinions of my society, so far as I can gather them, previously to my coming into connection with it. Personally, I have no experience of that during the last three years; fortunately, there has been no occasion for it.

17357. But do you think that these 4,421 members are entitled to a first-hand opinion on that subject?—I think they are entitled to an opinion on anything they like. We are all entitled to an opinion on anything we like.

17358. I am afraid we use the word "entitled" in a different sense. I meant were they in a position to form an opinion that this Commission ought to attach any weight to, on a question like that?—No, I do not say they are. They are not experts, of course.

17359. Then you said in your evidence that the difference between the canine and the human organs was so great as to make investigations conducted on dogs useless as a means of extending knowledge with regard to human diseases?—So I am given to understand.

17360. But, again, you do not wish to give scientific evidence as to that?—No, I have already said that I intend to bring a witness forward to speak specially on that subject.

17361. How does the question stand with regard to experimentation on dogs for the purpose of extending knowledge with regard to dog diseases. That argument would not apply, would it?—This particular argument would not apply.

17362. Do you think that knowledge with regard to dog diseases might be extended by experiment?—It might be, but it is very questionable whether it would be right to use that method.

17363. Supposing that it were a question of investigating the skin parasites of the dog, the flea and the louse, for instance—would it, in your opinion, be a moral proceeding to transfer those parasites from a dog in which you found them occurring naturally to another one, that they might breed upon it, and afford you an opportunity to investigate their life history?—No, we certainly think it would be quite wrong to do that. We see no reason in the world why that could not be studied on the animal which suffers naturally from it.

17364. But, assuming that it would be convenient to the investigator, and provide him with a larger quantity of material, you think it would be morally wrong?—Yes, I do, certainly.

17365. Then do you take up the view that it is morally wrong to cause pain in any circumstances to a dog?—Yes, quite.

17366. Is it wrong to chastise a dog with a whip?—Chastisement comes, of course, under a different category. I do not believe myself, personally, in it.

17367. It comes under the same category?—I do not believe it is at all necessary to chastise a dog. I never chastise my dog. I find that it does all I want without it, and does it better by kindness. I do not think it is necessary ever to do that.

17368. That was not my question. My question was: Do you think it is morally right or wrong to chastise a dog?—I think it is morally wrong to do it unnecessarily.

17369. Does your statement that it is morally wrong to cause pain to any animal in any circumstances apply to other animals than the dog?—You are asking for my personal opinion, I presume; my society does not go beyond the dog.

17370. You could not possibly give me the opinion of your society on that question?—I certainly think it is wrong to give unnecessary pain to any animal.

17371. But you have introduced a most important qualifying word, "unnecessary." That we would all assent to. But what you said before was that it was morally wrong to cause pain to an animal under any circumstances?—To a dog, I said. I see no necessity for it. That is why I say it is morally wrong.

17372. But that is not the question, because you introduce the word unnecessary, which begs the question?—I think I explained to you why I did not chastise my dog—because it is not necessary. I find I can do better without it.

17373. I did not ask you whether you chastise your dog or not. But you admitted that in your opinion, to inflict pain on a dog in any circumstances is morally wrong. Have you answered the question as to whether it is ever morally defensible in the case of other domesticated animals?—I still say that it is absolutely wrong to give unnecessary pain to any animal.

17374. But my question, please observe, was not whether it was right to cause unnecessary pain, but whether it was ever justifiable to cause pain?—I cannot say that there might not be cases in which it is justifiable. I cannot at the moment think of any, but I would not like to at once make a sweeping assertion that every possible case of that kind is wrong. But I certainly again say that every unnecessary pain inflicted on an animal is an evil, and morally wrong.

17375. I have not asked that question. But you say you feel unable to think of any circumstances in which the infliction of pain on an animal would be justified?—I cannot think of any at the moment.

17376. Then it was hardly necessary to introduce the word "unnecessary." Is it justifiable? Have you read the evidence given by previous witnesses on that subject?—I have read some of it; I have not read it all.

17377. Do you think it is morally wrong to kill an animal for food purposes?—No, I do not.

17378. Do you think that the pain inflicted in that case is necessary?—I certainly think they ought to be killed by the most humane methods possible.

17379. But supposing it were not possible to kill them without pain, is it still morally defensible to kill them?—But I think it is possible, from what I understand of the question; so that I do not see why we need discuss it.

(*Chairman.*) Shooting pheasants, for example?

17380. (*Sir John McFadyean.*) Is that painless?—I cannot say really. I do not know whether it is or not, but I say that if a pheasant is shot it ought to be shot in the most painless way possible, and no man has a right to shoot it unles he learns how to do that unerringly.

17381. (*Chairman.*) Supposing it gets shot in its body and not in its head, it suffers a good deal of pain?—I do not think that any man has a right to kill it unless he does it in the most humane manner possible.

17382. (*Sir John McFadyean.*) The most humane consistent with what—the pleasure of the man shooting?—I think the man's pleasure ought to be secondary altogether. The first thing ought to be the feelings of the animal that is being shot.

17383. I suppose you must be aware that shooting pheasants and other game, as it is ordinarily conducted, causes pain to the majority of the animals that are killed?—I suppose it is so.

17384. Is that morally right?—Well, of course, that raises the whole question of sport. I do not think it is.

17385. Is it morally more justifiable than the painless experimentation on animals with a view to extending human knowledge?—An animal that is killed for the purpose of providing food is, in my estimation, in quite a different category from animals killed for purposes of sport, although they may be used for food afterwards.

17386. Does that apply to pheasants bred on purpose that they may furnish sport?—Yes, I do not believe in that.

17387. Do you mean that you do not believe there is such a thing?—I do not believe it is right to breed pheasants or any other animal for the purpose of sport, though they may be used for food afterwards. They are not killed for the purpose of food; they are killed for the purpose of sport, and I disapprove of that.

17388. Is it morally less defensible than painless experimentation on animals?—I consider that they are both indefensible.

17389. Would you have the preservation of game for purposes of sport put down by law?—I would. I would like to say that I am not speaking for my society now; I am giving my own personal opinion.

17390. But supposing that your society is successful in its immediate objects, you would be in favour of their perhaps trying to influence public opinion so as to have sport put down?—There are societies for that purpose, and they have my warmest support personally, but I do not think my own society would enter into that at all, as it would be outside its province.

17391. Would it be justifiable to ride a horse to death in order to save the life of a human being?—That is a very difficult question. I do not think it would.

17392. It ought not to be a difficult question; I should not find the question at all difficult, and I expected you would not either; but it seemed to me to be a good case to define what is the attitude of your society with regard to these things?—I do not think it would be justifiable.

17393. Did you draft this petition to Parliament?—No, I did not.

17394. Do you approve of it?—Yes, I do.

17395. The second paragraph reads: "Your Petitioners are furthermore of opinion that, on account of this docility and obedience to the word of command, the dog has of late years been specially selected by vivisectors for extensive and peculiarly revolting and painful experimentation." Can you tell me what is the evidence that the dog was selected for experiments on account of its docility and obedience?—It is a well-known fact that the dog is very much more easily managed than any other animal.

17396. Than a sheep?—Than a sheep.

17397. Is it much more docile than a sheep?—I would not say more docile.

17398. As docile as a sheep?—Yes, quite as docile. It is more intelligent than the sheep, of course.

17399. But it was its docility that was specially mentioned?—That comes in "Docility and obedience to the word of command"—come in together. It is more intelligent than a sheep, and therefore more easily managed.

17400. Do you suggest that in experimentation a dog is managed by merely ordering it to do this and that—that the question of obedience is of real importance when an animal is to be operated upon by a physiologist?—I think so; it gives him less trouble, I should think, judging from what I gather.

17401. At what stage of the operation does it give less trouble?—At all stages.

17402. Do you mean that it is easier to catch a dog and secure it to a table than to catch a sheep?—Yes.

17403. You mean that the experimenter is in more danger of being bitten by a sheep, perhaps?—I do not mean that.

17404. In what respect is it more convenient to use a dog?—Because a dog can be persuaded to do almost anything.

17405. Do you suggest that when a physiologist proceeds to prepare an animal for experiment, he uses his power of persuasion, in the ordinary sense of the word?—I might refer to what Professor Starling said before your Commission. He said that he had known a dog which had undergone an operation and had been brought in after it had been kept for observation a certain time, and when it was brought in it licked the hands of the experimenter, and wagged its tail, and showed every sign of joy at seeing him again. I certainly repudiate most strongly the conclusion that Professor Starling came to from that. He wanted to make out that the dog was glad to see the experimenter, because he wanted to be experimented on again.

17406. (*Chairman.*) Do you say that Professor Starling said that?—That is the conclusion that he came to.

17407. I suppose from what he said?—I take it so, from what he said.

17408. Did he say that he believed the dog licked

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his hands because it wanted to be experimented on again?—I believe he brought it forward to prove that dogs are not averse to being experimented upon.

17409. You say that he meant the Commission to understand that?—Yes, I thought so.

17410. (*Sir John McFadyean.*) But that is really quite beside the point that I wished to receive information about?—I think that illustrates the question. You asked me why I think that a dog is more easily managed than a sheep. I say that Professor Starling himself has proved it. At least, he believes that, I should imagine from his evidence.

17411. Are you aware that, with the rarest possible exceptions, dogs when they are subjected to vivisection operations, are secured to a table?—Yes, I suppose they are—that is, for physiological experiments.

17412. That is for vivisectional experiments in the literal sense of the word?—Yes, quite.

17413. The dog has to be forcibly tied to the table? —Yes.

17414. In what sense are its docility and its obedience of advantage there, as compared with a sheep?—It is very much easier to persuade a dog. A sheep is stupid.

17415. But you do not persuade. You take the animal forcibly and lay it on the table, and tie its legs down. In what respect is it of advantage to the experimenter to select a dog for this purpose, and as a rule a strange dog?—I think that if I were going to experiment upon animals I would very much prefer to select a dog, because I know that I could manage it better than any other animal.

17416. Have you any knowledge of sheep?—No, I have not.

17417. So that it is possible that your opinion as to the advantage of a dog as compared with a sheep is entirely mistaken?—It might be, of course.

17418. Have you any knowledge of the pig?—I have no expert knowledge. I have seen one. I cannot say very much about it.

17419. Do you think it would be easier to secure an ordinary strange dog than an ordinary small pig?—I should think so.

17420 Why?—Pigs are so stupid, for one thing.

17421. But I have already pointed out that you do not ask for any assistance from their intelligence. You lift the animal forcibly off the ground, and tie it to the table?—That is where we must disagree, I think. It appears to me quite unnatural to think that an experimenter would not take advantage of the natural characteristics of a dog as compared with a sheep or a pig for the purpose of helping him in his experiment.

17422. Do you think that the experimenter relies on assistance from the dog?—I should think so. I should think it would be to his advantage to do so.

17423. (*Chairman.*) Are you not assuming that this dog is a dog that has known the owner for many years?—No, I am not afraid of any dog.

17424. You think that a strange dog would do all it could to assist the man in his experiment?—Yes.

17425. (*Sir John McFadyean.*) Supposing that the operation is to open the dog's abdomen to study some point of physiology in connection with the liver, would you please tell us the assistance that you would expect a strange dog to give you?—I cannot; that goes beyond my province. I cannot give you any view at all as to anything of that kind, upon the scientific side of the question. I do not know the nature of the experiment.

17426. You are aware that in most cases the first step, or a step at any rate before any cutting takes place, is the administration of anæsthetics?—I suppose so.

17427. Tell us what assistance you expect a dog to give you in that stage of the operation?—After the anæsthetic?

17428. Before; the assistance that you would not get from a pig or a sheep?—But it would not resist to the same extent.

17429. (*Sir Mackenzie Chalmers.*) But when it is under the anæsthesia what is the difference?

17430. (*Sir John McFadyean.*) Is the advantage lost as soon as the animal becomes unconscious?—I cannot say. I am told that it cannot be anæsthetised, of course.

17431. But you do not wish to give evidence on that point, do you?—No, I do not wish to give evidence on the question, because, as I said before, it goes beyond my province entirely.

17432. But still you approve of that statement in this paragraph?—So far as I know dogs, I think it is quite true, and I know them fairly well now. I have found dogs both docile and obedient to the word of command, and it stands to reason that it is easier to experiment on an animal of that kind than upon one devoid of those characteristics.

17433. (*Mr. Ram.*) Under anæsthetics?—No, before that.

17434. If it is under anæsthetics, where is the difference?—I am told that it is not anæsthetised. That you cannnot anæsthetise a dog.

17435. That is what you base yourself upon?—Yes.

17436. (*Sir John McFadyean.*) I did not doubt that you believed that; but I have spent about ten minutes in trying to get you to give reasons, which would satisfy the Commission as to the soundness of your conclusions; and you have not been able to give any that satisfied me?—I can give no better reason than my knowledge of dogs.

17437. But you admitted that you had no knowledge of sheep?—I have the same knowledge of sheep.

17438. Then why institute a comparison between a dog and other domestic animals?—I do not think it is necessary to have an intimate knowledge of any animal to institute a comparison.

17439. Do you know the statistics regarding the operations performed under a licence and certificates?—I cannot say. I have seen them.

17440. Can you tell me what particular operations are referred to in this petition as "peculiarly revolting and painful"?—I imagine the reference was, I do not know, to Professor Crile's experiments.

17441. What makes you think so?—Because I think this petition was drafted about that time, or shortly after that. I think they had that in view.

17442. So that if Dr. Crile's experiments were of a painless character, you would be hard put to it perhaps to justify that expression?—I do not think so. I think they would be able to find others.

17443. Will you tell me some others?—We consider that Professor Schäfer's experiments come under that description.

17444. They were "peculiarly revolting and painful," were they?—I think so. We considered them so.

17445. And others?—I do not know that I could tell you off-hand now. No doubt I could find a large number upon which you could base this. I have not the slightest doubt I could.

17446. You think you could find a large number that would be correctly described as revolting and painful?—I think so. But even if we could not I think the two cases that I have already cited are quite sufficient to justify this expression.

17447. But still you say you would have no trouble in getting a large number?—I think not.

17448. Will you undertake to look up a large number and add them to your evidence?—Yes, I will.

17449. Giving particulars as to time and place and so on?—Yes, I shall be delighted.*

17450. Then you do not think it was too strong language to use when they spoke of "Demonstrations of a prolonged and agonising nature before classes of students"?—No, I do not think it is too strong.

17451. Can you tell me any particular experimenter who has within recent years in demonstrating to a class of students caused prolonged and agonising suffering to an animal?—I cannot at the moment.

17452. Do you think you can find examples if you have time?—Yes, I think so.

17453. You think you could find a large number of cases?—A number of cases; I cannot say a large number of cases. I do not know how many, but I will find as many as I can for you.

17454. You will find as many as you can?—Yes.*

* *Further particulars were not supplied by the witness.*

17455. Thank you very much. You quoted Mr. Woodroffe Hill's opinion. Do you know whether he is entitled to be considered an expert on the anæsthetising of dogs?—I think so. He is a dog specialist. He has published several books on dogs, and has made a very careful study of dogs in every sense of the word.

17456. Is it within your knowledge that he has anæsthetised twenty dogs in his life?—I have only his word for it. He has told me that he has done so—at least tried to do so.

17457. He has tried to do twenty?—I do not know about twenty. I cannot say how many. He has told me he has tried to anæsthetise dogs.

17458. (*Sir Mackenzie Chalmers.*) And failed?—Yes.

17459. (*Sir John McFadyean.*) Would you admit that one might measure the value of his opinion by getting to know how many dogs he had tried it on?—I should say so.

17460. You would personally yourself attach most weight to the opinion of those who had most experience, other things being equal?—Naturally.

17461. At the present time your own experience and observation do not entitle you to form an opinion on this question?—Not at all.

17462. You are adopting the opinion of Mr. Woodroffe Hill, I think?—Yes, and others who have had experience in the matter.

17463. You would be prepared to abandon that opinion if you had laid before you the views of others of a contrary character based on a very much larger experience?—Quite so, if they convinced me that their experience was of such a character as to be reliable.

17464. I want to tell you that last week we had evidence given here from a gentleman, who had administered anæsthetics successfully to many thousands of dogs, who had actually administered it, and who had tabulated the results of the administration of anæsthetics to 1,200 consecutive cases, with only, I think, five deaths; and that many of these animals were under the influence of anæsthetics for a long period, up to five hours. Would you advise the Commission to attach greater weight to that evidence or to Mr. Woodroffe Hill's?—Of course the Commission is in a better position to judge than I am. I have not heard this gentleman's evidence. I take your word that it is so. The Commission are in a much better position to judge which of the two opinions to take. I would not presume to advise the Commission as to anything.

17465. Has the fact, which I have just told you, that we had this evidence given to us raised any doubt in your mind as to the soundness of the conclusions you had previously arrived at?—I am always prepared to accept the evidence of people who know more about a subject than I do myself. And I shall very carefully read the evidence to which you have referred, in order to be able either to revise or to justify my opinion.

17466. The witness is Mr. Hobday?—Thank you.

17467. (*Sir Mackenzie Chalmers.*) May I take it generally that your society hold the opinion that dogs cannot be anæsthetised?—Yes, that is the opinion of my committee.

17468. And even if they could be anæsthetised, that would not remove their objection?—Oh, dear me, no.

17469. In your petition you mention that dogs are used for "demonstrations of a prolonged and agonising nature before classes of students"?—Yes.

17470. You are not prepared with any evidence on that subject to-day?—No.

17471. Are you aware that under the law as it stands a dog in a demonstration must be absolutely under anæsthetics, and must be killed before it comes out?—So I understand.

17472. Do you know at all on what evidence that assertion was based?—No, I do not.

17473. It was before your time?—This petition was drafted before I became connected with the League.

17474. How long have you been secretary?—Just under three years.

17475. Do you devote the whole of your time to it?—Yes, practically.

17476. Then there is a paid secretary in London and honorary secretaries in the country?—Yes.

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17477. The present committee are responsible for this statement, of course?—Yes.

17478. This petition is still being used?—Yes.

17479. But you do not know on what evidence it was founded?—No, I can ascertain for the Commission.

17480. I suppose you will agree, at any rate, that if the statements are correct, the operations were illegal?—Yes.

17481. You do not know whether the attention of the Home Secretary was called to any of these illegal operations?—No, I am not aware that it was.

17482. I am afraid you cannot help us on that. I should have liked some information with regard to it. You know dogs suffer from distemper a good deal?—Yes.

17483. It is a painful disease, is it not?—Yes.

17484. Would your society hold that any experiments performed on healthy dogs for the purpose of finding out the cure or better treatment of distemper are justifiable?—They would, of course, highly approve the study of distemper in dogs which were already suffering from it; but they would totally disapprove of any inoculations or any experiments performed on healthy dogs for the purpose of giving them distemper, even with the view of finding out a cure or better treatment of the disease.

17485. Even for the purpose of saving future generations of dogs from distemper?—Yes.

17486. And that would apply to all canine diseases, of course?—Yes.

17487. Are you aware that veterinary surgeons perform very severe operations on dogs for curative purposes?—Yes, exactly the same as medical men do on human beings.

17488. Does your society hold that those operations are justifiable, if a dog cannot be properly anæsthetised?—Yes, I think so, if it is for the good of the dog itself.

17489. Whether it can be anæsthetised or not?—Yes, for that particular dog.

17490. If the operation is severe would it not be much more merciful to kill the poor beast?—It depends. If there were any hope of its recovery I should not say it would be more merciful to kill. If there is no hope of recovery, certainly it would be more merciful to kill it.

17491. Does your society agree that superfluous dogs ought to be destroyed—No, we do not believe in destroying dogs.

17492. For instance, take the Dogs' Home, where about 30,000 dogs are killed every year?—We do not approve of that.

17493. What do you suggest as an alternative?—We take measures to protect all stray dogs that come into our hands. We send them to our own home and find homes for them. We find people who take charge of them.

17494. But you are not prepared to provide for 30,000 canine orphans every year, are you?—I do not think we should find much difficulty in finding homes for them. We have no difficulty now in finding homes for hundreds which pass through our hands annually.

17495. Am I right that about 30,000 are now destroyed in the Dogs' Home in London alone?—I do not know the figures, but I know there are a large number.

17495A. However, your society disapproves of that?—We do not believe in it. If a dog is suffering from an incurable disease, then it is a greater piece of mercy to put it to death, and that we do not disapprove of; but we do not believe in the wanton destruction of dogs.

17496. I imagine that your society disapprove of the practice, for instance, of spaying bitches to prevent their breeding, which is very common?—As a society I do not know that they have any opinion as to that. I have not heard it expressed. I have heard individual opinions, of course.

17497. I was asking the views of your society?—I do not think they have any.

17498. I want just to ask you one question about Professor Schäfer's experiments. You know that most

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of his experiments were on dogs, which he had previously chloroformed and anæsthetised?—Some of them were, I understand; I do not know the proportion.

17499. Do you know that only three were performed on dogs that were not anæsthetised?—I know that some were.

17500. According to his evidence before us, three dogs were experimented on without anæsthetics. You know that in each of those cases the dog was not allowed to recover, the dog was drowned?—I do not know the particulars.

17501. It was killed by drowning, but at any rate that was an experiment that your society strongly objected to?—Yes.

17502. Are you aware that superfluous dogs in Edinburgh, where these experiments were performed, were destroyed by drowning?—We do not approve of that method of destruction. If they have to be destroyed we believe they ought to be destroyed mercifully in a properly constituted lethal chamber.

17503. I suppose everybody would agree that that is the best way, but would your society draw any distinction between drowning a dog as a superfluous dog, and drowning a dog in the hope of discovering a means of saving human life?—I do not think we draw any distinction whatever, because we do not believe in superfluous dogs.

17504. At any rate your society protested against Professor Schäfer's experiments, but did not protest against the destruction of superfluous dogs in Edinburgh?—We protest against the destruction of dogs by drowning everywhere, where it has come to our knowledge. We have discussed the matter with the Leeds Town Council recently, and persuaded them to discontinue it.

17505. (*Chairman.*) Does that include puppies; do you extend your care down to new-born puppies?—Yes.

17506. (*Sir Mackenzie Chalmers.*) Would you not allow a puppy, before it has opened its eyes, to be drowned?—That is a matter of individual opinion, I suppose. I cannot commit my society to an opinion upon that point.

17507. There are only two other points I want to ask you about. You mentioned certain Government laboratories; what laboratories do you refer to. I am not aware of any Government laboratories?—The Board of Agriculture have licensed laboratories for the purpose of vivisection, so I understand, and also the Local Government Board and, I believe, the India Office and the Home Office.

17508. I am not aware at any rate of the Home Office having any. I should have heard of it, I think?—I may be wrong about the Home Office.

17509. (*Chairman.*) You probably mean the Colonial Office?—Yes.

17510. (*Sir Mackenzie Chalmers.*) You may perhaps speak of the Lister Institute and the School of Tropical Medicine?—Yes.

17511. That is to say, the Government ask for experiments to be performed with reference to work in subjects dealt with in the departments?—Yes; the Return of the Home Secretary for last year makes special reference to these Government laboratories and laboratories which are supported by public money under the auspices of local authorities in different parts of the country. Those are what we mean.

17512. The only other point is this. You mentioned that some of the medical men who side with you found a difficulty in expressing their opinions, because of the strong opinion held by the heads of the medical profession. How do you account for it that men of the greatest eminence in the medical profession are all on one side, that the heads of the profession are so strong?—I cannot account for it. I do not profess to be able to account for it. I am very sorry that it is so.

17513. You admit the fact?—I am afraid that I must. Of course on the other side I say there are many eminent men who do not believe in vivisection.

17514. Many eminent medical men?—Yes, I believe so.

17515. (*Mr. Ram.*) Can you tell me their names?—Yes.

17516. (*Sir Mackenzie Chalmers.*) You have given some names. We should like to know the names of any existing men of acknowledged scientific attainments in the medical profession who are on your side?—We have often heard Sir Frederick Treves quoted; I am not at all prepared to say what his views are on the general subject, but he certainly expressed himself very strongly.

17517. On one particular point?—Yes.

17518. But speaking generally it is not from the heads of the profession, but from the other end that you have to draw your witnesses?—I do not know. I think you have had some fairly eminent men from our side before your Commission.

17519. (*Mr. Ram.*) Just a few questions as to facts, please. You have given us on several occasions the views of your society. How are they arrived at?—I do not quite understand. How do you expect me to answer?

17520. I leave you to find an answer. I put the question: How do your society arrive at the views which you have represented as being theirs?—As I told you, the society was started as an anti-muzzling society, and it has developed little by little until it became a General National Canine Defence Society. They have not arrived at these views suddenly, but it has been really a development, as with most societies.

17521. To take one example, for instance, which has been already referred to. You say that your society object to all experiments, because of the difficulty of anæsthetisation?—Yes.

17522. On what do they base themselves in saying that it is difficult to anæsthetise animals?—I think I explained before that we have received letters from medical men and veterinary surgeons, who give it as their opinion, and give it strongly as their opinion, that dogs cannot be anæsthetised.

17523. You gave us the name of one gentleman who said so, and who said that he had tried and failed on a few dogs. Can you give us the name of any other gentleman besides him?—I read to you an extract from Professor Woodroffe Hill's letter.

17524. That is the one to whom I am alluding. Can you give us any other names?—Dr. Lucas Hughes, of Liverpool, wrote us a very strong letter on the subject. We issued that letter from Dr. Hughes as a leaflet, and we also published it in our report.

17525. That is what made me anxious to get at it?—I am afraid that I cannot put my hand upon it at the moment, but it was a very strong letter.

17526. What was the date of it; do you know?—I should think it was about two years ago.

17527. Did he state how many animals he had attempted to anæsthetise?—No, he does not.

17528. You will get that information for me and send it to the Commission?—I will, with pleasure.*

17529. You have already been referred to the evidence of Professor Hobday as to anæsthetising animals. I want to read to you two sentences from the evidence of Dr. Dudley Wilmot Buxton, a Doctor of Medicine and past President of the Society of Anæsthetists, Consulting Anæsthetist to the National Hospital for Paralysis and Epilepsy, Anæsthetist and Lecturer on Anæsthetics in University College Hospital, who has had a very large experience in the study and administration of anæsthetics. He was asked this at Question 12439:—"Is there any difference, and if so will you explain it to us, in the method of operation of anæsthetics upon man and upon the lower animals?" And his answer is "None." Then he was asked, "Do you note their condition of insensibility, and the degree of it, in the same way with animals as you would with men?" And he said, "Always"?—That refers to all animals.

17530. That refers to all animals. Your society, as I understand, have been informed that it is specially difficult to anæsthetise dogs?—Yes.

17530*a*. Dr. Buxton was asked that question at Question 12489:—"We have been told by several witnesses that dogs are particularly difficult to keep under anæsthetics. Is that your experience?—(A) Certainly not. (Q) There is no difficulty in keeping a dog for several hours under anæsthetics?—(A) Not if you understand your business. (Q) We have also been told that it requires a very skilled anæsthetist to anæsthetise a

* Further particulars were not supplied by the witness.

dog. Is that so?—(A) That would depend entirely upon the apparatus employed." Then, at Question 12496:—"And, in your opinion, is any special skill or knowledge required for anæsthetising a dog in order to produce anæsthesia?—(A.) No, I think not. Of course, an inexpert person may produce an overdose by giving too much. I do not think he is likely to err on the other side." And again:—"If the dose of anæsthetic was insufficient, the dose of air would be insufficient, and the animal would die?—(A.) That is so. (Q.) That is your opinion?—(A.) That is my opinion." That is the evidence of a very distinguished man who has made it the study of his life to ascertain the effects of anæsthetics upon human creatures and upon animals. What is the evidence that you propose to offer to us on which you would ask us to set aside that evidence?—I am only prepared to give you the evidence that we have received, to some of which I have already referred, of such men as Professor Woodroffe Hill and Dr. Lucas Hughes of Liverpool.

17531. Those are two?—Those are two that I can remember at the moment.

17532. Our object is to see what evidence there is on either side?—I have also had conversations with several medical men, who have all said the same thing.

17533. In those conversations did you ever ask those medical men how many animals they had operated upon?—No.

17534. That is rather material, is it not?—Yes, it may be. I have simply asked them what their views were, and did they believe that this was correct, and they have said yes—that they thought the views expressed by those two gentlemen were correct.

17535. The views of your society, of course, are valuable entirely according as to whether they are or are not founded upon accurate knowledge?—Quite so.

17536. Can you tell me who drafted the petition which you presented to Parliament?—I really cannot tell you exactly who drafted it. It was drafted with the approval of the committee, but who the actual person was who drafted it I do not know. I daresay I could ascertain it.

17537. You have been good enough to hand to me a pamphlet just now, and I want to ask you about one or two expressions in it?—We do not issue that pamphlet, of course; it is a pamphlet which is issued by another society, which I simply put in my bag for the purpose of reference to the figures. I take no responsibility whatever for the pamphlet.

17538. Do your society adopt the statements that are made in it?—We simply adopt the facts as they are taken out of the Home Secretary's Returns. I could not put my hand on the Home Secretary's Official Reutrns, and therefore I put this in my bag instead.

17539. Do I take it that you have read this pamphlet yourself?—Yes.

17540. Do the statements herein made represent the views of your society also?—I do not know that they do exactly. I would not like to take the responsibility for them without going more deeply into them.

17541. Does your society think that the Act is "useless as a protection for animals, while protecting their torturers"?—That sounds rather strong, does it not?

17542. That is for you to say. Does that represent the views of your society?—I cannot say that we would put it quite in that form.

17543. I will just read you one other passage. Does your society think that "an actual painless vivisection is an impossibility"?—We do not take any responsibility for that statement.

17544. Then I will pass from that as it is not issued by your society. In the covering letter which you issued with the petition you say there has been a "terrible increase of experiments on dogs during the past three years"?—Yes.

17545. That is written in the year 1906?—Yes.

17546. "An increase proved beyond controversy"?—This letter is dated 1906; I believe it was drafted before that, only it was reissued in 1906.

17547. Will you tell me to which three years you refer in this letter when you say that there has been "a terrible increase of experiments on dogs during the past three years"?—I should think it refers to the years 1902-3-4.

17548. How can that be when it goes on to say that it is proved by a statement made by the Home Secretary in 1903. That cannot prove anything about 1904?—No.

17549. Nor can it prove anything about 1903, can it?—But this is only a statement made with regard to the year 1902, which I take to be the first of the three years referred to in the letter.

17550. (*Chairman.*) What is the date of the petition or letter?—The date of the issue of this particular letter which I have before me is April, 1906, but it was drafted, I am sure, before then, and was issued in the first instance earlier than that. This is a reissue sent out later.

17551. (*Mr. Ram.*) But it is on the faith of this letter that you are asking persons to subscribe and to take up the petition to Parliament?—Yes.

17552. Therefore, I want to test the accuracy of the statements in this letter. The letter is signed by your chairman and issued in April, 1906?—Yes.

17553. It says that in the past three years there has been a terrible increase in the number of experiments on dogs, as proved by the statement made in Parliament by the Home Secretary on May 11th, 1903?—Yes.

17554. I ask you, in this letter which is sent to me and many other people, which are the three years to which you refer?—I cannot tell you the exact three years; 1902 may have been the last of the three years for anything that I know. At all events, I do not think that the facts stated in the letter can be impugned at all, because there is a very great increase in experiments performed on dogs and other animals for all the years since 1897.

17555. (*Chairman.*) But this is all about dogs; there is nothing about other animals?—This is including dogs.

17556. (*Mr. Ram.*) Since 1897?—Yes.

17557. And other animals?—Yes.

17558. Please narrow yourself to dogs. How can you prove that there was a terrible increase in experiments on dogs during the three years preceding the issue of this letter?—I have not the figures with me with reference to dogs alone, but I feel pretty certain that it is so.

17559. Do you know how many dogs were employed by Professor Starling at his laboratory?—He says 155.

17560. Well, do you doubt that?—I do not remember exactly what year he refers to. If he refers to 1902 I cerainly do not doubt it.

17561. I will give you the reference. You will find it in Professor Starling's evidence on page 116 of the first volume, in an answer to Colonel Lockwood, at Question 3470: "I will take the year 1902, since at the request of the Inspector I separated the experiments made in that year on dogs from those made on other animals. In this year 155 experiments were performed on dogs in the physiological laboratory at University College. Of these, 151 were performed under licence alone. What does this mean? In experiments performed under licence alone the animal must, during the whole of the experiment"—(then quoting the Act)—"'be under the influence of some anæsthetic of sufficient power to prevent the animal feeling pain, and the animal must, if the pain is likely to continue after the effect of the anæsthetic has ceased, or if any serious injury has been inflicted on the animal, be killed before it recovers from the influence of the anæsthetic which has been administered.' That is a quotation from the Act. In all these experiments, therefore, the dog was first chloroformed. The experiment was then performed while it was fully anæsthetised, and at the conclusion of the experiment the animal was killed without ever recovering consciousness." That was true of 151 experiments out of 155?—Yes.

17562. You accept that statement of his?—I have no reason to disbelieve it. You see that we are saying that the numbers in 1902 were 232, and I find that that is quite consistent with what Professor Starling says.

17563. I am not disputing now whether the number is 232 or 155?—But I should like to explain that, because there seems to be a discrepancy.

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17564. If you please?—Professor Starling said that he used 155 dogs, which I find is absolutely correct. We say that there were 232, which is also absolutely correct, the difference being that in the Physiological Department of University College 155 dogs were used, and 77 were used in the Department of Pathological Chemistry; that is the answer of the Home Secretary to which we refer in our petition, and also in the covering letter, 155 in the Physiological Department and 77 in the other, so that Professor Starling is absolutely correct, and we are correct, too.

17565. I am willing to accept the numbers. What I want to point out to you is that out of those numbers it is only possible that four dogs can have suffered anything at all?—I do not say that.

17566. That is what Professor Starling states?—Yes, exactly, he says so, but I am afraid that we cannot accept that statement that only four out of 155 could have suffered any pain.

17567. Then will you tell me why you ask this Commission not to accept that statement?—Because neither Professor Starling nor anyone else can prove that the animals do not suffer. We have only to use our own common sense in the matter.

17568. But using common sense, if an animal is put under anæsthetics by which it is rendered wholly unconscious, and dies before it come from under the influence of the anæsthetic, how can that animal suffer?—I am not convinced that a dog can be anæsthetised.

17569. If you choose not to accept Mr. Buxton's and Professor Hobday's evidence, that is another matter; but hypothetically, if an animal is anæsthetised so as to be beyond all possible sensation, will you agree that it cannot suffer?—I suppose I must if it is beyond all possible sensation.

17570. And if the animal dies before recovery of consciousness, do you agree that there can be no kind of suffering in that animal?—I suppose I must. I should like, at the same time, to guard myself by saying that, even granting all that, we do not believe that vivisection of dogs is justifiable, on other grounds, of course.

17571. Following up that last remark of yours, granted that a dog is wholly anæsthetised and can never recover consciousness at all, on what grounds do you object to its being experimented upon?—We do not consider that man has a moral right to experiment upon any live animal for his own purposes. We think it is absolutely wrong to do so.

17572. That goes against any vivisection at all?—Yes.

17573. Do you draw no distinction then between dogs and other animals?—If you ask my own opinion, I do not personally.

17574. Does your society?—I have already said that my society does not go into the question of other animals. I believe that the great majority, if not the whole of the members, of my society do not believe in any vivisection at all, but *qua* the society we only advocate the non-vivisection of dogs.

17575. I will read you one other passage from Professor Starling's evidence, which you will find at page 115, in answer to Question 3451: "Though I have been engaged in the experimental pursuit of physiology for the last seventeen years, on no occasion have I ever seen pain inflicted in any experiment on a dog or cat, or, I might add, a rabbit, in a physiological laboratory in this country, and my testimony would be borne out by that of anyone engaged in experimental work in this country." Can you show the Commission any reason why they should not accept that statement?—All I can say is that I would not accept it.

17576. Why not?—Simply because it seems to me absolutely unreasonable to think that a man like Professor Starling, who has been engaged in this work for 17 years, never, as he says, saw pain inflicted on an animal. I am sorry to say that I cannot accept it. Professor Starling may think so, but my common sense will not allow me to accept it, and revolts against its unreasonableness.

17577. That is a direct statement of fact by Professor Starling?—No, of opinion, I take it.

17578. No, of fact?—So far as his opinion goes he has not seen any pain inflicted. I cannot accept that. That is all.

17579. You say that your common sense is the ground on which we ought to decline to accept that statement?—I am not dictating to the Commission; I should be very sorry to do that. I am simply giving my own views. The Commission are in a better position to judge as to what they should accept than I am.

17580. You quoted Professor Starling just now as to a dog being glad to be introduced into a laboratory. Let me read his exact words, and you will see what he said?—Thank you very much. I shall be glad.

17581. I thought you took it as implying that a dog shows pleasure at being about to be experimented upon again?—That is the impression that I had.

17582. Let me see if I can remove it. Will you turn to page 139, Question 4024—it is a question put by myself: "Such an animal, I presume, if it was an intelligent animal—a monkey or a dog—say, might have more apprehension than an animal which had never been so treated before, might it not?—(A.) I do not think so. I cannot, of course, judge of these inoculation experiments, but the experiments which I have in my experience are experiments in which the animal—the dog—has had an intestinal fistula established—that is to say, a little opening into a loop of its intestines. These animals are quite happy. When we want to observe the flow of juice from these fistulous openings, the dogs are brought into the room, placed in front of a fire in a little stand, and slung up in a duster to take the weight off their legs. Either I myself, or the laboratory boy, stays with them and talks to them, and meanwhile they are given some meat, and the juice drops from the opening into a flask placed below. These animals are happy to be brought in. They are taken notice of, and talked to, and given a good meal of meat. They are very happy to be brought into the laboratory, to be subject to observation under those circumstances, and they do not seem to be incommoded in any way by the fact that they have an opening into a loop of their intestines. (Q.) And during such time as they are not under observation in their cages, are they then in a state of suffering?—(A.) No, none whatever. (Q.) Even a well-established fistulous opening does not produce suffering while it is going on?—(A.) But what I wanted specially to insist on was that they were not even in a state of apprehension. They were perfectly happy. They liked to be talked to, and they behaved like a normal dog, in fact." That is the passage, I think, to which you alluded?—Yes.

17583. You observe there that the dogs were not brought in to be experimented upon again at all?—Yes, for the purpose of observation, I take it.

17584. They were not to be subjected to anything which was to cause them any pain?—That is the only respect in which I can modify my opinion. I am sorry that I said the other. I understood that it was for the purpose of experiment. At the same time that does not alter my views of Professor Starling's evidence, and it does not affect the point made when I adduced Professor Starling's evidence as a proof of the docility of the dog as compared with that of other animals.

17585. There is only one more question that I think I need trouble you with, and that is as to rabies. Your society, you say, doubted the existence of rabies to anything like a considerable extent?—To the extent to which it was returned.

17586. Did they doubt the existence of rabies at all?—No, I do not think that they doubted its existence at all. I think they believed that it did exist to some extent.

17587. Do they believe that it is now extinct in this country?—Yes.

17588. If rabies was not widespread, how do you account for the large number of deaths of persons who died of rabies—not of dogs, but of human creatures?—I do not think there were a large number.

17589. Do you know what the numbers were?—Not at the moment.

17590. Have you never had the numbers given you?—Yes, I have, but I cannot at the moment go through them, and my impression is that there was not such a large number.

17591. Your society, I understand, object to muzzling?—Yes.

17592. Have you ever put a muzzle on a dog?—Yes, I have.

17593. Have you ever had a dog which had a muzzle on for three or four days?—Yes.

17594. Did the dog mind it?—Yes, it objected very much.

17595. After it had worn it three days?—Yes.

17596. What sort of dog was it?—A Yorkshire terrier.

17597. Did it never get reconciled to its muzzle?—No, I do not think it ever did.

17598. I think it must have had rabies to start with; it is very different from my experience.

17599. (*Chairman.*) The muzzling was objected to on the ground of cruelty, I understood you to say?—Yes.

17600. (*Dr. Gaskell.*) There is just one question I want to be clear about. I understood you to say that in your opinion, and as representing your society, an actual painless vivisection in dogs was not an impossibility?—Oh, no.

17601. You said that just now; those were your words?—No, what I said was this. I was asked, assuming that a dog could be anæsthetised, assuming that what Professor Starling said was true, if a dog could be anæsthetised, then the experiment must have been painless, but I say that we deny, so far as we know, that a dog can be anæsthetised, and that if a dog cannot be anæsthetised, then it cannot be painlessly experimented upon.

17602. Then you want to withdraw that remark that you made just now?—I am not aware that I made it. I had no intention of making it.

17603. (*Sir John McFadyean.*) It may have been a misapprehension on your part?—Absolutely, because I have all along said that we believe that a dog cannot be anæsthetised, and therefore it cannot be painlessly vivisected. I understood the question to be whether if a dog could be anæsthetised and destroyed before it recovered consciousness, I believed that it suffered pain. I certainly say that if it could be anæsthetised it could not suffer pain.

17604. (*Dr. Gaskell.*) Did you not also just now say that in your opinion, and in that of your Society, a properly constituted lethal chamber was the best means of destroying dogs?—Yes.

17605. What do you mean by a lethal chamber?—A box designed for the purpose of putting the dog painlessly to death.

17606. In what way; how would it be put to death; what is the ordinary method in the lethal chamber; what is used?—Principally chloroform and carbonic acid gas, I think.

17607. But as dogs cannot be anæsthetised that is a painful method of putting a dog to death?—I do not say that a dog cannot be killed, I say that it cannot be anæsthetised. I said at the outset that in our opinion what we had been given to understand was that a sufficient dose cannot be given to a dog for the purpose of anæsthetisation without killing it, and that it cannot be made unconscious with a dose of chloroform or anything else without ending in death. We believe that dogs can be killed, of course.

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17608. Then that must necessarily be a painful death?—Not at all.

17609. Because they cannot be anæsthetised?—I do not see that, if the chloroform or other anæsthetic kills them.

17610. If the action of chloroform is first to anæsthetise an animal, and, in a lethal dose, to cause death, the period between when it is first given and the time of the animal's death, must either be a painful period or a non-painful period must it not?—I am not an expert on poisons, but I have seen a dog destroyed in the lethal chamber, and the whole process does not take a minute and a half.

17611. But during that minute and a half it is painful?—But it does not seem to have any pain at all.

17612. Of course it does not?—But, then, that is a very different thing from giving a dog a dose of chloroform when it is intended to be experimented upon by, say, a cutting process, when that dose is not sufficient to enable it not to suffer pain. I think there is a very marked distinction between the two.

17613. I was only on the question of the morality of putting it to death in the lethal chamber by means of a drug which does not anæsthetise, which seemed to me to involve a painful proceeding, and I wanted to know your opinion?—I do not think it is.

17614. (*Sir John McFadyean.*) Are you quite certain that none of these unconscious dogs in the lethal chamber could be revived?—No, they cannot.

17615. On what ground do you base that statement?—I have never known any to be revived.

17616. Is that clear evidence that they cannot be revived?—I do not think that they could always; and we have had veterinary surgeons present when they were lethalised.

17617. Have you had veterinary surgeons trying to revive them?—Oh, dear me, no; we do not experiment upon dogs.

17618. So that really you have no evidence of it?—Our object is to put them to death, when we find it absolutely necessary. We never try to revive them.

17619. Am I right, therefore, in saying that you have no evidence to show that they cannot be revived?—Because we have not experimented.

17620. So that you have no ground for asserting that they cannot be revived?—We have only the ground that they never have been revived.

Mr. JOHN ROSE BRADFORD, M.D., D.SC., F.R.C.P., F.R.S., called in; and Examined.

Mr. J. R. Bradford, M.D., D.Sc., F.R.C.P., F.R.S.

17621. (*Chairman.*) You are a doctor of Medicine, and a Doctor of Science, and Fellow of the Royal College of Physicians, and a Fellow of the Royal Society?—Yes.

17622. And you are Professor of Medicine at University College Hospital Medical School, Physician to University College Hospital and to the Seamen's Hospital?—I am.

17623. You are a member of the Advisory Board of the Army Medical Service, and a member of the Advisory Committee for the Tropical Diseases Research Fund of the Colonial Office, and a member of the Tropical Diseases Committee of the Royal Society?—I am.

17624. And you are Chairman of a Sub-Committee of the Royal Society for the Investigation and the Treatment of Sleeping Sickness and other Diseases?—That is so.

17625. I believe you desire to give evidence as to the value of experiments on animals principally in assisting the physician in actual practice?—Yes.

17626. We have had a great body of evidence as to the value of experiments on animals, and I will not take you into the whole history of the reasons why they are important; but perhaps you will just give us your views about the importance of it as a physician?—I have sent in a short statement already, and I am prepared to answer any questions to the best of my ability that any Commissioner may wish to ask me. I do not know that I have any reasoned detailed statement to make. If there are any points in this short summary on which any Commissioner would like to ask further questions to bring out the points I shall be very pleased to answer them, but, as I say, I have not got any reasoned detailed statement to put before you.

17627. So far as I have looked through your statement, you go through the principal services which have been done, in your view, by experiments on animals, by discovering causes of disease or remedies, very much on the lines that a great number of witnesses have gone through them?—Yes.

17628. I do not want to take anything in unnecessary detail?—I have divided this summary, roughly speaking, into two groups—two paragraphs—the one dealing, I may say, with the results which have been obtained in the various committees on which I happened to serve, especially those in connection with the Royal Society; but I believe you have had a very considerable amount of evidence bearing on that already brought before you, so that my position as regards that is that I am here to answer any questions that I am able to answer which anybody may wish to ask me with reference to that point. Then, with reference to the use of experiments on animals in the practical work of the physician, I have divided

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that into two short headings. One is as regards the necessity of every fully-trained physician having a sound knowledge of the healthy functions of the body, which you may certainly say could not have been obtained without the results of experimental science; and then I have mentioned two or three instances in which I thought that the physician in his every-day practice was directly dependent upon the methods he had to employ, involving experiments on animals to obtain the knowledge that was necessary for the treatment of the sick.

17629. Would you tell us what those are?—The instances I quoted bear, for example, on the necessity for accurate diagnosis in the early stages of disease. There are a great number of diseases which in the early stages do not give rise to what physicians talk of as marked signs; they cannot be detected by the handling and looking at the patient, but they can be detected by methods of investigation necessitating animal experimentation. I do not know whether you want me to give specific instances?

17630. Perhaps you would give us one or two?—A specific instance which would occur to one would be, for example, the case of a patient who had suspicions of tuberculous disease of the lung, at the top of the lung. What we talk of as the physical signs of that disease may be produced by other maladies than tuberculous maladies, and there are classes of cases in which the only satisfactory method of determining whether the patient is or is not suffering from tuberculosis is the inoculation of a guinea-pig with the material expectorated by the patient. That is an illustration of what I mean. If you wait until there are unequivocal signs of the presence of one or more of these two diseases, very much valuable time is lost.

17631. Are you speaking there of a disease that is of comparatively slow progress?—Yes.

17632. You could not use that sort of experiment to discover whether it was scarlet fever?—No.

17633. That would discover itself before the experiment was worked out?—Yes.

17634. But if it is like tuberculosis or suspected tuberculosis, is that experiment practically done now by physicians?—Yes, I have done it—or, rather, have had it done on several occasions on which there has been some doubt as to whether the patient was suffering from tuberculosis or from a malady that is known to physicians as bronchi-ectasis.

17635. (*Mr. Ram.*) Is the result on the guinea-pigs certain and final?—If it is positive, of course.

17635A. (*Sir William Church.*) But the same method is also of use in what you may call acute diseases, is it not?—Yes, certainly; but I thought the Chairman asked with regard to that simple experiment.

17636. But it is also of use for diagnosing purposes in acute diseases?—Yes, and that I have alluded to in the second paragraph, where I quote the instance of diphtheria and typhoid as illustrations of acute diseases.

17637. (*Chairman.*) How would it help you in a case of suspected diphtheria?—In a case of suspected diphtheria, the modern diagnosis is made by recognising the organism as such, the cultural peculiarities of the organism. But the foundations of our knowledge as regards the organisms are derived from experiments which in the past involved experiments on animals, as, for example, to determine whether an organism is virulent or not. That is no longer necessary now in the practical question of an individual patient at the present day; but our knowledge of the virulence of the diphtheria bacillus has been obtained by experiments on animals.

17638. I quite understood that distinction. I only put the question that I did in order to distinguish the two cases—the one in which you can actually test, by an experiment on an animal, whether your particular patient in a particular case has got a particular disease, and the other the general knowledge that you have derived from discovery?—Yes.

17639. I was not aware that it was ever done in practice—that physicians in a case like tuberculosis had a special experiment made on an animal by which to ascertain whether that particular patient had that particular disease?—Yes, that is done.

17640. I think you are the first witness who has referred to that?—Yes.

17641. (*Sir William Church.*) It is also frequently done, more frequently for tuberculosis in other organs of the body, is it not?—Yes.

17642. For instance, in the bladder?—Yes; I mention that in my summary. But I took the first illustration when the Chairman asked me.

17643. What is the next head upon which you desire to give evidence?—The next head is the one Sir William Church has alluded to with reference to the same method being applied in diseases of the urinary organs. They are facts of the same order, of course.

17644. Is the use of special experiments in particular cases common now amongst physicians, or is it only some very advanced ones who take it up?—I do not think it is a question of their being advanced. I think the number of cases where that is required is a very limited number, but they do occur. I have not been quoting hypothetical cases, I mean; I have been quoting actual cases that have occurred. Certainly, in cases of diseases of the bladder and diseases of the kidney it is often extremely difficult to be absolutely certain that tuberculosis is present without an inoculation experiment.

17645. (*Sir William Collins.*) Would the finding of bacillus of tuberculosis alone be insufficient?—You mean by other than inoculation methods?

17646. Yes?—I am not a practical bacteriologist, you must clearly understand that. I do not give evidence as an expert in bacteriology. I give evidence simply as a physician from this point of view. But I have always understood that there are difficulties in the recognition of the bacillus of tuberculosis in certain fluids—as, for example, the urine. And certainly putting that on one side, an inoculation experiment is a very much more satisfactory method, because you learn not only with certainty that the bacillus of tuberculosis is present, but you also learn the degree of virulence of the organism which (here I am only perhaps expressing my own personal opinion) I think sometimes is a fact of considerable importance.

17647. Is that true also of the sputum in the case of tuberculosis? Would the finding of the bacillus be insufficient without inoculation of an animal to verify the diagnosis?—I think that the recognition of the organisms in the sputum is more easy by other methods. There are not the same sources of fallacy as there are in the case of the urine.

17648. Then would it be sufficient without inoculation?—It might be. I do not know whether the Commission wish me to quote individual cases—whether it is worth while.

17649. (*Chairman.*) Certainly, where it illustrates your views?—In a case that I have very much in my mind of a man with what is spoken of as apical bronchiectasis, the illness was extraordinarily suggestive of tuberculosis. The examination of the sputum was negative, but yet the clinical picture was such that one had grave doubts about the accuracy of repeated examination of the sputum; and the inoculation experiment was also negative. And the subsequent history of that case proved the correctness of absence of tuberculosis, to the best of my judgment.

17650. (*Sir William Collins.*) In that case did not the bacteriological test and the inoculation test point to the same conclusion?—Yes, in that case, certainly.

17651. Can you give us an example in which the bacteriological test and the inoculation test pointed in opposite directions?—I could not recall a case actually at the moment; but I think I am correct in saying that in cases of pleural effusion one has often failed to find the organism in the fluid when the inoculation of the fluid has led to the development of the lesion in animals. I have not got an actual concrete case before my mind.

17652. (*Sir John McFadyean.*) It is a matter of common knowledge, is it not, among pathologists and bacteriologists that the inoculation test is a much more delicate one than the microscopic test?—That is so; that is my point, of course.

17653. You can use for one inoculation operation an amount of fluid that ten men could not examine in the course of several days?—That is so.

17654. (*Chairman.*) Do you attach importance to the freedom of the physiologist to experiment upon different kinds of animals?—Yes, I attach great importance to that.

17655. Will you tell us why?—First and foremost because I think that, in order to obtain a really sound knowledge of physiological processes, one wants to have the opportunity of investigating them under different conditions. Similar functions are performed in different animals by different agencies, and with slight differences in their physiology; and one in that way, by the use of different animals, gets a wider perception of the physiological processes in any given organ. I think that is one reason.

17656. We have been told that different animals resemble man in different parts of their bodies?—Yes; some animals resemble man more closely in certain parts of their structure and functions than other animals do.

17657. The monkey, with regard to the brain, I understand?—Yes.

17658. And I think we have been told by several witnesses that there are some parts of a dog—the digestive organs and the bowels—that closely resemble those of a man?—Yes; the general nutritive processes in the dog have a closer analogy to those in man than in the case of many other animals.

17659. I suppose there are other instances in which certain parts of an animal would not afford much assistance with relation to the knowledge of man?—Yes.

17660. The last witness that we had was one who belonged to those who protest very strongly against dogs being used for these purposes. Are there any purposes for which you consider the dog is essential?—I think that it is very difficult to carry out experiments on the circulation in other animals than the dog. When I say that it is very difficult, that is perhaps not quite the correct word to use. But the dog is the animal in which the circulatory apparatus has much more resemblance to that of man than in the case of many other animals. That is one instance, I think. Then, of course, the digestive processes and the nutritive processes generally, to which you have already alluded, is another illustration.

17661. (*Dr. Gaskell.*) Would you include in the circulation the lymph system, the lymphatics?—Yes, I think so. I have not yet myself worked at the lymph system, but I certainly think I would include it.

17662. (*Chairman.*) If dogs were excluded from experimentation, what would be the result to physiology. Many people think that they should be excluded, and I wish to know what, in your opinion, the effect would be?—I do not wish to use exaggerated language, but I am bound to say that I think it would cripple physiology in this country.

17663. (*Sir Mackenzie Chalmers.*) To a great extent or to a small extent?—To a great extent.

17664. (*Chairman.*) By not giving the same opportunities to physiologists of comparing results, in the dog and in man, in particular parts of the body, do you mean?—Yes, and in preventing physiologists making useful additions to knowledge in physiology.

17665. As a matter of research?—As a matter of research, and amongst other things, as a matter of direct practical value to man. The two things go together, I mean; they cannot be separated.

17666. I understand that there are a good many experiments that could not be satisfactorily performed and with satisfactory results if physiologists were confined to such small animals as guinea-pigs, rabbits and rats?—I think that is unquestionable.

17667. As to demonstration by experiments before students, what have you to say to that?—I think that it is essential for the proper education of students that there should be demonstrations. I understand by demonstrations, of course, demonstrations on anæsthetised animals.

17668. (*Mr. Ram.*) And animals that cannot recover from the anæsthesia?—An animal that is killed before it recovers from the anæsthesia.

17669. (*Chairman.*) Have you ever known, I ask you this because suggestions have been made before us to the contrary, of animals being vivisected for purposes of demonstration (I mean cut into with the knife for purposes of demonstration before students) without anæsthetics?—No.

17670. Never?—Never.

17671. You have had long experience?—Yes, I began working in a physiological laboratory in the year 1883. I cannot trust my memory as to exactly when I gave up my licence; it was three or four years ago. Since that I have not held a licence. Roughly speaking, I have been acquainted with physiological laboratories for twenty years.

17672. What is your view as to the value of demonstrations?—My view about that is, that it is quite impossible for a student to get really a correct knowledge of any fundamental phenomenon in physiology, such, for example, as the beat of the heart, without really having seen the heart beating. I do not think that that kind of real knowledge can be acquired from reading. If you take two students, one of whom has been told the stimulation of a nerve in the neck stops the heart, and the other has seen the heart stop with the nerve being stimulated, I do not believe that those two students have got an equal knowledge. I know that I never had what one might call a true knowledge of the thing until I had seen it.

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17673. (*Sir Mackenzie Chalmers.*) That, of course, is done under anæsthetics?—Yes.

17674. (*Chairman.*) Were demonstrations of that kind common when you were learning your profession?—No, when I learnt my profession, I think I am correct in saying that there were no demonstrations to the general class, and that the only demonstrations that took place were to students who were taking the higher course of instruction, the so-called advanced course. That is my recollection. I have no recollection of having seen demonstrations in the ordinary large lecture class. I am talking of 25 years ago, of course, and more than 25 years ago.

17675. Do all students now have an opportunity of seeing demonstrations?—I believe so. I cannot speak of that of my own knowledge, because, of course, I am out of touch with the teaching of physiology now, even at my own medical school, and I do not know for certain whether all the students see demonstrations or whether it is only those who are taking advanced courses. But you have had many witnesses before you who could answer that of their own knowledge. I am not prepared to say from my own knowledge.

17676. You think it is advantageous, at any rate, that they should have the opportunity?—I think it is more than advantageous; I think it is necessary.

17677. (*Sir William Church.*) You have seen the printed evidence before this Commission?—Yes. I cannot say I have read it all.

17678. I want to ask you a question about it. You will find on page 66, first of all, Question 5843, at the bottom of the first column, that a witness, Mr. Graham, alluded to some experiments of yours in the following words: "I only propose to give just one or two cases so that my argument may not be in the air. Dr. Rose Bradford cut into the ears of dogs, destroyed the tympanic plexus, scraped out the middle ear and poured in pure carbolic." Mr. Graham, of course, had no idea what was the object of that experiment there narrated. Is he quite wrong in his description of it? I should like you to explain to the Commission what was done, and what was your object in doing it?—I daresay it is within your knowledge that I was written to by the Secretary of the Commission with reference to this matter, and I have sent in a statement already with reference to it. These experiments were experiments on the physiology of secretion from the salivary glands—experiments on nervous regulation of secretion which involved the cutting of nerves supplying certain glands, the submaxillary gland and the parotid gland. The nerve going to the sub-maxillary gland is, of course, easily divided, the nerve going to the parotid gland cannot be readily divided, and so, in order to destroy that nerve, the ear was opened, under anæsthetics, of course, in the usual way in which a surgical operation on the ear is performed, and the interior of a certain portion of the ear was rubbed with a spoon and then one drop, one minim, of pure carbolic acid was allowed to run in, and was then wiped up with a piece of cotton wool and the wound was closed. That is a description of the operation. The object of the operation was to divide the nerves, or to destroy them, it comes to the same thing, supplying this particular gland.

17679. These particular experiments were done, I suppose, under Certificate B?—Yes.

17680. Then the witness went on to say that the operation was done under chloroform, but the dog lived for weeks afterwards in what must have been a condition of great suffering. I want you to tell the Commission whether, in your opinion, those dogs did suffer greatly?—It is quite untrue, of course, to say that they lived in a state of acute suffering. There was no evidence of suffering at all, and I may say that, of course, it is a common surgical procedure to scrub wounds with pure carbolic. It is, if anything, rather painless than painful.

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17681. It was done, first of all, whilst the dog was still under anæsthetics?—Yes.

17682. What is the action of pure carbolic acid when used in that way on the immediate parts?—It has a very successful but very complete destructive action; it just destroys what it is in contact with and does not, therefore, lead to any subsequent sloughing or inflammation when it is used in a small quantity like that, and is applied just in the form of one drop locally.

17683. And it also benumbs the part?—Yes.

17684. (*Sir Mackenzie Chalmers.*) How far is it a local anæsthetic?—It is a very powerful local anæsthetic.

17685. For instance, when applied to the painful nerve of a tooth?—It is only used for that purpose.

17686. As an anæsthetic?—It is applied, of course, to destroy the nerve as an anæsthetic, but to destroy the nerve as well.

17687. (*Sir William Church.*) Therefore what I want to get at is, that in your opinion, these particular experiments did not entail much after suffering in a dog?—I should have said they entailed no more than obtains in the healing of any healthy wound.

17688. In the first volume of evidence that was published, I do not know whether you have seen certain statements made about you. You will find on page 65, at the bottom of the first column, Question 1798, a statement made by Mrs. Cook that you performed various operations upon the kidneys of dogs at the Brown Institute?—Yes.

17689. She goes on to say: "Chloroform and morphia were used for the actual operation, and the animals were then placed in a glass case, with a glazed floor, for observation. Pieces were cut out of the kidneys, and they were mutilated in different ways. In the case of one dog, the operator cut a piece out of a kidney, and then tried to graft the piece and make it grow on another part of the inside of the animal. The animal died in four days." She, of course, had no idea of the object with which these experiments were done. I did not know whether you would like to mention that to the Commission?—First of all, these experiments were not done at the Brown Institution; I do not know that that is very material; they were done before I was at the Brown Institution. There was no operation of that kind done at the Brown Institution.

At this point Sir William Church took the chair.

17690. (*Sir Mackenzie Chalmers.*) How many years ago is it?—These experiments were begun in 1889, but they went on for many years. If you want the facts, I should have to look them up. I cannot pin myself exactly to a date, but they went on for six or seven years. But these particular experiments were not done at the Brown Institution. The object of these experiments was to see whether it was possible to produce in animals, by diminishing the amount of kidney substance, a condition at all analogous to what is found in human Bright's disease—that is to say, whether it was possible to produce a slow poisoning at all akin to that seen in Bright's disease—for the purpose, I need not say, of investigating Bright's disease. A number of facts were observed in the course of these experiments, perhaps the most important being that nothing at all akin to Bright's disease was produced by the diminution of the kidney substance. I mean that no proceeding like ordinary uremia was seen as the result of these experiments. A considerable amount of information was obtained, which was of value in these observation.

17691. Could you kindly tell us in what way it was valuable, because that is important?—One of the things which I think was of value is that one got accurate information as to how much kidney was necessary for life to be maintained.

17692. That affects prognosis?—Yes, prognosis. It might also conceivably affect operative interference. Then another thing that I think of some value, only this is rather a complex subject, is that it gave one a different idea of the significance of the quantity of urine that was passed. There were certain conclusions in human medicine that were drawn from a person passing much or little urine, and I think that those experiments showed that at any rate other factors had to be taken into consideration. That is an illustration of the kind of value of the experiments. But the most important thing of all was, I think, that no such condition as that known as human uremia was produced.

17693. (*Sir William Church.*) Would you tell the Commission, if you remember at this length of time, exactly what did become of these dogs. You see, in that paragraph, Mrs. Cook accounts for nine out of the forty-nine; she does not tell us what became of the other forty?—I should be very pleased to put the facts before you. You have had a copy of the published paper, giving the facts as regards each one, of course. To summarise the thing shortly, I would say that a great number of these dogs lived, of course, for very prolonged periods, in perfect health.

17694. Without apparently suffering?—Yes.

17695. They were in the condition of a man who has had an operation on his kidney?—Yes, they were perfectly well. Some lived, for example, for two years, and they were killed, not because they were ill, but because they served no useful purpose to keep any longer. Others wasted; they passed large quantities of urine, and got thin, but they suffered no more than that. They did not suffer at all, of course, except that they were thin. Others, where the quantity removed was greater, got so thin that they became extremely weak, and no doubt ill; they vomited occasionally; and those animals were killed immediately—there was no object in keeping them, of course. Then some of them, as stated by the witness, died within a few days of the operation. I remember this one alluded to here as having lingered thirty-six days, because it was a very remarkable one. That animal lived for thirty-six days, and died suddenly. I do not know from what cause, but it is quite easy to give you the full facts as regards each individual one if they are wanted. They are fully published.

17696. So that only two died from blood poisoning as the result of wounds, and one in four days. That is what she says?—Yes, I have no doubt that is true, but I should have to look it up to verify it.

17697. And the others were much in the condition of a man who has had a successful operation performed upon his kidney after he recovers?—Yes.

17698. That is to say, it is not likely that they were suffering tortures or pain?—There is no truth in the statement that they were suffering tortures or pain. And the only ones where there was evidence of the wound going wrong in the first few days after the operation were killed, of course.

17699. I should just like to ask you one question with regard to the use of animals in diagnosis beyond what Lord Selby asked you. Is not experimentation by means of animals of the greatest use in diagnosing diseases in the interest of public health. Is it not the case that an officer of health at a port would use animals, for instance, for the diagnosis of plague or cholera?—Yes, that is so.

17700. Those, of course, are acute diseases?—Yes, and, of course, at a time in the evolution of the disease when it might be impossible to diagnose it by other means.

17701. (*Sir John McFadyean.*) Do I correctly understand that you justify and advocate experiments on animals for teaching purposes, first on the ground that such experiments can be made absolutely painless, and, secondly, that they are of great value for fixing facts on the minds of students, and giving them a better understanding of physiological processes?—Yes, only I go a little further. I say that without such demonstrations it is impossible for them to get an adequate knowledge of physiological processes.

17702. But you would not seek to justify, and you would not advocate, such experiments were you not satisfied that they can be made painless?—No.

17703. Then, with regard to the question of declaring it illegal to use dogs for experiments, I suppose that for certain experiments an animal of about the size of a dog is necessary?—Yes.

17704. That is to say, there are certain experiments which are frequently done, and you hold that it is advantageous to do, which could not be done on a guinea-pig or a rabbit?—That is so.

17705. Other animals of about the same size might be the pig, or the sheep, or the goat, but do you think that a sheep or goat, or even a pig, could well be substituted for a dog in all the experiments for which a dog is now generally employed?—I think that is quite impracticable.

17706. For example, would it be possible to throw much light on the physiological processes of digestion and assimilation in men by experimenting on ruminants?—No.

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17707. At any rate, you do not think that information of value would be as likely to be obtained in that way as by using dogs?—Certainly not.

17708. And the same would apply also to the pig?—Certainly.

17709. (*Sir Mackenzie Chalmers.*) I only want to ask one or two questions from a purely lay point of view. You told us that it was necessary sometimes to have a diagnostic experiment in case of suspected tuberculous disease of the kidney?—Yes, and the urinary tract, yes.

17710. That is important for this reason, that according to the result the treatment would differ?—Yes.

17711. That is to say that if your diagnosis was wrong you would pursue a different treatment?—The treatment would be perfectly different, because the classes of affection that are likely to be confounded with tuberculosis of the renal tract are diseases associated with gravel, a totally different class of affection.

17712. Therefore the consequences to the patient of a wrong diagnosis might be very serious?—Yes.

17713. Would the same apply to what you told us about the tuberculous disease of the apex of the lung?—You would adopt a different form of treatment in the two conditions.

17714. Now as regards diphtheria, is it ever essential there to perform experiments on animals for diagnostic purposes?—No, I do not think it is, because the organism can be recognised by other methods, by cultural methods.

17715. But you have to diagnose the organism in diphtheria?—Certainly.

17716. You get the false membrane equally in laryngitis as in diphtheria?—Yes, there are other causes of false membrane besides diphtheria.

17717. Would the treatment differ according to whether you find the specific bacillus?—Yes. It is well known, of course, that in diphtheria it is of the utmost urgency to recognise it at the earliest possible moment; the life of the patient may depend upon it.

17718. As a practical physician I suppose you have seen much diphtheria?—Yes.

17719. Do you use the antitoxin?—Invariably.

17720. Have you read the evidence we have had about antitoxin here, that its efficacy mainly depends upon its early administration?—Yes, that is so, it very largely depends upon that.

17721. Have you any doubt yourself as a practical physician, that the antitoxin does save life?—I have not the smallest doubt.

17722. And not only saves life, but does it also cut short the disease?—I have no doubt of it whatever, having seen diphtheria before the antitoxin treatment, of course.

17723. Taking your opinion generally, you think that medicine owes a great deal in the past to animal experimentation, and that it must in the future, if it is to continue to make progress, depend on animal experimentation?—I have no doubt about it.

17724. As regards those experiments of yours on the ear, the whole experiment was performed under anæsthetics?—Yes

17725. Would the wound be closed in any way before the anæsthesia disappeared?—The wound would be completely closed and the dressing put on.

17726. (*Mr. Ram.*) An aseptic dressing?—Yes.

17727. (*Sir Mackenzie Chalmers.*) The ear is very sensitive, is it not?—Yes.

17728. Would there not be a danger of subsequent pain from this operation?—If the wound went wrong, if it became infected, but not otherwise.

17729. Not as long as it was aseptic?—No.

17730. Were not very sensitive nerves involved in the operation, and would not the ends of those nerves give rise to pain?—I do not think so. The nerves in the first place were destroyed by the carbolic acid, and the operation, I should like to point out, is not precisely similar, because the structure of a dog's ear is a little different from that of the human ear; but putting that aside the operation is practically identical with the very common operation in human surgery of opening that portion of the back of the ear.

17731. For the purpose of getting at an abscess in the ear?—Yes.

17732. (*Mr. Ram.*) Is that followed by pain in the human subject?—Not so far as I know. There may be pain where the thing is due to inflammation, for which the operation is done.

17733. (*Sir Mackenzie Chalmers.*) The actual operation does not give rise to great after pain?—Certainly not

17734. In the human subject at any rate?—No.

17735. Therefore there is no reason to suppose that it does in the dog?—I have no hesitation in answering that question, because I know that these dogs ran about just like normal healthy dogs, and they showed no signs of pain. Every physiologist would tell you that if he performs an operation, and the wound becomes infected the thing is to a great extent spoilt for his purpose.

17736. Then the dog is destroyed?—Then the dog is destroyed.

17737. I just want to clear up this further point. In the case of kidney operations you extirpated a portion of the kidney in certain dogs?—Yes.

17738. That was done, of course, under anæsthetics?—Yes.

17739. The wound was healed up and dressed under anæsthetics?—Yes.

17740. Was it ever necessary again to open that wound, or not?—I rather fancy that I sometimes had to take out a stitch; there may have been a stitch abscess. I never had to open the deeper parts.

17741. In fact your object was that the wound should heal?—Certainly, but some of these wounds become infected, as was stated.

17742. Then you killed the dog?—Yes.

17743. After a time?—I am speaking from memory; I have not looked up these paricular series of experiments, but my impression is that they were killed at once.

17744. What I wanted rather to get with reference to some other evidence that we have had was this: The wound was not reopened for the purpose of inspecting it, or anything of the kind?—No. When you asked me the question whether I ever reopened a wound my impression is that I did sometimes take out a stitch.

17745. It has been suggested to us that under Certificate B the operation is performed under anæsthetics, but that subsequently exploratory operations are performed without any anæsthetics—is that so?—That is not so.

17746. You never heard of such a case in your long experience?—No.

17747. You have read the evidence given to us as to various tropical investigations?—Yes.

17748. We have had evidence as to sleeping sickness, yellow fever, Malta fever, and plague?—Yes.

17749. Have you anything to add to the evidence that we have already had?—I do not think so. Sleeping sickness and plague are the only diseases of the kind that I have been directly concerned with, because I have been on the Advisory Committee controlling both.

17750. You were a member of the same Committee as Dr. Charles Martin, who gave us evidence?—Yes, the Committee at the India Office.

17751. And you agree with his evidence?—Yes.

17752. (*Mr. Ram.*) With regard to these experiments on dogs, when you extracted part of a dog's kidneys, as I understand, it was, of course, necessary that the dogs should survive after the anæsthesia passed away in order that you might see the effect?—Yes.

17753. In the majority of cases I understand that the dogs throve and showed no signs of illness?—In a large number of cases.

17754. In other cases the dogs, I suppose, were ill in consequence of the effect of the abstraction of more or less of the kidneys?—Yes, they wasted. Wasting was the prominent symptom of illness.

17755. In respect of some of those dogs, I suppose, there was pain, or at all events discomfort, in consequence of the illness?—I should say that there was discomfort. I should not say that there was pain; but there was in those animals in which there was the smallest amount of kidney left a very considerable physical weakness and inability to take food, and therefore I have no doubt that there was discomfort. There was never any evidence of local pain, and there was no reason for supposing that there should be.

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17756. Were the dogs in a condition in which they were lying about in discomfort moaning?—No.

17757. It did not amount to that?—No, I never heard one of these dogs moan.

17758. Or give any actual signs of acute suffering?—No.

17759. There was an expression used by Mrs. Cook, I think, which was put to you just now—I do not know whether you adopted it—that one dog lingered for thirty-six days, and died suddenly?—Yes, my impression is that it died suddenly, but I will look that point up if you wish it.

17760. Only to this extent, the phrase she used, I do not know whether it was adopted from you or invented, that it lingered for thirty-six days, gives an idea of great discomfort?—I do not adhere to the word lingering. I do not think I used it. I should prefer to look it up, and send you an account of it.

17761. It was put to us as a case in which it was proved that the dog suffered pain for a considerable time—thirty-six days. Anything that you could give us as to that particular case I should be glad of?—I have no hesitation in answering at once that the dog did not suffer pain. I do not know in what particular form of words I described its life for thirty-six days.

17762. One other matter, please. In your statement you speak about the advantages obtained by inoculation to enable you to form a right diagnosis with regard to, for instance, tuberculosis. Does it come to this, that the instances so far as you have found them yourself tend to correct the diagnosis, to prove whether the diagnosis is right or wrong?—Yes.

17763. It tends, therefore, I suppose to indicate the proper treatment?—Yes.

17764. And may be the means of saving life if the wrong treatment were negatived, and the right treatment indicated?—Yes, that is so.

17765. Is the good which has been obtained by the discovery of these antitoxins, and so forth, confined, do you think, to correct diagnosis, or is it in any cases used in a curative way for the patients themselves?—It is certainly so used in a curative way in the case of diphtheria, for example.

17766. We have heard a great deal of Sir Almroth Wright's experiments lately, and you have been speaking of such instances as acute forms of pleurisy, lung diseases, and tuberculosis of the urinary tract. Tuberculosis of the urinary tract, on which some witnesses have given us evidence, is a matter, I suppose, that can only be treated through the system?—It can be treated surgically, of course.

17767. But apart from surgically, it must be treated through the system?—Yes.

17768. So far as you know, is the serum used by Sir Almroth Wright used curatively?—On his patients, I believe so.

17769. Have you known any cases in which, for instance, in tuberculosis of the urinary tract, this serum has been used?—I have no personal knowledge.

17770. Do the books disclose any cases?—There have been cases recorded in literature where it has been successfully employed.

17771. But you have seen cases in books?—Not in text books.

17772. I use the legal phrase, in books?—I thought you meant text books.

17773. No, I mean in literature. You have seen instances there recorded, in which it has been useful as effecting a complete and permanent cure of such a matter as tuberculosis of the urinary tract?—Yes.

17774. (*Dr. Gaskell.*) You have had experience of experimentation upon various animals, have you not—dogs and others?—Yes.

17775. Have you ever found the slightest difficulty in anæsthetising dogs?—No, none.

17776. You would not put them in a different category from other animals in that respect?—No, I should not put them in a different category. I should have said that I have had personally more facilities in anæsthetising dogs, and that I have found them very susceptible to certain anæsthetics, chloroform particularly.

17777. (*Mr. Ram.*) Meaning, that they are more liable to die under the anæsthetics?—Yes.

17778. (*Dr. Gaskell.*) But when you have succeeded in keeping them alive, you can keep them well under anæsthetics?—Yes, you can get the most perfect anæsthesia in dogs.

17779. You have had a licence and various certificates for some time?—Yes, I have held, I think, all the certificates. I never held a demonstration certificate. I think I have held all the others.

17780. Do you consider that the present Act puts trouble in the way?—I think there is a little trouble. Personally, I cannot say that I have had any serious amount of trouble. I have never been delayed in the course of any experimental inquiry that I have been engaged in, but I think I have known of instances where delay has occurred.

17781. Do you think it would be better if the present Act were abolished altogether?—No, I do not think that. I think that experiments on animals should be regulated by an Act.

17782. Have you any suggestions to make to this Commission as to any alterations in the Act that would be beneficial, or have you not thought about that?—I have not thought about it of recent years, I must confess; but I think I can answer that question, at any rate, to a certain extent. I think that certain individuals, at any rate, should perhaps have more general powers given them; that instead of having to apply for certificates for different categories of animals, they might be all covered by one certificate. Some years ago, when I was conducting some observations on a disease allied to sleeping sickness (nagana) in animals, I had to do those experiments on a very large number of different kinds of animals—horses, cattle, and a number of different animals—and I had some little inconvenience in getting those certificates; but it never amounted to anything very serious.

17783. (*Sir Mackenzie Chalmers.*) Were those experiments in nagana done in England?—Yes, they were done at the Brown Institution.

17784. (*Dr. Gaskell.*) That inconvenience, I presume, also affected the people who had to sign the certificates?—Yes.

17785. It was a bit of a worry for them, was it not?—Yes. I think there is a grave misconception in the mind of the public with reference to the wording of some of these certificates—Certificate B especially. There is one certificate which on the face of it rather implies that you are doing the experiment without anæsthetics—it is Certificate A—and in many cases you are really using an anæsthetic.

17786. (*Sir Mackenzie Chalmers.*) Certificate A only authorises feeding experiments or experiments which do not involve more operative procedure than the prick of a hypodermic needle?—I think I am under a misapprehension; I think the certificate was altered a few years ago; but there was a time when I held the certificate, and the particular certificate was the certificate dispensing with anæsthetics.

17787. That refers to the old E before EE?—Yes; there was one certificate which you had to apply for, which dispensed with anæsthetics, and you had to apply for that certificate when you were going to do an experiment on a particular animal under anæsthetics, and therefore the public who could see an intimation that So-and-so held a particular certificate, acquired the idea that they were doing experiments without anæsthetics, when really they were not.

17788. We had that in the evidence of Sir Victor Horsley. It was the old Certificate E, afterwards abolished by Mr. Asquith, and EE substituted for it?—Yes. I think there might be some modification with regard to the certificates.

17789. (*Dr. Gaskell.*) I may take it then that you would like to have some modification with regard to certificates?—Yes.

17790. And larger powers given to certain individuals?—Yes.

17791. Would you specify those individuals?—No, I would not specify them. I think that ought to be determined by the people who sign the certificates; they might act as referees with reference to that. They might be defined perhaps as teachers and professors of physiology in universities and teachers in medical schools.

17792. It has been suggested to us that they might be defined as heads of laboratories?—I think that would be quite a suitable thing.

17793. (*Sir William Church*). Would you be in favour of doing away with all certificates?—No, I do not think I should. I think there might still be certificates for certain animals.

17794. But you would be in favour of doing away with all certificates for persons who were in the position of head of a laboratory?—Yes, I think so.

17795. But for men who held less responsible and less well-known positions, you would still require Certificates A, B, C, and EE?—I think the certificates might be simplified.

17796. (*Dr. Gaskell.*) Have you read Sir Victor Horsley's evidence on that point?—No. It is not a thing that I have given much thought to, but I certainly quite agree that heads of laboratories ought to have further facilities.

17797. (*Sir Mackenzie Chalmers.*) You told us that you thought the Act ought to be kept up?—An Act.

17798. Is that for the reason that having an Act tends to keep up a high standard of humanity, or for the reason that it tends to allay public apprehension?—That it tends to allay public apprehension. I do not think there is any necessity to set up a standard of humanity for the gentlemen engaged in work of this kind, because their object is to relieve humanity.

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17799. Would there not be danger without the Act, of experiments being performed by quite a different class of people, who might not have the same qualifications or the same standard of conduct?—I imagine that those individuals come under the Prevention of Cruelty to Animals Act—Martin's Act.

17800. They would now; but if you abolished this Act, would there not be a danger of unscientific people in the name of science performing reckless experiments?—I am not in favour of doing away with an Act.

THIRTY-NINTH DAY.

Wednesday, 27th November, 1907.

PRESENT:

The Right Hon. The Viscount SELBY (*Chairman*).
Sir W. S. CHURCH, Bart., K.C.B., M.D.
Sir W. J. COLLINS, M.P., M.D., F.R.C.S.
Sir M. D. CHALMERS, K.C.B., C.S.I.
Mr. A. J. RAM. K.C.
Mr. W. H. GASKELL, M.D., F.R.S.
Mr. J. TOMKINSON, M.P.

Captain C. BIGHAM, C.M.G. (*Secretary*).

Mr. HENRY HEAD, M.A., M.D., F.R.S., F.R.C.P., called in; and Examined.

Mr. H. Head, M.A., M.D., F.R.S., F.R.C.P.

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17801. (*Chairman.*) You are a Doctor of Medicine, a Fellow of the Royal Society, and a Fellow of the Royal College of Physicians, I believe?—Yes.

17802 And Physician to the London Hospital?—Yes.

17803. You desire to offer some evidence to us, I believe, with reference more particularly to the relation of diseases of the nervous system to experiments on animals?—Yes.

17804. First, I think, you desire to point out how far experiments on animals have led to the elucidation of difficulties in the structure and functions of the nervous system?—If I may, I want to point out that all rational diagnosis and treatment must be based upon knowledge of the structure and functions of the nervous system. Sixty years ago little was known of its structure, and still less of its functions. Charles Bell and Majendie had proved that the anterior roots were motor and the posterior roots sensory in function. Marshall Hall was describing reflex action in the spinal cord. It was then an entirely new idea. A few diseased states were known to be accompanied by gross changes, but none of the paths by which impulses pass to and from the brain were then known. I thought it might interest you to hear what Sir Thomas Watson, in his text book (Lectures), said in 1845. He explains the peculiar difficulties to which the physician was exposed when dealing with nervous disease by saying, "One source of difficulty lies in the circumstance that the structure of the nervous system has no perceptible or understood subservience to its functions. We do not discover in the mechanism of this system that adaptation of means to an end which is so conspicuous in many other parts of the body; and consequently, though such adaptation doubtless exists, we are not able to trace the reason or the manner of its interruption"; and again, "It is a further source of difficulty that physiologists have not yet been able to determine with anything like precision or certainty what share the several parts of the brain and spinal cord have in regulating, respectively, the functions which all physiologists acknowledge to belong to the nervous system in the aggregate. There are many and convincing reasons for believing that the brain is a complex organ; but we can seldom put our finger upon this or that portion of the nervous matter which composes it, and say here resides the influence that governs this or that particular function." That was 60 years ago.

17805. Sir Thomas Watson was then, I suppose, a gentleman who might be relied on to give the up-to-date knowledge of his time?—I think so. Sir Thomas Watson was particularly interested, I believe, in matters of this kind. When the part of the book on the nervous system is read, it is very difficult to say how it reads, the ignorance is so profound. When you read the rest of the book on the other systems much of the knowledge given could be transferred to an ordinary text-book.

17806. We may take that as showing the high-water-mark at that time, then, of the specialist's knowledge on this subject?—Yes, I think so; because he even alludes to the very recent knowledge of reflex action; he mentions Marshall Hall, and he puts in the recent knowledge that was only published shortly before; so that I think we may take Sir Thomas Watson as fairly representative of the professors of medicine of that day, and of the knowledge that was given to students at that time. The whole of the knowledge which we now possess of the structure and functions of the nervous system is due to the close co-ordination of the results of animal experiment and the consequences of localised disease. The anatomist (this is the point I want to make first about anatomy) of the dissecting-room can help us very little. He cannot even say whether the biggest tracts visible to the naked eye conduct upwards or downwards. The beginning of our present knowledge came with Waller's discovery that a nerve degenerated when separated from its nutritive centre. This law was the direct outcome of experiments on animals, and its application to the brain and spinal cord has been responsible for the greater part of that knowledge we now possess of the structure of the nervous system.

17807. When you speak of a nerve degenerating do you mean losing all its functions?—Not only losing all its functions, but undergoing retrograde destruction. When a nerve is separated from its nutritive centre it becomes gradually functionless, but also it becomes gradually destroyed entirely, so that no nervous matter is left behind. Let me explain by a simile how the anatomy of the nervous system depends upon experiment. I am not talking of the functions at all; I am talking simply of the anatomical structure. Let me just take a simile. Imagine a wall covered with creepers arising from several stems. If we wished to know from which of these any one branch takes its origin, we could cut one stem and every leaf arising from it would die, marking out among the healthy foliage the offshoots of the divided stem. This is the principle that has been used in tracing the paths in the nervous system. Gowers, by applying this method, discovered the ascending tracts in the lateral column of the spinal cord.

17808. When you say, by this method, do you mean by experimenting in that way upon living animals?—By applying this method to localised lesions in the spinal cord.

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17809. (*Sir Mackenzie Chalmers.*) Accidental lesions or lesions caused by disease?—Yes.

17810. (*Sir William Collins.*) Did Gowers himself employ both methods?—No, I am just taking Gowers' as an application of this method to an accidental one first.

17811. But did Gowers employ both methods?—Gowers, by this method, discovered the ascending tracks in the lateral column of the spinal cord which had degenerated in consequence of injury. This is an instance of the fruitful application of a law discovered by experimentation on animals to a case of an accidental lesion in man, because, but for Waller's law Gowers could never have used the injury in man to discover ascending tracts in the lateral column. But there was a great deal of dispute about them until Mott and others settled their anatomical connections once for all by animal experiment. Gowers' own observation was not able to give us the complete distribution of these tracts; that only arose where the experiment could be repeated several times under set conditions in animals. From that time the preliminary observations made by Gowers have been verified by several others, and made perfectly certain now by animal experimentation, and we now know the greater part of the distribution of this path. The lesions of disease are usually too diffuse, and the opportunities for *post-mortem* examinations too uncertain, to give clear answers to a direct question. By experiments on animals a tract or set of tracts can be divided precisely; the animal is kept alive until degeneration has taken place, and is then killed. By suitable means the dead parts can be coloured so as to stand out clearly in the microscopical picture. The method by which these dead structures are made to show up clearly against the healthy parts was discovered by Marchi from experiments on animals. Waller's law and Marchi's method applied to material obtained from experiments on animals and from disease or injury in man, are responsible for almost all our knowledge of the anatomical paths in the nervous system. But for experiments on animals we should not have had, so far as we can tell, Waller's law; and the method of colouring the diseased parts (which had been the greatest difficulty in applying Waller's law to the nervous system) was discovered entirely by experiments on animals by Marchi.

17812. (*Mr. Ram.*) The knowledge of one was made available by the method of the other?—Yes. If I may just be allowed to say so, our present knowledge of the anatomy of the nervous system depends to an enormous extent on methods discovered from experiments on animals, and in the utilisation of these methods upon material derived from the results of disease or accident. When we turn to the knowledge of the functions of the nervous system still more do we require controlled experiments to correct and amplify the uncontrolled results of disease. For a knowledge of ingoing sensory paths we are obliged to fall back mainly upon disease, because man alone can tell us of his sensations, but disease produces irregular and diffuse destruction, and the most instructive conditions rarely come to post-mortem examination. Even here, when observation of disease on man starts with a preponderating advantage, in that man can tell us of sensations, experiments on animals are necessary to supplement the rare opportunities offered by disease. On the motor side experiments on animals have the overwhelming advantage that the destructive lesion or the stimulus can be localised accurately, and observations can be made, under the direct conditions of experiment, time and place being convenient. Hughlings Jackson discovered by long and laborious observation that when certain parts of the surface of the brain were the subject of disease, certain movements were produced in the limbs. But these results remained a brilliant hypothesis until Hitzig in Germany, and Ferrier in England, showed by simple direct experiment that stimulation of the surface of the brain in animals produced movements of the limbs such as Jackson had described. In spite of a generation of clinical observation since that time, many important details of cortical localisation still remained undecided until Sherrington and Grünbaum's experiments on the anthropoid apes; that is to say, that although Dr. Jackson, by the most brilliant intuition and most laborious observation, said that stimulation of certain parts of the brain marked by disease were associated with these convulsive movements, few, if anybody, believed him, because everybody said that it is impossible to stimulate the brain. They held exactly the idea that is given in Watson—that the brain was a sort of independent structure, in which no function was placed in one part or in another—that the brain was the seat of a sort of generalised function. But it might be asked: Why not experiment on men who offer themselves voluntarily? I hold very strongly that no man has the right to be the subject of an experiment unless the whole conditions surrounding that experiment are previously known as well as they can possibly be at the time. In my own case the most careful observations were made upon patients suffering from accidental injuries to nerves. Then the experiment was designed so that the question which could not be answered by these accidental lesions was put directly, and the answer given clearly.

17813. (*Sir Mackenzie Chalmers.*) In the case of an animal?—In the case of myself. No man has the right—I want to insist upon that—to experiment upon himself unless the whole of the conditions of the experiment are known beforehand, so far as it is possible. That is to say, that vicarious experiments are useless, and that an experiment on man, if it occurs—self-experimentation—must be the culminating point in a complete series of thoroughly thought-out data, which can only be obtained by a long course of observations of disease, together with animal experiment. Every now and then, for the relief of pain, man is operated upon under conditions that closely represent those of animal experiment. I refer particularly to such operations as the division of the posterior roots for pain and the operation of the removal of the Gasserian Ganglion at the base of the skull for persistent neuralgia. From such instances, when they occur, we can obtain the most valuable information, but such cases can only occur with excessive rarity, and, as in the case of the Gasserian Ganglion, it is always the same structure which is removed.

17814. (*Chairman.*) When you say such cases, do you mean cases in which a surgeon would venture on such an operation?—Yes; I obtained most valuable information from two of Sir Victor Horsley's patients, in whom he had divided the posterior roots in the back for persistent neuralgic pain; but such cases occur with excessive rarity.

17815. When you say information, do you mean that you questioned the patients about their sensation?—I examined them as if they were animals only, with a human consciousness. People may say that there occur in human beings opportunities in consequence of surgical operations which are equally good with those of animals, and give you the advantage of having a man to deal with. But those are excessively rare, and not only that, but when they are not excessively rare, as in this modern operation of removing the ganglion at the base of the brain for this persistent neuralgia, it is always the same structure that is removed. I do not want at the present stage to see any more patients in whom the Gasserian Ganglion has been removed.

17815A. Will you explain?—At the base of the brain the whole sensory nerves of the face and head come together, and form a sort of knot. This knot or junction point is the part from which all the ingoing lines pass into the brain, so that they carry the whole of the sensations from that half of the head into the brain. Now, for extremely severe neuralgia this knot is cut, and the junction is removed, so that the pain impulses no longer pass into the brain.

17816. Do you mean pain impulses coming from the rest of the body?—No, from the face and head; it is facial neuralgia. They no longer pass inwards, and the whole of that part of the face is made entirely devoid of sensation.

* 17817. Is that an operation that is successfully performed now?—It is a brilliant operation; it is brilliantly successful.

17818. Is it successful in its after results?—Yes, brilliantly successful, if it is done with care by the people who can do it.

17819. (*Sir Mackenzie Chalmers.*) Is it a dangerous operation?—Yes, but the mortality is going down enormously; now that people are getting skilled the mortality goes down every year.

17820. Does the destroying those sensory nerves produce any other ill-effects afterwards? Have you

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watched cases for a number of years?—Yes. One particular case that I have under observation now was done about 1897—ten years ago.

17821. (*Chairman.*) Does it affect the vitality of the patients at all generally?—No, not generally. It enormously improves it, because they have been the subject of this terrible pain over long years, and the relief of that pain often turns them into entirely different beings from the patients you have seen before.

17822. Does it alter the appearance of the person's face?—Yes, because you must divide the motor nerve for certain parts of the face, too; you cannot get the one away without the other.

17823. Does it take away the power of movement?—Part of it, not altogether.

17824. (*Mr. Ram.*) Is the whole of that side of the face affected permanently?—Yes, permanently.

17825. Is it dead to all sensation?—Very nearly.

17826. Does it feel a touch or a prick?—To a varying extent. There are parts which are entirely insensitive to everything; and pain, of course, disappears with it.

17827. (*Sir William Collins.*) Is the nutrition of the parts supplied by the divided nerve affected?—Somewhat.

17828. Detrimentally?—Not with care; it does not heal so easily quite on that side if there is an injury, but not otherwise.

17829. Is the eye affected?—That depends entirely upon whether you have taken care or not. If you have taken care, in the present day, the eye is not affected, for you can be careful to shield it.

17830. No ulceration of the cornea follows?—Not necessarily. In the modern operations the eye escapes altogether. In the old cases the eye used to suffer a good deal.

17831. (*Sir Mackenzie Chalmers.*) Was that from accidental division of nerves affecting the eye?—The eye loses its sensation.

17832. (*Sir William Collins.*) You spoke of the mortality having fallen enormously. Was the mortality originally enormous?—Yes, it was high at first.

17833. About what was it?—I would not like to say exactly what the first mortality was. I should have to look it up, but it is going down rapidly.

17834. (*Chairman.*) You only performed this operation in cases where the pain was so severe that it made life intolerable?—Absolutely. That is the question you always ask; Is life intolerable?—That was not quite the point that I wanted to make. The point I wanted to make was that these occasions arise very rarely, and although you might say—Ah, yes, but you have got, let us say, statistics of hundreds of Gasserian Ganglion operations to deal with, yet it is always the Gasserian Ganglion.

17835. (*Mr. Ram.*) You want to learn about other nerves?—We want to learn about others; you cannot go on for ever learning one particular thing. I have not the slightest doubt that years hence, when our knowledge advances, we may go back to this and learn more. What I wanted to prove was that animal experiment has been necessary, not only for the study of the function, which is more easily, perhaps, understood, but also has been absolutely essential in the progress that we have made towards our knowledge of the anatomy of the nervous system. And, then, it is not animal experiment only, but it is animal experiment, closely interwoven with other forms of experiment; that is to say, I want to show how closely related animal experiment and the utilisation of material obtained from disease or injury are to one another; that but for animal experiment we could not have utilised in the way we have done material obtained from disease. And I may perhaps be allowed to pass on to tell the story of a small research, which happens to illustrate this close inter-relation. The problem of this particular research was given by disease, and it was—Why does pain caused by irritation of some internal organ radiate on to the surface of the body or into the limbs, and why do these parts become tender? It had long been known that when an internal organ (let us take the heart) was affected, the pains were not always situated over the heart; they seem to go into other parts of the body. Now, in consequence of the work of Dr. Gaskell, some of us suspected that this was a definite phenomenon, and was the way in which the internal organs expressed themselves when they were diseased. First of all, Dr. James Mackenzie and myself independently attacked this problem, and we examined for a number of years the actual facts as they occurred.

17836. (*Chairman.*) In patients?—In patients in the hospital. We selected instances in which one organ, if possible, was affected. Take, for instance, such a disease as stone in the kidney, where we watched the pain produced by the stone, and then we saw the stone removed, and saw the pain disappear, and by a long and laborious series of observations, extending over a number of years, we determined that when an internal organ was affected in this sort of way it expressed itself by pain radiating round the body, associated with areas of tenderness on the skin. That is a good instance of the phenomena of referred pain. But, of course, I need scarcely say that if you attempted to describe phenomena like that people might say—Oh, yes, we know that it happens, but it is all hung in the air; it has no relation to anything whatever that we know. So little had it any relation to anything that was known that most people doubted that these areas occurred at all. But throughout the whole research we were much comforted and supported by Sherrington's experiments on the distribution of the posterior roots. Sherrington found by a most ingenious method of experimenting, which I do not think I need explain, that supposing you worked out the parts supplied by one posterior root you found that they spread over the body and limbs, and were distributed in a sort of zone, like a belt round the body; they went round like a half-belt; and it was the fact that I was in touch throughout the whole of my observations with Sherrington, who was at that time working out these zones which were previously entirely unknown in animals, that made me perfectly certain that my observations on referred pain were correct, and enabled me to propound a set of figures, what you may call a map of the body. This I should never have dared to put forward had I not known that in the nervous system there lay the possibility of a series of zones of this kind; that is to say, it was knowing by experiments on animals that the posterior roots followed a course somewhat of this kind, that made me much more confident that what I found in disease was really the fact.

17837. Were the experiments on animals for the purpose of verifying an idea which had presented itself, apart from experiment?—No, Sherrington was working entirely independently. He went purely for knowledge. What is the distribution of the posterior roots is what he asked himself; that is, how are they distributed, and he worked that out by animal experiment. That was undertaken entirely apart from any thought of disease or anything to do with it, only it so happened that we were both influenced by Dr. Gaskell's ideas, and so he took the experimental side of the work, quite independently, and I wanted to see how far this idea could be borne out by the examination of cases of disease of patients.

17838. By observation?—By observation. But what I wanted to impress upon the Commission was that taking this small piece of work I would never have dared to put forward a map of the body worked out in this way from observation only, had I not known that the structure and functions of the nervous system contained something which was similar.

17839. For that purpose how long did you carry on experiments on animals?—I did not do any then at all. I was observing human beings.

17840. How long did Sherrington carry on experiments on animals?—I think some four or five years.

17841. And in that time he operated on a great number, I suppose?—A considerable number.

17842. 200 or 300?—I could not tell you without looking it up.

17843. What kind of animals did he use?—Monkeys and cats.

17844. Those were the most suitable for that purpose?—The monkeys particularly.

17845. Then he would use a good many, I suppose?—I do not know exactly how many.

17846. Would it be necessary in that case to get a certificate for letting the animal come to?—Yes, because he divided the roots; he cut the lines below and cut the lines above, leaving the one in between untouched; then the animal had to come to, and the sensory ideas were marked out later on when the animal was quiet.

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17847. When the animal was under anæsthetics the only operation was a cutting one?—Yes.

17848. Was the wound then allowed to heal?—Yes, the wound was allowed to heal in many cases.

17849. Then after the operation had been performed there would not be artificial irritation of the nerves of the body?—No, there was none. The only way in which it was done was by stroking or pricking the skin.

17850. To see whether there was sensation—not pain?—No, not pain at all. Sherrington would take a pencil, for instance, and rub it along, and then when the animal moved he knew that the movements must have been supplied by the other root that he had left there which was sensitive.

17851. (*Mr. Ram.*) Would the animal be in any state of suffering on account of the operation?—Not the least, except for possible post-operation discomfort.

17852. The healing of the wound?—Yes. The whole point in the operation was that the animal should be as normal as possible, otherwise it was no good.

17853. (*Mr. Tomkinson.*) Then after the wound was healed, would that monkey be operated on again for further experiment, as a rule?—Not necessarily; there was no necessity.

17854. But it might be; it had not to be destroyed?—That depended upon what the nature of the experiment was.

17855. In certain experiments was the whole object to see how the healing took place?—No.

17856. How the animal recovered from it?—No, it was allowed to recover that its sensations might be tested.

17857. Tested for what?—Tested for touch. You see, Sherrington had taken away sensation above and taken away sensation below, and left a little oasis of sensation in between in a desert of loss of sensation. You passed through the desert of loss of sensation, and the animal made no response; you came into the little oasis of sensation, and it responded at once; put its hand out, or whatever it was, and then you knew that it must be supplied by the only root he had left uncut.

17858. (*Sir Mackenzie Chalmers.*) That, of course, was a painless proceeding?—Yes.

17859. Merely tactile?—Yes, or no more than the prick of a pin.

17860. (*Chairman.*) As regards further operations, I suppose that for physiological purposes the animal might be fit for other operations on different parts of the body?—Yes, just as a man who has had his Gasserian Ganglion cut out of his face.

17861. But whether that can be done is rather a question of law than for you?—Yes, that is what I meant.

17862. (*Mr. Tomkinson.*) Would it be a perfectly healthy animal after the operation?—It would be as healthy as a man who had had the nerves of his face divided.

17863. Nature would not restore the lost part, would it?—No; arrangements were made that it should not.

17864. Do I correctly understand that there was a certain severing and taking away of the nerves above and below?—Yes.

17865. Those nerves would not come together again?—Arrangements were made that they should not, just as in human neurectomy.

17866. That leads up to a question I wanted to ask. You know, no doubt, that there is sometimes performed what, I think, is a very cruel operation on a horse, unnerving a horse to make a horse which is lame in the feet insensible in the feet altogether, to practically give it dead feet?—Yes.

17867. I understand that a piece of nerve is taken out?—Yes.

17868. And for a time the horse's feet are absolutely insensible; it will go on hard ground that it could not go on before?—Yes.

17869. But in course of time that nerve grows together again?—Yes, because it is not done in such a way as to prevent the nerve coming together again. But it can easily be done in such a way as to prevent the nerve coming together.

17870. (*Chairman.*) Whether or not that is a cruel operation, so far as regards the operation itself on the horse, could it be done under anæsthetics if the veterinary surgeon performing it thought right?—Yes, perfectly well. And not only that, but if it was done so that the nerve did not come together, it would not be cruel in any sense of the word.

17871. It would be a painful operation, I suppose, if the horse was not under anæsthetics?—It is difficult to say how painful. I should not think it would be excessively painful.

17872. And not lasting?—If you did it extremely quickly it would be a momentary violent stab of pain and then the thing would be over; but the only pain that can come in is when the nerve regenerates, not when the nerve is divided. If the nerve remains divided, no pain can come in. It is when the nerve regenerates that the pain comes in.

17873. I introduced that in the course of what you were saying, because I was anxious to bring what you said to bear on the subject of our inquiry, namely, the law relating to operations on animals. Will you now proceed?—I wanted, in this account of the small research in which I was personally concerned, to bring forward how closely all these methods are united together, how they are all woven up together. The next stage was to find out what happened if these parts that Sherrington had been working upon were irritated in man. That, of course, could not be done by an ordinary experiment obviously; but it so happens that there is a disease which is known to everybody, shingles, which produces the most violent irritation of this particular part. That is well known now ever since 1860.

17874. Which particular part?—This posterior root, the ganglion. But by working on the shingles, by working on the material, the dead-house material, microscopically, and using the method for colouring that had been discovered on animals, we were able to show that the irritation of the ganglion was able to produce these zones on the body closely resembling, but differing on the other hand to a certain extent from, those obtained by Sherrington. That last step in the research was given by the working up of dead-house material, of the material on dead human beings, by means of a method which was discovered by experiments on animals. At the end of this stage we at once saw how little we knew about the distribution of the nerves on to the surface of the body. Generations of anatomists had dissected the nerves of the human body, and it was thought that we knew all that was to be known about their distribution. Yet if a workman cut his wrist and came to the hospital with one nerve divided, the actual anatomical facts could not explain what we saw in any single case. Anyone who was honest when a workman came to the hospital, any physician or surgeon who saw him and was honest with himself, had to say, our knowledge gained from dissection tells us nothing with regard to the sensory condition of that man's hand. But there did not seem to be any way of working it out at first, and we were so impressed with the absence of knowledge on these points, that we set to work first of all to observe what actually happened when by accident various nerves were divided. We began to suspect that there were certain laws underlying the whole thing which had never been formulated. But good as hospital patients are (and they are extraordinarily good from the point of view of many observations) it was obvious that no working man could give the time required or the attention for a long and elaborate series of observations. Therefore after everything had been thought out in the most careful manner, and after we had made several hundreds of observations on ordinary hospital patients, I determined to have three nerves divided in my own arm. This was done with the most successful results; the question that we put by our operation was answered immediately with a clearness that we could not obtain by any other means, and for five years we have been working out the results of that experiment.

17875. Do you mean that you ascertained exactly what parts of the hand lost their use or their sensation in consequence of the severing of those nerves?—We knew before from our observations what parts would do that; from previous observations we knew what parts would probably lose their sensation. But Nature, or rather accident, cuts the wrist or the arm, say, with a circular saw, and you cannot tell a circular saw that you only want sensory nerves cut—it cuts everything it comes across. Therefore the man loses not only sensation but motion. The problem that we wanted settled was, if only the skin is made anæsthetic what is the result? That was the point.

17876. Only the skin?—Not the parts supplied by the nerves which go to muscles. The problem we wanted to solve was this: There go to the hand, nerves which go to the muscles, and nerves which go to the skin. Ordinary accidents divide both. We wanted to know what happens if you divide only the nerves that go to the skin and leave all the nerves which go to the muscles untouched. The day after the operation that question was solved, and we thereby obtained a knowledge of an entirely new form of sensibility; a form of sensibility which had never been described before. A direct answer was given to a direct question.

17877. Could you take your three nerves, for example, and could you say what was the result of the severance upon the sensation in your hand or in what part of your body?—We desensitized the skin over the whole of the part. (*Describing the same.*)

17878. Did you find that it destroyed sensation in the skin in that part only?—Yes, we took only the skin nerves and left all the nerves which go to muscles, that was part of the operation; and the result was that we discovered that under this sensation, which is what we all have when we touch ourselves like this (*describing*), there lay an entirely unappreciated sensory mechanism, which is responsible for an enormous amount of our sensation, but that it runs with muscular nerves, which was an entirely new fact, and a thing which has revolutionised the conception of sensation altogether. Because in old days we would have been told, if you cut all the sensory nerves going to the skin you desensitize the part. It is nothing of the kind. You must cut the sensory fibres in the muscular nerves before you do that.

17879. You did satisfy yourself by an experiment on yourself; but could you have satisfied yourself by experiments on animals?—No. I was going to point that out to you, because we could not test the sensation of animals. But the whole point is, that running alongside came a series of experiments on the way in which nerves united in animals, which were necessary to show how these reunited nerves conducted. That has been going on upon animals, and can only be done on animals. Therefore the points that I want to make so strongly are, that in every inquiry into the structure and functions of the nervous system, observation of disease and experiments on animals are intimately interwoven; the one amplifies the other, and neither can be fruitful alone, because, as I have shown before, the very methods by which we examine disease are discovered from experiments on animals. Experiments on animals cannot tell us about what a human being feels; for that we require disease; but on the other hand in many of the cases where we have to depend upon human beings, we only know what is the matter with them because we have made that lesion in animals. So that the whole thing is all woven together like a fabric, and you cannot pull out one coloured thread without altering the pattern. Lastly I did not know whether it would be of any value simply to put one's self forward to answer any questions; as for practical purposes I am an animal that has been operated upon under Certificate B.

(*Chairman.*) I will leave that to some of the gentlemen who understand the matter more than I do.

17880. (*Sir William Church.*) You have given us a very interesting account of experimentation on animals and on yourself with regard to the nervous system, but I suppose you would say that, *mutatis mutandis*, all that you have said applies in a very large measure to researches into other organs in man?—Yes. I have been particularly interested in the nervous system, and also the nervous system was within so comparatively short a time a *terra incognita*. Our knowledge of the nervous system as exhibited by Watson is like the old maps of Africa, undiscovered country. Now we know its paths, because within a comparatively recent time we have learnt such an enormous amount about them; that is why I chose it particularly. But, *mutatis mutandis*, it is absolutely as you say.

17881. But you would, I suppose, agree that the same methods of observation and experiment are required for researches into our knowledge of other groups of organs in man?—Absolutely. I think it is only a small example, taken from the nervous system, of great general methods.

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17882. You personally, of course, do not maintain that experimentation upon animals should take the place of observation upon man in what we call clinical observation?—Most certainly not; the one can only be fruitful with the other.

17883. The two should be worked together?—Yes. It is absolutely necessary that the two should be worked together. The great progress in our knowledge of the paths of the nervous system came from Waller's discovery, which was made on animals.

17884. So that the great value of experimentation on animals is that by that means you can put a definite question as it were to Nature, and get a definite answer?—That is it exactly.

17885. Therefore, as your research into any group of organs goes on, so it is necessary for you to put definite questions and obtain, if possible, absolutely certain answers?—Yes.

17886. So that in your view the great value of experimentation on animals is in addition to our knowledge either for scientific purposes or for curative purposes?—Yes. I should like also, just as a corollary to that, to point out the uselessness of experimenting for a practical end. Men have been experimenting for practical ends and observing for practical ends with regard to pain and disease with comparatively little progress.

17887. Because they have not sought for knowledge but for practical results.—Yes. It was only when we determined to see whether we could not apply the theoretical results obtained by Dr. Gaskell to the structure and the functions of the nervous system that we discovered these other laws, which are of daily practical importance.

17888. Consequently those who object to experimentation on animals upon the ground that immediate practical results are not obtained simply do so because they do not understand the way in which practical results can be obtained?—Yes; certainly. Practical results are the bye-products of a manufacture of which knowledge is the aim.

17889. (*Sir William Collins.*) May I ask you one or two questions with regard to the very interesting experiment that you carried out upon yourself? It would not have required a licence under the Vivisection Act, of course?—I am not a domestic animal.

17890. It would not, therefore, have required a licence under the Vivisection Act?—No.

17891. I understood you to say that that experiment which you made entirely revolutionised our conception of sensation?—If the results that we have recorded are right, it has done so because it has discovered for us something that we did not conceive before. It has discovered for us the fact that nerves which run with the muscular nerves which were supposed to have certain comparatively inferior functions, have very much higher functions and are of very much more importance in the field of sensation that was ever supposed.

17892. You say, if the results that you derive from your experiments are correct. Have you any doubt as to the results?—I have not, but other people may have. That I leave to time; only time can tell that.

17893. The difficulty that you apprehend is in convincing others?—No, I think it is like all new things. They have to undergo the test of time.

17894. (*Sir Mackenzie Chalmers.*) One experiment is not conclusive?—It is one in a series of advances.

17895. (*Sir William Collins.*) I am anxious to know why you qualified that previous answer by saying if the results you obtained were to be regarded as correct?—I think that is the attitude of every one who is a scientific worker. Personally I do not doubt it.

17896. You have no doubt that this experiment has revolutionised our conception of sensation?—Certainly it has revolutionised my conception.

17897. And that you discovered a form of sensation which was never described before, I think you said?—Not in full.

17898. Would you tell me what are the three nerves that you divided?—They are the radial, and the two branches are what is called the external cutaneous, one of which runs down the front of the forearm, and the other down the back of the forearm.

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17899. Was it not surmised previously to your experiment that the radial nerve was concerned in cutaneous sensation of the extensor surface of the fore-arm and hand?—It was supposed to be proved that it was not. If you look up Bowlby's book, you will find that he showed that when it is divided, there is no change of sensation.

17900. Do you mean that I shall not find in any physiological or anatomical work prior to your work any such suggestion?—I do not for a moment mean that. That was not the experiment.

17901. That is my question?—No. The anatomist has always said that the radial played a considerable part, but when the radial was divided in man we could not find out that anything of the kind happened. That was one of the most characteristic instances where anatomy and fact did not go together. The anatomist always described (as you know from your books) a large area supplied by the radial. He put it down with very marked local borders like the borders between Switzerland and France and other countries. But when the radial was divided, nothing apparently happened.

17902. You stated that anatomical knowledge told us absolutely nothing as to sensation of the hand?—Quite so.

17903. Do you think that anatomical knowledge has not told us anything as to the radial nerve being associated with sensation of the extensor surface of the forearm and hand?—But it did not explain why, when you divided the radial nerve, you could not get the result that anatomists said that you should get. That was the trouble.

17904. Will you tell us what was the result of dividing the radial nerve in your own case?—I did not divide the radial nerve only; I divided it together with two other branches.

17905. Will you tell me what the result of the division of the radial nerve in your own case was?—I did not divide the radial nerve only. I divided it together with two others, so as to make quite certain that I got a loss of sensation which I found that I could not get by dividing the radial nerve only.

17906. Have we any knowledge of the part played by the ulnar nerve in respect of sensation motor power?—The motor power was accurately known; the sensation was very imperfectly known.

17907. Was nothing known of sensation of the hand in reference to the ulnar nerve prior to your experiment?—I am afraid I do not quite understand you. My experiment had nothing to do with the division of the ulnar nerve.

17908. But I understood you to say that anatomical knowledge had told us absolutely nothing as to sensation of the hand, and you said that the distribution of the ulnar nerve suggested nothing as to sensation of the part of the hand that it supplies?—That has nothing to do with my experiment. That has to do with my observations on man.

17909. It has to do with your statement that anatomical knowledge has taught us nothing as to the sensation of the hand?—That is perfectly true, but that has nothing to do with my experiment; it has to do with the observations made by Sherrington on animals and myself upon human beings, where the ulnar nerve had been divided. It was found in those cases that if you gave the anatomist a patient in whom the ulnar nerve had been divided, he could not tell you whether it had been divided and partly re-united, or whether it had not been divided, except by looking at the motor supply. If by any chance the motor supply was uninjured, he could not tell you in what condition that nerve was; he could not tell you whether to cut down and suture that nerve, nor could he tell you if you did cut down upon it what you would find.

17910. Had it been observed by surgeons that when the ulnar nerve has been divided at the elbow joint, there has been loss of sensation in the little finger and in the adjacent portion of the next finger?—But that is not sufficient to tell you whether it has been divided.

17911. Did we know absolutely nothing of sensation in the hand from such observations as that?—Nothing that would be of practical importance to you; so much so that it was the habit to neglect sensation and to depend entirely on the motor condition.

17912. Will you be surprised to hear that a surgeon seeing a case of division of the ulnar nerve at the elbow joint, and finding loss of sensation in the little finger and the adjacent portion of the next finger, had cut down upon that ulnar nerve and re-sutured it and found sensation restored?—Not in the least, because he cut down without knowing, except on the motor side, what he was going to find. Time after time I have known the ulnar nerve cut down upon in ignorance that it had reunited and reunited. Recently a patient of my own, in whom the ulnar nerve was united, was operated upon and the nerve exposed, simply because it was not known whether that nerve was united or not. We knew that it was united, and it proved to be so. Anybody who has had experience in this side of practice knows how difficult it was to come to a correct conclusion.

17913. What is the form of sensation which you are able to describe now, which was unknown before your experiment?—You are passing on to my experiment now. What is called deep sensibility.

17914. Would you tell us a little more about it, please?—It was found that after the operation a touch with the finger could be appreciated and localised although the whole of the skin had been rendered insensitive.

17915. (*Mr. Tomkinson.*) By considerable pressure?—No, only a touch. Secondly, it was found that by means of pressure pain could be produced with the same amount of pressure as on a normal hand.

17916. (*Sir William Collins.*) You mean that the sensory fibres apparently travel along with the motor fibres?—They do. Are you surprised to hear that? The surprise in your question is the measure of the new idea.

17917. I express neither surprise nor the reverse. I am asking you a few questions on the result of your own experiment?—The surprise of that question shows the new idea.

17918. I am afraid that you must not argue surprise from the question. Does that experiment then necessitate a revision of our opinions as to the distinction usually drawn between motor and sensory nerves?—No, between muscular and cutaneous nerves, which is a different matter.

17919. Do you distinguish between touch and pain and sensation of temperature?—I do not quite understand the question.

17920. When one speaks of sensation, is it necessary to discriminate between sensation of pain, sensation of touch, and sensation of temperature?—Do you mean from the point of view of the skin or the deep structures, or from the point of view generally?

17921. Either?—Do you mean from the point of view of the spinal cord or the brain or the peripheral nerves, the nervous system you are concerned with? That is the point.

17922. Do you, or do you not, consider it necessary to make a distinction in speaking of sensation generally between sensation to touch, sensation to pain, and sensibility to temperature?—Yes, discriminating also heat and cold.

17923. Would you tell me whether those forms of sensibility travel along with the motor nerves or with the cutaneous nerves?—One form of touch travels along the cutaneous nerves, and one travels along the deep fibres; one form of pain travels along the cutaneous nerves, and one travels along the deep fibres. Heat and cold travel solely along the superficial, along the skin fibres. Localisation of a spot touched travels along the deep fibres. Localisation of a spot touched travels along the superficial fibres; but the localisation of a spot touched travelling along the deep fibres does not carry the power of distinguishing two points; sensory impulses travelling from the skin carry the power of discriminating the two points. You see why I had a difficulty in answering the question.

17924. I am very much obliged to you. May I take it that those views are generally accepted by physiologists now?—No; they are new.

17925. Since when?—Since the work has been going on. I suppose the first publication of it was in 1905.

17926. With regard to your experiments in association with Professor Sherrington?—I was not associated with him. Sherrington was working independently entirely.

17927. I thought that you urged the importance of associating the results that you obtained with those that Professor Sherrington obtained?—That is quite right. We met, but we were marching entirely independently. Sherrington published his results in the usual way, and it was my knowledge of his publications that helped me so profoundly in my research.

17928. As the result of those experiments and observations of yours and Professor Sherrington's, whether in association or not, do I correctly understand that you are able to localise the position of stone in the kidney, in the ureter, or the bladder according as the pain is distributed in the thigh or the buttock?—Not by pain only. There is no touchstone in medicine. That is only one of a set of facts which have to be taken into account in making a diagnosis. No diagnosis is made on one method of examination or one set of facts. A diagnosis is made and acted upon only by co-ordinating a large series of facts brought together by different means, and this is only one of them.

17929. Then do I rightly understand that localisation of pain will not localise a stone by itself?—Certainly it will not. A better instance in some ways is the pain down the inner side of the arm, produced by heart disease, which is a thing that was not understood at all.

17930. You spoke of the degeneration of nerve. Have you worked at the regeneration of nerve?—Yes.

17931. Am I right in thinking that there are two schools as regards the regeneration of nerves: those who argue for the peripheral regeneration, and those who argue for the central regeneration?—Yes.

17932. May I ask whether you can tell us your own view on that subject?—No, it would be impossible, because we are beginning to see that the problem is not so simple as that. It would be quite impossible here to explain the position that I hope to adopt in the matter.

17933. We have been told that there are peripheralists and centralists, and one witness, at any rate, classed himself with the one or the other. You are not able to do that?—That is too dogmatic. The problem is a more complex one than that.

17934. Have experiments been made with a view to elucidate that problem?—Yes; those are the very ones that I have been making.

17935. Have both peripheralists and centralists claimed, as the result of experiments on animals, the accuracy of their view?—Yes, certainly, because it is like all those things; we have to work on both sides of the question before we ultimately reach the truth.

17936. You alluded to Sir Charles Bell's work?—Yes.

17937. It has been claimed, I think, that Sir Charles Bell's work was the greatest that had been done in physiology since Harvey. I do not know whether you estimate it as high as that?—I think the knowledge that the posterior root is sensory, and the anterior root motor has had an enormous effect upon our knowledge of the nervous system; that is the way in which I can put it.

17938. Was the knowledge of the nervous system obtained by Galen valuable?—I am afraid I do not know; because I am afraid that I know very little about it.

17939. We have been told by one previous witness, I think, that much of the knowledge of Galen was valuable to-day, and others, I think, have rather disputed that view. I was wondering whether as a neurologist you could give us your view?—The only way in which I should put it is that in 1845 it had no fruitful effect upon knowledge as judged by the ordinary standards of the time.

17940. The experiments of Hierophilus on human beings have also been alluded to; do you consider that they yielded any knowledge?—I do not know anything about them. The point is that 60 years ago our knowledge of the paths in the nervous system was exactly in the condition of a map of Africa, and now we know a number of paths, a number of tracts, and a number of territories. That is the point.

17941. We have had our attention called to the opinion of Sir Charles Bell as to the comparative uselessness of experiments on animals for the purpose of ascertaining the structure and functions of the nervous system. He said in one of his works "Experiments have never been the means of discovery, and a survey of what has been attempted of late years in physiology will prove that the opening of living animals has done more to perpetuate error than to confirm the just views taken from the study of anatomy and natural motion." I gather from what you have told us to-day that you do not agree with that view?—It is not a question really of whether I agree or not; it is a question of pure fact. Sir Charles Bell's statement there is not worth any more than the statement of any other person on that matter. The point is how much more do we know now in consequence of direct experiment. Bell did not know what we know. You cannot foretell. He only made that statement because it was his impression of his own time—that is a long while ago. We know a great deal more since then, and not only do we know a great deal more, but we also know how much more work we have got, so to speak, on our hands.

17942. That was in 1836?—Yes.

17943. At that time had Sir Charles Bell himself advanced our knowledge of the nervous system?—He stated the hypothesis that the two roots had different functions.

17944. Was it only a hypothesis in Bell's time?—That, of course, is a very disputed point as to how far his statement could be taken as a definite proof; it was definitely accepted 10 years later, in 1845. But perhaps I may just read you one passage which you will be interested to hear: "It is commonly believed," says Watson, "that disease affecting the *anterior* columns only of the cord, will be likely to disturb, or to suspend, the *power of voluntary* motion in the corresponding parts" (that is the direct outcome of Sir Charles Bell's statement) "to produce *spasm* or *palsy*, and that disease affecting the *posterior* columns alone will be likely to alter or abolish the *faculty of sensation* in the corresponding parts; to cause *pain, tingling, numbness*, or *complete anæsthesia*. But I have mentioned certain facts which contravene this opinion. Suspend your judgment respecting it. Neither the minute anatomy nor the physiology belonging to the question are yet conclusively settled. There seems no reason to doubt that disease affecting the *lateral* half only of the cord will be likely to derange both the sensibility and the power of movement, in the corresponding parts on the *same side of the body alone*." So you see the state of knowledge in 1845.

17945. Did Brown-Séquard make further observations on the functions of the roots of the spinal nerves?—Mainly on the functions of the spinal cord; most of his observations were on the functions of the spinal cord; there were some on the posterior roots.

17946. Did Brown-Séquard contest the conclusions that Sir Charles Bell had arrived at?—I am afraid I cannot tell you without looking it up. But Brown Sequard was led by experiments on animals to give an immediate answer to that last statement that Sir Thomas Watson put forward wrongly.

17947. You alluded to the localisation of the cerebral functions as as example of the advance that has been made in our knowledge of the nervous system. Is the association of the left lower frontal convolution with the function of speech, one of the cases of localisation of the cerebral function?—You mean the association of Broca's convolution with speech?

17948. Yes?—Yes, but that does not stand on the same level as others.

17949. Do you mean that it stands on a lower level or a higher level?—It has not been proved in the same way as others; that is just one point at present in dispute.

17950. Do you know a recent paper by M. Marie?—Yes.

17951. He, I think, asserts that the third left frontal convolution does not play any special rôle in the function of language?—Yes, it is just that very point that is in dispute. It is one of the points that we cannot settle by experiment, and it is one in which we have to wait for observations, and we have waited all these years.

17952. Was it one of the earliest localisations?—Yes, one of the earliest and still one of the most disputable.

17953. I think you laid it down that no man has a right to be the subject of experiment unless the whole circumstances are well understood?—Certainly.

17954. That was in regard to himself, and I suppose *a fortiori* in regard to others if they voluntarily agree to be operated upon?—Certainly. An accident may happen which may put things wrong; that is where the risk comes in, but you should take every possible precaution to avoid that accident.

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17955. And, as you said, experiments on animals cannot tell us, of course, what human beings feel. That, I take it, is why you call our special attention to your own experiment, as in your opinion solving a problem which no experiments on animals could have sufficed to solve?—No, what I worked upon were the experiments on animals which worked into the whole network of the research. Self experimentation, microscopial anatomy, and experiments on animals all worked in together.

17956. But previous to your experiment, no experiments on animals, I understand, had suggested that sensory fibres travelled with muscular fibres?—Most certainly they had. It was because Sherrington found by experiments on animals that such a large number of the nerve fibres which run with the muscular nerves did not degenerate like motor nerves at all, but degenerated in the opposite direction, that made us determined to test the thing on man.

17957. But I thought you claimed that this revolution in regard to our knowledge of sensation had been the result of your own experiment?—As regards sensation, that is perfectly true. All we knew was that these muscular nerves contained a very large number of fibres which were evidently not motor. Therefore in my experiment I determined to put that to the test from the point of view of sensation. Sherrington could not, of course, test sensation, but he told us that there were a large number of afferent fibres, whatever those afferent fibres did.

17958. But without your experiment on yourself, by experiment on animals alone, could this revolution in our conception of sensation have been attained?—Not of sensation, but the very fact that Sherrington had told us that there were these afferent fibres made it perfectly certain that there must be a sensory function attached to them. What that sensory function was, was a thing that only a human being could tell us, was what we set ourselves to show; but the idea would not have come to us had we not had this elaborate work of Sherrington's, which told us that a large proportion of the fibres in the muscular nerves were not motor.

17959. Was that arrived at by the method of Marchi, which you have already described?—Yes. Waller's law of degeneration and Marchi's method.

17960. (*Sir Mackenzie Chalmers.*) As regards this operation on yourself, did you have it done under local anæsthesia or general anæsthesia?—General anæsthesia.

17961. After the division of these nerves, did you suffer pain above the division; was there great pain above the division?—Do you mean in the other parts of the arm?

17962. Yes—you divided the nerve?—Yes.

17963. Above the division, of course, the nerve would be connected with the central nervous system?—Yes.

17964. Was there much pain there during the healing?—Absolutely none.

17965. Therefore, presumably, in these experiments on animals the animal suffers no pain?—There is nothing to show that it suffers at all, and I did not. I went about my work as usual, with my arm in a sling.

17966. And it healed in the ordinary way?—It healed by first intention.

17967. How long did it take before the nerves again rejoined and re-established sensation?—Fortunately, one of the stitches was cut out, so that I still have a patch of anæsthesia left.

17968. Insensitive?—No, in the peculiar form of sensibility which we are particularly interested in studying, so that I hope it will last. But all the others are healed.

17969. (*Mr. Ram.*) As I understand, the experimental investigation that you were carrying on at the same time as Professor Sherrington was making experiments on animals, was a case in which a correct theory originated by clinical observation, and was confirmed and proved by experiments on animals. Is that the correct way of stating it?—Hardly. It was the general trend of knowledge at the time, experimental and otherwise, led to the correct theory; it was the whole thing put together.

17970. But you were pursuing it by clinical observation?—Yes.

17971. And the theory that you were led to adopt by your clinical observation was proved and, perhaps, corrected by the experiments made by Professor Sherrington?—It was enormously helped, because it was only by his experiments, which were going on at very much the same time, that we knew that there was anything of this kind—namely, that the central nervous system was distributed in this sort of way. That is the way I put it. Let me be quite clear. Dr. James Mackenzie put forward exactly the same view, backed up by admirable observations on similar cases, but he did not carry it as far, because he was not so familiar with the work on the posterior roots.

17972. That is why I put my question as I did. With regard to the experiment on yourself, do I sum it up rightly by saying that it showed the co-existence of nerves affecting the skin with another series of muscular nerves which conveyed the sensation independently of the skin nerves?—Yes, that is quite right.

17973. Does the knowledge that you so arrived at, in your opinion, directly tend to the amelioration of future suffering by more skilful and expert treatment that that knowledge will enable?—Certainly, it has opened up and enabled us to understand things which we did not understand before.

17974. And it tends, therefore, to correct errors either in diagnosis or treatment, which may have existed in the absence of that knowledge?—Yes.

17975. (*Dr. Gaskell.*) I only wanted to make this matter quite clear if I could. Before your experiments had various surgeons sewn together nerves going to the skin which had been accidentally cut?—Yes.

17976. In some cases had they found that sensation was well localised immediately after the operation, when it was tested?—Yes, that is true.

17977. And in consequence of finding out that recovery of sensation, was the conclusion come to that those nerves had healed remarkably quickly?—Yes.

17978. And that therefore the whole process of re-regeneration as ordinarily taught was wrong, and that the two nerves could heal together by direct contact very quickly?—Yes.

17979. That was shown by your experimentation to be absolutely erroneous, because the outcome of it was that the localisation of the deep nerves was very much more accurate than ever had been before suspected?—Yes.

17980. The only other thing that I should like to know is whether in that regeneration of your nerves going to the skin there was a very great difference in time between the regeneration of those fibres which gave you tactile sensation and those fibres which gave you painful sensation?—Yes, there was a difference of nearly nine months before the tactile sensation began to recover so far as the shaved skin is concerned —so far as the naked skin is concerned.

17981. And you gather from that that those fibres which absorb the sensation of touch are not the same as those which absorb the sensation of pain in the skin?—Yes.

17982. And that the nerves which conduct the painful impressions from the skin regenerate more quickly than those which conduct the tactile sensation?—Yes.

17983. Can you tell me at all where you would say the perception of pain is located in the central nervous system?—It is very difficult to say.

17984. You would not say, for instance, that the perception of pain could be located in the spinal cord? —Certainly not.

17985. Therefore, an animal deprived of the upper part of its brain could feel pain?—No, certainly not. I do not think there is anything to show that.

17986. Do you consider that an animal deprived only of the cerebral hemispheres could feel pain?—I should not like to say. I have not enough experience of that at present.

17987. (*Mr. Tomkinson.*) The result of this experiment on yourself, I understand, was a valuable discovery of something that was not known before?—It was an addition to knowledge.

17988. And that was because you, as a human being with the power of speech, could tell the result?—Yes.

17989. And if that experiment had been made upon an animal the same result could not have been arrived at?—You would not have made it on an animal for that reason—not on the sensory side of the nervous system.

17990. Then how does that bear upon the utility of the practice of vivisection on animals?—It was given as an instance of how the whole of these various methods are all woven together, and the one depends upon the other. That is the point—that they are all woven together; that the one is useless without the other. They are like the re-agents on a chemist's desk. You give him so many re-agents, and then you take away one of them from him; you say to him: You must do all your analyses, but you must not use one particular re-agent, say hydrochloric acid. There are a number of experiments which he could do without that particular re-agent, but in order that he may do his analyses properly he must have his whole set of re-agents in front of him to use. In the same way, these are all methods which are used; and it so happened that in this small research which I have instanced all the methods were used each in its own particular place.

17991. Still, in this particular instance the experiments might have been made on animals for a long time without the discovery which the experiment made upon yourself did bring to light?—That is why I submitted myself to the experiment, otherwise I should certainly not have done it. It would have been an illegitimate thing to do.

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17992 (*Chairman.*) Had the experiments on animals been useless, do you mean?—No.

17993. Do you mean that you experimented upon yourself because you had not arrived at a final conclusion from experiments on animals?—No, because it would not be the kind of question to which you would expect an answer in that way. You would not expect an answer to a direct question concerning those finer forms of sensation.

17994. Your answer to Mr. Tomkinson's last question seemed rather to suggest that experiments on animals had been thrown away. I did not understand that to be your intention?—No, not at all.

17995. You had explained that before?—Yes.

Mr. A. D. WALLER, M.D., LL.D., F.R.S., called in, and Examined.

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17996 (*Chairman.*) You are a Lecturer at the University of London, I believe, and a Doctor of Medicine, a Doctor of Law, and a Fellow of the Royal Society?—Yes.

17997. And you have been there a good many years, I think?—I have been five years at the University, but I have been a lecturer at a medical school for 20 years.

17998. You do not propose, I understand, to go into general questions about the value of experiments on animals or the morality of them—we have had a good deal of evidence on those points already, as you know—but principally you desire to vindicate, if I may say so, the University of London from some of the suggestions in Miss Lind-af-Hageby's book, "The Shambles of Science"?—Certainly, and I wish to restrict myself to facts within my own knowledge.

17999. You were present, either as lecturer or otherwise, at a good many of the operations therein described?—Yes, I have drawn up a list of the operations which I could identify.

18000. The first was a lecture on December 3rd, 1902. Dr. Pembrey was lecturing then?—Yes, I was present then.

18001. Then there were lectures on February 3rd, 1903; February 19th, 1903; February 26th, 1903; and another on the same date?—That is a point of doubt in my mind. I have identified the dates from my notes as the occasions on which I was present, but this date February 26th is twice quoted in "The Shambles of Science," and one of those dates must be wrong therefore.

18002. Do you mean to say that there were not two on that date?—There were not two on that date.

18003. However, as regards the one that Miss Hageby describes as "A dog injected with a substance derived from a lunatic," and the other as "A troublesome dog." Those two, whatever the date was, you were present at?—I was present at the lecture, and I can identify the "dog injected with a substance derived from a lunatic." As regards "a troublesome dog" on going through the description, I could only say that I saw nothing of the sort.

18004. Do you mean by that, that you were not present, or that you may have been present, but you did not see what she says took place?—I mean to say that I think it is a mistake. I think probably this is relating to some other place. I do not think that this lecture, so far as I can identify it, took place at the University. I cannot identify it.

18005. You say that it certainly did not take place at the date that Miss Hageby puts it at?—It cannot have done so, because I have the laboratory books here, and another lecture took place on that date, namely, "A dog injected with a substance derived from a lunatic." That lecture did take place on February 26th. In "The Shambles of Science" an experiment headed "A troublesome dog" is described as taking place at that date, but it cannot have done so.

18006. Your evidence about that is that you are confident that it did not take place on that date at the London University?—Quite.

18007. And you have not been able to find any trace which would enable you to identify it?—I cannot identify that lecture.

18008. It may have taken place somewhere else?—Yes, it is probably a mistake on the part of Miss Lind-af-Hageby.

18009. Then the next lecture is on March 12th. "The only completely satisfactory method." That is one of the lectures you were present at?—Yes, I was present at that.

18010. I would like to ask you generally about Miss Hageby's book; you read it at the time, I believe?—Yes.

18011. And you made annotations on your copy?—I annotated it.

18012. Which you are willing to leave with us?—Certainly.

18013. Generally, do you consider that the book gives a fair and accurate representation of what took place?—Oh, no; it is a highly coloured representation; a representation coloured by the sentiments of this lady—in perfect good faith, no doubt. I do not say that they are in bad faith at all, but they are obviously seen through her own coloured spectacles, and the amount of discoloration that those spectacles have produced is really very remarkable.

18014. As regards those lectures that you were present at, would you just tell us as regards December 3rd, 1902 (and I will take you through the others very shortly), what the points are that you picked out as being, in your opinion, mistaken?—I will quickly run through my notes. She states on page 11, "He inserts one point of a pair of forceps under the skin behind the skull for a moment."

18015. Are you quoting from the original edition?—Yes. I have not got the later editions. That means that he cut the head off. Then she says lower down, "The animal begins to struggle, quickly moves its limbs to its back, and tries with hands and feet to thrust away the instrument." My note is, "The ordinary reflex movements that do not imply any sensation."

18016. You were present at that lecture?—Yes, and I made these notes.

18017. Have you a remembrance of the lecture, and what took place?—I have none now, I had at that time. On page 12 she says: "The wound is cauterised." My note is, "Presumably the cord is destroyed." That means pithing.

18018. Would that involve a cautery?—No.

18019. You mean that there was no cauterising?—Yes. Then on page 13 is the episode of the frozen rabbit left in an ice-box, and so forth, and the description of its start with horror on the next page. I have made some comments on that, but it is an impossible thing.

18020. (*Sir Mackenzie Chalmers.*) Would you tell us

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what really happened?—What happened was that the rabbit was put in an ice-chest, a refrigerator, to lower its temperature, but the refrigerator was not cool enough as a matter of fact, and the animal's temperature was not lowered

18021. Was it anæsthetised or unanæsthetised?—It was not anæsthetised. It was simply put into the refrigerator to show that its temperature did not go down as that of a cold-blooded animal would.

18022. Nothing whatever happened?—No. I have this memorandum "Minimum temperature=8°." It was not low enough to act.

18023. 8° Centigrade?—Yes.

18024. What would that be Fahrenheit?—I make out 46°.

18025 (*Chairman.*) But what do you mean by its temperature? Do you mean its internal temperature?—That was the intention, no doubt. The temperature of a cold-blooded animal in a cooling box would go down; the temperature of a warm-blooded animal would not go down, or at any rate not so quickly. But there was no vivisection at all.

18026. I understand that a rabbit's natural temperature is a little higher, if anything, than man's?—About 37° or 38°.

18027. About the same. I do not understand your bringing it down to 45.—That is the temperature of the box, not of the animal.

18028. I was asking what you brought the rabbit down to.—I cannot tell you. I have no idea.

18029. (*Mr. Ram.*) But there was no vivisection?—There was no vivisection; there was no freezing of the animal. That is pure imagination.

18030. (*Sir William Church.*) When she states, "It was found to be beyond the stage for observation," that leads the reader to think that the animal was dead. I understand from you that the animal was not dead?—The description is, "The animal was taken out of the freezing machine quite unconscious, but frozen stiff, like a piece of wood. With all signs of terror the animal springs back trying to get away, but half paralysed by the cold and half fascinated." My comment upon that is simply that all this is "impossible."

18031. (*Sir Mackenzie Chalmers.*) Was the animal never exposed to a lower temperature than 45°?—No; the cooling box did not act properly, as a matter of fact. I remember that much; but the details about the animal I do not remember. I generally remember this description of an animal frozen stiff, and, with all signs of terror, springing back, and think to myself that was an imperfect description.

18032. (*Chairman.*) There must be some imperfection when they say at the same time, "frozen stiff like a piece of wood" and "the animal springs back"?—I have simply got a joking comment of this as being an obvious impossibility. Then my general comment upon this lecture at the time was, "No vivisection was performed at this lecture (at which I was present)."

18033. Do you mean that none was performed at that lecture or that none was performed whilst you were present?—I was present at the whole lecture, and no vivisection was performed while I was present. My comment continues, "One or two frogs were killed, and their hearts used for experiment. The lecture was on animal heat, and some animals hot and cold-blooded were cooled in an ice-chest and their temperatures compared. That experiment was on December 3rd, and that is misdated again in "The Shambles of Science" as December 2nd, but that is simply a mistake of Miss Lind-af-Hageby's. It should have been the 3rd, according to my laboratory book, which I have here.

18034. Then the next is February 3rd, 1903—"An experimental production of blood clotting by injection of a nucleo-proteid"?—I remember that particularly well, because I was there, and I gave the anæsthetic myself, and there is a description on page 30 of the anæsthetist looking "most careful, as if he were attending the precious life of some human patient," and so forth. It simply means this: It was one of the very first lectures delivered in the laboratory, and I laid great stress upon anæsthesia, and gave the anæsthetic myself, in order to, so to speak, show how it was to be done when done by other people in the laboratory. I have got a long comment here, but I do not think it is germane to anything that you wish to know.

18035. You have made a special subject of anæsthetics, have you not?—Yes; I have worked at it a great deal in the last fifteen years.

18036. You have nothing special with regard to that experiment that you wish to say?—There is a statement, "Have these preliminary operations been done under an anæsthetic?" on page 29, the inference being, of course, that they have not. My comment here is, "Yes, I took charge of the anæsthesia."

18037. What was the animal there, a grey rabbit?—I forget the colour of the rabbit, but it was a rabbit, no doubt.

18038. I might ask you now, as regards all these experiments that you were present at, was there any case in which the animal was operated upon either without anæsthetics or without sufficient anæsthetics?—Certainly not.

18039. Are you satisfied that in every case there was complete anæsthesia during the operation?—Certainly, and I state this in my summary. I state that no experiment has been performed in this laboratory in which an animal has not been completely insensitive to pain.

18040. If there were movements in the animal—if that is accurate—it would follow that it was not from sensation of pain?—Certainly not, in my opinion. They were reflex movements; the ordinary convulsive movements that happen.

18041. Is there anything else you wish to say with regard to the experiment on the 3rd of February?—They ask the question again on page 32, "Now, was the rabbit anæsthetised or not?" My comment is, "Certainly." I was there. Then they say, "The animal began to struggle violently." My comment is, "The usual anæmic spasms."

18042. (*Mr. Tomkinson.*) Is that a dog?—No; the same animal—a rabbit. Then they go on, "And would not the tetanic spasms that followed counteract the effect of a possible anæsthesia." My comment in pencil is, "Certainly not." That is all I have to say about that.

18043. (*Chairman.*) Then we come to February 19th. "An experiment that is not supposed to be useful." Have you any observations to make upon it?—Yes; I was present at that lecture. On page 59 they say, "The lungs are exposed, and artificial respiration kept up by means of a pump attached to the operation table, and worked by electricity." My addition to that is "pumping air plus chloroform vapour"—that is to say, the artificial respiration apparatus was attached to an apparatus for producing anæsthesia.

18044. Was that the usual method?—That was the usual method.

18045. And it was satisfactorily performed on this occasion?—Yes, it cannot be otherwise with that automatic apparatus, I think. Then, on page 60, "We can be almost certain that the dog has been also curarised, because it is absolutely still, with the stillness that is characteristic of the 'Hellish Woorali.'" My comment as to that is, "It was not."

18046. It was not curarised?—No; it was not curarised. Then I have a further amplification to this effect: "The animal was absolutely motionless under artificial respiration through a chloroform flask giving between 2 and 3 per cent. of chloroform vapour to the insufflated air. I was present." I had been measuring the percentages at about that period.

18047. Would that be sufficient to account for its being completely still?—Absolutely; and it would run the risk of being overdosed with that percentage.

18048. Then there is the lecture on the 26th February: "A dog injected with a substance derived from a lunatic." Dr. Brodie is again the lecturer, as on the 19th?—Yes; that lecture did take place on February 26th, according to the notes in my laboratory book—"Lecture on the Circulation, by Dr. Brodie," and I find Miss Lind-af-Hageby and Miss Schartau's names as being present there, so that I identify that as the experiment of the 26th February; therefore I do not identify the other one.

18049. You say that it was a lecture on the circulation?—A lecture on the circulation, on the effect of choline, which was the particular thing—one of the alkaloids, one of the ptomaines.

18050. She described it as "A dog injected with a

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substance derived from a lunatic"?—Yes; that is all right. It is a description, after a fashion, of choline; it is a substance derived from a lunatic. It was in that case.

18051. I think we have had that explained?—Yes. It is stated on page 76: "The demonstration begins. The lecturer tells us that the animal is anæsthetised with morphia and some chloroform." My statement is, "The animal was fully anæsthetised." I have no other remarks at all on that lecture.

18052. I will not ask you to repeat the general observations that you made about the choline that was given.—That applies to the whole.

18053. (*Sir William Church.*) Miss Hageby, when she was giving evidence on this case here, took exception to the way in which the dog was killed. She states in "The Shambles of Science" that the dog was killed "By blowing a little air into the jugular vein," and the first time that was done she said that the levers continued in motion, "Showing that the flame of life is not blown out." "The lecturer declares that the dog will now be killed. How exceptionally kind and considerate! It is to be killed by blowing a little air into the jugular vein." She says that it was not killed when the air was first blown into the vein, and she rather led one to think that in the intervals between the first blowing into the jugular vein and the second the animal was not under the influence of chloroform?—This is in 1903. I have no comment on that, and I cannot remember every detail of what happened to this animal in 1903; but it does not sound to me probable at all. I have this comment written down, that "The animal was fully anæsthetised"; therefore I do not think it probable. This was written down as soon as I got the book.

18054. Might I read the passage to you, because she made a great point of it in her evidence: "It is to be killed by blowing a little air into the jugular vein. The assisting demonstrator attaches a glass tube to the cannula in the jugular vein and blows some air down the tube, but the levers are still in motion, showing that the flame of life is not blown out. He repeats it and then sits down among the audience. The dog has been killed strictly scientifically." She rather implied I think, in her evidence, that there was a considerable interval between those two blowings into the jugular vein by the assistant demonstrator. Can you tell us at all whether that was the case?—I have no recollection of any such interval.

18055. (*Sir Mackenzie Chalmers.*) During which the dog was suffering?—I have no recollection of any interval, and I am absolutely certain that the dog was not suffering.

18056. The dog was fully under anæsthesia the whole time?—The dog was fully under anæsthesia the whole time.

18057. (*Sir William Church.*) That is what I wish to get out?—Yes, certainly.

18058. (*Chairman.*) Have you finished then with February 26th?—Yes, but I am puzzled by this other thing called "A troublesome dog."

18059. I understand you to say that you cannot trace it?—When I got this copy I realised that I had been present on February 26th, and then I set work to try and remember a troublesome dog, and all my comments are that I never saw anything of the kind; and now my comment is that I do not believe that that lecture ever took place at the University of London.

18060. So I understand. What you tell us with regard to that is that you do not believe there ever was such a lecture delivered at the University of London. Will you tell us about the 26th of February?—That is the one we have just finished.

18061. But Miss Hageby also puts this down as the 26th of February. In your laboratory book is there any mention of such a lecture?—No, there would not be. It would only be in my memory, and I do not remember the experiment.

18062. Dr. Brodie is down there as lecturer on the 26th February?—Yes.

18063. Is he down a second time?—No, only once.

18064. And you have given an account of that one?—Yes.

18065. Then there is a mistake obviously somewhere—I mean on Miss Hageby's part; but whether it is a mistake altogether for some other place, you do not know?—I do not mean that there is an invention at all. It is simply a mistake.

18066. Now, as to March 12th, what do you say? That was a lecture by Dr. Brodie, I think?—Yes.

18067. And the title is "The only completely satisfactory method"?—Miss Hageby begins by saying: "He has doubtless found the remedy to be curare," on page 129. My comment is, "No," no curare was used.

18068. You are quite sure that there was no curare used there?—Absolutely. So far as my memoranda go, the footnote I put down is, "Incorrect. The animal was under artificial respiration through the chloroform flask, getting between 2 and 3 per cent.," in the usual way.

18069. You wrote your notes, you say, about how long after the lecture?—The lecture was in March, and the book was in my hands in July, that is four months.

18070. You were clear at that time that there was no curare used?—I was clear at that time there was no curare used.

18071. Is there anything else that you have a note of on that lecture?—Oh, yes. There is a remark made on page 134 to the effect that at the end of the lecture one other—the director of the laboratories—came to try the action of betain. I remember that even now, because it was a preliminary trial of betain that was made, and if any point has been made of that I will explain it; if not, I will not detain the Commission. This is germane to the inquiry of the Commission, I think, on page 135: "When an anæsthetic or narcotic has been given to the animal the students are told so, or they see the anæsthesia being kept up by repeated doses of the anæsthetising agent." My note is, "Not necessarily," but on this occasion the anæsthetic was certainly given.

18072. What do you mean by not necessarily?—One does not mention it on every occasion. At every turn the animal is anæsthetised, but it may often happen that one does not mention it.

18073. When it has been given, the students, she says, are told so?—Sometimes.

18074. She does not say that the students are told about every fresh dose?—No.

18075. "Or they see the anæsthesia being kept up." They would not necessarily see it?—No.

18076. As regards telling them at the beginning; is it usual to tell them?—I really do not know. I should think it might be or it might not be. I do not see any custom. I often do not say anything about it. I take it for granted. I do not think I should necessarily say so myself.

18077. Is it the invariable practice in these operations at a laboratory to give an anæsthetic?—Certainly. I believe that a lecture that I gave immediately after the publication of this book has been put in the hands of the Commission. The first two pages contain my immediate answers to the allegations in this book, but the University was cognisant of it, and took no action, because action was being taken from elsewhere, from University College, and as a matter of fact, the publication of that lecture was delayed for a week or a fortnight, until after a parallel action on the part of University College was over, on the part of Dr. Bayliss.

18078. We have that pamphlet already, have we?—Yes.

18079. There is only one thing, I think, that I want to ask you. You have told us that no operations have been performed in the laboratory except upon animals that were under complete anæsthesia?—That is so.

18080. And that they were so kept?—Yes.

18081. But some witnesses, and I think Miss Hageby amongst them, have drawn a distinction between light and deep anæsthesia, as if light anæsthesia meant that the animal was subject still to pain and deep anæsthesia meant that it was not. Is that so?—It is a question of definition. My definition of light anæsthesia and deep anæsthesia implies that there is no sensitiveness to pain in light anæsthesia, any more than there is in deep anæsthesia.

18082. That is a point I wanted to know. We have had it explained by other witnesses to us, and I do not want to take you through it at length?—I can do it very shortly, I think. I gave a definition of it a few months ago that I came across. "The finger-post between this first stage and the next is quite clear; if when the conjunctiva is touched the eye winks, the

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anæsthesia is 'light,' if the eye does not wink the anæsthesia is 'deep.'" That is my definition of light and deep anæsthesia, which I gave in the lecture.

18083. The point I wanted to know is, is the use of the word "light" or "deep" any indication whatever of any degree of pain?—None. In light anæsthesia all sensation and voluntary motion are lost. In deep anæsthesia the movements are more profoundly affected; the eye does not wink in reply to a touch on the conjunctiva.

18084. So that the use of the expression "the animal was under light anæsthesia," would not convey to anyone who understood the use of the word in anæsthetics, any suggestion that the animal was suffering pain?—Not to a well-instructed person who understood the use of language.

18085. As the word is used by surgeons or anæsthetists?—Yes.

18086. Is there anything else you wish to state to the Commission?—In answer to Question 7234, Miss Hageby refers to my "Exercises in Practical Physiology," and says that I give a caution in regard to the use an anæsthetics on page 47: "Unless the animal is too deeply anæsthetised, salivation is acceleration. I wish to state that it is not a caution, but an absolute statement of fact. The only implication contained in the statement is that the animal is liable to be too deeply anæsthetised.

18087. (*Sir William Church.*) I should like to ask you one question, not on the subject of "The Shambles of Science," but you have paid great attention to the administration of chloroform to both human beings and animals, have you not?—Much less in the case of human beings, but I make no distinction really.

18087A. We have had some evidence given before us by a witness, who said that the fall in blood pressure which takes place on the administration of chloroform is produced really by asphyxia and by that alone, and not by the chloroform itself. What would be your opinion upon that point?—I should say that chloroform of itself tends to produce a fall, but that asphyxia tends to produce a rise.

18088. I think the witness's view was that you only got a fall in the blood pressure when you got the respiration interfered with; is that so?—You have respiration interfered with and the heart interfered with, and the respiration activity diminishes and the heart activity diminishes, and the vasomotor centre relaxes, and you have a fall of the pressure and diminution all *pari passu*.

18089. You have no hesitation in saying that the action of chloroform does at times diminish the blood pressure?—No, I have no hesitation in saying so.

18090. Quite apart from any connection with interference with the respiration?—Yes, I should say so.

18091. (*Sir William Collins.*) Did the Senate of the University of London pass a resolution in favour of giving evidence before this Commission?—Yes, I think so. A vote was taken.

18092. What witnesses were to appear on behalf of the University?—Dr. Bradford, Sir Edward Busk and myself.

18093. I think you are the only witness appearing on behalf of the University?—I understood that Dr. Bradford was appearing on behalf of the University in respect of the Brown Institution, which is an institution under it, and I am appearing an behalf of the University in respect of the physiological laboratory, which is an institution under it. That is the status, I believe, respectively of Dr. Bradford and myself.

18094. Was a letter written by Sir Arthur Rücker to the Under Secretary of State for the Home Department in reference to statements contained in the work by Miss Hageby and Miss Schartau?—Yes, in reply to a communication from the Home Office.

18095. Would you desire to put it in?—I mentioned it in my *précis*.

18096. Perhaps you might read the last paragraph but one?—"I may add that when the laboratory was first established, and on several occasions since, I have discussed the performance of experiments on living animals with the director, Dr. Waller. I informed him of my personal wish that no such experiments should be used for the purposes of demonstration except those which were necessary for the instruction of senior students; and I found that this was in exact agreement with his own wishes and intentions. He used the phrase that a vivisection should be regarded as a 'solemn thing.' I have every reason to believe that this view is that in accordance with which the laboratory is being worked." That is the paragraph.

18097. May I take it that it is in accordance with that view that the practice in the laboratory is conducted?—It is so.

18098. You have, have you not, at the laboratory a panel of lecturers containing many distinguished names?—Yes.

18099. And many important publications have proceeded from the laboratory?—A considerable number.

18100. Would you care to make any quotation from the lecture which you have handed in dealing with the particular case of the "Shambles of Science"?—I only refer to it in my *précis* as indicating my state of mind towards the general question, and in reply to Miss Lind-af-Hageby and Miss Schartau's statement in the "Shambles of Science." I would like to place before the Commission what I said. "If the two young ladies under whose name the publication entitled 'Shambles of Science' has appeared, came simply to get copy,' the case is easily understood; but I am unwilling to believe this; and, indeed, the case looks to me much more like that of a pair of very ill-advised busybodies supporting each other through a trying ordeal for the sake of what they imagined to be an errand of mercy. I am sorry they came. I should have told them or any other non-professional women that they had better not come to such lectures, for they would run the risk of being misled and pained by experiments where no suffering to living animals was caused. Indeed, one would look very much askance at the young woman who should be able to look on while an animal was, as she imagined, undergoing torture. For the naked details of even a properly conducted vivisection, ignorantly considered, as must be the case if they are considered at all by an unprofessional person, appear revolting—so do the details of a surgical operation or of the slaughter of an animal for food, or of the proceedings in the dissecting room, and the *post-mortem* room. The minute and graphic account of such details by ignorant women is from every point of view mischievous and deplorable, and I very much regret that these two unfortunate ladies should have obtained the opportunity to torment themselves and their readers by their conscientious misrepresentations. Physiologists, when vivisection is necessary, do not take either pleasure or pain in its naked details, and they systematically take due precautions to fully anæsthetise animals required for experiment. The incredible motives sometimes attributed to physiologists by well-meaning people, and the unlimited adjectives and substantives by which they are held up to public reprobation can only be left to the antidote of their own excess. It is really labour lost to be constantly pleading 'not guilty' to this, that, or the other quite outrageous statement; we can only wonder that well-meaning people can so quietly harbour such infamous thoughts. Nor have we failed to question our own conscience. We well know that physiologists are subject to the common laws governing the human mind, and that habit must tend to engender inattention, and that inattention would be cruelty. We are on our guard against our own inattention, but knowing that we are human, we do not resent as indignantly as might be expected of us the reminder to be on our guard that is contained in the denunciations of our critics. We also know that there is evil in the appearance of evil, that things right and proper in themselves, but offensive of appearance ought not to be exposed. And if exposure by even competent authority must offend and injure the imaginations of ordinary men and women, how much more injury and offence to the public mind will be committed by incompetent and self-deluded women who have entered, what is to their minds, nothing more than a chamber of horrors, where in reality the 'quivering flesh' and 'palpitating heart' and 'tortured nerves' are more often than not only the living parts of dead animals, and where, in any case, an animal, even if alive, has been absolutely deprived of sensation. When one attempts to realise what kind of images must hold possession of an ignorant, prejudiced, and sensitive person—man or woman—who has seen, without understanding, he ceases to wonder at bad language, yet what can he hope to say that shall reach the mind of the deluded fanatics whose pity has been fanned to hatred by agitators. And can it be expected of us that we should say anything at all to persons who can employ sensational literature to poison the wells of human sympathy. In this laboratory—and no doubt in others—our very first concern is to administer anæsthetics properly when anæsthetics are required. And I make the deliberate statement that animals in this laboratory are anæsthetised with as great certainty and accuracy as are the patients in any hospital in the United Kingdom. Can it be necessary to tell you that we do not juggle with bottles of 'colourless and odourless' liquid as imagined and stated by the two ladies who have visited these 'shambles'? You shall see for yourselves what takes place outside the lecture room whenever it is necessary to anæsthetise an animal. I have chosen cats for my demonstration, since the demeanour of these animals is most familiar to us."

18101. May I ask whether the evidence tendered on behalf of the University of London has been submitted to the Senate?—No, I put it to the Physiological Committee.

18102. Is there anything that you desire to add on behalf of the University of London?—No.

18102A. (*Sir Mackenzie Chalmers.*) Where is the laboratory at which these experiments were conducted? —At the Imperial Institute, on the top floor.

18103. Is it specially under your supervision?—Yes.

18104. Is that laboratory only used for demonstration experiments, or are there research experiments carried out there also?—Research experiments are carried out there as well.

18105. Under your supervision?—Yes.

18106. How long have you been engaged in research and demonstration experiments?—At that place or generally do you mean?

18107. Generally?—Twenty-nine years.

18108. And at this laboratory?—Five years.

18109. May I take it from you generally that as regards the experiments witnessed by Miss Hageby and referred to by her, you are satisfied yourself that the animals were properly anæsthetised and kept under anæsthetics until death?—Certainly.

18110. And that any movements which she saw, and such movements may no doubt have been seen, were reflex movements and not conscious movements of pain?—Yes.

18111. I suppose you have a good deal of experience of physiological laboratories?—Yes.

18112. May I ask whether you are a physiologist only or a practising physician also?—I am a physiologist. I do not practise.

18113. But you have medical qualifications?—Yes; in fact, I have practised formerly.

18114. Now you are wholly a physiologist?—Yes.

18115. Then I can ask you two questions that I want to ask. In the first place, we have been told by a witness that when artificial respiration has to be kept up, and for that purpose tracheotomy is performed on the animal, the tracheotomy is performed before any anæsthetic is administered, is that so?—Not in my practice, most certainly.

18116. Are you aware of any laboratory in England in which tracheotomy would be performed on an animal before the administration of some anæsthetic?—I know of none.

18117. That practice has never come before you?—No.

18118. Would you approve or disapprove of it?—I disapprove of it.

18119. So far as you are aware it has not been done, and it would be both wrong and illegal?—Yes, I think so, certainly.

18120. A great many physiologists now, as you know, are not medical men?—There are such physiologists, distinguished physiologists, even.

18121. And some very distinguished physiologists?—Yes.

18122. As regards the licensing of what I was going to call beginners, who are not medical men, is there not a difficulty. In the case of a medical man you have the guarantee of his profession; but in the case of a man who wishes to begin experimenting as a physiologist have you any such guarantee?—But surely he would be vouched for by some person of experience? I should imagine so. I should think it would always be the case.

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18123. That is what I wanted to ask?—A young man would not be licensed in a laboratory to work by himself generally. He would be under the supervision of the director or professor of the laboratory if he were a beginner.

18124. Before his application for a licence was signed would there be sufficient opportunity for making inquiry?—Surely; it has to be endorsed by certain professors and authorities. That works all right, I imagine.

18125. You think that there would be no more difficulty in examining his qualifications than in the case of a medical man?—I do not think there would be. I should not make any distinction for that purpose.

18126. Are there any students of your own studying wholly as physiologists and not men going to take medical degrees?—I have, so to speak, no students directly. Everybody in the laboratory of the University of London is more in the nature of a colleague than a student. The students come to lectures, but I simply see them at lectures. That does not make them practical students of mine.

18127. They are not under your tuition in any way?—No, they are not.

18128. (*Mr. Ram.*) With regard to any of these experiments of which you have told us, and at which you were present, was any curare used?—No; there was no curare used to my knowledge at all right through.

18129. Have you had experience of the use of curare?—Yes.

18130. We have been told that, according to the belief of some witnesses, it is impossible to sufficiently anæsthetise an animal under curare. What do you say to that?—I have no experience of curare in combination with an anæsthetic. But are you asking me whether an animal immobilised by curare could be anæsthetised?

18131. Yes?—It could be.

18132. Have you ever known curare administered without anæsthetics?—Never.

18133. By an anæsthetic we mean, of course, one producing complete insensibility to pain?—Certainly.

18134. (*Chairman.*) I think the question asked was whether, if you gave an anæsthetic to an animal which had been curarised you could have an assurance that it really was under the influence of the anæsthetic?—Certainly.

18135. You could always ascertain and know whether you had it completely anæsthetised or not?—It is immobile, so is a plant immobile, but I know if you give it 2 per cent of chloroform vapour it is of necessity under the influence of the chloroform vapour.

18136. At any rate, you think that you could always satisfy yourself if you were giving an anæsthetic to an animal under curare that it was completely under the influence of the anæsthetic?—I should be satisfied. If artificial respiration is going on with the percentage of chloroform vapour, I should be satisfied.

18137. That is what would satisfy you?—Yes.

18138. (*Dr. Gaskell.*) Because you know that that percentage of chloroform vapour would always keep an animal of that size completely under the anæsthetic?—Yes, and I know that I am on the safe side as regards pain to the animal, and on the unsafe side as regards the life of the animal. I am more liable to lose the animal than to let it out of anæsthesia.

18139. May I take just a question with regard to this attack of Miss Hageby with regard to the dog injected with the substance derived from a lunatic, because she says in answer to Question 7507 "The method of killing is really what is mostly criticised in this chapter besides the nature of the experiment—that the assisting demonstrator did not even take the trouble to see that the animal was really dead," and then she goes on to say in reply to Question 7509: "The demonstrator attached a glass tube to the open jugular vein, and first we were told that the animal was to be killed. Then he blew a little air into the jugular

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vein and then sat down, so far as I remember, among the audience, and the lecture went on and the animal was supposed to be dead; but after a minute or two the dog began to struggle and to show signs that it was not dead, and for some time nobody observed it." "(*Sir William Church.*) In what way did it show signs that it was not dead? (A) By struggling. 'It has begun to struggle and seems to be in great pain, at last the lecturer happened to look at the "dead" dog. He says something to the assistant demonstrator, who leisurely rises from his seat, and strolls up to the animal.'" Do you remember anything of all that?—I do not remember the details. They do not sound to me very likely, but what I should imagine might be the explanation of what she saw, or thought she saw, would be that with air in the veins convulsions happened, and in order to make sure, as any person would do, as I should do under such circumstances, the lecturer said "Kill the animal," meaning simply to put the scalpel into the medulla. That would be one's natural action if one saw a convulsion on the part of an animal that had been so treated. One would oneself put a scalpel—I would have done it myself—into the medulla.

18140. Is it not a common thing by that method of blowing air into the jugular vein for struggling to take place?—Yes, it is common, of course, to have struggling take place when the air is in the vascular system.

18141. It is of the same nature as the struggles of asphyxia?—Or anæmia.

18142. (*Mr. Ram.*) There is no sensibility to pain?—Absolutely none. There is just one point which occurs to me which I should wish to mention about the marmot, as I have seen all about this marmot, and an extremely interesting point it is. It was a marmot coming out of its winter's sleep, and coming out of its winter's sleep the front half of the marmot gains its temperature in about one hour before the back part of the marmot, and that marmot came out of its sleep, and was fully awake as to the front part of its body and still asleep as regards the hinder part of its body, and bit the demonstrator.

18143. (*Chairman.*) That is at the time of the lecture?—At the time of the demonstration. I have seen a great deal about it in the evidence before the Commission, and it puzzles people that an animal—it puzzled Miss Lind-af-Hageby quite honestly no doubt—should normally for a time be half-paralysed; but, as a matter of fact, it is an extremely interesting phenomenon. If you study the temperature curve, given by those who have studied it, there is an interval of about an hour while the animal resembles a paraplegic animal completely as to its hind legs just as if it were paralysed, whereas the front end was all alive—so much so that it bit the hands of the demonstrator, a pretty good bite.

18144. (*Dr. Gaskell.*) With regard to the question of light anæsthesia, I should like you as an anæsthetist to tell the Commission how you can tell when a sufficient amount of an anæsthetic has been given, so as to prevent the animal from feeling pain?—One's ordinary quick and practical test is the conjunctival reflex. If the eye winks, more anæsthetic is wanted; if the eye does not wink, it is all right.

18145. You have just told us that that is evidence rather of a deep condition of anæsthesia, but when it is in light anæsthesia, how do you know it? We have been told that there is a very distinct difference between movements that are purposeful and movements which are in the nature of reflex movements?—I should have difficulty in giving you a sharp test while the eye reflexes are present. I suppose one would go by the character of the movement, but then that is not a sharp difference. The differences between the reflex and automatic movements and the convulsions are often very deceptive as indicative of conscious voluntary impulses; but still one would tell by the behaviour of the animal whether the movements were indicative of sensation or not, simply from their general character.

18146. From their general character you think you could tell. As an individual who has given anæsthetics to animals considerably, would you feel any doubt as to being able to tell. One allegation brought here against physiologists is that the physiologist keeps the animal under light anæsthesia, and that really is not true anæsthesia at all. The question has been asked again and again what is meant by light anæsthesia?—In the state of light anæsthesia, one knows that the first movements in the case of a man to disappear are the purposive movements, the obviously purposive movement, and one knows that when that happens there is no sensation; and one judges from the character of the movements that the animal is in an analogous state, having no sensation. Only, as a matter of fact, my practice, and I think the practice of most physiologists, is to go as far as the conjunctival reflex.

18147. Is it not the surgical evidence, and therefore one may say also with respect to animals that when you have once obtained complete abolition of the conjunctival reflex a very considerable time elapses, in which no chloroform is given, before any sensation of pain ever comes back?—Yes.

18148. So that there you have a very considerable time after that conjunctival reflex has been abolished, in which you may be perfectly certain that there is no sensation of pain?—Yes; and I will go further than that. You know that you can keep your animal anæsthetised; that if you have brought it down by 2 per cent. you can keep it there at $1\frac{1}{2}$ per cent., 1 per cent., and a $\frac{1}{2}$ per cent, as time goes on. I have had an animal as long as 12 hours under anæsthetics, and at that time it wants only a very low percentage indeed to keep it under.

18148A. But the practice of all physiologists is to anæsthetise the animal absolutely fully first, so that all conjunctival reflex disappears?—Yes.

18149. And then for a long-continued operation afterwards, smaller doses of chloroform are given, but still you feel confident that the animal is not feeling pain?—Certainly.

18150. (*Mr. Tomkinson.*) Do you consider morphia an anæsthetic?—I should not call it an anæsthetic myself. I should call it a narcotic. It is a question of words again.

18151. Do you consider that the state of stupor arising from morphia or alcohol is a state of anæsthesia?—Not in the ordinary way, but I can imagine the stupor being so profound as to amount to anæsthesia.

18152. You said, I think, that you had no difficulty in keeping animals for a very long period under anæsthesia?—No, none at all.

18153. Dogs?—I have kept dogs under anæsthesia for a considerable time, but my longest periods were with cats. I have kept cats under for a very long time, but dogs I have kept under for three or four hours.

18154. In perfect anæsthesia?—Yes.

18155. You have found no difficulty in anæsthetising dogs?—I have not found any. I have not lost a dog for several years.

18156. The boundary line between insensibility and death is not such a very perilously narrow one?—Yes, it is fairly perilously narrow, but if one administers the anæsthetic strictly quantitatively one can reduce the danger very materially.

18157. But whether the risk of death is run or not, you have no difficulty at all events in keeping the dog absolutely in a state of anæsthesia?—No.

18158. And immunity from pain?—Quite so.

18159. (*Sir Mackenzie Chalmers.*) I want to call your attention to Mrs. Cook's evidence on page 70 of the First Report, Questions 1947-52. What she said was: "I have witnessed some experiments at the Imperial Institute, but only baking and freezing." I asked her "On animals?—(A.) Yes, living animals. (*Sir William Collins.*) Were they cruel experiments in your opinion?—(A.) Certainly.—(Q.) Was there any anæsthetic used in those cases?—(A.) Yes, in one; and it was so difficult to give the anæsthetic successfully that the experiment was given up." Then later on she was asked who the experimenter was, and if you look at Question 2027, she said when "Dr. Waller works there." Have you any explanation that you would like to offer the Commission as to the kind of experiments: can you tell us what they were?—I believe it is the case of the stiff rabbit; it sounds to me like it. My attention is called for the first time to this evidence at this moment, but as I see it, it sounds to me like Mrs. Cook's description of the "frozen rabbit."

18160. (*Chairman.*) Was Mrs. Cook at that lecture?—I have the name of Cook occurring in my attendance book. I do not know what her initials may have

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been. I will look it up. I forget which lecture the "frozen rabbit" was.*

18161. She said she thought it was 1903?—Yes, but I could identify it.

18162. (*Sir Mackenzie Chalmers.*) Perhaps you can tell us this. She was asked at Question 2028, "What animals were used, do you remember?" and she said, "I saw a black cat used, and I saw two cats used and a rabbit"?—I remember the black cat very distinctly; I think she is correct there. I have no doubt it was this experiment.

18163. Can you tell us what the nature of the experiments was; was there any pain involved?—There cannot have been. But I do not identify her description yet. First I jumped to the conclusion that it was the stiff rabbit that she was describing, the freezing box and baking looks like that; an ice-chest you may say is a baking machine if you like to think so.

18164. Was any painful experiment performed?—Oh, no, none was performed at any time, of course.

18165. There was no experiment that would expose the animal to such a rise of temperature or such a fall of temperature as to cause real pain?—No heating experiments are ever made in the laboratory. The only ones ever made were on reducing the temperature.

18166. The baking is imagination?—Pure imagination absolutely, and the freezing was never there. I notice the temperature of the ice chest at the time is noted as 8° Centigrade.

18167. And no animal was exposed to a temperature below the freezing of Fahrenheit?—No, certainly not.

FORTIETH DAY.

Tuesday, 3rd December 1907.

PRESENT:

The Right Hon. The Viscount SELBY (*Chairman*).

Colonel The Right Hon. A. M. LOCKWOOD, C.V.O., M.P.
Sir W. S. CHURCH, Bart., K.C.B., M.D.
Sir W. J. COLLINS, M.P., M.D., F.R.C.S.
Sir J. MCFADYEAN, M.B.
Sir M. D. CHALMERS, K.C.B., C.S.I.
Mr. A. J. RAM, K.C.
Mr. W. H. GASKELL, M.D., F.R.S.
Mr. G. WILSON, M.D., LL.D.

Captain C. BIGHAM, C.M.G. (*Secretary*).

Mr. J. LORRAIN SMITH, M.A., M.D., called in; and Examined.

Mr. J. L. Smith, M.A., M.D.

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18168. (*Chairman.*) You are a Doctor of Medicine?—I am.

18169. What position do you hold in the University of Manchester?—Professor of Pathology.

18170. You have been requested by the University, I believe, to give evidence on their behalf?—I have.

18171. I gather from your *précis* that they have passed a resolution or given you some sort of express mandate as to what they wish you to state on behalf of the University?—That is so.

18172. But, besides representing the University, I believe you have also been requested by the Pathological Society of Great Britain and Ireland to give evidence?—Yes.

18173. Then, perhaps, as regards the University, we had better take their expressed view, and we will examine you more generally as representing the Pathological Society?—If you please.

18174. I think you have in writing the views of the University?—Yes. The University is pleased to comply with the invitation of the Commission to give evidence regarding the experiments carried on in several of its departments. The laboratories of Physiology, Pathology, Pharmacology, and Public Health are registered for experiments on animals. Apart from original research, the experiments necessary for the public health investigations required by the Sanitary Authorities for Manchester and the surrounding districts are carried out in the Public Health laboratories of the University. The work carried on in the Public Health laboratories is highly valued by a large number of local authorities, representing about five millions of people, and Officers of Public Health find the reports issued from them of great value as regards the detection and prevention of the spread of disease. Some of the hospitals also avail themselves of those reports, which are often very helpful in diagnosis. The University authorities have endeavoured to comply with the regulations imposed by the Act to the best of their ability. The laboratories are visited by the Inspector several times each year, without previous warning, and the fullest information is given to him regarding the researches which are being carried on. The Inspector on his part thoroughly investigates and examines all the animals under experiment. Any representation which he may make regarding accommodation for animals, etc., is carefully considered, and, so far as can be gathered, has been hitherto dealt with to his satisfaction. The University authorities consider that adequate care is taken to further those objects which the Act was framed to accomplish, and that the present legislative provision for controlling this type of investigation is sufficient. The University has never had reason to find fault with any of the licensees for the manner in which the experiments have been carried out. In view of the care which has been exercised in conducting this type of work in the departments of the University and of the thoroughness of the administrative control by the Home Office, the University considers that further restrictive measures are unnecessary and undesirable.

18175. You say, apart from original research, experiments necessary for the Public Health investigations and so forth. Are those experiments on animals?—Yes, for diagnosis in the Public Health laboratory.

18176. Does that include experiments by way of cutting operations, and also by way of inoculation?—It is practically all inoculation.

18177. In your laboratory, then, the experiments which are necessary for the Public Health Act are practically all inoculations, we may take it?—Yes, that is in the Public Health laboratory. The Professor of Public Health undertakes that work.

18178. When you say the Physiological laboratory, I thought it was the laboratory of the University of Manchester?—The University has a laboratory of Pathology and of Public Health; they are separate.

18179. But are they both under you?—No; the Public Health laboratory is under my colleague, Professor Delépine.

18180. I was going to ask you whether the local authorities that you speak of lower down have laboratories of their own, because I understand the Corporation of Manchester has one in the University?—They arrange with the University laboratory that this type of work shall be carried out there.

18181. But have any other local authorities there a laboratory; has Salford, for example?—No, it has not a laboratory.

18182. (*Sir Mackenzie Chalmers.*) All the local authorities make use of your laboratory?—Yes.

18183. Do they contribute to the expense; do you charge so much for an investigation?—Yes, there is a scale of charge.

18184. There is a scale of charge for a particular investigation?—Yes.

18185. Could you give us one or two instances of the sort of investigation that is useful?—Diphtheria diagnosis and tuberculosis diagnosis.

18186. Will you tell us how the diphtheria diagnosis is done?—I have given you instances of the vivisection part of it.

18187. Will you tell us how animal experiment is used by the local authority, if it is used?—As a rule diph

* *The witness subsequently wrote: The name M. Cook occurs in the attendance-book under November 19th,* 1902 (*Lecture on Animal Heat, by Dr. M. S. Pembrey*).—*A. D. W*

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theria diagnosis is carried out simply by culture, but in a doubtful case you would inoculate a guinea-pig.

18188. Is there anything else?—Tubercle diagnosis is carried out in cases of milk which are suspected of being tuberculous, or in excretions from the human subject which require diagnosis if it is doubtful. They may be inoculated into an animal.

18189. What number of animals, for instance, are used for public health purposes?—Over 2,000 animals a year are used for this purpose.

18190. For sanitary authorities?—Yes.

18191. (*Colonel Lockwood.*) Do your experiments include inoculations in the eye?—Not for diagnostic purposes.

18192. But for any other purposes?—They might use the eye for investigation.

18193. (*Chairman.*) In the Public Health department do you mean?—Yes, if they happen to be doing an investigation on that subject.

18194. (*Mr. Ram.*) In research?—In research.

18195. (*Sir John McFadyean.*) Have you ever known a case?—No, it would only be for a specific purpose.

18196. (*Sir William Collins.*) In what proportion of cases of diagnosis of diphtheria do you find it necessary to resort to other than cultural or microscopic?—A very small proportion.

18197. Less than one per cent., would you say?—I could not say off-hand how many there would be.

18198. Are you able to speak with certainty as the result of inoculation experiments?—With practical certainty.

18199. What are the results in the animal which you regard as satisfactory in cases where the diagnosis remains doubtful as the result of cultural or microscopic test? — You get the evidence of pseudo-membranous inflammation.

18200. You get the false membrane in animals?—Yes.

18201. What animal do you use?—The guinea-pig.

18202. And in cases of tuberculous diagnosis do you find it necessary for Public Health purposes to resort to inoculation?—Yes.

18203. Is that because you are not satisfied with the finding of the bacillus?—Because you fail to find it in doubtful cases.

18204. Then in cases where you fail to find the bacillus are you able to establish the diagnosis as the result of inoculation with material on the animal?—Yes.

18205. Are there any other cases besides tuberculosis and diphtheria in which for Public Health purposes you find it necessary to resort to inoculation of animals?—Those are the chief ones; and possibly in anthrax, but that is a comparatively uncommon condition.

18206. (*Mr. Ram.*) And tetanus?—You might in tetanus.

18207. (*Sir John McFadyean.*) And meat poisoning?—Yes, meat poisoning; but these are things one comes across very occasionally.

18208. (*Sir William Collins.*) In the course of the past year have you had occasion to resort to inoculation of animals for Public Health purposes for other than the diagnosis of tubercle and diphtheria?—It is not my department, of course; I could not answer it without reference. I can add the answer afterwards by consulting Professor Delepine.

18209. (*Sir Mackenzie Chalmers.*) The animals used are guinea-pigs and rabbits chiefly for Public Health purposes?—Yes.

18210. (*Dr. Wilson.*) In injecting into a guinea-pig to test the diphtheria culture would you inject it into the peritoneum?—You might inoculate the throat.

18211. Is that often done?—No, not very often.

18212. Then where is the false membrane found; only when the inoculation is into the throat?—Yes.

18213. You would not expect to find it in the peritoneum?—You would get an inflammation that would not correspond closely to the mucous membrane inflammation.

18214. And what experiment do you carry out in your own department?—Investigation chiefly; I might have a diagnosis, but it is very rarely that that is asked for.

18215. You have conducted a good deal of original research, have you not, at Belfast?—Yes.

18216. And amongst those original researches, researches into conjunctivitis?—Yes.

18217. Those were inoculation, I suppose?—Yes, practically.

18218. Would any anæsthetic be used for the inoculation?—It was self-inoculation really; it was not inoculation in the sense of taking a definite bacillus or streptococcus of a known virulence; it was rather an accidental inoculation which might arise from merely wounding the surface.

18219. Introducing a foreign body into the eye?—In a wound of that sort.

18220. Would not those experiments be attended by a considerable amount of pain?—So far as I could observe there was no great pain.

18221. Did the eyeball slough?—No, never—only a little conjunctivitis. The aim of this set of experiments to which you refer was to avoid severe effects on the part.

18222. (*Colonel Lockwood.*) What happened to the animal after the experiment?—To which animal do you refer?

18223. This animal that you are referring to that was operated on in the eye. Was it destroyed?—Yes. The eye was examined.

18224. How long afterwards was it destroyed?—It was kept for a period, I forget exactly how long—I think a month or two afterwards. Then they were examined microscopically to see if you could discover any trace of inflammation or any degeneration, or even the presence of microbes.

18225. (*Mr. Ram.*) Examined after autopsy?—Yes, but it was all negative; we discovered nothing really by that method.

18226. (*Chairman.*) Is conjunctivitis very painful?—It may become painful, and in severe cases it is painful; but very frequently it is not painful.

18227. (*Sir William Church.*) With regard to Public Health work, could you tell us whether, in the event of suspected ptomaine poisoning, there is any other method by which a result can be arrived at so sure as that of animal experimentation?—No.

18228. (*Chairman.*) You have told us that you also propose to give evidence to us on behalf of the Pathological Society of Great Britain and Ireland?—Yes.

18229. Who are the members of that society?—The pathologists of the United Kingdom.

18230. That is to say, they are all of them educated pathologists?—There is a membership of about 200, and that, of course, includes physiologists and some anatomists, and also a good many physicians and surgeons who are interested in pathology.

18231. But they are what you might call professional men, either professional surgeons, or physicians, or physiologists, or pathologists who have not taken a degree?—Yes, I think you might say that almost without exception. There might be a chemist elected as a member; I am not quite sure. The basis of election is that they shall have done work which bears definitely on pathology.

18232. And there are about 200 members, you say?—Yes.

18233. What is the object of the Society?—To promote the study of pathology.

18234. That means the study of disease, I suppose?—Yes.

18235. Would you tell us a little more in detail how they follow it out?—The first aim of pathological investigation is to describe the structural and other changes which may be observed in the bodies of those who have died from disease. The tissues of the diseased organs are compared with those of normal organs, and it is found that certain changes are the result of the disease. It then becomes possible to explain how, with the occurrence of these changes, there arise the symptoms which the patient showed during life. Pathology has therefore, with the sciences of medicine or surgery, the common purpose of obtaining a knowledge of disease. It attacks the problem, however, by the exact methods of the physical and biological sciences. The method of investigation by a study of the dead body has been a most fruitful one, especially since Virchow introduced the doctrine of cellular pathology as the basis of investigation, and the working out of this doctrine has been the main occupation of pathologists. For example, the process of inflammation has been carefully studied in order to discover the structural changes which it involves, and by this means much knowledge has been obtained regarding the part played by various kinds of cells. Similarly the processes of degeneration, in which the chemistry of the cell is disturbed, have been the subject of prolonged and careful inquiry, and in the main the observations have been made on the dead body. Till quite recently malignant growth was almost altogether investigated by the study of the structure of tumours obtained from the dead body, or by means of those removed at surgical operations. However valuable the study of pathology on these lines has been, it must be admitted that the method has clearly marked limitations. The tissues which are taken for investigation are no longer living, and therefore our observations are not direct. The evidence we gather concerns what has taken place in the past, but the actual occurrence we do not observe. In the next place a difficulty arises from the fact that we are observing the terminal conditions, and we are left to guess at the nature of the early changes. These conditions may be not only terminal, but also of a secondary nature, and it is of fundamental importance in pathology to distinguish the primary and secondary changes. The primary changes are those which occur when the process of the disease commences, and are directly due to the cause of the disease. It often happens, however, that when once the balance of the tissues is destroyed in the early stages, it is never restored, and in place of normally arranged tissues adapted to each other, and supplying the conditions whereby nutrition is maintained, we may have a progressive disturbance set up which ends only when it has brought about the death of the patient. In this stage of the development of disease the secondary changes may play the chief part. One element of a complex tissue may, for example, be stimulated to overgrowth by an irritant, while the other tissues of the organ are merely damaged by it. As the overgrowth continues, the other elements of the tissue are destroyed. Now since we are studying disease with a view to arrest its progress, it is of the first importance to gain knowledge of the earlier changes. It is during this stage that the opportunity of recovery is afforded. When secondary changes are in progress it is not recovery only, but regeneration, that is required. Recovery may be, and often is, complete, and health is restored, but regeneration is always imperfect. In order to make direct observations in the early stages of disease, we must have recourse to experimental methods. The study of the process by observations on the dead body may afford us certain hypotheses as to the laws governing the phenomena of disease during life, but when we desire to test the truth of these hypotheses we can appeal only to the experimental method. We must reproduce the phenomena and determine the truth of our interpretation of them. The nature of my argument will be most easily seen if I adduce two or three examples as illustrations. I am purposely leaving out of the argument all reference to the bacterial etiology of acute diseases, or to the treatment by bacterial vaccines or antisera. This side of the experimental work in pathology has already been dealt with by Dr. Martin and others. The Pathological Society, in asking me to give evidence before the Commission, desired me to deal with general pathological processes and diseases other than acute infectious conditions, and I have chosen my illustrations with this in view.

18236. This is as to the importance of examining the disease at all its stages in the living body?—Yes. and the fact that the point arrives in the investigation of disease when progress is practically stayed unless one has recourse to experimental methods. Arterio sclerosis is a disease of the walls of arteries of insidious origin, commencing usually at about middle life and gradually progressing until it becomes directly or indirectly a cause of death. It is an extremely important form of disease not only because of its grave effects but because it is so common. It leads to hæmorrhage and softening of the brain, to various forms of heart disease and to a slowly developing form of Bright's Disease of the kidneys. So noteworthy is this condition that it has been said with a rough approximation to the truth, that a man's life lasts as long as his arteries are sound. The cause of this condition is exceedingly obscure. It develops slowly in the course of years and it is therefore difficult to single out one definite factor which can be described as the essential cause. There has been endless investigation of it as it is seen in the dead body. Its various forms have been carefully described, but we do not know its cause, and to gain this information we require to have in our possession some agent which will set up the process. My argument is that this point is reached in all investigations of disease. The means of research which are known have been applied to the problem so far as it can be solved by the study of the dead body, and if we are then possessed of the requisite knowledge we have recourse to the experimental method. Josué made the important discovery in 1903 that repeated injections of adrenalin into the circulation of rabbits set up a form of arterio-sclerosis in the walls of the vessels. This discovery has already led various pathologists to take up the question, and the first paper on the subject from a laboratory in England was published in the Journal of Pathology last October.

18237. (*Mr. Ram.*) October of this year?—Yes. The value of adrenalin for physiology has been explained, I observe, already, and also its value for surgery in arresting hæmorrhage. It promises to be of still greater value, since it may enable us to discover the cause of arterio-sclerosis.

18238. (*Chairman.*) That is, in larger doses than you would administer for the purpose of stopping hæmorrhage, I suppose?—Yes. You apply it locally to stop hæmorrhage on the oozing surfaces. The next illustration which I wish to describe is taken from the pathology of respiratory diseases. Pneumonia is one of the most common of acute diseases, and it may be studied in two ways:—(1) The bacteria which cause it, and (2) the conditions of the patient which render him liable to become infected. The second only concerns me now. We carry about with us in our mouths and elsewhere the bacteria which cause pneumonia.

18239. You mean that we carry them about in a healthy condition?—Yes. Hence the question before the pathologist is not merely to explain where the infection comes from, but the conditions of the patient which permit the pneumococcus to spread from its usual haunt in the mouth and throat and find its way to the lungs, where it sets up inflammation. In order to work out these conditions, one point to which to direct attention is the lungs themselves. Can we find an agent which in itself may be used to cause pneumonia without the aid of bacteria? Such an agent has been recently discovered by the experimental method. It has been found that to submit an animal to an atmosphere of oxygen will, ere long, bring on pneumonia.

18240. What do you mean by an atmosphere of oxygen—pure oxygen?—Yes, pure oxygen.

18241. Can an animal live on pure oxygen?—Yes.

18242. Without any of the other gases that make atmospheric air?—Yes. The simplest possible way therefore of working out these conditions so far as the lung itself is concerned is to place the animal in an atmosphere richer in oxygen than the air we normally breathe. Pneumonia has been very fully studied by the methods of morbid anatomy, and the data which have been obtained are of great value in enabling us to understand the cause of the disease but we have yet much to learn regarding the activity of the lung and its relation to the onset of inflammation. It is at this stage of the problem that experimental investigation becomes necessary in order to test the truth of hypotheses suggested by the data which we have obtained. The third illustration I have taken from a study of derangements in metabolism. The commonest and most important of these is fatty degeneration. When the cells of a tissue are damaged by the action of poisons they very commonly show in their substance the accumulation of globules of fat. Such a change is seen, for example, in cases of poisoning by arsenic; but this disturbance may be due to a large variety of causes. It occurs more or less universally in gland cells, and its importance may be

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gathered from the fact that in the muscular fibres of the heart wall it forms one of the most serious types of heart disease. In regard to this condition a great amount of information has been obtained by means of starvation experiments. It was thought that fat appeared in the cells, say, of the liver in such conditions from dilapidation of the cell substances, but by means of these experiments evidence has been obtained to show that the presence of fat is in many cases to be explained by the absorption by these cells of fat which has been transported by the blood from the tissues under the skin and elsewhere which normally contain fat.

18243. Would you tell us exactly what you mean by starvation experiments?—The starvation experiments which have been used in this case are those where the animal is deprived of food.

18244. What sort of animals?—Dogs and birds, in order to study the kind of fat which is found in the process of degeneration.

18245. What is the process of the experiment; do you starve the animal until it dies?—No.

18246. Or do you take off one or more of its meals as it were?—You deprive it of food until its body contains little or no fat.

18247. (*Mr. Ram.*) Are they kept absolutely without food?—Yes.

18248. With water only?—Yes, with water. You want to deprive them of fat.

18249. (*Chairman.*) You give them no food?—We give them no food.

18250. For how long, for example?—For a week or two, or even for a month, I have seen some experiments recorded.

18251. Will a rabbit or fowl live for a month with only water?—Yes, and dogs also.

18252. Will they live as long as a month without anything but water?—Yes.

18253. At the end of that time will they recover if they are fed carefully?—Yes, they are fed up again.

18254. Do they show any symptoms of pain?—No.

18255. They are unhappy, I presume?—Yes, they are unhappy; there are no particular symptoms. In fact, there is an experiment on record where the milk of a bitch was studied after a period of starvation in this way.

18256. You mean that it had puppies?—Yes.

18257. Was it able to suckle them?—Yes; it was a study of the kind of fat that appeared in the milk.

18258. Do you usually recover animals after a period of starvation?—Yes, for this experiment certainly.

18259.—For this experiment you are speaking of?—Yes.

18260. Your object would not be to kill the animal?—No, they are fed with fat of other animals which is of a different character.

18261. They are fed with fat?—After they have been starved of their own fat. Then you can follow the relation of this abnormal fat to other tissue changes.

18262. That is part of the experiment—feeding them up again with fat from other animals?—Yes, by that means a great deal of light has been thrown on the process of fatty degeneration. Another important branch of this investigation is exemplified by nervous disease. It has been for years a well established fact that one of the earliest changes in nervous disease is a breaking down of the nervous tissue system into fat, which is absorbed by lymphatics and disappears. These changes are found in the dead body in damaged nerves in the spinal cord and brain, and the investigation of them has been carried out by the methods of morbid anatomy with great energy and success. Since it has been discovered that changes of this nature are found in the spinal cord associated with the occurrence of local inflammation elsewhere, as for example, the presence of pus in the pleural cavity, this form of tissue disintegration has been taken up experimentally, and it has been shown that slow absorption of the poisons generated by bacteria takes place along the nerve sheath, and that these poisons on reaching the spinal cord set up fatty changes in the tissue of the spinal cord at the point where the nerve branches off. In other words, we have obtained a new means of attacking the obscure problem of the causation of such diseases as General Paralysis, Locomotor Ataxia and Recurrent Insanity.

18263. Do you mean to say in detecting the approach of the disease?—Yes.

18264. And stopping it?—And ultimately stopping it.

18265. Have you been successful with animals in stopping cases which you believe you saw?—It is rather early to say that, because the research is just in process. I have purposely taken researches which I know are going on at the present time for most of my illustrations.

18266. However, that is the object of the research?—Yes.

18267. And you consider that there is a reasonable prospect of success?—Yes, a very reasonable prospect. Finally, I wish to refer to a disease regarding which we have as yet no knowledge of the causation, rheumatoid arthritis and osteo arthritis. This is a distressing joint disease or group of diseases regarding which a large amount of work has been done, but so far it has been confined to clinical observation of the conditions and a description of the pathological changes in the structure of the joints affected. Quite recently a hospital has been opened in Cambridge for the study of diseases of this type. What in this case is required above all things is the means of experimental investigation of the disease. We do not know the cause, and our resources for arresting the disease are correspondingly small.

18268. Have animals that same disease—can it be created in animals?—Something very similar to it is found in them.

18269. At any rate they are not naturally subject to it, are they?—I could not answer that.

18270. It is not an ordinary disease?—It is merely an illustration, a disease which illustrates my argument that the research is arrested in a sense.

18271. I only wanted to see how far the research was carried on by means of living animals?—So far we have not been able to reproduce it.

18272. Are your researches on this particular group of diseases carried on at all at present by means of experiments on animals?—So far as I know they have not—at least not with any success. The idea of explaining it by bacteria of course suggests that various experiments have been carried out in that way, but there is no definite teaching as yet on this point.

18273. (*Mr. Ram.*) Is it contemplated in this school that is founded now for the investigation of this disease that animals will be employed?—Ultimately they will go on to experimental work. Meanwhile they have merely the hospital for observing and publishing accounts of cases. One volume of reports has been printed.

18274. (*Chairman.*) Are these curable diseases?—No.

18275. (*Colonel Lockwood.*) Have you any objection to the present restrictions placed by law upon operations on living animals?—No.

18276. Do you think that it has placed Englishmen at a disadvantage as compared with other nations?—I do not know that it has very seriously, anyhow.

18277. Can you suggest, or would you have any objection to, any further restrictions as regards, for instance, publicity of the names of those holding licences or a restriction of the number of licences?—You mean to make it more difficult to get a licence?

18278. I will not say more difficult, but to make more public the names of those who hold licences. I will put it in this way: should you have any objection to further restrictions upon the issue of licences?—I think I should be expressing the feeling of pathologists that further restrictions would delay research, and so far they would object to them, I think.

18279. Have you yourself seen many experiments upon living animals?—Yes, a fair number.

18280. Have you ever seen any cruelty connected with those experiments—what I roughly call cruelty?—Certainly not

18281. Have the animals always been under anaesthetics in all the experiments that you have witnessed—not inoculations, cutting operations?—In cutting experiments, yes.

18282. The animal was completely anæsthetised?—Yes, sufficiently anæsthetised.

18283. And the Act always strictly carried out?—So far as I have observed.

18284. As regards the starvation experiments, have you witnessed many of those?—None at all.

18285. Then what were those experiments that you alluded to as regards fat—you said with dogs and fowls—They were not my own experiments.

18286. Nor did you witness them?—No.

18287. Were they carried on in England?—No.

18288. (*Sir William Collins.*) I did not quite gather the point that you were pressing upon the Commission as regards investigations in connection with arterio-sclerosis. Would you be so good as to explain that?—That the investigations into the causation of arterio-sclerosis by the methods of morbid anatomy had practically come to a standstill until this observation of Josué.

18289. Had Gull's and Sutton's work been of value?—Yes.

18290. What have we learnt in addition to that by experiments on animals?—We have learnt that we have got a substance that will cause arterial sclerosis.

18291. Is there anything besides that that we have learnt as the result of experiments on animals on the subject of arterial sclerosis?—They are working out the effect of this substance.

18292. But in addition to the fact that you state that adrenalin is found to be productive of arterial sclerosis, have we added to our knowledge of the subject by experiments on animals?—So far as Josué's experiments are valid we have added to our knowledge.

18293. Does that involve anything more than the statements that you have made that adrenalin is productive of that degeneration?—That gives the means of studying the development of the condition. It is quite a new investigation.

18294. Has it given us the means of preventing it?—Not yet; it is quite a new investigation.

18295. Is that your own?—No.

18296. Did you witness the experiments in connection with it?—No.

18297. Then in regard to the causation of pneumonia, did I correctly understand you to say that the submission of the animal to an excess of oxygen contributed to the production of pneumonia?—It causes pneumonia itself.

18298. Apart from any organism?—Yes.

18299. That pneumonia can originate without bacterial cause?—Yes.

18300. Is that an example of inflammation without bacteria?—Yes.

18301. And is pneumonia in man also of non-bacterial cause?—No.

18302. Never?—The usual pneumonia is caused by bacteria.

18303. But is there any case in man of an analogous disease process called pneumonia similar to that which you can produce in animals by the administration of an excess of oxygen?—It is conceivable that in the caisson disease you might have a certain element of oxygen effect of that sort.

18304. Is the result of your investigations to suggest that oxygen ought not to be employed in the treatment of pneumonia?—I have debated that point with physicians like the late Dr. Dreschfeld. It is rather difficult to answer it definitely. I think he did not use it when he saw these experiments.

18305. If oxygen produces pneumonia, the use of oxygen in pneumonia would be rather a homœopathic remedy would it not?—Yes.

18306. We have been told by a previous witness that the use of oxygen in some respiratory diseases had been the outcome of experiments on animals. Is the result of your research to suggest that oxygen ought not to be employed in respiratory diseases?—That is a debatable point as to how far you should go in using oxygen in acute pneumonia.

18307. It implies a caution?—It implies a caution. I go that length.

18308. And that is a very recent research, I understand?—Yes.

18309. When was it published?—In 1899.

18310. Did I correctly understand you to say that morbid anatomy had been of great value in arriving at conclusions in some of the cases to which you referred; I took down your words to that effect?—Yes.

18311. You do not wish to disparage the importance of morbid anatomy as a means of research?—Not in any way. I merely wished to indicate that the natural development of the study of any disease was to commence with morbid anatomy, and that the experimental work was required to test the conclusions.

18312. So I understood you. Then you indicated certain researches that you thought had a useful bearing upon the cause and possible prevention of general paralysis. Do you then accept the bacillus paralyticans as the cause of paralysis?—The experiments that I have seen myself were done by a variety of toxin-producing microbes; the bacillus coli was tried and Gaertner's bacillus and a mixture, I think, of bacilli.

18313. What was the result of those investigations?—That you get definitely degenerative lesions in the spinal cord at the point where the nerve emerges.

18314. Does the evidence point to spinal paralysis being a bacillary disease?—No, on this particular part probably does not; but there has been a large amount of evidence published lately to connect it with a definite bacillus, the diphtheroid bacillus. This investigation was rather to show that experiments have found out a mode of introducing the toxin into the nervous system in a form in which it has local effects.

18315. May the toxin be of various kinds?—Apparently, so far as I have seen the experiments, it may be of various kinds.

18316. Am I right in thinking that the bacillus paralyticans has been alleged as the cause of general paralysis?—Yes.

18317. Is the result of your investigations to prove it or to disprove it?—We have not worked with that particular point before us, as to whether that particular bacillus is the cause or not.

18318. Have you been able to produce the results that you identify with those of general paralysis without the use of this paralyticans bacillus?—Yes. I should say that we have, with ordinary bacilli such as you find in the intestines, been able to produce effects of this order.

18319. And with poisons derived from other than bacillary sources have you tried the same experiments?—No.

18320. Would you tell us where those fasting experiments on dogs were performed?—They were performed by Rosenfeld and Lebedeff.

18321. Where did they work?—That I am not sure.

18322. In Germany?—Yes. Rosenfeld published them in the "Zeitschrift für Klinische Medicin."

18323. When?—I will supply the date. I have not it in my mind at present.

18324. Is that also a recent research?—Comparatively recent.

18325. Have any similar investigations been made in this country?—Not so far as I know.

18326. (*Sir John McFadyean.*) Your own experiments and all those that have come under your observation at Manchester University have been carried out in strict conformity with the Act?—Yes.

18327. Have you any reason to suppose that anywhere else the law is violated in experiments on animals?—It has not come under my observation.

18328. You have seen experiments conducted in other laboratories?—Yes. I began work in Oxford at the Physiological Laboratory. I worked in Cambridge in the Pathological Laboratory. Then I was ten years in Belfast and saw experiments and a good deal of work going on there, both in physiology and pathology; and then I have worked lately in Manchester.

18329. So that as a means of ensuring compliance with the Act, it would be in your opinion entirely un-

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necessary to legislate, so as to give greater publicity to the names of those who are engaged in vivisection, and to give in fuller details for public information the nature of the experiments?—Yes.

18330. You are familiar to some extent, I suppose with what might be called anti-vivisection literature?—To some extent. I have not studied it very far.

18331. Have you read any of the evidence given before this Commission?—Yes, I have read some of it.

18332. You are aware that there is a pretty widespread opinion among the laity that a great deal of painful experimentation is carried out in this country under the Act, especially by physiologists?—Yes.

18333. You do not agree with that?—I have not seen it.

18334. Do you believe that there is painful experimentation carried out?—No, I have no reason to suppose there is.

18335. You are aware that great trouble has been taken to persuade the public that that is so?—Yes.

18336. Would it in your opinion be justifiable to call these attempts misrepresentations of the facts?—Yes, the statements about the cruelty and pain which are inflicted are misrepresentations, so far as I know.

18337. Do you think that that misrepresentation of the facts may be an honest misrepresentation?—Yes.

18338. But that it might be continued and even increased if the laity were provided with details even of painless experiments?—Yes.

18339. Would it occur to you that that would be one serious objection to publishing fuller information with regard to the details of pathological and physiological researches?—Yes.

18340. Might I ask is this oxygen pneumonia a progressive pneumonia; I mean progressive in the sense in which the bacterial pneumonia is progressive, that you can bring it on by causing an animal to respire an atmosphere of pure oxygen?—Yes.

18341. Does it continue and tend to a fatal issue afterwards?—Yes.

18342. And the tissue at the time of death is found pneumonic?—Yes, consolidated, congested.

18343. But germ free?—So far as I have been able to see I have discovered no germs.

18344. I just want to ask you a question or two about these milk experiments which are carried on so extensively at the Public Health laboratories. Am I right in supposing that these are carried out with the intention of taking advantage of the special powers which Manchester possesses regarding tuberculous milk?—Yes.

18345. The object of the experiments and of the tests to which the milk is subjected is to diminish the number of tubercle bacilli supplied to Manchester people in their milk?—Yes.

18346. Do you happen to know whether it is the opinion of the Public Health Authorities there, that the work has been in any respect successful in that direction?—I can judge from the Medical Officer of Health's report, which contains endless references to the work that is carried on.

18347. Approving of it?—Yes, and relying on it.

18348. From your own knowledge, can you offer any opinion to the Commission as to whether it would be of any real value in that connection to merely make a microscopic examination of milk?—I have tried it frequently without satisfactory results.

18349. Why is microscopic examination unreliable when the result of the examination is a negative?—The result of the examination is that bacilli may still be there, although you have been unable to discover them.

18350. Is that because it would be practically impossible to examine even half a pint of milk in six months?—Yes.

18351. Is there a difficulty in recognising the tubercle bacillus with certainty by the microscope in milk taken from a shop?—Yes, there is, you get it in various conditions.

18352. Are there other bacilli that have the same reactions and agree with it morphologically?—Yes, the butter bacilli and others.

18353. On the other hand, do you think that the inoculation test is a very delicate one?—Yes, it seems to be very delicate.

18354. Can you say whether, as a matter of fact, as employed in Manchester it has led to the detection on different occasions of cows which are actually the subject of tuberculosis of the udder?—Yes, I believe so.

18355. (*Sir Mackenzie Chalmers.*) As regards the inoculation experiments done in the Public Health laboratory, I suppose as soon as the animal unequivocally develops the disease you destroy it?—Yes, so I understand.

18356. It is not necessary to let it die?—No.

18357. You can destroy the animal as soon as the disease unmistakably shows itself?—Yes, say in the glands.

18358. The starvation experiments that you referred to were somewhere in Germany, but you do not know where?—Yes.

18359. In England we have starving men, of course?—Yes.

18360. But by mere observation of them you cannot get the necessary information?—No; you cannot define it; you cannot test it in the same way.

18361. In fact you would want to kill them and make a *post-mortem* examination?—Yes.

18362. What certificates do you hold?—"A" and "B" just now.

18363. "A" is inoculation. Under "B" the animal must be anæsthetised during the experiment?—Yes.

18364. But is allowed to recover?—Yes.

18365. Are you following any particular research under Certificate B now?—I have got the licence, but when I got it I was led off to another investigation.

18366. You are not using it?—I am not using it.

18367. You have experimented under Certificate B?—Yes.

18368. I want to ask you this. When you experiment under Certificate B, do you understand that not only the cutting operation itself, but any subsequent painful part of the operation, such as stimulating a nerve, or sewing up the wound, must be done under anæsthetics?—Yes.

18369. There is no doubt about it in the physiologist's mind?—I should think not.

18370. Are you a practising physician as well?—No.

18371. Your work is professorial?—Yes, and hospital work. I am pathologist to the Royal Infirmary.

18372. You represent the Victoria University. May I ask, do you come here giving this evidence on behalf of the University as a whole, or on behalf of the Medical Faculty?—I represent the Council of the University.

18373. I ask for this reason. In March last we had a witness from the Victoria University, Manchester, Mr. John W. Graham——Not from the University may I be allowed to say.

18374. He described himself as Principal of Dalton Hall in the University of Manchester?—Yes.

18375. Did he represent any Faculty of the University?—No, my evidence is from the Council of the University, which is the Executive Body.

18376. Is Mr. Graham a member of the Council?—No.

18377. (*Mr. Ram.*) What is Dalton Hall?—A hall of residence for students.

18378. Are they taught there?—There may be a little tutorial work.

18379. They study at the University and live at Dalton Hall?—Yes.

18380. (*Dr. Wilson.*) When you speak of the Council do you mean the Medical Council, or the Council of the whole University?—The Executive Body of the University.

18381. Arts?—Arts and Sciences, and Law, and Music, and so on.

18382. (*Sir Mackenzie Chalmers.*) How long have the laboratories been open at Manchester?—I could hardly say that.

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18383. Some years?—Oh, yes.

18384. Do you know whether Mr. Graham made any inquiries there as to what went on in his own University laboratories. Has he made any inquiries from you?—Not from me.

18385. Would it be open to him to obtain information as to what went on if he wished?—I should be very pleased to show him anything he might desire.

18386. So far as you know, no inquiry was made by him as to what went on locally in his own University?—That is so.

18387. I suppose as Principal of Dalton Hall he holds a University position, or does he not?—I could not say offhand the relations of these halls to the University. They are controlled in some way or other, but I think he is appointed by an independent committee—the Governors of the Hall.

18388. One question with reference to what you said about genera' paralysis. You referred to general paralysis of the insane. Is it not commonly supposed that general paralysis of the insane is a sequel of syphilis?—Yes.

18389. Has your work tended to negative that in any way?—Not necessarily. Our work has been rather on the method in which toxins are absorbed so as to localise the lesions in the spinal cord. The toxin variety of toxins may be used for the purpose of the experiments described.

18390. Various toxins might produce the same effects?—Yes, the same degenerative effects.

18391. (*Mr. Ram.*) With regard to these starving experiments you told us that the animals appear to suffer discomfort and so forth?—I am speaking from hearsay only. I have never seen them.

18392. You have never seen a starvation experiment?—No.

18393. Can you tell us what generally happens to the animal at the end? Is it allowed to recover, fed back into life, or destroyed?—It is fed back and then examined afterwards.

18394. Can you tell us of any discovery which has had an actual beneficial effect upon mankind, which has resulted from these starvation experiments?—This of course, bears upon the pathology of fatty degeneration of the heart; that it why I adduced it.

18395. Has the treatment of fatty degeneration of the heart or medical knowledge with regard to it been advanced by these experiments on animals?—Yes, I think it has.

18396. May we put it so high as to say that knowledge has been acquired by these fasting experiments which has been directly beneficial to mankind in the treatment of fatty degeneration of the heart and other diseases?—Directly beneficial in the sense of contributing to our knowledge of these conditions.

18397. Has the treatment with regard to fatty degeneration of the heart, for instance, changed at all in consequence of knowledge derived from these experiments?—It would be difficult to say that.

18398. You do not know that it is so?—I do not know that it is so.

18399. When you say that the animals are brought back to life and are examined, are they examined as live animals?—No, they examine the chemistry of their organs; there is a chemical examination.

18400. The animal is killed?—Yes.

18401. And then by an autopsy the organs are examined?—Yes.

18402. (*Dr. Gaskell.*) Have you read the evidence of the Honourable Stephen Coleridge?—Yes.

18403. I think it would be rather a good thing for this Commission if you would just tell us a bit about that case of Mr. Cecil Shaw to which he referred. His statement is at Question 10491, "Is it now your suggestion that these two gentlemen were saying what is not true, and that Dr. Cecil Shaw did these experiments? (a) That is my suggestion." And then he goes on afterwards to say, "My complaint is with respect to the Irish Government, that they immediately accept what I call the absurd explanation of these two vivisectors." Is it true that Mr. Shaw did experiment without a licence?—No.

18404. Can you tell us what were the facts?—I did the experiments.

18405. Is it necessarily the case that Mr. Shaw implied that he did the experiments when he speaks of "my own work"?—No.

18406. He uses the first person, as though that experimental work had been carried out by himself?—Yes, as if he had done the whole of it.

18407. Will you tell the Commission about it?—The circumstances of the investigation are these. The late Dr. McKowan, an eminent opthalmologist in Belfast, appealed to me to undertake this research. I was not specially acquainted with Opthalmology, but I tried to meet him. The best way of doing it was to associate myself with an opthalmologist and it was in these circumstances that the combination came about.

18408. And what part did Mr. Cecil Shaw take in it?—Of course we discussed the whole situation. He practically did the whole research except the vivisection part of it.

18409. Why did your name not appear in connection with it?—It should have done, I admit; it would have been less ambiguous.

18410-11. Still it was not your research really, although you did the experimental work?—Yes. I was working, of course, at this question and have been working at it since. These experiments which I have just recounted with regard to the absorption of toxins along the nerve sheaths is in the line of this investigation; it has engaged my attention more or less continually ever since.

18412. Mr. Coleridge states here "The letter from Dr. Lorrain Smith, who had a licence, to his very good friend Dr. Shaw, who had not, is as follows. . . . Both of them are dated the same day; they must have met in the street and exchanged them." He implies that you met Mr. Shaw and then in the street casually concocted this arrangement of letters and the evidence. Is there any truth whatever in that kind of suggestion?—No. What else could I do? Shaw had written the paper, and, as I explained in my letter to the Irish Office, "I appealed to him at once for what statement he had to make about this, and I enclose his reply." I do not see the ground for any such insinuation. I have explained the whole circumstance.

18413. And your Irish Home Office were satisfied with your explanation?—Yes, they were satisfied.

18414. The experiments were done in Belfast?—Yes.

18415. (*Sir Mackenzie Chalmers.*) At the time of these experiments Dr. Shaw held no licence from the Irish Government?—No.

18416. But you did hold an Irish licence?—Yes.

18417. (*Dr. Gaskell.*) The returns are sent in by you?—Yes. There is a reference that I do not understand to Dr. Shaw having applied with regard to monkeys and dogs to the British Medical Council on the 22nd July last. I know nothing about it whatever. I left Belfast in 1904.

18418. He called it corroborative evidence that he, Mr. Cecil Shaw, is the person who did those experiments, the corroborative evidence being that he applied to the British Medical Council in July last?—Concerning experiments which happened in 1897; I do not follow it.

(*Chairman.*) I do not think it impressed anybody very much as being corroborative evidence.*

18419. (*Dr. Gaskell.*) You mean with respect to the date?—Yes, I had been away from Belfast for over three years. I was attacked another time about doing experiments without a licence myself in a research on bacteria of the intestines of dogs, although I stated in the investigation that the intestine was removed after the animal was dead. I had the same sort of correspondence with the Irish Office as to whether my licence should be taken away and my laboratory shut up.

18420. Do you consider that these experiments did cause the rabbits a good deal of pain?—No, they did not. We did not cause the iritis; we did not succeed in causing iritis.

18421. So that the jequirity did not cause a great deal of pain afterwards, after the operation was over?—No, it was a very minute quantity of jequirity, just

* *The National Anti-Vivisection Society subsequently wrote (31st December, 1907) that the words "British Medical Journal of 22nd July last" in Q. 10496 should read "British Medical Journal of 22nd July, 1899," which correction Mr. Coleridge omitted to make in revising the proof of his evidence sent to him in June.*

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the least speck of dust on the point of a camel-hair brush.

18422. No inflammation was caused?—No, except conjunctivitis. It was very like an experiment with tuberculin, Calmette's experiment for the diagnosis of tuberculosis in man.

18423. Then the net result of those experiments is negative?—Yes, in no case did we cause iritis.

18424. There is one other question I want to ask you, and that is at Manchester do you demonstrate to students?—I do not.

18425. Are there demonstrations in pathological experiments and experiments involving vivisection?—No.

18426. Do you consider that such experiments might be useful to students?—I do

18427. Would you encourage them?—In pathology?

18428. I am rather thinking of pathology and pharmacology; I was thinking of the course as a whole?—I could quite imagine their being of enormous teaching value. I know how I was impressed the first time I saw a physiological experiment myself. One of the things that stands out in my recollection of Professor Rutherford's teaching almost more clearly than anything else in the experiment of blood pressure.

18429. (*Dr. Wilson.*) You have just mentioned Professor Rutherford. You graduated in Edinburgh University?—Yes.

18430. And, of course, you have often seen his experiments before his class?—Yes, that is to say, I was a year there, and I saw the two or three experiments that he did.

18431. In carrying out those experiments on blood pressure, when it was necessary to keep up the respiration of the dog, for example, by inserting a tube into the trachea, how did Professor Rutherford proceed, I mean as to the anæsthetisation?—I could not tell you what anæsthetics he used. It is no use trying to recollect the details of over 20 years ago. I could not charge my memory.

18432. Was the animal anæsthetised before the tube was inserted into the trachea?—Oh, surely. I have no specific recollection of the point, but the animal seemed to be anæsthetised throughout, and I am sure it was.

18433. That is to say it did not strike you that any cruelty was inflicted during even the severest experiments?—That is so.

18434. And, of course, in all these class experiments the animal was destroyed before recovering from the anæsthetic?—I believe so. I was only a student, of course, amongst hundreds of others.

18435. Then with regard to these experiments which are being conducted to elicit the conditions of arterial sclerosis, is it not generally assumed that experimentation on animals is justifiable in order to discover, if possible, the causation of the disease?—Yes, I think so.

18436. But you could not say that arterial sclerosis is ever caused in the human being by adrenalin?—We are not ready really to say anything about it yet. We are only working it out. We have not yet, properly speaking, defined the form of arterial sclerosis and its causes.

18437. Would you, as a pathologist, say that you can ever induce in an animal the exact disease that you find, even in a diseased animal?—If you mean absolutely identical in every respect, no, I suppose not.

18438. You think not?—Some bacterial diseases are extremely difficult to distinguish, anthrax, for example.

18439. You could not say, then, that this arterial sclerosis which Josué has been able to induce by administration of adrenalin is the same sort of sclerosis that you find in diseased conditions of human beings?—In any case we are not far enough on yet to say so. The first English investigation, as I say, has been published from Dr. Woodhead's laboratory in last October. There are a few others.

18440. Then you say that fatty degeneration of the heart can be caused artificially, or by experiments on animals by starvation experiments?—No, I did not say that. I said that starvation was one of the modes of investigating the nature of fatty degeneration of the heart.

18441. You do not cause it by starvation?—No, you cause it in other ways.

18442. But how is the condition brought about then if after starving animals you find a certain form of fatty degeneration?—You may cause degeneration, say by giving a little phosphorus or arsenic.

18443. But that is not the usual way in which it is caused in human beings?—It is caused there, too. It is a very curious thing that you can cause it in a great many ways. It is caused by lesions, toxins, poisons.

18444. A question was asked you about deciding as to ptomaine poisoning by experiments on animals. May I ask what is the condition of the animal which will enable you to say that a certain food contains ptomaines?—You would kill the animal with the ptomaine.

18445. But what conditions would you expect to see in the animal?—You might even get it infected with the bacillus. There are various forms of bacilli described.

18446. The Gaertner bacillus?—Yes.

18447. Would you expect to find that?—Yes, I have found it in a case.

18448. But you would not expect to find it always?—Not always. I found it in one case that I investigated, and it seemed to have come from meat poisoning, so far as I could make out.

18449. In those laboratory experiments which are carried out for the Corporation, how long would it take to be able to decide in a doubtful case of diphtheria, for example?—I should say within 48 hours.

18450. But supposing that you have to resort to a guinea-pig?—Yes, within 48 hours.

18451. Could you tell from the condition of the guinea-pig within 48 hours that you are dealing with the Klebs-Löffler bacillus?—Yes, I think so.

18452. How long would it take in using the guinea pig to decide whether the bacillus in question is the tubercle bacillus or not?—I think, as a rule, within six weeks. The usual time is about a fortnight or three weeks.

18453. It is now admitted, is it not, that it is exceedingly difficult to dissociate the tubercle bacillus from a number of other bacilli which are found in milk?—There are various ways that would dissociate them experimentally.

18454. The tubercle bacillus would bring about certain pathological conditions in the guinea pig?—Yes.

18455. Animals do not suffer from rheumatoid arthritis as far as you know?—No.

18456. You mean by rheumatoid arthritis, enlargement of the joints?—Yes, and wasting of the structure in the neighbourhood.

18457. But nothing definite has been yet discovered through experimentation with regard to that disease?—No.

18458. (*Sir William Church.*) You have had considerable personal experience in laboratories in which experimentation on animals goes on in Edinburgh and in Oxford, in Cambridge, and in Belfast, and in Manchester?—Yes, I did not have much experience in Edinburgh, except as a student. I have been in laboratories there a little, but not very much.

18459. And I think also you have probably visited other laboratories in this country?—Yes.

18460. We had a witness before us lately who seemed to be under the impression that when an animal was kept under chloroform by means of having a tube introduced into the trachea, and then vapour of chloroform supplied to the animal through that tube, the tube was introduced without the use of anæsthetics; that is to say, the wound was made without the use of anæsthetics. Have you ever seen it done?—Never.

18461. Not in Edinburgh when you were a student?—No.

18462. There is no difficulty whatever in keeping an animal under anæsthesia under those circumstances?—No.

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18463. (*Chairman.*) You, I believe, are Honorary Secretary of the Personal Rights Association?—I am.

18464. What is that Association?—It is an Association for the defence of the personal rights of men and women, of course, and secondarily of animals.

18465. That is what I asked you the question for. In personal rights do you include the personal rights of animals?—No, we should not call the rights of animals personal rights, but we are obliged to take into consideration the rights of animals for this reason, that if we did not recognise that the animals have rights, we should be obliged to consider every prosecution for cruelty to animals as an attack on the personal rights of the person prosecuted; therefore in that secondary way we are obliged to take into consideration the rights of animals.

18466. Has your Association any Hall or place of meeting?—32 Charing Cross.

18467. You have offices there?—We have an office at 32 Charing Cross. I may say that we have been in existence since March, 1871, and we have had quite an illustrious band of vice-presidents.

18468. And how many members have you?—From 120 to 150. We are only a small number of people meeting in an upper room, but I hope that will not militate against our evidence being considered valuable.

18469. It is a question we ask of most witnesses who come to represent particular bodies?—Quite so.

18470. I should like to say first before I examine you, and other gentlemen examine you, that we cannot admit as part of your evidence that pamphlet which is appended in print to your *précis* called "The Ethics of Vivisection"; we cannot print it bodily as evidence; but we shall ask you questions on the points mentioned in your *précis* and if there are any answers that you wish to support by the arguments used in your pamphlet, of course you can use them for that purpose?—I am very sorry for that, because although my *précis*, including the appendix, has been on our shelves for nearly twelve months, I consider that if you would read it through——

18471. I have done so?—I am very much obliged to you, my Lord. I consider that it is almost a sufficient answer in itself to the evidence that was given before this Commission by Lord Justice Fletcher Moulton, that is to say, it anticipates that evidence, and I think replies to it.

18472. I have told you what I propose to ask you; it may be that other members of the Commission will take a different view and go into this matter more than I should do, but I propose to ask you with regard to the first two pages of your *précis*, which, I think, open the whole ground of your views?—Yes.

18473. You are going to tell us what you consider to be the case set up by those who are in favour of vivisection as regards its morality?—Yes, that case is almost—if not entirely—based upon the direct balancing of the pain inflicted on the animal and the pain which the vivisectors suppose to be avoided by the knowledge which they may obtain in that way. I have contended, in the portion of my *précis* which is not to be considered now, and I believe that I have contended successfully, that that is not sound ethics at all—that the hedonistic ethics, which Lord Justice Fletcher Moulton put before you (and which I believe to be sound ethics at the bottom) cannot be applied directly in that way; that you have to obtain from it your *axiomata media*, as Austin contended, and apply them, and that any attempt to apply the hedonistic end directly, by the balancing of the evil and the good, is altogether a mistake and unsound ethics. I could show you that by taking a case. Supposing that a man who has a wife and children at home wanting food goes into a railway carriage, and sees there a bag of sovereigns marked "Rothschild." Shall he steal those sovereigns or not? By direct balancing, the ethical verdict would have to be in favour of his taking them; the loss inflicted upon Rothschild could be a very small matter indeed, while the gain in avoiding the pain to his family would be very great. If you decide that matter in the way of direct balancing, the decision will be in favour of the man committing the theft. The proper way, however, of dealing with the matter is to arrive at some mediate axiom—that of honesty in the matter of property, which would decide exactly the other way. And so it is with regard to this vivisection: if you argue it by way of direct balancing you come to one conclusion; if you argue it by your mediate axiom of morals you come to another conclusion.

18474. I see in your *précis* you state the grounds upon which those who approve of vivisection base that approval?—Yes, there are three grounds.

18475. You have dealt with the second of them just now; that is to say, that the pain or cruelty inflicted is more than compensated by the knowledge obtained which enables the medical profession to lessen pain and loss of life, so that the balance on the whole is in favour of happiness?—Yes.

18476. The first I see you state is that animals vivisected within the scope of British law feel no pain, or at all events not such a degree of pain as to constitute cruelty?—Yes.

18477. But do you contend that if an animal is operated on under complete anæsthesia, and is not allowed to come out of it, that is cruelty?—No, you will find on the next page of my *précis* that I deal with that; that if an animal is operated upon after the sentient life of that animal is completed; that is to say, if the animal is under complete anæsthesia and is not allowed to recover from that anæsthesia, so that it dies in that state, then the operation on that animal although technically during its life is after its sentient life is ended and, as I should view the matter, it is more one of anatomy than of vivisection; it is more properly to be called anatomy than vivisection.

18478. Then as regards the first ground, you say that if every animal that was operated upon was operated upon under complete anæsthesia, that is an answer?—Yes, if we could get any adequate guarantee that the animal is operated upon under a complete anæsthesia, and in the preliminary stages of putting that animal under anæsthesia there is nothing done that is objectionable, and the animal is not allowed to survive, that is to say, is not allowed to come out of the anæsthesia, but dies before it does so, then for all practical purposes that animal has been operated upon after its sentient life is ended, and therefore from that point of view may be regarded as a dead animal and the operation itself may be regarded as anatomy. But I am bound to tell you that I very much doubt whether you will get guarantees for anything of that kind being done—that is the point.

18479. You are not yourself, I understand, a physiologist?—I am not, and I do not propose to give evidence in that connection.

18480. We have had a very large number of physiologists before us, the purport of whose evidence has been that the Act has been enforced fairly and properly as regards anæsthesia; if that is so that would deal with the experiments under a simple licence?—Yes; but I do not share their opinion.

18481. But it does not deal with the cases in which the animal is allowed to recover, or where it is desired that the animal should recover afterwards, and be watched, or with the cases of inoculation for disease?—No.

18482. To those cases you object, I understand?—I do. I would like the Commission to understand that when I say that I should have no objection whatever to an animal being vivisected under complete anæsthesia, and killed or allowed to die in that state of anæsthesia, I do not commit myself to saying that any guarantees which have been proposed up to the present time appear to me sufficient for that purpose.

18483. But you would admit that that is a question of fact, would you not? The Act has been at work for 30 years, and it is a question of fact and evidence as to whether or not it has worked satisfactorily so as to prevent anything in the nature of cruelty?—I quite agree with that, provided that it is distinctly under-

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stood that I am not saying that I consider it satisfactory.

18484. Quite so. May I take it that you do not see any justification for allowing animals to come to after an operation in order to watch the results?—Provided that there were no pain connected with it of any serious kind, I should see no objection even to that; it is only a question of whether the animal is subjected to serious pain in the matter. I see no objection to an experiment on an animal as such. If anybody proposed to try the experiment of whether an elephant could carry a threepenny bit from, say, Trafalgar Square to the Houses of Parliament, I should not object, and if the load were increased I should also not object up to the point beyond which the load became such that the experimenter would be prosecuted under the Act for Prevention of Cruelty to Animals.

18485. And do you object to inoculation, giving an animal a disease, on ethical grounds?—I consider that the whole of the present practice of the propagation of disease through animals is an abomination which will be swept away at some future time.

18486. Would that be your opinion, although you were satisfied that good results for mankind, and the treatment of diseases of mankind, were obtained by reason of those experiments—would that alter your view?—I do not believe that good results can be so obtained.

18487. I understand that?—But even if you could show me some good physical results which could be obtained in that way, I believe that there are other considerations connected with the matter which ought to condemn the practice, and I feel quite sure that, in the future, the medical profession generally will condemn the practice.

18488. That is to say, that you do not think that the saving of a great number of human lives or a great deal of human pain would justify the operations by way of inoculation of giving a disease to a score of animals?—It is the same thing with regard to all questions of morals. This question does not arise simply with regard to vivisection or with regard to some animal disease; it arises with regard to every moral law that we have. Take the question of lying or the question of murder. Supposing that by committing a murder you think you can save several other lives, the same question arises. Take the case of Eugen Hahn, which I have put in that statement of mine, on which, my Lord, you are to examine me. This surgeon, who took a bit of cancerous matter out of a woman's bad breast and inserted it into her healthy breast. Suppose Eugen Hahn could show me that by his doing that to that woman he could save 50 other lives, 100 other lives, or a thousand other lives, I would say, "No, you do not prove to me that you have done what ought to be considered right. The evil to humanity by allowing such things to be done far transcends any medical gain that you could possibly get in that way." I would contend also that—as I have shown there, and shown most conclusively—these experiments are not confined to animals, and never have been confined to animals; they have always been performed on human beings, as well as animals. Any of you gentlemen who are acquainted with this book by Dr. Smidovich will find the eighth chapter of it full of instances, with references to pages in magazines and medical journals, in which the authors of those crimes have themselves put on record the fact that they have committed them.

18489. That case of Eugen Hahn to which you have referred was a case, judging from your account, in which a surgeon whose duty it was to do his best for his patient took the opportunity of poisoning her?—But why is a surgeon bound to do the best for his patient? Suppose that he can, by poisoning a patient, produce a great deal of good for the rest of humanity, and save a lot of lives, on vivisectionist ethics he ought to do it. Why, if it is a question of direct balancing, should it not be done?

18490. You are putting vivisection ethics somewhat differently from the way in which they have been put before us. You have a right to do it, I suppose, in arguing the ethical question?—I think I have a right to put it so. If it is a matter of direct balance, one way or the other, why should not Eugen Hahn do that which he did?

18491. You think that on the principles of the vivisectors he ought to do it?—I think that if they were carried out logically that is what they would do. I think that they dare not, generally, do it; that is to say, they dare not carry out their principles logically.

18492. Your second ground about the balance in favour of happiness you have already dealt with?—I have dealt with that, I think.

18493. Then the third ground which you put into the mouth of the vivisectionist is that vivisection is part of the procedure of science, and moralists have no right to dictate to science, but, on the contrary, should take their direction from it?—Yes.

18494. Is not that rather a controversial way of putting your adversary's argument for him?—Then let me put my adversary's argument in words out of his own mouth.

18495. Who is the person speaking?—I have prepared an answer to that.

18496. Out of whose mouth?—Not merely out of the mouth of one, but I will give you several. The vivisectionist doctrine, as an ethical doctrine, is the primacy of science over morals. In other words, vivisectionists claim, as scientific men, to be unrestricted by moral considerations, or by public opinion. Professor Metschnikoff, at the conclusion of the Harben lecture on the 30th May of last year, after advocating the vaccination of young girls with syphilitic material— with which he had succeeded in infecting monkeys—in order to fit them for a career of prostitution ("Journal of Preventive Medicine," page 459), declared that "morality should not attempt to lead hygiene, but should rather follow her," and that "modern hygiene, having become an exact and infinitely more precise science than it was formerly," ought "to reign supreme over all moralizing doctrines." This statement was met with loud applause and stamping by an English scientific audience.

18497. Were you present?—I have a letter from a person who was present, and I can put that letter in as evidence, if you like. The lecturer was complimented at the close of the lecture, special reference being made to what he had said about morals and hygiene; and amid enthusiastic applause he was presented with the Harben gold medal. Under these circumstances I cannot dissociate British vivisectors from their foreign coadjutors. One of the frankest of these, Dr. Smidovich, in his book, "Confessions of a Physician," page 152, says: "There is but one way out of the dilemma—that of stifling the reproaches of conscience, of choking down pity, and closing one's eyes to the living agony of the animals sacrificed." In his reply to Questions 3737 and 3738 Professor E. H. Starling said: "It is the greatest asset which a nation can have to have among itself a number of men endowed with this "mere curiosity," men who will put everything second to the advancement of knowledge." In "Nature," a fortnight before this was said (6th December, 1906, page 122), Professor Starling wrote: "A clamour has been raised by certain agitators for a restriction of experiments to those which can be shown to have a direct utilitarian object. Such a restriction is impossible. . . . These researches must be undertaken in a spirit of pure curiosity, from a love of knowledge itself." It will be observed that the argument is everything is second to the advancement of knowledge: vivisection is necessary to the advancement of knowledge; therefore everything is second to that for which vivisection is necessary. Dr. de Watteville wrote to the "Standard" (23rd November, 1883): "I think we, as medical men, should not attempt to conceal from the public the debt of gratitude they owe to the *corpora vilia*—for such there are, and will be as long as the healing art exists and progresses." He pleaded that moral and pecuniary support should not be "refused to hospitals on the ground that their inmates are made use of otherwise than for treatment." He contended that "no amount of hysterical agitation and so-called humanitarian agitation will alter the laws of Nature, one of the plainest of which is that the few must suffer for the many. Sentimentalists who think they know better, who uphold the abstract 'Rights of Man,' and want to push them to their logical consequences, have no other alternative in the question now before us than to condemn the modern course of medical studies." And he concludes: "Whilst defending the moral grounds upon which experimental medicine rests, I allow that there are limits, narrow limits, beyond which it would be imprudent or criminal to go. But I must emphatically protest against the tendency of men nowadays—and I am ashamed to observe that a few are to be found within the medical profession itself—who act upon the supposition that the public at large form a proper tribunal to decide upon what constitutes a transgression of those limits. Those alone are competent judges who are able to form a correct opinion on the one hand of the ultimate utility, on the other of the proximate consequences, of any investigation *in corpore vili*." Will your Lordship allow me to add that Lord Justice Fletcher Moulton, in his evidence before this Commission, really sustained the same ground—although he put it in words that are more consonant with English feeling, perhaps—when he said in answer to Question 12783: "'Conscience' is too often used where the true expression should be 'emotion.' Emotion may be a good motive power, but it is a bad guide, and in my opinion there is a very great deal of force in the phrase which somebody has used: 'If you want to do good in a particular way and want to know how you can do it effectively, give your heart a rest and your brains a chance.'" You see conscience is turned into emotion, emotion is turned into the heart, and you have to put your heart away; that is to say, you have to do what Professor Metschnikoff tells you, you have to put your conscience away. That is just what Dr. de Watteville tells you; it is the same thing throughout. The ethical ground of vivisection, I contend, is really a repudiation of morals and a contention that the pursuit of knowledge is to be unrestricted by moral considerations.

18498. You are putting it now in a somewhat different way from that in which it is put in your *précis*. You say there that the argument is that vivisection is part of the procedure of science, and moralists have no right to dictate to science, but, on the contrary, should take their direction from it?—Those are almost exactly Professor Metschnikoff's words.

18499. That is not the view which has been taken by the great bulk, I think, of the witnesses who have been before us?—I am very glad to hear it, but it is undoubtedly the view of Professor Starling; it is undoubtedly the view of Professor Metschnikoff, and, so far as I can see, it is the view also of Lord Justice Fletcher Moulton.

18500. (*Colonel Lockwood.*) You said just now that you had reason to believe that anæsthesia was not always complete; have you any authority for that statement?—My idea respecting that has been gathered from what I have read and from the fact that curare is used along with the anæsthetic. I do not see how you can really guarantee that you have got the animal under an anæsthetic if you use curare at the same time.

18501. Then is it a sort of general statement, for which you have no authority?—My conclusion has been gathered from my own reading. As I have told the Commission, I have not come here to-day to give evidence upon any point except the ethical side of the question.

18502. (*Sir William Church.*) Why do you place a limit upon the rights of animals?—Rights must be limited by responsibilty; you cannot make animals responsible in the same way that you can make a human being responsible. I limit the rights of animals; I also limit the rights of children, and I limit the rights of idiots and lunatics, simply because you cannot make them responsible. And there is also another ground for the limit, namely, the ground of sensibility.

18503. You said, I think, that you thought that we had a perfect right to kill an animal for experimental purposes; that if the sentient life of the animal was dead you had no objection to experiments?—None whatever.

18504. What right have you to kill an animal to make an experiment upon it?—If the animal is a domesticated animal it is kept by us—we keep it, and there is no possibility of our carrying on any such intercourse with animals unless we have certain rights over them. We are bound to kill in order to live ourselves, with regard to a great number of animals. We are bound to kill them even for vegetable food; we cannot get any sort of food without killing animals, and there is no reason why this should be limited to the mere obtaining of food. If we can get any other good from them without inflicting upon them any wrong, then we should undoubtedly do it.

18505. If we have a moral right to kill animals to feed ourselves, why have we not a moral right to kill animals to protect ourselves from disease?—We have such a right; I have never denied it.

18506. Then you maintain that we have a right to kill, but we have not a right to pain?—Not to torture.

18507. What do you mean by torture?—I mean serious pain that is inflicted for some unjustifiable purpose.

18508. Then it is justifiable to feed ourselves, but not justifiable to try and protect ourselves?—Exactly, it is not justifiable to protect ourselves in that way.

18509. I do not quite understand what you said with regard to the propagation of disease through animals. Animals are used experimentally for the diagnosis of disease, not the propagation of it?—They are used for the diagnosis of disease; that is to say, they are given certain diseases for that purpose; and they are also given diseases, not for the purpose of diagnosis, but for the purpose of experimenting on these diseases; and, in answer to a question from the Chairman, I said that I believed that the whole of that practice of the propogation of disease through animals was a wrong thing; that it would pass away, and that the medical profession themselves would come to that opinion. I give that as my opinion.

18510. I still do not understand what you meant by the propagation of disease?—I mean this. If you give a number of animals a disease, I call that the propagation of disease in those animals. Those animals would not take the disease in the course of Nature, in the ordinary way. If you give an animal a disease voluntarily and deliberately, I call that the propagation of disease.

18511. For the sake of a special observation and experiment?—I do not care for what you do it; that is the propagation of disease. I am not in the least denying that vivisectionists, in a large number of instances, at all events, perhaps universally at the outset, are actuated by good intentions. I have never said a single word against that.

18512. Have you any ethical objection to the use of what are now called sera in the treatment of disease?—If those sera are obtained by vivisectional processes, of course, naturally, I object to that, not because they are sera, but because they are obtained vivisectionally; it is only asking me the same question that I have already answered in another form. I would not allow myself, or any child of mine, to be operated upon seropathically. I may tell you, if you are interested in knowing it, that some years ago, when I was visiting Paris, I was attacked by some disease, and the doctor there proposed subcutaneous injection. I said—What are you going to inject; if it is serum or vaccine, I will not have it on any consideration. No, he said, it is morphine. I said—You may use morphine, if you like. But I would not allow myself to be operated upon seropathically.

18513. Why not?—In the first place, I do not believe in seropathy——

18514. That will do; you need not go any further.

(*After a short adjournment Sir William Church took the Chair.*)

18515. (*Sir William Collins.*) The paper appended to your evidence deals largely with a discussion which you had with Huxley, does it not?—Yes.

18516. I understand you to agree with Huxley that the field of morality is a reclamation from a state of nature?—Yes, I do hold that, and I hold it to be a very important statement of his, which is a landmark, as I think I have said, in British ethics.

18517. I gather that you think that to take the cosmic process as our moral exemplar is fatal to the ethical ideal?—Certainly.

18518. I understand that you quote John Stuart Mill as in agreement with yourself to the effect that the doctrine that man ought to follow nature, or, in other words, ought to make the spontaneous course of things the model of his voluntary action, is irrational and immoral?—That is certainly my principle.

18519. And you lay emphasis, I gather, upon the evolution of the moral sense?—Yes.

18520. That is to say, that things formerly regarded as not immoral may be held to be immoral to-day, and things regarded as moral to-day may be re-

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garded as immoral at some future date?—Undoubtedly.

18521. I see that you lay stress upon an opinion which you share that our consideration for animals is not so much subtracted from a fixed stock of sympathy resulting in less being available for our fellow-men and women, but is rather the sowing of a crop of tender emotions?—Certainly; I hold that to be a very important principle. Usually it is argued that it is the opposite—that philozoic sentiment is something subtracted from philanthropic sentiment. On the other hand, I think that not only could it be shown *à priori*, but it could be verified from experience that those who are kindest to animals tend to be kindest to human beings also.

18522. You think that it is part and parcel of the same moral sense that dictates our action towards mankind and towards animals?—Certainly; I think that the basis of all our moral actions is sympathy, and that that sympathy ought to be as wide as sentient existence.

18523. In regard to the question of the balance of advantages, the theory put forward by Sir John Fletcher Moulton, I see that you call attention to the judgment of Lord Chief Justice Coleridge in the case of the "Mignonette"?—Yes, that is a very important point. You will notice there that Lord Chief Justice Coleridge's decision is almost the exact opposite to that of Lord Justice Fletcher Moulton.

18524. I see that the Lord Chief Justice then said:—"Law and morality are not the same; many things may be immoral which are not necessarily illegal; yet the absolute divorce of the law from morality would be a fatal consequence"?—Yes, certainly it would.

18525. You agree that there may be things which may be regarded as immoral, which we ought not to punish by law?—Undoubtedly; it would be fatal to attempt to regulate our whole field of morality by law. And I may say with regard to this question of vivisection, that, although I think something may be obtained by law, still I myself rely more upon such arguments as are put forward in the appendix to my *précis* to affect the medical profession, and scientific men generally, and I think that they will gradually come round to the view which I have put forward.

18526. You agree that not everything that is morally wrong need be punishable by law?—Certainly.

18527. And that it may be a question for this Commission to decide, or to advise the Government upon, as to whether vivisection is or is not one of those things which, though you regard them as morally wrong, should yet be not punishable by law?—Yes, I think that there may be some things in vivisection which we may hold to be wrong, and endeavour to discourage, but still we should not be wise to interfere with them by means of penal enactments.

18528. I see that you rather sum up the opinion of Lord Justice Coleridge in the "Mignonette" case by saying that what would otherwise be a crime is not justifiable by the plea of a specific balance of advantage?—Yes, that is my contention throughout the whole of that essay.

18529. We have often had the question raised in this Commission, whether it is right to ride a horse to death to save a human life. What do you say with regard to that?—If it is a question between the life of a horse and the life of a human being, I should ride the horse to death as I would risk my own life.

18530. Is that adopting the balance of advantages argument or not?—I am placed between two alternatives. I have to do one thing or the other; I have to let the human being die or the horse die, and I am bound to take my choice. But I would not vivisect a horse to save a human being.

18531. You think that is a sound position?—I think so. It is one that has been put to me, and which I have to answer just offhand. I think I would take the risk of the horse dying.

18532. Have you not rather put to us that in the case of vivisection there may be a conflict between the objects of science and the dictates of morals?—Yes, certainly there may be.

18533. Do I correctly understand from you that when that conflict occurs morals should be supreme?—Yes, morals should always be supreme over everything; otherwise moral law has no meaning at all.

18534. I did not quite understand how Sir John Fletcher Moulton applied his balance of advantages theory in the case of man, because I see that in answer to Question 12735 he says:—"I unhesitatingly say that our right to experiment on men is extremely limited. In the first place, we have no right voluntarily to allow death to occur, even though the man would be willing to permit it." And he says:—"I do not think that we should have the right for experimental purposes to allow a man to infect himself under special circumstances, even though they would assist science with a disease like syphilis in order to add to knowledge." And again he says:—"I do not think that you ought to lessen in any way the feeling of sanctity of human life." What do you say with regard to that?—I do not think the sanctity of human life would be decreased by a man voluntarily submitting himself to experiment, but it certainly should not be done except under the strongest public guarantees that no wrong is being done, because evidently anything like that would be open to the gravest abuse if the public had no means of verifying what was done.

18535. Do you think it would be sound ethically to say that while the balance of advantages argument is applicable in the case of animals, it is inapplicable in the case of man?—No, that is not the case. If an animal could voluntarily give itself up to be experimented upon, I should see no objection to the animal doing it, but I object to an animal being made a vicarious sacrifice in the way that is done, and I object to a man being made a vicarious sacrifice in the same way. If the woman in Eugen Hahn's case, which I have mentioned already, had said:—"I allow you to experiment with my healthy breast," under due supervision, so that it could be verified by the public, I would see no harm in it.

18536. You draw a great distinction between cases in which the voluntary assent of the patient has been obtained and those in which it has not?—Certainly; voluntary self-sacrifice happens every day on the battlefield and elsewhere.

18537. As Lord Chief Justice Coleridge said in his judgment:—"To preserve one's life is, generally speaking, a duty; but it may be the plainest duty, the highest duty, to sacrifice it"?—Certainly; and as that is so in military matters, I see no reason why it should not be so also in scientific matters.

18538. We have had the name of Pirigoff mentioned before the Commission already. I see you make some reference to his work; do you desire to quote from it?—That is a quotation in my essay from the book of Dr. Smidovich.

18539. What does he say?—Pirogoff says, "In my younger days I was pitiless to suffering. One day, as I remember, this indifference to the agony of animals undergoing vivisection struck me with such force that, with my knife still in my hand, I involuntarily exclaimed, turning to the comrade who was assisting me, 'Why, at this rate one might cut a man's throat.' Yes, much can be said in favour of and against vivisection. There can be no doubt that it is an important aid to science . . . But science does not entirely fill the life of man; the enthusiasm of youth and the ripeness of manhood pass, and another period of life ensues, and with it an inner call for introspection; and it is then that the recollection of the violence used upon, the tortures inflicted on, and the sufferings caused to another creature commence to pull at one's heartstrings involuntarily. It seems to have been the same with the great Haller; so it was with me, I must confess; and in these later years I would never be able to bring myself to perform the same cruel experiments upon animals which at one time I carried out so zealously and with such nonchalance." You will observe that Dr. Smidovich says in reply to this, "None of this can be denied. *Mais que faire?* To renounce vivisection were to place the future of medicine in jeopardy, to condemn us doctors for ever to the uncertain and barren paths of clinical observation."

18540. Is it you or Dr. Smidovich who makes this further observation, "As one of the ablest thinkers on medical matters has said, it is not so much the amassing of further facts that is wanted as the ability to think out the logical consequences of the facts already within our knowledge"?—It was I who said that, and the doctor referred to is Dr. Rablagiati, of Bradford.

18541. Is Dr. Smidovich alive?—I do not know at all. I think he is. This book of his has only recently been published.

18542. What is the date of it?—It is quite recent.

(*Sir William Church.*) It is 1904. It appears to be a young Russian doctor, and it is more a work of medical romance.

18543. (*Sir William Collins.*) Then you made some allusions to some experiments in regard to syphilis?—Yes.

18544. Are they vouched for by Smidovich?—Yes, if you turn to Chapter 8 in his book you will find a heap of experiments on human beings with syphilis, and the references to the medical and other papers in which they were confessed by their authors; and he points out that those experiments in venerology took place because animals could not be experimented upon for that purpose. Since that time, of course, as you all know, Professor Metschinkoff has succeeded in communicating syphilis to monkeys.

18545. Were those experiments made on human beings without their knowledge and consent?—So it appears in Dr. Smidovich's book; some of them with their consent, some of them not.

18546. Is there anything else that you desire to add in reference to the ethical evidence given by Sir John Fletcher Moulton?—I want to point out in the first place that his doctrine is hedonistic, but one-sided; he treats it only from the negative side—namely, salvation from pain. You see at Question 12704 that it is based on direct balancing. We have these words, "He would look at the inflicted pain, which would be to the bad side of the ledger." Those words evidently mean direct balancing. Now this view of direct balancing is argued against by me in detail in the *précis* respecting which I have just been answering, but, in reality, when you come to the after part of Lord Justice Fletcher Moulton's evidence the specific balancing really—or rather I should say the *quid pro quo*—is withdrawn, because at Question 12717 you find him arguing that you should experiment even without that *quid pro quo*—that it should be done as "a matter of pure curiosity," as Professor Starling says. Now this, as I have already said, is exactly the opposite to the view of Lord Chief Justice Coleridge. Lord Justice Fletcher Moulton proceeds by steps. He first says: "I say that if you look round the world the only way in which we can diminish pain is by human action." Well, I suppose it is; the only thing we can do in any direction is by human action; in fact, what we do and human action mean exactly the same thing. But I would point out that science never has any right to dictate human action. However far your generalisations may carry you in science, and to whatever height you may get in what you do in science, your conclusions will always be summed up in the indicative mood. When you pass from the indicative mood to the imperative you pass out of science altogether, and you come to ethics, and the categorical imperative must be decided by ethical considerations. Then Lord Justice Fletcher Moulton said at Question 12708, "I shall trust to show to this Commission that the pole star by which we steer is knowledge." Surely there was no need to demonstrate any such thing to this Commission or to any human being outside of Bedlam. We all agree, of course, that the pole star by which we steer is knowledge. But then he proceeds further on to say that no man who knows anything of science has any doubt that the right way to advance knowledge is by experiment, and then at Question 12716 you have the statement that "The more complex the subject is the more factors there are at work the more essential the experimental method is, and the most complex of all phenomena are those that relate to living beings." I believe I know something of logic, and this startles me as a great novelty, because the fact is really that the more complex phenomena become the more impracticable does experiment become, owing to intermixture of effects, and that is why experiments on animals or on human beings are to a large extent illusory; all sorts of contradictory conclusions are drawn from the same experiment performed by different individuals.

18547. (*Sir William Church.*) Do you make that statement from your own knowledge?—Yes; I am speaking as a logician.

18548. Not as an experimenter?—No; I am speaking as a logician. I am speaking as one who has for a great number of years studied the science of evidence, and who knows the conditions under which knowledge can be obtained. But it must not be supposed for that reason that I am in any way opposed to experiment; that is quite out of the question. As I have said earlier to-day, I am not opposed to experiment, even on animals, under limitations.

18549. (*Sir William Collins.*) Sir John Fletcher Moulton's point was largely that if medicine was a merely observational science it would crawl, but that if it were an experimental science it would advance rapidly?—Yes; I know he said that; he drew a distinction between an observational science and an experimental science, and he said, "You can take the whole range of the sciences, and I would challenge an opponent to name one in which advance, if it has been rapid and striking, has not been through experiment." I suppose it will be allowed that great advance has been made in astronomy and mathematics, and I suppose it would also be allowed that no experiment has helped in that advance.

18550. Did not Sir John Fletcher Moulton say that he thought the science of volcanoes was the only observational science that he knew?—Yes; I did not know that there was a science of volcanoes separate from seismology, but it is evident that that was chosen because it was a laggard piece of science. The fact is that the words "rapid and striking" afford a loophole of escape from any criticism. If they refer to modern medicine it begs the whole question, because those who hold the view that I am putting forward challenge, of course, the advances which have been made in this respect—that is to say, with regard to Pasteurism especially I certainly would challenge it. Then how does Lord Justice Fletcher Moulton prove his hedonistic theory? He gives us first a history of what was done with a dozen guinea-pigs.

18551. Have you been able to find out the origin or source of that experiment?—No; I have not.

18552. I did not get it from the Lord Justice himself?—I observed that you asked him what those experiments were, and that Lord Justice Fletcher Moulton said: "I cannot tell you the name of the doctor because it was not given me, but I heard it long ago in the early days of research in tuberculosis, and I believe it to be true." If that had been brought forward here, not by a Lord Justice, but, say, by some ignorant anti-vivisectionist, I venture to say that you would have laughed very much at such a piece of evidence being brought forward. Then Lord Justice Fletcher Moulton went on to say: "If it was not actually performed it would still hold good as a typical example." A typical example if not actually performed! That is a very curious sort of example. If it is a typical example at all, it is an example of the sort of reasoning that we are expected to accept in this matter. Then further on, at a suggestion of the Chairman, he said: "You may take it simply as an illustration." If we get no evidence that the thing occurred at all, what is the value of it as an illustration? Then there was another illustration that he gave—the illustration of rats in a ship. This is an analogical argument, and, as anybody knows with regard to analogy, the whole of the value of the argument depends upon the justice of the analogy, and there is no analogy here at all. The rats in the ship were not vivisected. I suppose that the passengers, if they were rational human beings, would have been quite willing that the rats should be destroyed, whether they were the cause of the plague in the ship or not; certainly I would, and I do not know any anti-vivisectionist who would take up the ground that Lord Justice Fletcher Moulton there puts forward as that of an objector.

18553. Does that complete all that you have to say with regard to the evidence of Sir John Fletcher Moulton?—Yes, I have looked through the remainder, and I can find nothing in Lord Justice Fletcher Moulton's argument, except what I have put forward—nothing at all. I consider that my essay appended to my *précis* is a complete answer to the main position that Lord Justice Fletcher Moulton took up.

18554. (*Sir John McFadyean.*) I understood you to say that in determining for oneself whether any action was right or wrong one should not endeavour to cast up any account of gain or loss, but should apply cer-

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tain mediate axioms. Will you tell us how you arrive at those mediate axioms?—You arrive at them either by induction or deduction from your main principle of ethics, that is to say, the promotion of happiness.

18555. Is that the greatest happiness of the greatest number?—No, the greatest happiness. I have nothing to do with the greatest number.

18556. The greatest happiness of whom?—Of all sentient beings.

18557. That is the greatest happiness of the greatest number, is it not?—No.

18558. Well, what is it?—It is the greatest happiness.

18559. The greatest happiness of all sentient beings?—Yes.

18560. But supposing that they cannot all be happy, is not that deciding what course of action will bestow the greatest happiness on the greatest number. Distinguish, please, between that and the utilitarian doctrine?—There is no distinction; it is the utilitarian doctrine.

18561. You accept that?—Yes.

18562. Do you only object to the hedonistic principle, because you think the account is cast up wrong?—I do not object to the hedonistic principle. If you read my *précis* you will see that I accept it.

18563. But I understood you to say a minute ago that we were not to apply the hedonistic principle, but mediate axioms?—No, what I object to is not the application of the hedonistic principle, but a certain mode of applying it—applying it in the mode of direct balance of advantages, of pleasure and pain. That is what I have endeavoured to show.

18564. Yes, I understand. You think that supposing there might be freedom from pain there are countervailing disadvantages?—No, if you look at the portion of my *précis* in which I have quoted from Austin you will see that Austin himself tells you exactly.

18565. Perhaps you can explain it yourself?—I would rather read what Austin says. He says, "According to that theory"—that is the hedonistic theory—"our conduct will conform to rules inferred from the tendencies of actions, but would not be determined by a direct resort to the principle of general utility. Utility would be the test of our conduct ultimately, but not immediately; the immediate test of the rules to which our conduct would conform, but not the immediate test of specific or individual actions. Our rules would be fashioned on utility; our conduct on our rules."

18566. So that if vivisectors believe that by vivisection they are promoting the happiness of sentient beings, they are acting logically and in a way that you would commend?—Certainly, so far as our judgment of the men is concerned; but, in judging the morality of acts, motive is irrelevant.

18567. That is all I want on that point. You were asked a question as to the rights of animals, and I understood you to say that these rights were limited by the fact that animals are irresponsible, and you classed them along with lunatics and children?—Yes, for some purposes.

18568. It appeared to me that that was an unfortunate classification, because the fact that the individual is a child or lunatic extends his rights as regards protection from pain. Are the rights of animals extended with regard to protection from pain, owing to their being irresponsible, more or less?—I deny your premise. I deny that the rights of children and idiots are extended.

18569. Well, in what respects are they affected? I am merely quoting, so far as I can, what you said yourself; you said that they were limited?—Yes, they are limited; they are limited by being interfered with. A child is not allowed to do exactly what it likes in the same way as an adult human being is.

18570. That is beside the question. We are discussing pain; we are not concerned with limiting the actions of animals?—You asked me a question with regard to the limitation of their rights, and I am answering that question.

18571. I will put it to you again. You were asked with regard to the rights of animals, not with regard to their limitation, and you said that their rights were limited by the fact that they were irresponsible?—Yes.

18572. How does that bear on the question of causing pain to animals? Does the fact that they are irresponsible limit their rights as to freedom from pain caused by human action?—Yes, it does.

18573. That is what I thought?—A moderate chastisement to a horse would be perfectly justifiable.

18574. But is it more or less justifiable owing to the fact that the horse is irresponsible?—It is more justifiable on account of the horse being irresponsible, and the horse is placed under restraints which would not be justifiable if it were responsible as a human being is.

18575. But that is only pain inflicted for that individual horse's good?—I do not know that it is for that individual horse's good. If I drove a horse and gave it a slight flick with the whip, it would not be altogether for the horse's good, it would be for my good in getting forward.

18576. So that that is a case you would think in which it is quite permissible to cause pain to a horse?—Yes, a very slight amount of pain.

18577. Where do you draw the line; what mediate axiom enables you to draw the line between permissible pain and unpermissible pain, which is not for the animal's good?—You cannot do it in practice.

18578. I am content with that answer?—You cannot do it in practice, and you will find that under the Cruelty to Animals Act (Armstrong *v.* Mitchell, 88 L.T. 870), the question of cruelty is reserved as a question for the judges; that is to say, it must be decided in each case by men who have got some empirical idea of what amount of chastisement shall be allowable and what shall not. In all those cases it is so. Nature is not made up in separate compartments for us.

18579. May it be decided in the same way how much pain it is permissible to inflict on animals with a view to extending useful human knowledge?—Certainly.

18580. May I ask whether you would consider it a greater crime to kill a man or to cause him a considerable amount of non-fatal pain?—Certainly, I should say to kill him.

18581. Does that apply to animals also?—No.

18582. What is the mediate axiom which enables you to discriminate in these cases?—With regard to animals, the killing is a necessity of our life upon earth. No man can help killing animals; we could not help it, as I said previously, if we were vegetarians. The ploughshare, when it goes through the ground to get the corn to make my bread, kills, vivisects, if you like, innumerable earthworms; do what we will, we cannot avoid killing animals, but we can avoid torturing them, and it is avoidable evil that we should avoid.

18583. I am afraid I do not quite follow that. What is the evidence that man must everywhere kill animals?—Can you tell me how to avoid it?

18584. Certainly; I could suggest that if men ceased to interfere with the domesticated animals they probably would die out, and it is only the domesticated animals that I am dealing with?—Yes.

18585. Then, supposing that they had died out here, do you suggest that it would be impossible to maintain existence without them?—No, not without them, but you would be obliged to kill other animals in order to live.

18586. Why? Are you a vegetarian?—No, but that does not matter to the point you are asking me about.

18587. Except that we have had people here who are vegetarians, and who are maintaining their existence well enough?—Never mind; those persons who are vegetarians, as I have explained just now, cannot obtain their bread without killing earthworms, and they cannot obtain their cabbages without killing caterpillars.

18588. So that you freely admit that man is entitled to cause a certain amount of pain to animals in protecting himself, for instance, and in feeding himself?—Yes.

18589. In some of which cases he may be justified in killing them, although his life is not threatened but his comfort simply is threatened?—Yes.

18590. What is the mediate axiom that justifies a man in killing an animal and causing pain in killing it to prevent discomfort, but yet forbids his causing it any pain whatever in order to extend useful human knowledge?—I have never said that he may not cause a certain amount of pain to animals in order to extend useful human knowledge.

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18591. If you admit that then I am quite content?—Very likely you and I would draw the line at very different places.

18592. Yes, I think that is very likely. Is it justifiable to cause pain to one animal in order to secure benefit to another animal?—It might be so. If I could tie up my dog, and cause it a certain amount of discomfort by being in its kennel, in order that another dog should be free from annoyance by it, I should feel perfectly justified in doing that.

18593. I will put a concrete case to you. Supposing that one had a lot of cattle in which a serious disease had broken out, and one had knowledge amounting to certainty, that by taking blood from one of those animals and injecting it into the others, one could save their lives, and save them a certain amount of suffering too (I ask you to assume that that is so), would a man be justified in doing that?—I think so, so far as I can see.

18594. Would you explain what is the difference in principle between that and infecting an animal in order to secure from it some material which would be valuable for treating other animals?—In the first place, there is no absolute certainty with regard to that. You have given me absolute certainty with regard to the other case. There is no absolute certainty in this.

18595. But I ask you to assume in this case also that there is absolute certainty that by infecting an animal which at present is healthy, you can afterwards obtain from it material which would be valuable for saving the lives of other animals. Assuming that to be certain, would it be permissible?—I do not know all the circumstances. I could not decide that offhand.

18596. Then why did you decide the other one so easily?—It appeared to me that that is a perfectly easy case, so far as I can see. You must recollect that it is by no means an easy matter to decide offhand at a Board like this all the cases of casuistry that you might possibly put to me.

18597. Apparently not?—I am quite willing to acknowledge it. I do not in the least deny it.

18598. Do you mean that it is difficult to decide on the spur of the moment, whether it would not upset a conclusion previously arrived at?—I do not mean that in the least. If, instead of asking me these cases now, you were to write them down and give me a little time to think out each case, then I should be able to give you a much better answer.

18599. If I may I will re-state it, and make it absolutely concrete?—I mean to say this, if you take the greatest lawyer in the land and you put him down at this table and then ply him with all sorts of difficult questions of the application of the law he will not be able to answer you offhand. Very often judges take days to consider their verdict upon these matters, and I should say the same with regard to these matters of casuistry.

19600. Would you like me to understand that the question is too complicated for you to answer offhand?—The question is one that requires consideration.

18601. May I call your attention to this statement in your *précis*: "If there is any validity in the vivisector's logic it cannot stay at the lower animals." That is to say, the logical consequence of performing an experiment on the lower animals is to go on to experiment with man?—No.

18602. Well, what does the statement mean?—The logical consequence of vivisection—that is to say, the direct balancing—is that human beings, as well as animals, will be vivisected if there is a direct balance of advantage.

18603. I beg to call your attention to the fact that you yourself suggested it to be logical that they should go on to vivisect criminals and lunatics?—Yes. I do say that.

18604. Supposing that I point out where the flaw is in this reasoning?—I should be very glad.

18605. If there is any validity in the logic of those who justify the killing of animals for human food, you cannot stay at the lower animals. Then why not kill idiots, lunatics, and babes for human food? It seems to me to be just as logical to stop at the one as at the other?—I do not think it is at all logical; at all events, it does not point out any flaw in my reasoning.

18606. Is it not quite logical for vivisectors to start with an axiom which says that one thing may be permissible with regard to one's actions on the lower animals, but not permissible on man?—One may quite do that. But I say that, given hedonistic direct balancing, you cannot stop at the lower animals logically, and I will go further and say that vivisectors have not stopped at the lower animals. If you take Dr. Smidovich's book, the whole of Chapter 8 is full of experiments in which vivisectors have not stopped at the lower animals, and they have not stopped with regard to cruelty.

18607. Does your objection to causing pain to animals extend to the whole of the animal kingdom?—Of course it varies with regard to the sensibility of the animal. One would not say the same with regard to a black beetle as one would with a dog. There is a difference in sensibility. When the question is one of cruelty the sensibility of the animal must, of course, be taken into consideration.

18608. What do you mean by cruelty?—I have already defined it as serious pain inflicted for an object which cannot be justified.

18609. But as applied to pain caused by vivisection it is begging the question, is it not. To call the pain caused in vivisection cruelty is begging the question which is in dispute?—I never called all the pain inflicted by vivisection cruelty.

18610. Which part of it do you call cruelty?—I say that a great part of the pain inflicted by vivisection is cruelty, and if I had come here to give evidence on that portion of the matter, which I have not done——

18611. Then please do not?—I could give you plenty of proof of it.

18612. That is like saying that you have not come here to give it, but you are ready to give it?—Yes.

18613. But you would not offer to give expert evidence on that?—No; I am not an expert on that matter. I should have to give you the evidence of others.

18614. What do you think about the right of man to cause pain in field sports?—I have noticed that questions of this kind have been put to other witnesses—questions designed to show that there are grievous inflictions of pain on the lower animals, in connection with the rearing of animals designed for food, with the slaughter of those animals, with what is called "sport"; and that those who desire to prevent vivisectionist cruelty are inconsistent in not proceeding at one and the same time against all these forms of cruelty, that by eating ordinary butcher's meat they are *participes criminis* in the castration of animals and the horrors of the slaughter-house. I wonder that the argument was not pushed a step farther, and that it was not pointed out to vegetarians that they obtain their bread by the painful extermination of animals. When the ploughshare—which is an instrument used in the procuring of the corn from which bread is made—passes through the ground it cuts up alive innumerable earthworms; and rabbits and other animals which compete with us for the possession of our vegetable food are killed by methods which undoubtedly involve serious pain. Under the conditions of human existence I do not see how we could avoid killing and the infliction of some pain. In so far as, in the matters referred to, the infliction of pain is avoidable, it ought to be avoided. But I decline to pursue the matter farther. I am reminded of the case of the boy who was told that he might have all the nuts in a certain jar, but that he must take them out with one handful. He found, however, that when he took them in his hand his fist was too large to get through the opening of the jar. I would be glad to abolish all forms of cruelty at a single blow; but I would find the matter in hand too large, and would be unable to get my fist through the opening of the political jar. But why commence with vivisection? Because vivisection sanctifies cruelty by investing it in the garb of science, by making knowledge the primary object of pursuit. It is not only hideously immoral in practice, but sets up an ethical theory of the primacy of scientific curiosity, which is ruinous to the ethical ideal, debauching to the intellect, and demoralising to mankind.

18615. Do I gather from that answer that you would approve of field sports, in so far as they are painful, being put down by law?—No; I must decline to go into that.

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18616. Do you mean that you decline to answer?—Yes.

18617. Why do you decline to answer?—That is my answer—simply because I will not go into any further matter now. I am attacking vivisection. I do not want my hand too full to get through the opening in the jar.

18618. But that is not the question. I think one has a perfect right to put questions to find out how far you are consistent?—I do not think you will ever find out that I am not; you may try that as much as you like.

18619. But if you do not answer we cannot find out?—I have given you my answer.

18620. No, you have not, excuse me. I asked you whether you would approve of field sport being put down by legislation in so far as it is painful?—That does not appear to me to be the question.

18621. You decline to answer?—Yes; further than I have already answered.

18622. Do you justify the killing of animals in order to convert their hides into leather?—Yes.

18623. You think that is justifiable?—Yes.

18624. Could substitutes for leather not be found, perhaps to man's discomfort, but still, you would not represent it as absolutely essential that men should be shod with leather?—No, I say that if by progress of dietetics and economic progress of the kind you show that we can get rid of killing animals for food and leather and other things altogether, no one would rejoice more than I should, and I think that the future may bring us to it.

18625. But is any question of right or wrong involved in rearing animals in order to kill them to provide us with shoe leather?—I do not think the thing is carried on. I never heard of it being done.

18626. Do you think that the value of an ox's hide does not enter at all into the agriculturist's calculations?—It does, but the ox is not reared for that only.

18627. It is reared in part for this?—It enters into the consideration, but it is reared for its flesh and its horns, and its hide, and its hair; it is reared for the whole. It would be putting a part for the whole to say that it was reared for the purpose of its hide.

18628. I said, was it reared in part for the purpose of its hide?—Yes.

18629. Is there any ethical side to that question at all?—I do not think that there is, as we are situated now.

(Sir John McFadyean here took the Chair.)

18630. (*Sir Mackenzie Chalmers.*) In reference to Professor Metschinkoff's lecture, were you quoting from the lecture, or were you giving us a summary of what you thought it meant?—It was a quotation. I have the "Journal of Preventive Medicine" here for you to verify it, if you like.

18631. I refer especially to a passage which I did not understand to be a quotation about the inoculation of syphilis?—That is not a quotation.

18632. What is that?—It is a description of a passage in Professor Metschinkoff's book, evidently too short to be a quotation.

18633. I read the lecture myself, and I should not have abstracted it in the same way?—Perhaps not, but I contend for the accuracy of my abstract, and I shall be quite willing to append to my evidence the passage of which it is an abstract, in order that readers may see whether it is a fair abstract or not.

18634. That perhaps might be useful.* As regards the case to which you have referred of inoculation of cancer by somebody named Eugen Hahn, can you tell us who Eugen Hahn was, and where he performed this experiment?— You will see it in Dr. Smidovich's book.

18635. Can you tell us the date and where it was performed?—You will find it on page 125. "In March, 1887, a woman, suffering from cancer of the mammary gland, applied to the surgeon, Eugen Hahn, of Berlin. The performance of an operation was impossible. 'Not wishing to divulge before the patient the hopelessness of her condition by declining to operate upon her, and so as to relieve and reassure her by the psychical illusion of having performed the operation,' Dr. Hahn removed a portion of the tumour of the patient's diseased breast, and transplanted it into the other healthy one" (*handing in the book*).

18636. The object of this experiment seems to have been to prevent the poor woman understanding that her case was hopeless?—No, that was not the object of putting a portion of the cancerous material into the healthy breast.

18637. I am only reading the words of the book: "'Not wishing to divulge before the patient the hopelessness of her condition by declining to operate upon her, and so as to relieve and reassure her by the psychical illusion of having performed the operation,' Dr. Hahn removed a portion of the tumour of the patient's diseased breast, and transplanted it into the other healthy one; the inoculation was successful." It conveys that idea to me?—But it does not convey that idea to me, and if you notice, the book itself from which this is described is called "Ueber Transplantation der Carcin"; that is to say, the book itself is on the transplantation of cancer, and this was an experiment in the transplantation of cancer.

18638. (*Sir John McFadyean.*) Then do you maintain that that is a false suggestion?—No, I do not.

18639. How do you reconcile that with your interpretation of it?—What do you mean?

18640. This represents that the main purpose of the experiment was to reassure the patient; is that false?—That was a reason for performing some operation on her, but it is not the reason for the transplantation of a portion of the cancer from the one breast to the other breast.

18641. (*Sir Mackenzie Chalmers.*) The passage speaks for itself?—I do not think it does in the sense you mean.

18642. Then we cannot get any further. This book was translated in 1904?—It was published in 1904. I do not know when it was translated; it might have been translated in 1903.

18643. I see that the author of the book "The Confessions of a Physician" seems to have two names?—He published the book under the name of Veresaeff, but his real name is Smidovich.

18644. Is anything known about him except that he was a young Russian doctor?—I do not know very much about him. I know that his book has been translated into various European languages, and from the reading of the book, I judge that although he is a very strong and passionate vivisectionist, at all events his truthfulness has been unaffected, and it gives me the idea of a man who speaks very frankly indeed respecting what he has done. That is the idea the book gives me, certainly.

18645. Have we any reason for knowing that any of his statements were personal experiences, and not a sort of medical romance?—I do not think that anybody who has read that book would say that it was a romance; and the reviews that have taken place (you will find one of them, from the *Times*, appended inside the cover of my book), are of a kind to show that it is a serious book.

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* The passage is as follows:—"Only to those persons who are exposed to the greatest risk of acquiring human syphilis could one propose vaccination with an attenuated virus. In point of fact, the hypothesis that this vaccine might provoke more serious complications than true human syphilis is not admissible; at any rate, it is not based on any scientific data. The persons liable to benefit from vaccination would at first have to be selected from among the beginners in prostitution. It is an established fact that these women only in exceptional cases escape syphilis. As almost all of them are, therefore, most liable to acquire this disease, vaccination would be an advantage for them as well as for the men having intercourse with them; but in practice the carrying out of this proposal meets with serious difficulties, since the beginners in prostitution are very young, and almost always minors."

FORTY-FIRST DAY.

Wednesday, 4th December 1907.

PRESENT:

The Right Hon. The Viscount SELBY (*Chairman*).

Colonel The Right Hon. A. M. LOCKWOOD, C.V.O., M.P.
Sir W. S. CHURCH, Bart., K.C.B., M.D.
Sir W. J. COLLINS, M.P., M.D., F.R.C.S.
Sir M. D. CHALMERS, K.C.B., C.S.I.
Mr. A. J. RAM, K.C.
Mr. W. H. GASKELL, M.D., F.R.S.
Mr. J. TOMKINSON, M.P.
Mr. G. WILSON, M.D., LL.D.

Captain C. BIGHAM, C.M.G. (*Secretary*)

Mr. W. E. DIXON, M.A., M.D., called in; and Examined.

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18646. (*Chairman.*) You are, I believe, Professor of Materia Medica and Pharmacology at King's College, London?—Yes.

18647. And also Assistant to the Downing Professor of Medicine in the University of Cambridge?—Yes.

18648. And you come here as representative of the Therapeutical Section of the Royal Society of Medicine?—Yes.

18649.—You have given us several heads under which you propose to give your evidence; we will just take them in order, please. First of all, you propose to deal with the advantages which Pharmacology has derived from experiments?—Yes.

18650. Will you give us your views upon that, please?—Perhaps I might say, first, that I do not want to re-duplicate evidence unnecessarily, and that I understood subsequently to sending in my *précis* that I was specially required to give evidence on anæsthetics, so that if you think this first head is unnecessary, I am perfectly willing to eliminate it.

18651. Speaking for myself, the first of your heads seems to be one on which we have had an adequate amount of evidence, and really you are only saying ditto to witnesses whom we have had before, whose views put the matter fully before us?—That is true, I understand.

18652. I think if you will come to anæsthetics, in which you have had considerable experience, there is a good deal in what you say there that we have heard before, but still there has been so much said about anæsthetics, and especially about the efficiency of particular anæsthetics, or rather of different drugs which are used as anæsthetics to produce complete anæsthesia, that perhaps you will give us now in your evidence about anæsthetics and narcotics?—The first point that I wish to make perfectly clear is that there is no essential difference between an anæsthetic, a narcotic, and a hypnotic. There are two classes of narcotics, the one I am not dealing with. There is the class of drugs acting on the brain, which we call specific; I refer to morphin and hyoscin; I am not dealing with those; I am dealing with those drugs of the alcohol series which act on the brain. That is a very large group including alcohol, chloroform, ether, paraldehyde, chloral, urethane, and many more. These drugs all have fundamentally the same action on the brain, they act in precisely the same way, and their differences are determined entirely by differences in the rate of absorption and excretion. For instance, chloroform and ether are absorbed extraordinarily rapidly, because we administer them by inhalation, and they are absorbed from the huge area of lung capillaries, so that they are taken into the blood almost immediately, and their effects are produced almost at once. And in the same way, when you stop administering these drugs they are excreted equally rapidly, so that the effect is a transient one; as soon as you have stopped, or very soon after you have stopped, administering a volatile anæsthetic, its action ceases. If you administer one of the solid substances like chloral, or urethane, it requires a certain length of time to act; it is not absorbed quickly. Chloral, by the mouth, takes from half an hour to three hours getting into the circulation, and it is being absorbed all that time. Supposing it is taken by the mouth, it passes slowly from the alimentary canal, and when it reaches the blood it is not excreted rapidly, so that you get chloral existing in the blood over a very prolonged period. Perhaps I can make my point quite clear if I say that if chloroform had been a solid substance (of course, it is not), then it would have had exactly the same action as chloral, and if chloral had been a liquid substance it would have had exactly the same action as chloroform. With animals, of course, I personally prefer in many experiments to use urethane (urethane I use mostly myself) to chloroform. First of all, perhaps you give the animal chloroform to get it under quickly; then you give the urethane either intravenously or subcutaneously, or into the peritonial cavity—it does not matter which—and it gets into the blood and produces anæsthesia, which lasts from six to eight hours; and then, of course, the total effect does not pass off probably for 24 hours afterwards.

18653. But with a dose of that kind the animal would survive if it were not killed by some other cause?—Yes, it would survive. Perhaps I may make my point quite clear if I may be allowed to read what is the usual teaching to students at the present time, from a page in my Text-Book on Pharmacology. I say there on page 50:—"Narcotics or hypnotics administered in large doses give rise to complete anæsthesia. Urethane, for example, when given to a rabbit in doses of one or two grains behaves as a mild hypnotic, but if an injection of 100 grains is made, it forms an excellent and complete anæsthetic. Such an anæsthetic would not be suitable for man on account of the prolonged action of the drug, about 24 hours, and on account of the difficulty of just gauging the dose, so that whilst anæsthesia is complete, the medulla retains its activity." Of course, the medulla retains its activity, or the respiration would stop. In the case of an animal it does not matter

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a bit if respiration does stop; we simply do artificial respiration, everything is ready. In a great number of experiments you do give so much anæsthetic that the medulla gives out, and then you simply perform artificial respiration by means of the pump, which is always handy. The term anæsthetic has come to be used in a limited sense for drugs which are very rapidly absorbed and excreted, *i.e.*, the volatile drugs, chloroform, ether, etc. With such the effect only lasts so long as the administration is continued. They are quite unsuitable as hypnotics because they act for too short a time." There is no fundamental difference between the action of chloroform, ether, ethyl-chloride, and ethyl-bromide, on the one hand, and chloral, urethane, chloretone-paraldehyde, etc., on the other.

18654. You have some evidence at the foot of page 5 of your proof as to the doses for rabbits and man in particular; would you just give us that?—The dose of chloral per os or per rectum necessary to produce complete anaethesia in rabbits is one gramme per kilo of body weight; that would mean for a healthy man weighing 70 kilogrammes a dose of over two ounces; whereas the common hypnotic dose for a man is only 10 to 30 grains, or about 1-70th part of this quantity. In dogs and cats smaller doses are given relatively to the body weight to produce complete anæsthesia. In man chloral is also employed as an anæsthetic. Perhaps I ought not to have put it in this way. It is very little use at the present time for obvious reasons, although I give an example where a Frenchman has used it as an anæsthetic for children in 10,000 cases. He gives it in 3 to 10 gramme doses.

18655. (*Sir Mackenzie Chalmers.*) As an anæsthetic?—Yes, as a complete anæsthetic. It causes, as he says, complete anæsthesia lasting from three to six hours.

18656. (*Chairman.*) We used not to go into that. It is not generally used for men?—No, it is not generally used for men; it is dangerous.

18657. (*Sir Mackenzie Chalmers.*) Dangerous to life?—It is dangerous because it might make the medulla give out; and moreover we do not want to keep the men under it in a trivial operation for six or eight hours.

18658. (*Chairman.*) We are not examining into operations on human beings except in so far as they throw light on the question of operations on animals. But is chloral used for animals generally as an anæsthetic?—Yes, chloral is used for animals.

18659. You were going into the experiments of Mr. Bouchut, speaking of what he could give to a child?—Yes.

18660. You can take it rather short?—Then I give the doses necessary in cases of urethane. The dose necessary to anæsthetise a rabbit is 1½ grammes per kilo body weight, and for a dog or a cat about one gramme per kilo body weight. According to this reckoning the dose to produce anæsthesia in man would be about 70 grammes or 2 ozs., but for reasons already mentioned, it is not employed for that purpose.

18661. Then the next drug you wish to mention is morphin?—Morphin or morphia as it is sometimes called, is of course a little different in its action; it belongs to an entirely different group of drugs. These drugs which I have been speaking of up to now act on all the nerve cells in the brain in much the same way, except that in every case they paralyse the sensory nerve cells before the motor—that is a most important point—so that you can have complete anæsthesia whilst you can still get movement; that is to say, on stimulating an afferent nerve you can get movement when the sensory cells are all obliterated from the brain.

18662. Are you speaking of all drugs now?—I am speaking now of the drugs that I have previously mentioned, the alcohol group of hypnotics.

18663. (*Colonel Lockwood.*) Of which morphia is one?—No, not morphia; drugs other than morphia, those alcohol drugs which act on all nerve cells, motor and sensory, but sensory first. In the case of morphin you have a difference. Morphin is what we call a specific drug; it is not allied to other drugs at all. Morphin acts quite differently; it is quite a distinct thing altogether, and it only attacks the sensory nerve cells; so that if you produce complete anæsthesia from morphin you can still get, indeed you do get, very distinct reflexes. You can administer morphin to man in anæsthetic doses, and you can still—I have frequently done it—obtain quite distinct reflexes, although when the man has subsequently recovered he has never felt anything at all.

18664. (*Chairman.*) When you say that you can obtain reflexes, what exactly do you mean?—I mean that if you simply do something quite as trivial as stroking the abdomen you will produce motion; but if you apply what to an ordinary man is a very painful stimulus such as pressing the supra orbital nerve with your thumb you get no effect at all, not even a reflex, and as I say subsequently the man knows nothing about it. So that we do not use (or very rarely) morphin alone as an anæsthetic, not because it is not one, but because it leaves the motor cells active, and the animal is reflex.

18665. You mean that it would disturb the operation by reflex action?—Yes; by movements. The action of morphin on dogs is precisely the same as that on men.

18666. Is it used for dogs?—Nearly always.

18667. Alone?—No; not alone, because it is necessary to give something to cut out these movements. I should have gone on later to say that it is not used for man for that reason. But it is quite commonly used for man now in Germany, when it is given with some drug which also paralyses the motor cells as well as the sensory, and so in Germany, and to a limited extent in England, it is given with hyoscin (the so-called morphin scopolamine narcosis) for operations where chloroform is deemed unsuitable.

18668. In that combination it both produces complete anæsthesia and prevents reflex movements?—Yes. Then there are two other minor points that I should like to draw attention to. The first is the fact that when surgical anæsthesia is once produced the amount of anæsthetic necessary to continue that anæsthesia is very small. Thus, if it is necessary to give 1·5 per cent. chloroform to anæsthetise a dog, the anæsthesia may be continued indefinitely by the subsequent administration of 0·2 or 0·3 per cent., or even a smaller quantity. Of course, that is perfectly well known to every anæsthetist; it is hardly necessary, really, to draw attention to it.

18669. How are you guided as to when you administer the additional doses—by the appearance of the patient or the condition?—The test which is commonly employed is what we call the reflex test; you simply put your finger on the cornea, and if there is a reflex then you know that the motor cells are beginning to recover their activity.

18670. I was asking the question rather with reference to a case, of which I understand there are some, in which curare is given, as well as a complete anæsthetic. How would you know, in that case, when to renew your dose?—Do you mean a volatile anæsthetic now?

18671. Like the one you have been describing—any anæsthetic which would be sufficient to produce complete anæsthesia, and a proper one to administer to a dog. In some cases we have been told that curare is administered in cases where great stillness is necessary?—May I leave that point? I have put a note on curare at the end.

18672. Very well?—The second point is the great value of giving morphin before administering chloroform. This combination produces a very perfect anæsthesia, the quantity of chloroform necessary being much smaller than would be required if no morphin had been given. These facts have been commented on by many, and are universally recognised, so that I will only refer to the observation of Bernard on dogs, and of Nussbaum, of Munich, Rigault and Sarasin, of Strasburg, and Labbé and Curon on men. Then I refer again to the paralysis of the sensory cells; all anæsthetics paralyse the sensory cells before the motor.

18673. Then you come to curare?—What I would like to say about curare is, first of all, to say briefly what its action is. It is not an isolated drug, having this peculiar action all by itself. There are lots of other drugs having the same type of action. It is, perhaps, the one which has the most characteristic action on the motor nerve endings, but there would be no difficulty in picking out a whole host of others that do the same. All this group of drugs paralyses the nerve cells, the brain, and every one of them paralyses the motor nerve endings, and they may all cause convul-

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sions by acting on the spinal cord. Those three facts apply to all of them. Some members of the group have one action well defined, and others another. Thus, nicotine first paralyses the nerve cells, and later the motor nerve endings, whilst hemlock (conium) paralyses the nerve cells and nerve endings almost together. Curare first paralyses the motor nerve endings and later the nerve cells, whilst hemlock (conium)—the poison that killed Socrates—paralyses the nerve cells and nerve endings, roughly, about the same time. I picked those three examples from a group to show the various stages, how one produces its action at one time and another at another time. I mention this to show that even curare given alone is a complete anæsthetic, if enough is given, although we in England, conducting experiments, assume that curare has no action on the nerve cells, and always give enough of some other anæsthetic to completely paralyse the brain. Of course Claude Bernard really started this idea that curare acts on the motor nerve endings and not on the sensory nerve endings or cells. But Claude Bernard's experiments only apply to the spinal cord; he did not prove anything else at all; all that he showed was that the sensory cells in the spinal cord are not paralysed by curare. That was all his experiments meant. None of these other drugs paralyse the sensory cells in the spinal cord in moderate doses. Chloroform, except in the largest doses, does not paralyse the sensory cells in the spinal cord.

18674. But what we are told is that curare is consistent with the animal feelng pain, although it is unable to move, and that that is not the case with chloroform: that if the animal is reduced to apparent insensibility under chloroform it does not feel?—My point is that these ideas about curare originated with Claude Bernard, whose experiments simply showed that the sensory cells in the cord were not paralysed. They showed nothing else at all. Nevertheless, we believe that small doses of curare would paralyse the motor nerve endings before the brain cells were paralysed, but that large doses of curare will paralyse the whole of the brain like chloroform.

18675. But (reducing it to purely lay language) does that mean that the animal would cease to feel pain with a large dose of curare?—Absolutely.

18676. But that with a small dose it might continue to feel pain, though unable to express it by action?—Yes; it would, I think. That is the point I just wished to bring out there—that curare was a member of a certain definite group of drugs.

18677. There is one matter with reference to anæsthetics that I would like to ask you about as it is not in your *précis*. When you are operating on animals, do you have an anæsthetist to assist you, somebody who is skilled, I mean, in the use of anæsthetics?—No, I usually have a box; I put the animal in the box, and pour in chloroform, in the case of cats, and ether in the case of rabbits, and in the case of dogs one has to be more careful, of course, and I give the chloroform by hand very slowly. The whole point about anæsthetising a dog is to go slowly; dogs very easily die of chloroform, but if one goes sufficiently slowly they never die. I never kill a dog by any chance whilst administering anæsthetics to them now.

18678. So far as regards giving an anæsthetic then you do give the animal the best chance; that is to say, you are the most skilled person in your laboratory and you are the person who administers it and reduces the animal to anæsthesia?—Yes.

18679. But as regards the renewal doses which you say may be very small and should be administered at the proper time, of course, whenever necessary, who looks after that?—Usually there are no renewal doses. The animal is placed on the table, and I give it the urethane or chloral (usually, almost always, urethane), and then the animal is anæsthetised and would remain deeply anæsthetised for very much longer, of course, than is required.

18680. I think you told us for six or eight hours just now?—At least that. I have never tried it longer.

18681. (*Mr. Tomkinson.*) How do you give the urethane first?—Inject it into the blood.

18682. (*Chairman.*) And that is what you usually do. And how do you give chloral?—Chloral I do not use much, but I generally give it into the peritonial cavity. Chloral, like chloroform, depresses the heart.

18683. Does it produce the same effect as urethane, complete anæsthesia lasting a long time?—Yes, exactly the same.

18684. So that in almost all operations, I understand that the one dose would be enough, and would not require renewal?—The one dose would be enough.

18685. Then supposing it is an operation in which for one reason or another, the dose requires to be renewed either because of the nature of the drug used for anæsthetising, or from the excessive length of the experiment, or whatever it may be, who does that?—I should do it always.

18686. Would you if you were performing the operation at the moment be able to watch better than or as well as anyone else?—I should watch it the whole time; that is what I should be doing. You watch the experiment the whole time. The preparation of the experiment perhaps takes half an hour for an average experiment. Then when it is prepared your duty is simply to watch and observe.

18687. Supposing, for example, that the managers of the hospital or laboratory in which you were working were to say that they were prepared to give you a skilled anæsthetist to assist you in these operations, would you reject it as unnecessary?—Absolutely. There is no anæsthetist required at all. We are not giving anæsthetics—the anæsthetic has been given. There is one other point about curare which I think is extremely important, and that is that curare is excreted with extraordinary rapidity; so that if you give an animal curare it only remains curarised for a very short time, so quickly is curare excreted that you cannot poison a man by the mouth, because as fast as the drug gets from his stomach into his blood it goes out by the kidneys. I myself have taken a large dose of curare, and have never had any symptoms at all, yet if I had had a cut on my skin and let the curare get in, the curare would, I have no doubt, have paralysed or depressed my motor nerve endings. The explanation of that is that the curare is simply excreted, we know this absolutely, very rapidly. The importance of that is that if you give an animal an anæsthetic plus curare, the curare disappears very quickly, the anæsthesia still existing.

18688. If you give it through the mouth?—No, supposing that I injected curare into a vein, the animal remains curarised, say, for half an hour to three-quarters of an hour. If that animal has got urethane plus curare, the curare action is over in half an hour or three-quarters of an hour, while the anæsthetic action of the urethane continues for eight hours at the least.

18689. But supposing that the animal were not completely anæsthetised, the most painful part of the operation would be without anæsthetics?—But you anæsthetise the animal before you give the curare.

18690. But it is not everybody who is satisfied that the animals are completely anæsthetised, and one has to deal with that. It is suggested that animals are not completely anæsthetised, and at the same time are given curare. In that case, the fact that the curare was worked off in half-an-hour would not be so material because the animal would have suffered the greater part of the pain in that half-hour?—Yes, the only condition in which that could apply would be when giving volatile anæsthetics. Supposing that you were administering chloroform with curare, then it might be said that you are not giving enough chloroform. But you can see whether you are giving enough by looking at the blood-pressure. The blood-pressure is just as delicate an indication of the action of chloroform as the reflexes.

18691. (*Mr. Tomkinson.*) For how long do you take the blood-pressure?—It is always taken.

18692. During the whole operation?—During the whole operation, at least during the whole of my operations. I am speaking of my own operations.

18693. (*Chairman.*) By what is it taken?—By a cannula connected with an artery to what we speak of as a mercurial manometer. It is being recorded automatically the whole time.

18694. (*Mr. Tomkinson.*) Is that inserted under anæsthetics?—Yes, the animal is given an anæsthetic to begin with, and it never recovers from it.

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18695. By nerve endings I suppose you mean what we call nerves?—The nerves are white fibres, and they end in either glands or muscles, or something or other, and the drug can act on various positions in various places, and the nerve endings are just one place. A nerve ending is not a muscle, and it is not a nerve; it is something in between the two.

18696. (*Chairman.*) Do you often use curare yourself?—No, I use it very rarely.

18697. In what cases do you use it?—I should use it where vaso-motor nerves, vessel nerves, were concerned where we require to investigate the action of a certain kind of nerves which go to the vessels.

18698. Blood vessels?—Yes, because those vaso-motor nerves, the vessel nerves, are mixed up with the motor nerves, and you must separate the one from the other. You can cut out the nerves to the voluntary muscles by curare and leave the nerves to the blood vessels.

18699. How is it that you can do that under curare and not without curare?—That is what we call the specific action; the drug has just got that specific action; it just attacks those particular nerve endings and leaves the others.

18700. That does not explain it to my mind clearly?—Do you wish to know why the curare goes for those particular nerve endings?

18701. I want to know why curare renders it easier—what is the state of things produced by curare which makes you able to perform an operation when the animal has curare and not able to do it when it has not?—Supposing that I wish to stimulate a nerve, I excite the nerve in one way or another, and I should get, if the muscles were intact, simply contraction of muscles, and I could not observe my blood vessels at all. But if I have eliminated the muscle effect I have only got the vascular effect left.

18702. Those are the cases in which you would use curare?—Yes. It is very rare.

18703. (*Sir Mackenzie Chalmers.*) How often in the last two years have you actually used curare yourself?—I should say three or four times, but I could not give it to you just offhand. It is not common at all; I mean that it is not once a week or once a fortnight—it is nothing like that.

18704. Two or three times in the last two years?—Perhaps more than that—perhaps four or five times. I could let you know, of course.

18705. (*Chairman.*) If you were forbidden to use curare it would only operate upon you, at any rate, to that extent. You would be prevented from performing some two or three operations a year?—Yes, but they would be operations of the greatest importance. It might spoil a whole research.

18706. I am not offering any opinion about it. I am only wanting to know the extent of it. Have you been always satisfied that the animal has not felt pain under curare?—Always—absolutely.

18707. As satisfied as if it had not had curare given it?—Yes, absolutely.

18708. Is that all that you have to say about curare?—Yes.

18709. Then as to your head of modern work, I think really we have had all this before?—You have had some of it, but I would just like to mention one thing, if I may; and, to show you the great advantage of the pharmacological laboratory, I would just like to mention the investigation into West African boxwood. Up to Lancashire, when the masters introduced a new wood, the men making shuttles got symptoms of poisoning, and, of course, the men said that it was due to the wood, and the masters said that it was not. I should also like to say that the doctors said that the men suffered from symptoms which agreed with a cardiac action, and that this drug must have an action on the heart. We got samples of this wood, the masters sent us samples, and we investigated it. We got the alkaloids out, and we found that the alkaloids had a curare-like action; in fact the stuff, so far as it was concerned, might have been curare. It is not curare, it is a different substance, but it is the same type of drug. The essential thing that the men suffered from was great difficulty in getting their breath because their motor nerve endings to the chest wall were depressed—not absolutely paralysed.

18710. How did the poison reach them, because they do not handle the shuttles much?—No, it was the turners, the men who live in an atmosphere of sawdust in the workshops, and get the dust into their chests.

18711. Not the men who look after the working of the looms?—No, it was the men actually mixed up in the sawdust. The place in which they work is, I am told, a cloud of dust.

18712. (*Mr. Tomkinson.*) When once made the reel was inocuous?—Yes.

18713. But it was in the process of making the reel, all the dust in the turning?—Yes, and then in regard to the first part of my *précis*, may I just mention the difficulty of obtaining evidence from clinical experiments, because I really think it is of some importance. I happened to be on a Committee of the British Medical Association for suggesting what drugs shall go in the new British Pharmacopœia, when it became necessary to find out the value of certain new silver compounds. Accordingly we sent out notices—I think we sent out 300 notices—to the leading ophthalmic surgeons, and surgeons attached to Lock hospitals, asking them what was the value of these new compounds, and the results which came in were the most remarkable I have ever seen. It would be thought that in dealing with the action of silver compounds used for eye work, conjunctivis, and various forms of inflammation of the eye, that if it is possible to make any definite statement from clinical observations it would be about a disease which you can actually see; you can apply the drug and say at once whether it has any value or no value. And yet the decisions of these expert physicians and surgeons as to the value of these new drugs were exactly divided. Some of them said, "This drug is our sheet anchor in the treatment of disease." Others regarded the same drug as quite inert and worthless. How are you going to determine between those two views? Of course, the explanation is simple. It means that the conditions in a clinical experiment are never the same; there is no control experiment. You have not two men exactly the same in every respect, except that one is being treated for this particular disease. You have no control, and you never know what action the drug has. You can give a drug, and the patient may get well or get worse; you cannot say in the case of an unknown drug whether it has made him better or made him worse.

18714. Have these compounds that you are speaking of been tested by animal experiment?—No; they have not. It is not necessary to do that by animal experiment. I was simply referring to the difficulty of making clinical observations and determining the action of drugs from clinical observation. It is practically impossible.

18715. I do not quite follow why, if you can get no satisfactory results from clinical observation as to the action of the drug, you cannot expect to get any information from experiments on animals?—In an experiment on an animal you have got exact conditions and control.

18716. But at the same time you say that it is not necessary to test it on animals?—Not these particular drugs, because they are not used for absorption at all; they are used only externally for inflammation.

18717. With regard to the discovery of the poison in boxwood and its operation, was that done by experiments on animals?—Entirely.

18718. Who was applied to to make the experiment?—In the first place the men—the Union—sent to Liverpool and got the wood examined and reported upon by a botanist, Professor Harvey Gibson, who isolated the alkaloid, and tried the effect of the poison on an isolated heart—a heart taken out of the body and working by itself—and he got an effect which corresponded to potassium.

18719. A heart taken out of a human body?—Taken out of an animal body. He got the alkaloids out, and then he tried those alkaloids on isolated hearts obtained from animals.

18720. (*Sir William Collins.*) Frogs' hearts?—Rabbits.

18721. (*Mr. Tomkinson.*) What is an isolated heart?—A heart taken out of the body and hung up and perfused.

18722. Made to beat by electricity?—No; it beats by itself if you send fluid through it. Any part of

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the body will go on working by itself outside of the body if you treat it properly.

18723. For how long?—For many hours. A child's heart has been taken out six hours after death and been made to beat for several hours—not in this country, of course.

18724. (*Chairman.*) Detached from the body?—Yes; cut off and hung up.

18725. Made to beat by electricity?—No; by sending fluid through the vessels to it.

18726. You mean if a certain artificial action is applied to it?—No; it beats by itself. The reason why the heart beats is because fluid is going through it's vessels.

18727. That is what I meant when I said some artificial action.—Yes; that is what I meant.

18728. (*Dr. Gaskell.*) Would you not rather say some nutritive action?—Yes. Then the masters, of course, did not approve of this report, and they sent the stuff to us, and a chemist, Mr. Harrison prepared the alkaloids and I did the experiments on animals, which showed that it was a curare-like poison.

18729. (*Chairman.*) You did the experiments?—Yes, and I gave evidence about this drug to the Dangerous Trades Committee.

18730. Was that at King's College?—No; it was at Cambridge.

18731. (*Colonel Lockwood.*) How many experiments on dogs have you conducted during the past year?—Perhaps 18 to 20. I cannot remember.

18732. I understand from you that there is no difficulty in keeping a dog under an anæsthetic provided it is given slowly at first: that you never experience any difficulty?—Never. The danger is that if you give a dog 6 per cent. of chloroform vapour it dies in a minute or half a minute. If you give it 0·1, then go to 0·2, then to 0·3, and so on, it never dies.

18733. How long did these experiments that you have been conducting generally last?—Perhaps two or three hours; never more than three hours, I should think.

18734. And the animal you operate upon is always killed before it recovers?—Yes, always.

18735. Who does that?—I do that.

18736. How?—Generally by injecting air into a vein. You see I have a vein always prepared. I blow in a little air which goes straight to the heart, gets behind the valves of the heart, and the heart stops beating. That is the simplest way.

18737. (*Sir Mackenzie Chalmers.*) You blow into a vein?—Yes, you blow air in.

18738. (*Colonel Lockwood.*) And that is done at the conclusion of the experiment?—Yes.

18739. Before the class?—If it is a class experiment, before the class.

18740. You show these experiments probably among others to classes?—I give a series of demonstrations regularly to classes.

18741. Have you ever seen any levity such as is alleged by one of the witnesses on the part of students?—I have never seen any levity.

18742. They look upon it as a serious business?—It is the most important part of their curriculum without any exception. Lectures can be got up out of books, and they do not matter a bit. If I may just show you, that is a record taken in one of my class experiments (*exhibiting a diagram*). *There* is the heart. *That* is the effect of chloroform on the heart. The blood pressure goes down because the chloroform is given: the heart beats more feebly, therefore the blood-pressure goes down. The men see that actually taking place.

18743. Are you in favour of conducting experiments, the result of which is already known, for the instruction of students?—I think it is of vital importance. I could not over-estimate the importance of it.

18744. In your opinion it is absolutely necessary?—Absolutely necessary, and all my colleagues at Cambridge agree with me.

18745. Do I correctly understand from you that chloroform and curare, if the curare is given in sufficient doses, have both practically the same effect of anæsthesia?—If curare is given in big doses, not in small doses.

18746. But it paralyses the sensory nerves of the spinal cord?—The sensory nerves in the brain. Chloroform does not necessarily paralyse those in the cord.

18747. To put plainly, an animal that was thoroughly curarised with a big dose would absolutely not feel any pain?—I believe not; it could not.

18748. Is that your own idea only, or is it supported by other evidence? Is it a well accepted fact by the profession?—Yes, I think it is a well accepted fact.

18749. Then why do the profession always give curare accompanied by an anæsthetic?—I think there are very few professions which are quite so conservative as the medical profession, and when a drug has once been stated as being used for a certain purpose, although it is shown for years and years to have no such action, it is still given for that purpose. Tannin is still given to stop bleeding from the lungs, although it has no effect; it is all destroyed in the alimentary canal; yet it is still given.

18750. I suppose you are aware that a great portion of the public have looked upon this question of curare with very great feeling?—Yes, I am perfectly aware of it, and I think it ought always to be given with an anæsthetic.

18751. You do think that?—Certainly, because as I have said, it is only in large doses that this central effect, so far as we are certain, comes on.

18752. Then if it is effectual, given in large doses, why not give a large dose?—One objection to that is that it is very difficult to get curare; it is almost unobtainable.

18753. Is it not on the market?—No; you can buy stuff that is called curare which does not contain any curarine.

18754. At all events the public may understand that you can obtain complete anæsthesia from curare alone?—Yes, certainly.

18755. (*Sir William Church.*) We have had a great many positive statements made to us by witnesses that morphia is not an anæsthetic. I should like to ask you one or two questions on that point. On page 25 of the third volume at Question 7308 with regard to the evidence given during the trial of Bayliss v. Coleridge, the witness said: "These attempts to prove that morphia is an anæsthetic are futile," and a little lower down in the same paragraph: "Claude Bernard quite appreciated the utility of morphia from the vivisectors' point of view, though he was honest enough to acknowledge its utter uselessness as an effective anæsthetic for vivisected animals. These are his words: 'The animal still remains sensitive, a touch on the cornea induces the closing of the eyelids, but he is quite still, and lends himself without a movement to the most delicate operation. . . . He feels the pain, but has lost the idea of self-defence.'" You would not agree with that?—I should like to point out in the first place that there is a mis-translation there. The animal remains, the word is "*sensitive*" in French. That is not correctly translated as "sensitive"; "reflex" it the correct translation I should say there. The animal still remains reflex, not sensitive. That is quite a different thing. That is the first thing that I should wish to say. And then, secondly, of course Claude Bernard was perfectly correct in stating that morphin is not a suitable anæsthetic in vivisection experiments, because the animal is reflex, it can move; it is only the sensory cells which are really depressed excepting in enormous doses, of course.

18756. If you would turn to Question 9266, which is on page 105 of the same volume, you will see there "Dogs are but slightly susceptible to the action of morphia." "'Caldwell gave a dog 17 grains without producing serious symptoms; whilst to a number of dogs he gave 12 grains hypodermically, with no bad effect.' Then Claude Bernard refers to a dose of 30 grains." Do you agree with what is stated there to be a fact, that Caldwell gave a dog 17 grains without producing serious symptoms?—No, I do not know how he gave it. Supposing this is absolutely true, then I say that the morphin was given by the mouth, and was never absorbed. People are always talking about drugs; but if they give the drugs by the mouth, how do they know that the drugs have got into the circulation? Many drugs are not absorbed at all.

18757. He is also stated to have given 12 grains hypodermically, with no bad effect. That did not

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mean, I imagine, without any effect at all, but without any prejudicial effect to the particular class of experiment that he was conducting?—Yes, you can give enormous doses of morphin to dogs, and nothing happens, except that the respiration gives out, and the animal dies in that way. But morphine has not a depressent action on the heart, so that you can give grammes upon grammes to dogs if you do artificial respiration; indeed I have done so.

18758. (*Sir Mackenzie Chalmers.*) Can you give grammes and grammes without producing anæsthesia; that is the point?—No, you cannot; you will produce anæsthesia then. But you would not kill the heart.

18759. (*Sir William Church.*) So far as regards the action upon sensibility to pain goes, is the action in the dog different from that which it is in man?—It is exactly the same.

18760. Does it require a very much larger amount of the drug to produce insensibility to pain in a dog than it does in man?—No, I should say much about the same.

18761. Therefore you would consider that 3 grains of morphia would undoubtedly produce insensibility to pain in a dog?—Yes, I should.

18762. And as Professor Starling says here, 15 grains would be quite an excessive dose for that purpose?—Quite an excessive dose, quite an unnecessary dose.

18763. By excessive I meant unnecessarily large?—Yes.

18764. At Question 9498, on page 113—this again is with regard to what Claude Bernard says about morphia—a quotation is made from Claude Bernard that the action of morphia differs entirely from anæsthesia. You would not agree with that?—Morphin, I tried to point out, does differ from chloroform. I made two distinct groups.

18765. That is to say, in its action it belongs to an entirely different series of drugs?—Yes.

18766. But I am speaking only with regard to anæsthesia alone?—There would be no difference.

18767. As to insensitiveness to pain it has much the same action upon sensibility to pain as the other series of drug?—Exactly the same.

18768. And it affects, we believe now, the same cells in the nervous system?—Yes.

18769. You have done much work with regard to the standardisation of drugs?—Yes, I have done a good deal.

18770. Is there any way by which many drugs can be standardised, except by watching their effects upon animals?—There are five drugs—digitalis, stropanthus, squill, ergot, and cannabis indica, the chemistry of which we know so little, that we cannot standardise them in any way other than physiologically. Of these two drugs, namely digitalis and ergot, are of the most important in the Pharmacopœia. There are ten different ergots there (*handing in a list*). Those ergots were bought at different chemists' shops. This is simply to show the variation in the action of ergot. A practitioner, say, having to treat with a case of a woman in labour, gives one of those ergots, and probably in three cases out of four it is inactive. The same applies to digitalis.

18771. (*Chairman.*) How do you get rid of those difficulties? Do you go to some special places for your drugs, where you avoid that risk?—You have your drugs standardised.

18772. (*Sir Mackenzie Chalmers.*) When you say that you standardise your drugs, do you mean that you standardise once for all, or do you have to standardise each brew?—Each brew. Certain wholesale chemists standardise each lot that they sell to the public.

18773. (*Chairman.*) The only way by which you can ascertain their action is by experimentation on animals?—Yes.

18774. (*Dr. Wilson.*) What is the test; is it blood pressure?—In that case it is blood pressure for ergot—the height to which the blood pressure rises.

18775. (*Sir William Church.*) Has the absence of this accurate knowledge of the strength of the drugs led to most diverse opinions being expressed by medical practitioners as to the value of certain drugs?—Certainly.

18776. In fact, it has been held that even ergot has no action upon the uterus?—Yes, and the stuff that is sold in many chemists' shops has no action upon the uterus.

18777. Cannabis indica is another drug about which there has been a great difference of opinion?—Yes, it is more usually non-active than active as bought in shops.

18778. Are there any drugs in which chemical processes cannot at present show their real composition and strength?—Yes, for those drugs that I have mentioned chemistry is at present quite worthless.

18779. In these drugs through your not being able to isolate the alkaloids from them with ease, is it very difficult to tell what is the strength of any given specimen?—Yes, that is so; sometimes active principles are sold, and in one case, for example, the "active principle" was extremely active. In another case the so-called "active principle" was beautifully crystalline, and it had no action at all; as soon as you begin to purify them they lose their activity.

18780. Ergotin is an example?—Yes.

18781. (*Sir Mackenzie Chalmers*). Is that passive or active?—It may be either.

18782. (*Sir William Church.*) In Chemistry, microscopy cannot tell the difference between an active crystal and a non-active one?—No.

18783. (*Sir William Collins.*) Then I rightly apprehend that it is not so much by the composition or the ingredients of the pharmaceutical compounds in future, but by their action based on standardisation that we must look for their therapeutical differences?—Not at all; most drugs in the Pharmacopœia can be, and are standardised by chemical means. It is only certain kinds that cannot be, the five that I have mentioned.

18784. Does the remark that you have been making apply only to those five?—There are a few others, but only of those five have I an experience. No one would dream of standardising atropin in any other way than by weighing it, or morphin. Nor would we do so with these drugs if we could standardise them in any other way.

18785. Is the composition of curare ascertained with any certainty?—Curarine has been prepared in Germany, but, as I say, you cannot get it; you cannot buy it. I have once seen a crystal of it as a great curiosity, and that is all. I do not think its chemical composition has anything known about it.

18786. I was going to ask you whether you could give its chemical formula?—I do not think it is known.

18787. I gathered from what you told us that amongst the gauges for determining whether anæsthesia is present or not, are the reflexes and blood pressure?—Yes.

18788. Are there any other gauges besides those?—Those are the two which I myself use. There are other things, of course, like the pupil and the depth of respiration, and so on, showing the condition of the medulla. But the two that one chiefly uses in animals are the reflexes and blood pressure; at least, those are what I go by.

18789. Is it possible in human beings to go by the blood pressure?—No, it is not.

18790. We have been told that there is a mechanism by which blood pressure in human beings can be accurately gauged?—That is so. When I say that it is not possible, I mean that it would be very inconvenient while the anæsthetist was anæsthetising, to have a man taking the blood pressure and recording it all the time, besides which the blood pressure mechanism as at present made for man will only give you the blood pressure for any one moment. You will then have to start it over again.

18791. I wanted to ask you whether any attempt has been made to utilise the blood pressure gauge for the purpose of determining the amount of anæsthesia in human beings?—No, not so far as I am aware.

18792. Is that because you think that the mechanism employed for gauging the blood pressure, apart from the manometer, is unreliable?—My point is, that in man (we have been doing a lot of work on this point lately), you can take the blood pressure, say, in the arm, at a certain moment, but you cannot take it continuously. You see, it is taken by blocking the pulse, but you cannot keep the pulse blocked; you have to let it out and then take it a minute or so afterwards, so that you get periodic readings.

18793. Did I correctly understand you to say that in the case of morphia the test of the presence or absence of reflex was inapplicable?—Yes, that is so, of the lower reflexes.

18794. By a reflex action do you mean a conscious action?—Oh, no.

18795. An unconscious action?—An unconscious action. Most of our reflexes, of course, are unconscious.

18796. But may not a reflex action be conscious?—No, not conscious.

18797. We have had a notable case, that a Commissioner suggested, that sneezing was a reflex act, and that sneezing was conscious. What do you say to that?—I should say you would still sneeze if you were unconscious. Breathing is a reflex action; we do not know that we are breathing, but, nevertheless, you can breathe consciously, and then it is no longer reflex, so to speak.

18798. Is a reflex act a voluntary act?—No, it would not be reflex.

18799. Is it part of the essence of being reflex that it is an involuntary act?—Certainly.

18800. (*Mr. Ram.*) Would it be accurate to say that a reflex action may be sometimes controlled by consciousness?—That is quite correct.

18801. But that consciousness is not necessarily present to a reflex action?—That is quite correct.

18802. (*Sir William Collins.*) You spoke of the sensory cells in the spinal cord in connection with the curare point, I think?—With chloroform.

18803. Was it not curare?—With chloroform.

18804. At any rate you spoke of the sensory cells in the spinal cord?—Yes.

18805. Is the spinal cord sensitive?—I do not quite know what you mean by sensitive.

18806. Can the spinal cord feel?—Oh, no.

18807. When you speak of the presence of sensory cells in the spinal cord, you do not wish us to assume that the spinal cord can feel?—I mean the cells that receive impulses. There are receiving cells.

18808. Cells receiving afferent impulses?—Yes.

18809. The last Commission referring to curare and Claude Bernard, put this on record in their report: "It has, however, been positively stated by perhaps the highest authority on such a subject, Claude Bernard, to have no effect in producing insensibility to pain," that is curare. Do I correctly understand that you contest that view?—Yes. You cannot expect anything to stand still. That was the best knowledge that the Commission had at the time, but it is not the best knowledge now.

18810. We were told by a previous witness in answer to Question 3617 in the first Report, that "the evidence before us really is not sufficient to my mind to demonstrate conclusively that curare does or does not abolish sensation. On general considerations I would think that it probably abolishes the functions of the higher part of the brain, that is to say consciousness; but we have not sufficient evidence for that." What do you say in regard to that?—In the first place you will remember that I told you that curare must not be regarded as a sort of unique drug, or by itself, that it belongs to a definite group of drugs which all have these three actions; they all paralyse the brain; all of them produce convulsions under certain conditions, and all paralyse the motor nerve endings. Curare first of all belongs to that group very definitely—that is the first point. And secondly, I have repeated Claude Bernard's experiments, and given large doses of curare, and have succeeded in completely cutting out the sensory effect.

18811. Do you, or do you not, agree with that previous witness, who said that we have not sufficient evidence to say whether it abolishes the functions of the higher parts of the brain?—I think there is sufficient evidence that in big doses it does. In small doses I agree with him, no; in big doses, yes.

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18812. Is that the result of your own experiments?—To some extent it is the result of my own experiments.

18813. When did you conduct these experiments with curare?—Some three or four years ago now. There have been odd experiments done at odd times, many of them on dead animals, of course—technically dead.

18814. Then did you test whether or not the higher functions of the brain were abolished?—In those experiments that I was referring to, I was doing them on the spinal cord, the receptor cells of the spinal cord.

18815. I was asking you for the evidence by which you were led to conclude that the higher parts of the brain were abolished, as regards their function, by curare?—The only reasonable evidence of that is that curare, as I say, belongs to this definite group of drugs which have these three actions in common.

18816. Do you argue by analogy from its congeners, as it were?—Yes; it is practically impossible to obtain direct experimental evidence.

18817. Why?—Because you have the motor nerve endings gone. You cannot stimulate the brain, of course.

18818. Must it ever remain in doubt then as regards curare itself, apart from analogy, whether it does or does not abolish consciousness of the higher parts of the brain?—No, it need not; but the only experiment which would prove definitely whether small doses of curare abolish pain would be on man. Any volunteer must have the curare injected and artificial respiration must be ready, and some operation must be done on him.

18819. I understood you to say that you had taken curare yourself?—Yes, by the mouth.

18820. In what doses?—A big enough dose certainly to completely curarise a dog.

18821. Did it have any effect upon your own sensibility?—It had absolutely no effect at all.

18822. Or on your motor nerves?—It had no effect of any kind at all. I ought to say that every dose of curare varies. You get one sample and it is very active, and the next sample is inactive, so that you cannot weigh out a dose and say I am taking so much. The only way by which you can be sure that it is very active is by trying it on frogs.

18823. Is it possible to standardise curare?—Yes, on frogs.

18824. (*Mr. Ram.*) Did you standardise all that which you took?—Yes, I knew all about it.

18825. (*Sir William Collin.*) Was it an active or inert specimen?—It was a very active specimen.

18826. Does the commercial curare vary in quality?—Yes, tremendously; it varies from great activity, which is rare, to inertness, which is common.

18827. What is the price of curare in the market?—I would not like to say, but I believe it is about 5s. a gramme. I am not at all sure. Of course, it is a resin, and there is very little active alkaloid in it.

18828. You told us, I think, that there were a great many other drugs that acted in the same way as curare?—Yes.

18829. You said a host of them?—Yes.

18830. You mentioned nicotin, I think, as one?—Nicotin I mentioned.

18831. Conium has been mentioned before this Commission already. Is conium a powerful poison in man?—Yes.

18832. It was denied or doubted by a previous witness. Is it a powerful poison?—Yes, it is the poison that Socrates died of, you remember—hemlock.

18833. Would you name any others in this host of drugs that act in the same way as curare?—Nicotin, cocaine, lobelin, galseminin, apacodin, as I say, this boxwood, and a great number more keep cropping up in literature periodically. I am not personally acquainted with them.

18834-5. You have named some six or seven, but when you speak of a host one rather assumes a larger number?—I meant perhaps twenty or thirty.

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18836. You know that there is a restriction under the Act with regard to curare. Should the same restriction apply to the host of other drugs that resemble it?—As I pointed out at the time, most of the other drugs paralyse the brain before they paralyse the motor nerve endings. Let me give you an example. Nicotin paralyses the brain first. Then there is no question there. In the case of curare, it happens to be the other way round; it goes for the nerve endings first, and then to the brain. So that we have difficulty. That is why there is all this fuss about curare.

18837. Would not that suggest that the analogy between curare and the other members of this host is not very close?—Certainly not. I think in all text books on pharmacology you will find them grouped together as one definite group of drugs, having certain three or four actions in common.

18838. And you think you can argue from the action of some of these of which we know more to what the action of curare should be?—You can gauge it at once. For instance, when I got this African boxwood, I found that the African boxwood paralysed certain nerve cells. As soon as I discovered that, I classified that drug immediately into this group, and the other action simply fell out, as a matter of course. I knew what was going to happen before I tried as regards all the other effects.

18839. Is it a characteristic of this group that the ends of the motor nerves are paralysed?—Yes.

18840. Or is it that the ends of the motor nerves are paralysed before the abolition of the higher centres?—No, it is sometimes one and sometimes the other; they vary; and as I told you conium comes in between the two.

18841. The important point for us is to know, if possible, the order in which that paralysis takes effect—as to whether a person's or an animal's consciousness may remain after the absence of movement has ceased?—The only two with which I am acquainted, where the effect comes on first in the motor nerve endings, are curare and the boxwood. In the others the paralysis begins at the nerve cells first.

18842. So that in this important question there may be a lack of resemblance among these members of the same group?—I do not follow you.

18843. You suggested that there are a host of drugs which act in the same way as curare?—Yes.

18844. And that one may argue the action of curare from the known action of some of these congeners?—Yes.

18845. But the very important question of whether the paralysis of movement precedes or not the paralysis of consciousness, is a matter upon which there is a variation within that group?—Yes.

18846. Then is it possible to argue, from what we know of some members of it, what the action will be in the case of other members, in respect of the order of paralysis of motion and consciousness?—No, not with an unknown drug. The order varies. The type of action is the same in all.

18847. It may be that while in the case of curare the paralysis of motion precedes by a considerable length of time the paralysis of consciousness, in other members of the group the reverse may be the case, or there may be a great difference in the time intervening?—Yes, that is correct.

18848. Then you cited the replies of some practitioners in regard to the use of silver compounds in diseases of the eye?—Yes; may I pass you the Report (*handing in the same*).

18849. Thank you. I see you say, "Wide differences of opinion are, however, expressed. Regarding protargol, an ophthalmic surgeon remarks that it is 'inert and quite useless'; while another observer states that, in his experience, it is 'by far the most reliable salt for injecting or irrigating in cases of chronic gonorrhœa.'" I understand that your point in quoting this was to show the variation of opinion by observers of the drug under observation?—Yes, and of the extreme difficulty of making use of clinical experience.

18850. But it points rather to inaccuracy of observation, does it not?—At first sight it does, but that is not, I believe, true. It points to the fact that the conditions are so extremely variable that it is very difficult to draw conclusions.

18851. You may have a plurality of causes in operation?—Yes; quite so.

18852. I suppose that may be true also in the case of an experiment upon an animal as well as in clinical observation?—You have always got the control—that is the point.

18853. Do you mean that in the case of an experiment on a living animal you never have to do with plurality of causes?—Yes, you have plurality of causes; but you have the cause and the effect. You know that the effect is due to the cause, but you do not know exactly the intermediate steps between; you can differ on that.

18854. Is it not a fact that different observers watching the same experiments on animals have drawn different conclusions?—No; they do not differ on the facts. The facts remain the same. They only draw different conclusions as to how the effect is produced.

18855. But even in the case of watching an experiment accuracy in the observation of facts is required?—Yes, absolutely.

18856. Have you not come across instances in which facts have been interpreted differently by different observers in the same sets of experiments?—What I mean is that, supposing you observe such a simple thing as a rise in blood-pressure, there is a fact written down, and you cannot deny it. As to the origin of it, how it has been produced, you may have two schools, one saying that it is produced one way and the other saying it is produced the other way.

18857. Given agreement in observation, you may have variety of interpretations?—Yes.

18858. (*Sir Mackenzie Chalmers.*) You mentioned, I think, that you had a Therapeutical Section of the Royal Society of Medicine?—Yes.

18859. Would you kindly tell us, who are not members of the profession, what the Royal Society of Medicine is?—It is a collection of most of the learned Societies of Medicine joined together recently into one big society, which they call the Royal Society of Medicine, and with various sections; that is to say, the Therapeutical Section is in one group, Diseases of Children is in another.

18860. Has it a local habitation?—Yes, in Hanover-square.

18861. And is it representative of a large body of medical opinion?—A very large body.

18862. There were some parts of your evidence that I did not quite understand, which I should like to know a little more about. You referred to morphia as exercising a specific selective action?—Yes.

18863. I suppose there are a good many other drugs in the same group with morphia, or are there not?—There are a few others, but not very many.

18864. One, for instance, is the substance used in India called Bhang?—Yes, that is Indian hemp, cannabis indica.

18865. Does hachish belong to the same group or not?—Yes; that is the same thing. I classify it in the same group myself, although it has a different type of action.

18866. Does it destroy sensation?—It produces such a weird kind of intoxication that it is difficult to observe. It does to a very large extent, but nothing like to the same extent as morphin.

18867. In your opinion, for certain animal experiments, morphia is a safe, proper, and effective analgesic?—Yes.

18868. You are perfectly satisfied of that?—Absolutely satisfied.

18869. Do you think there is much room for idiosyncrasy in animals as regards the action of morphia?—No; no room at all, I think.

18870. We were told, I think, by one witness that pigs were absolutely insensible to morphia. What do you say to that?—How was the morphia administered?

18871. We were not told that?—There are all sorts of statements of that kind made; one is continually coming across them. For instance, they say henbane is not poisonous to hogs and it is to hens; but that is not true, of course. It is poisonous equally to both of them if you give it in the correct way. Some drugs are given by the mouth and are not absorbed, and people say they are not poisonous.

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18872. What I wanted to get at was this: Assuming that the morphia is injected in the way in which you use it in laboratories, has it a very different effect on different animals?—No; it has not. It has precisely the same effect, although it is interpreted differently. If, for instance, you inject morphin into frogs you first of all produce complete paralysis and narcosis; 24 hours afterwards the frog gets strychnine-like convulsions; that is to say, a frog gets convulsions and man does not. How is that? Because you cannot give a dose of morphin to a man like that and then let him recover. If you could he would undoubtedly get similar convulsions; indeed, it is quite common to see a man with these twitchings in exactly the same way, on recovering from morphin poisoning.

18873. Take the case of a man attempting suicide by morphia. Would you get similar results?—Yes.

18874. Is there any reason for supposing that a dog is less sensitive to the action of morphia than other animals of the same size and weight?—There is no reason at all. I should say that it is more sensitive.

18875. And you do not think that there is much idiosyncrasy in dogs?—None at all. I have never seen any.

18876. You have had large experience of giving morphia to dogs?—Yes.

18877. I think we were told that there was one particular family of human beings who are curiously insensitive to morphia?—Now we are coming to a different question. If you take morphin for a certain length of time, you learn to destroy it just as if you take alcohol for a certain length of time in sufficiently big doses, you learn to oxydise it completely.

18878. Or a pipe of tobacco?—Yes, and you learn to use your morphin just as if it were bread and butter. That is done by means of a ferment. This ferment, this power of destroying morphin, might conceivably (although I do not know it, I never heard of such a thing) be hereditary.

18879. Then as soon as you got to that stage you would have what is called an idiosyncrasy?—Yes.

18880. But as regards many drugs there is a marked idiosyncrasy, is there not; for instance, quinin. Some people can take quinin and some cannot?—Some people cannot take quinin because it produces an objectionable action so extremely easily.

18881. (*Sir William Collins.*) Would what you say amount to this: that given a substance which is a poison to any animal if that substance is only properly got into the blood of any other animals it would be poisonous to them too?—Yes, it amounts to that. That is correct. You see there are no drugs with which we are acquainted which have any different action on the heart or on the kidneys of animals from that in man. The only differences which occur are on the brain. For instance, cocaine in big doses causes convulsions. Cocaine acts on the cerebral hemisphere, the big part of the brain. In the frog there are practically no cerebral hemispheres. You can give an enormous dose of cocaine and you can hardly get any twitchings. In the rabbit you want a relatively smaller dose, but still a very big dose. In the dog, in which the cerebral hemispheres are better developed, you want a still smaller dose, in a monkey still smaller, and in man the smallest. So that you can tell the dose of cocaine necessary to produce convulsions by weighing the cerebral hemisphere and then the body; the dose of cocaine varies in relation of these two exactly.

18882. (*Sir William Collins.*) What you are saying if true of chemical poisons will not hold in the case of bacterial poisons, will it?—You mean such as diphtheria toxin?

18883. I am thinking of the case of septicœmia of mice, which we have been told is harmless to one variety of mouse and fatal to another?—They are proteid substances, or bodies allied to proteid substances, and that would not apply, of course. The horse serum, the serum from a horse may be poisonous to one animal and not to another.

18884. You were speaking of the chemical poisons of the pharmacopœia?—Yes.

18885. (*Sir Mackenzie Chalmers.*) Chemical poisons do not multiply in the blood itself?—No.

18886. Some bacterial poisons multiply in the human body?—Yes.

18887. However, the practical point is, that you feel no doubt that as morphia is administered in laboratories in England, it absolutely protects the animal from pain?—Absolutely.

18888. Urethane, I understand you, is a general anæsthetic?—Yes.

18889. I am sorry to say that I do not know what it is?—It is a curious substance, because Schiemerderberg thought out the sort of substance that he wanted. He thought to himself, "I want to get a substance different from chloroform which will have the action of chloroform but which will not depress the medulla." So he made up a formula on paper and then built up urethane from it. It is a combination of urea and alcohol, so to speak.

18890. It is a synthetic substance. Is it made in the laboratory?—Yes.

18891. Then does it produce a sort of uræmic poisoning, or produce alcoholic paralysis?—No, ureia does not produce uræmic poisoning. It produces an action very similar to that of chloroform, but without any uepressent action on the heart. It has the exact action of chloroform, or any other member of the group, on the brain. All these drugs of which I call the alcohol group collect in the brain.

18892. (*Dr. Gaskell.*) And you can tell that it will have that action by its formula?—No, you cannot, but Schmiederberg thought that you could, and he built up the formula.

18893. (*Sir William Collins.*) There is a considerable relationship, is there not, between the chemical constitution and physiological action?—There is some relationship, but it is not considerable to the pharmacologist at present, because a small change produces a tremendous difference in action sometimes.

18894. (*Sir Mackenzie Chalmers.*) It is not worked out?—It is not worked out. There must be a relationship.

18895. In your opinion for animals, urethane is a useful and reliable anæsthetic, and an analgesic?—I think it is the best.

18896. There is no danger, if it is properly administered, of the animal suffering any pain?—No.

18897. I did not quite understand, I am afraid, what you said to Sir William Collins, that when either chloroform or curare is administered, the sensory cells of the spinal cord still remain active?—No; it was chloroform. I said that chloroform paralyses the sensory cells. I said that all the anæsthetics paralyse both sensory and motor, but they paralyse the sensory in every case first.

18898. Then I misunderstood you. I understood you to say that even after you had administered chloroform, the sensory nerves of the spinal cord might still be active?—Yes, they might still be active in light anæsthesia. That is quite true. It is complex, and it is difficult to explain everything at once. When you administer chloroform you begin by paralysing the higher centres first. You paralyse control in the brain, power of paying attention and judgment; they go first, and therefore you get, as we may say, a riot of the lower centres; you begin to reel in your gait and do not consider what you say, you lose your politeness, and so on; you lose all external conformities with a polite existence. Then you get the depression progressing still further, and it goes to the motor area, and then it runs from the brain down to the cord; it misses out the medulla; lastly, it paralyses the medulla; but always, in whatever part of the central nervous system the action occurs, you have evidence to show that it paralyses the sensory cells before the motor. Nevertheless, paralysis progresses in this order.

18899. But as long as the sensory cells in the spinal cord would reply to a stimulus might there be pain?—No, appreciation of pain may have gone long ago. But with a smallish dose of an anæsthetic you may pinch the tail or hind limb and get a leg drawn up.

18900. That is purely reflex action?—Yes, that is purely reflex; but the sensory cells are intact in the spinal cord. With a big dose of anæsthetic that action is gone; I mean that the sensory cells are paralysed. In the second case, probably the motor cells also, although these become paralysed only very late.

18901. You spoke of the effect of a large dose of cocaine, but cocaine acts as a local anæsthetic simply

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and simply cuts off communication with the brain, does it not?—Yes, it paralyses the fine nerve fibrils. If you inject a little the fine nerve fibriles all round are paralysed by the cocaine. It does not paralyse the motor fibrils because there are not any in subcutaneous tissues.

18902. It simply stops the message going to the brain?—Yes.

18903. When you told us how a large dose produces convulsions, that was not applied locally?—No, that was given internally, of course.

18904. By injection?—Yes.

18905. There is no doubt whatever, I suppose, as regards curare, that it paralyses the motor before it paralyses the sensory nerves?—There is no doubt about it; but it paralyses the sensory cells as well.

18906. That being so, it is essential to keep the provision in the Act that it must only be administered with another anæsthetic?—Yes, I think that is a good provision.

18907. Because otherwise there would be a period of time during which there would be sensation without motion?—Yes, that is the point; that is right.

18908. May I ask you what was the object of your personal experiment in taking curare?—It was taken accidentally. We commonly take our drugs up in pipettes, and while I had a pipetteful to my lips I was attempting to talk to someone, and sucked up the whole, and swallowed it before I knew what I was doing.

18909. And you had an uncomfortable quarter of an hour, I suppose?—Well, I did not know what was going to happen, but nothing happened at all.

18910. Has any attempt been made to determine chemically the constitution of curare?—Yes, in Germany an attempt has been made; they have got pure crystals of curarin, but the supply of curare has given out. We are still dependent, you see, on the natives, who keep the sources of curare a secret.

18911. And for the purpose of chemical analysis you require a considerable quantity, I suppose?—Yes.

18912. You told us about some interesting experiments with boxwood. The result of those experiments was that the dust of boxwood was capable of poisoning human beings through inhalation?—Yes.

18913. Producing heart symptoms?—No, no heart symptoms. The essential symptom was this tremendous difficulty in getting breath; the men turned blue in the face and gasped for breath. The doctors who examined them found nothing wrong with their hearts, except that the heart sounds were very feeble, but they regarded it as and called it cardiac dyspnœa; that is to say, they thought the difficulty in breathing was due to the heart, whereas in reality the difficulty of breathing is due to partial paralysis of the motor nerve endings.

18914. As the result of that, what has been done practically?—I have not seen the report of that Departmental Committee, but I fancy that they are either going to forbid the use of the wood altogether, or they are going to compel the people to wear respirators.

18915. If they were efficient respirators it could be handled with safety?—Yes, it could be handled with safety.

18916. You mentioned that you thought it was most important to do certain experiments before students. What number of students pass through your hands annually?—I should think 150.

18917. Would your experiments be pharmacological experiments or physiological experiments?—Entirely pharmacological.

18918. Do you think it is important that students should see any physiological experiments or not?—Yes, there are certain things which I assume that they know all about when they come to me.

18919. May I ask, are your students third-year men?—Yes, the third-year men; after they have done their anatomy and physiology and whilst they are beginning their work in the wards.

18920. And you think that at that stage it is essential to show certain experiments on animals to the students?—I regard it as the most important part of their education.

18921. It has been suggested that it has a deteriorating effect on their character and humanity. Have you any opinion on that matter?—I regard that statement as absolutely ludicrous. Watching an operation does not produce that effect; it rather produces the other effect.

18922. It is said that in watching an operation you approach it with a different mental attitude, because the operation is entirely for the benefit of the patient, whereas the other is simply to illustrate a scientific fact?—It is entirely for the benefit of the watcher certainly.

18923. We have been told that it would have necessarily a harmful effect on the students' mind?—I do not think that for a minute. I cannot see how it could possibly affect their minds detrimentally at all.

18924. You mentioned in your *précis* a series of researches, some of which we have not had any reference to?—These are only some of them. They are typical ones.

18925. Have any of these been carried out in your laboratory?—Yes, all of them either are being or have recently been.

18926. Take the first—lymphocytosis. The causation and mode of production of this condition, you say, has been determined. Is that a recent research?—Yes.

18927. Has it any practical effect on treatment?—All knowledge, of course, must have a practical effect. May I explain what it is roughly? There are, roughly, just for the purpose of description, two kinds of white blood corpuscles. There is one kind which lives, so to speak, in the spleen and lymphatic glands, and the other, which has its habitat in bone marrow. In the spleen and lymphatic glands there are muscle fibres, and what we have shown is, that we can send out at any moment many of these leucocytes which live in the spleen and lymphatic glands. And we can do that at any moment by means of pilocarpin. If I want to produce lymphocytosis in a man, I can do it in a few minutes by giving a small dose of pilocarpin.

18928. Excuse my ignorance; these white corpuscles are what are sometimes called phagocytes?—Yes, but of these are mainly the bone marrow variety.

18929. And not a phagocyte?—Not to the same extent as the others.

18930. So that its action would not have a direct curative effect on the wound?—I do not think it would personally, but you know the tendency in phthisis (or it was the tendency a short time ago) was to give drugs supposed to increase the leucocytes. I do not think it is used much now.

18931. But still this research may have, at any time in connection with some other research, an important effect on treatment?—Certainly. It is a fact about pilocarpin which I expect all students to know now.

18932. I suppose this work is still going on?—Yes.

18933. Then the next is "arteriosclerosis." You say that it has been shown that this condition arises from any cause which temporarily raises the blood pressure?—Those are most interesting experiments, because they show this. You know that as we get older we all get certain changes produced in our vessels. One type of this change we can produce artificially. It is due to any cause which raises the blood pressure. In order to produce arteriosclerosis, we raised the blood pressure with one's fingers compressing the abdominal aorta in a rabbit. A rabbit has got a stiff abdomen as we have; you can put your hand down and quite easily compress the aorta. We did that once a day every third or fourth day. Then what happened? The rise of blood pressure cracked across into the elastic fibres in the blood vessels—the blood vessels are full of elastic fibres. Inflammation occurred round these, fibrous tissue developing, and calcareous material subsequently being deposited in the artery. It is a practical point, too; it teaches us what not to do as we get on in years.

18934. But as we get older it is a more important thing to prevent this arteriosclerosis than to produce it artificially?—Yes, but if we know how to produce it artificially we are a long way towards prevention.

18935. So that the pressure caused by nicotin in smoking might be the cause of arteriosclerosis?—Yes, any cause which raises the blood pressure to a fair extent will produce arteriosclerosis.

18936. Would you feel inclined to say, then, that as we get older we should diminish our smoking?—No; but there is this other point to consider, which you can learn from experiments. If you take a boy and give him a cigar his blood pressure goes up, and later he gets collapsed and blood pressure comes down; if you give him another cigar a week afterwards his blood pressure goes up as before. If you, however, give that boy a cigar every day, after a time his blood pressure does not go up; he has learnt to destroy the nicotin just as you may learn to destroy morphin.

18937. Is the moral of that to begin smoking as early in life as you can?—No, but I think the moral is that it is better to be a moderate and continuous smoker than to smoke in fits and starts.

18938. And never to discontinue your smoking?—Yes.

18939. I see you mention, among other things, the action of tobacco smoke and the origin of nicotin immunity. Is that at present being investigated in your laboratory?—Yes.

18940. In tobacco smoke itself very little nicotin is contained, is it not?—There is a good deal. Practically the whole effect of a cigar is caused by its nicotin; the other constituents are unimportant.

18941. Has it a slightly paralysing effect, or what is its effect?—The first effect of nicotin is to excite. It excites and raises the blood pressure for half an hour; it stimulates everything for half an hour, and then after that you get the depressant action.

18942. (*Sir Mackenzie Chalmers.*) I see among other matters that you are investigating in your laboratories are the action of new cardiac tonics, the action of caffein, urea, and saccharin on the kidneys?—Yes.

18943. Are those experiments animal experiments?—Yes.

18944. Have they an important bearing on human treatment?—In connection with caffein, urea, and saccharin on the kidneys. The point is that we know nothing, at least I think I am right in saying nothing as to the cause of renal disease how it originates. I had some sort of conception that renal disease might be caused by taking tea and coffee, and therefore we used caffein. Or it might conceivably be due to excessive urea, and therefore we tried urea. Urea has a marvellous effect on the kidneys. Thirdly, saccharin is a drug which is used to produce sweetening in the case of people who suffer from diabetes. A drug allied to saccharin I know will produce renal disease. We knew that, and so these drugs are being given at the present time by the mouth to dogs to see whether there is any effect; that is to say, they are simply being fed, some taking caffein as we take tea, some having their food sweetened with saccharin and some having urea put in their food.

18945. Saccharin at any rate is largely taken by certain classes of people, and it is important to find out whether it has any evil effects as well as good effects?—Yes.

18946. And to some extent, I suppose, the result of your experiments may be either to acquit or convict tea and coffee and certain other agents?—These are excessive doses of caffein.

18947. Still, long continued small doses may possibly have some effect?—Yes, as we know that they have such a marked effect on the kidneys.

18948. You are also investigating the relative action of the new local anæsthetics?—Yes.

18949. Including cocaine or eucaine among the new ones, or strovaine?—No, cocaine is at present a local anæsthetic in the pharmacopœia. e object to cocaine because in a considerable number of cases it produces toxic symptoms, and because it is irritant. Accordingly this Committee of the British Medical Association asked me to have the newer local anæsthetics investigated, so that we could make a report as to which of the many on the market supplied by wholesale druggists and chemists was the best, and this research is just about completed.

18950. It will have a most important effect on minor operations on human beings you think?—Most important. We are every day doing more and more operations under local anæsthesia. Now we inject the local anæsthetics in quite a number of cases into the spinal theca. You inject it through the back, you make the patient sit up and lean a little forward, and inject it into the spinal theca, where, of course, it paralyses first the sensory cells, and the man loses all sensation to pain. Then touch goes next. Pain goes first, then tactile sensation, and lastly motion. That is the sequence that always occurs; no matter what anæsthetics you use the paralysis goes in that order—pain, then touch, then motion. The man cannot feel anything, but he can still flex his toes. If you put these drugs——

18951. (*Mr. Tomkinson.*) Pain goes first?—Yes.

18952. What second?—Tactile sensation; that is to say, a man can feel that you stroke his leg, but he cannot feel any pain. You can put a hot iron on his leg and he would look at it and feel you do it, feel all the sensation, but would feel no pain.

18953. And what goes last?—Motion.

18954. Where does consciousness come?—Consciousness is retained all the time. This is local anæsthesia.

18955. (*Sir Mackenzie Chalmers.*) How do you tell with an animal when the sense of pain is abolished by local anæsthetics? I can understand that you can investigate the action of these things with a human being, who will tell you his sensations, but I cannot understand how you can get any trustworthy results from an animal?—There are several modes, of course, of using those drugs. In this case a number of experiments on the anæsthetic point were done on men, on ourselves. I mean that a man had his arm bared and one drug was injected there, and another drug given here, and you can test it in that way. But where the animals came in mostly was as to the toxicity of these things. The anæsthesia is one property, the toxicity is another. The irritability is another, the toxicity and irritability are tested on animals.

18956. The problem is to find a thing which is a perfect anæsthetic, and at the same time a non-toxicant and a non-irritant?—Yes.

18957. Your name has been referred to in the evidence, I do not know whether you have read the evidence given before this Commission, but I think Dr. Gaskell will ask you about those particular experiments. Has your attention been called to that?—Yes, I have not read it, but I know the point to which I am referred.

18958. (*Mr. Ram.*) Sir Mackenzie Chalmers has asked you nearly all I wanted to ask you, and there are only two or three questions left that I wish to put. With regard to these local anæsthetics, of which you have just spoken of, you said that you could inject one or more of them into the spinal cord and produce anæsthesia by that means?—Yes.

18959. Would that be local anæsthesia or general anæsthesia?—I should not like to say which. It is anæsthesia below the point of injection. You inject it just where the spinal cord ends into the sub-dural fluid there, and it paralyses the lower part of the spinal cord. It is local, of course, in that it does not affect the other part of the body.

18960. It affects the portion of the body below the point at which you insert it?—Yes.

18961. And affects that part of the body universally?—Yes.

18962. And equally?—Yes.

18963. And completely?—Completely.

18964. When once you have arrived at a local anæsthetic which is not toxic and not irritant, is it safer to use a local anæsthetic than such an anæsthetic as chloroform or ether or the A C E mixture?—It is not a question so much of safety as of convenience and necessity, I think.

18965. But there are certain dangers we know affecting what we generally know as anæsthetics, chloroform, ether, and so on?—Yes.

18966. Are there any dangers, so far as you know, incident to the use of local anæsthetics?—With some of them there are considerable dangers. With cocaine you may, in certain people, accidentally inject the cocaine into a vein when you inject it, in which case it produces a toxic action on the brain and a toxic action on the heart. Cocaine is a toxic body. And, secondly, you may get sufficient irritation at the seat of injection to produce objectionable symptoms. I

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think we have now got a local anæsthetic; as the result of this work we have certainly determined the best, and are going to recommend that that one should be put in the pharmacopœia.

18967. With regard to all these researches that you have told us of, I gather from what you said that experiments on animals are not necessary for all, but for some?—Yes.

18968. In the case, for instance, of subjecting animals to the action of caffein, urea, and saccharin to see how they act on the kidneys, do the animals suffer any pain?—No, none at all. You can give saccharin to a man; it simply sweetens his food. He may object to having his food sweetened, but you cannot call that pain.

18969. And the same with animals?—Yes.

18970. (*Chairman.*) The animal is not operated on afterwards to find what the result has been?—No, we kill it afterwards.

18971. After what?—After three or four months; after they have been subjected to this experiment for three or four months the animals are killed—they die.

18972. (*Mr. Ram.*) And are dissected as dead animals?—Yes; and sections are cut of their various tissues.

18973. Have you at present found any result from the experiments as to those three matters, caffein, urea, and saccharin?—We have only killed one dog at the present time, and we killed that dog because we found that it was getting ill, that it began to lose its hair, and began to pass albumen in its urine, so that it was killed and its kidneys were found diseased. That was with urea.

18974. Do you attribute the disease of the kidneys to the action of the urea that you had administered to the dog?—We do. We cannot be certain until we have done two more experiments to make sure that it is a constant effect.

18975. With regard to the effect of drugs on blood clotting, have you experimented on animals with regard to that?—Yes, they were all done first on animals, only subsequently on man.

18976. What is the nature of such an experiment as that?—The nature was to take a drop of the animal's blood first. There is a convenient vein in the ear of a rabbit, and you just make a prick with a needle, you get a drop of blood, and take its clotting time with a certain instrument. Say, it is three minutes; then you give the animal a dose of the drug, the effect of which you want to determine. Supposing you give citric acid, you take its clotting time at various periods afterwards, and you find that the citric acid will prolong the clotting time from three to four, and five, and ultimately to fifteen minutes; and then there is gradual recovery.

18977. Does it make the blood more slow to clot?—It would make it more difficult to clot.

18978. When you say ultimate recovery—recovery from what?—The animal is none the worse for the citric acid.

18979. Then, with regard to the standardisation of drugs, you spoke about the necessity of using animals for experiment, and I see in this pamphlet that you have handed to us, which you put it in I think to show the varying nature of the strength of ergot?—Yes.

18980. This caught my eye: "For my part I unhesitatingly express the belief that many hundreds of patients die annually from digitalis and its allies not possessing the virtues which are required of them."—Yes.

18981. That is your belief?—Yes.

18982. If you could get a perfect standardisation for digitalis, for instance, then a doctor administering it would know exactly the strength that he was giving?—Yes.

18983. At present I suppose in many cases he has to give it in absolute ignorance of the strength of the drug he is administering?—In absolute ignorance.

18984. Intending to give perhaps a strong dose of digitalis, and giving a quantity which in a proper preparation would be strong, in effect he gives next to nothing to the patient?—Yes, he gives none.

18985. And the patient dies perhaps?—Yes.

18986. Is that true of what you call the allied drugs which I think you said were strophanthus and squills?—Yes.

18987. When you have arrived at a perfect knowledge of what the dose ought to be, and the strength ought to be, would it be possible to cause the sale of the standardised drug to be universal?—I think so, certainly. If a drug is not sufficiently active, it ought not to be used, and if it is too active it can be diluted to a certain definite strength.

18988. I do not think I made my question quite plain. I am supposing that you have got a quantity of a drug of exactly what you consider the right strength—that you have satisfied yourself of the actual strength by tests on animals?—Yes.

18989. That quantity is sold out to different chemists?—Yes.

18990. Does it retain its standardised strength?—That is the point the Pharmaceutical Society specially asked us to investigate, and we have done it in a number of cases now; it retains its strength for eighteen months, and it is not necessary to go longer.

18991. It would be possible then for any chemist in the country or anywhere else to supply himself, if he went to a proper place, with an amount of this drug or any other similar drug of what was known to be the proper strength?—Yes.

18992. You spoke of taking curare yourself accidentally by the mouth. How is curare generally administered to an animal?—Intravenously. It has no action, of course, when given by the mouth.

18993. You proved that to your own satisfaction?—Yes.

18994. I should just like to ask you one thing before you leave the standardisation of drugs. Is there any pain caused to the animal that is experimented upon, beyond the mere prick of a needle?—I do not think so. Of course we standardise digitalis on frogs which have very little brain.

18995. I ought to have asked you that before, what animals are generally used for standardising drugs?—In the case of digitalis, stropanthus, and squill we use frogs; in the case of ergot we use cats, that is under licence alone and anæsthetised the whole time; and with cannabis indica we use dogs. The drug is injected, the dog becomes intoxicated, it recovers completely, and it is none the worse for it; indeed cannabis indica intoxication is rather pleasant, there is nothing objectionable in it at all. It is greatly used in India as a pleasure.

18996. Then, in point of fact, it comes to this, that in all this standardisation of drugs either the animal is under anæsthetics, or there is no suffering at all?—Yes.

18997. (*Dr. Gaskill.*) I think perhaps some members of the Commission did not quite understand what you meant when you spoke of anæsthetics causing the sensory cells in the spinal cord to be put out of function, while the motor ones still remain, so that reflex actions are still possible?—What I mean is, that you can get a stage in chloroform anæthesia when you can show definitely (this is not a question of opinion at all, it is a question of absolute fact—it is in all the text books of repute) that in the spinal cord the sensory cells are paralysed and the motors are still active.

18998. What do you mean by the sensory cells in the spinal cord. That is what I think the Commission did not quite understand?—There are two sets of cells in the spinal cord, the cells which receive and the cells which give out impulses, and we use from analogy the word sensory to mean receptor—receiving.

18999. Is it possible for reflex action to take place without those sensory cells being involved?—Yes.

19000. So that really the sensory cells, which would be the communication between the cord and the higher centres, would be put out of action; while a simple reflex would still be possible because the motor cells would not be put out of action. That is what you mean?—Yes.

19001. I see that you say in your *précis* that a dose of morphine such as two grains administered to man would lead to deep sleep, from which he is aroused with difficulty, while larger doses are followed by coma and complete unconsciousness?—Yes.

19002. So that you would consider that two grains to a man leads to complete absence of pain?—Yes.

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19003. And deep sleep?—Yes.

19004. What I want to know is, does the dose of morphia required bear a distinct relation to the body weight of the animal used?—No, it bears relationship to the amount of brain substance.

19005. And that bears a relation to the body weight?—Yes.

19006. What I mean is that a small dog and a large dog would not require the same dose to produce the same effect?—Certainly not.

19007. Could you draw any relationship between the dose for a man and the dose for a dog, considering the difference in size of the man and the dog?—I should not like to draw any relationship.

19008. But I meant rather if two grains is a dose for a man to produce very deep sleep, would you consider that two grains, or more than two grains, or less than two grains would be required for a dog very much smaller in size than a man?—One grain in a dog, of course, produces a very marked effect. You want much less relatively to the two animals.

19009. Would that be partly on account of the size of the animal and partly on account of the development of brain substance?—Yes.

19010. Both those things would combine in the same relation?—Yes.

19011. And cause a lesser amount of morphia to produce the same effect in a dog as in man.

19012. Then I see that the Hon. Stephen Coleridge included your name in one of the five cases, I think it was, that he prominently brought before us as having infringed the Act—he referred to your paper by Dixon and Brodie in the "Journal of Physiology," Vol. XXIX., No. 2, page 144?—Is that all that was mentioned? I am afraid that I have not read it.

19013. That is what he refers to in his evidence here. The point of it is this, if I might just say what he accuses you of. At No. 10371 he says: "On page 144 these two vivisectors make the following statement: 'In studying these reflexes we have found it of the utmost importance to avoid the use of chloroform or ether as the anæsthetic. The experiments must therefore either be performed upon unanæsthetised animals, upon animals anæsthetised with morphia, or upon decerebrate animals. Our experiments were usually conducted under one of the two latter conditions, but in a few instances were repeated upon animals lightly anæsthetised with chloroform.'" This point is that, as you have said, they must be performed upon unanæsthetised animals, or other, when you speak of light anæsthesia, you really mean unanæsthetised animals. That is what I understand.

(*Dr. Wilson.*) I should say incomplete anæsthesia.

(*Witness.*) I think he ought to have referred in the same paper to the beginning, to page 102 of the same research, which I have here, under the heading of "Animals, Anæsthesia, etc." In that heading it says: "The animals mainly used were cats. With the exception of a few experiments, in which the animals were killed by pithing, all were anæsthetised, usually with ether, but at times with chloroform, or with the ACE mixture. In experiments on dogs we generally administered morphia as well. In a few cases we anæsthetised first with ether and then injected urethane, either intravenously or intraperitoneally." That is the first thing, at the beginning of the paper where one always describes the anæsthetic effect. I know, of course, Mr. Coleridge's Society reported the statement on page 144 to the Home Office, and they replied by pointing out to him this page 102 where we definitely state that all the animals were anæsthetised. If I may just refer to page 144, the words which are quoted are perfectly correct, but in order to understand exactly what they mean, the context is really necessary. These experiments were in connection with asthma, and they were to show that when, by any cause, you excite a certain portion of the septum of the nose, you get constriction of the bronchioles. You never get that constriction of the bronchioles if there is much volatile anæsthetic in the lungs, because a volatile anæsthetic in the lungs paralyses the nerve endings. Accordingly, we state: "The experiments must therefore either be performed upon unanæsthetised animals," which is, of course, quite impossible, first of all because it is not allowed, but also because you cannot do an experiment on an unanæsthetised animal where the whole point is absolute stillness. You must not even let a man walk across the laboratory when an experiment of this kind is going on. The movement of a hair would spoil the whole thing, so we say, "Upon animals anæsthetised with morphia or upon decerebrate animals. Our experiments were usually conducted under one of the two latter conditions"; that is to say, the animals were decerebrate or they will have had chloroform first and then morphia, and then subsequently when everything was quite still, no more chloroform being given, the excitation of the nose was done. The least little bit of sensation of any kind would, of course, immediately have produced a big effect on the animal absolutely vitiating the experiment. That was the experiment. Then we say at the end that the experiments "in a few cases were repeated upon animals lightly anæsthetised with chloroform." There is really nothing to comment upon on that. They were anæsthetised. An ordinary surgeon, when he does an operation, only deeply anæsthetises his patient when he wants to relax the muscles, when he wants all the muscles, say, of the abdomen to be relaxed so that he can feel a tumour at the back of the peritoneal cavity. Then he puts the patient under deep anæsthesia. But in ordinary cases he uses light anæsthesia, and, personally, I think the lighter the anæsthesia the better.

19014. (*Dr. Gaskell.*) Mr. Coleridge's comment is: "They cover up the anæthetisation, in my opinion, by speaking of them as 'lightly anæsthetised with chloroform,' because if they were anæsthetised properly with chloroform, according to their own description, the experiments would be valueless"?—Of course, if a man says that the animals are not anæsthetised, it is his business to bring evidence to show it. All I can say is that if there were the very slightest movement or were the sensory nerve centres acting in any way, the animal would move, and one movement of any kind would vitiate the whole experiment. May I pause to show you the delicacy of the tracing referred to? It would be impossible to take a thing like that with the slightest movement (*handing in a tracing*).

19015. Then, so far as I understand it, your remark, "The experiments must therefore either be performed upon unanæsthetised animals," etc., did not in the smallest degree imply that you yourself ever performed experiments on unanæsthetised animals?—Certainly not. That is stated clearly at the beginning of the paper. The pages are given on the outside of the book. There are two pages—144 and 102.

19016. Now I think you would like to tell us some few points with respect to the administration of the Act as at present. You have not referred to anything of that sort, although I see it is down in your *précis*. We should like to know what, in your opinion, are the difficulties and the annoyances which the present Act causes to workers?—At Cambridge we encourage the men when they are qualified to come and spend six months in the laboratory learning to observe, doing a piece of useful research work which shall enable them to take their M.D. in research. Those men come, and I suggest some useful object for them to work upon in their six months; but often they cannot start; they are kept delayed sometimes as long as two months waiting about until they get their licence to begin. They lose heart; it is difficult to give them anything to do, and altogether it is a most discouraging thing to the men.

19017. (*Dr. Wilson.*) They have the M.B.; they are qualified?—Yes, they are qualified medical men, but we require a man to show something of his own capacity before we give him the M.D. Then I have had a good deal of correspondence with the Home Office, because I wrote up and asked for certificates for painless experiments, and they replied, "If you want to do painless inoculations you do not want any certificate at all; you can do those as much as you like—they do not come under the Act." But they added, "If you do them you must do them on your own responsibility," which means that one can be prosecuted at any time for doing these experiments, and as one does not quite know how the Act is going to be interpreted, one has, of course, to say that they are not painless, but painful, and you get your certificates. Then at the end of the year, when you send in your report, you have to state whether the experiments performed were painless or painful.

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19018. What sort of experiments are you referring to?—Inoculation experiments.

19019. (*Sir Mackenzie Chalmers.*) The results may be painful?—The results that I am speaking of in these cases were painless.

19020. And you knew they would be painless?—You have moral certainty. For instance, the injection of caffein must be painless; it cannot be otherwise. Yet you must have a licence, and you have to put in at the end that the experiment was after all painless. It seems to me rather absurd to have to go through all that. Of course, the Act at the present time says, "Experiments calculated to give pain." In my opinion most experiments are not calculated to give pain; in fact, in ordinary vivisection none of it is calculated to give pain. I have a perfect right, according to that, to perform experiments without a licence at all, because they are not caluclated to give pain. If they were calculated to give pain you would find very few people who ever performed experiments. Then, as a second point, what I recommend in order to avoid this trouble to be rid of all these complications and make the thing as simple as possible, would be to have one licence and to give a comprehensive licence to the head of a laboratory or to any other responsible person.

19021. (*Dr. Gaskell.*) Do you mean by that to abolish all certificates?—Yes.

19022. But upon the licence you would state the experiment that was intended to be done?—No, I should not. I should give a complete comprehensive licence to responsible persons, heads of laboratories; and to workers, to people who were not so responsible or who were not heads of laboratories, and so on, I would give an endorsed licence, only allowing them to do certain experiments, and indeed only doing those experiments under the control or supervision of someone who had had more experience in research than they had.

19023. Again without any special certificates?—Yes.

19024. In fact, you favour the abolition of the certificates altogether?—Altogether.

19025. Then, I suppose, you would require, were such licence to be given, that a report of the whole experiment should be sent into the Home Office at the completion of the experiment?—Certainly, of everything that was done. I think that would be an excellent provision. At the present time, supposing you start a research, it does not matter what it is, it never leads to where you think it is going. As soon as you get into it you find that there are branches in all directions. You have to follow one branch; you have to use different animals from those you put in your certificate. There is delay of a fortnight, three weeks, or a month, it may be, before you get that certificate back. You may have to perform inoculations interperitoneally instead of subcutaneously, and there is more delay again in getting fresh certificates. At the present time I have had, I think, several certificates A, each fresh one allowing me to do a different thing. I have had a number of Certificates A—I do not remember the exact number.

19026. With respect to these senior students, would you restrict this licence limited by endorsements to students who had already taken a degree, or would you allow it to students who were in *statu pupillari*?—If the experiments are done under the control and supervision of a responsible person, I do not see why any intelligent and educated man should not be a fit and proper person to do them, whether he happened to have a degree or not.

19027. It is hardly necessary to ask you would you consider it for the benefit of the men themselves, who are sending in these theses for M.D., this form of research is valuable and good for humanity at large?—I think it is one of the most valuable parts of their education. I am quite sure that the men who have done six months' research work look upon the subject in an entirely different fashion. They have learned in that six months to observe; they see things in the simplest experiments that they never observed before.

19028. And you would rather encourage an increase in the number of advanced students doing the research work of this kind than discourage it?—Certainly, I would thoroughly encourage it.

19029. (*Sir Mackenzie Chalmers.*) I suppose that if advanced students were allowed to do research work you would impose this condition, that it should be done under the immediate supervision of the head of the laboratory, and that the anæsthetic should be given by some person who understands anæsthetics?—Yes, I think that is most important. With regard to anæsthetics, anæsthesia at the present time is a very simple process. We do not use chloroform; it is altogether too much trouble; you have to be continually looking at your animal and its reflex the whole time, adding a little more and stopping it.

19030. You prefer urethane?—I use urethane.

19031. Because the worst that can happen is that the animal if kept too long under the anæsthetic may die?—Yes.

19032. Have you ever had to perform a painful experiment on an animal without any anæsthetic?—Never. I have never seen a painful experiment on an animal.

19033. Without anæsthetics?—No, I have never seen a painful experiment on an animal.

19034. (*Mr. Ram.*) Would you advocate students being allowed to vivisect animals under a lethal dose of an anæsthetic in order to acquire manual dexterity?—No, I would not under licence.

19035. You think that manual dexterity can be sufficiently acquired without using animals?—Do you mean for surgical operations?

19036. Yes?—It is a very large question, I think. It is used at the present time largely in America. The man has to go out into practice. Where does he get his practice now? He gets it by practising on man. A country practitioner who is qualified, and gets his M.D. at Cambridge or London, may have had no experience, perhaps he has been house surgeon; but wherever he goes he has to learn to do his surgery on human beings.

19037. That is why I asked the question. Do you think it would be well to allow the use of animals under a lethal dose of anæsthetic to enable students who are perhaps qualified medical men to gain manual dexterity in the performance of operations on human creatures?—Yes. I think it would be a wise provision to make for the sake of humanity.

19038. Do you think that experimenting on the bodies of animals would give such manual dexterity? Is the formation of an animal's body so similar to that of a human body that a man by experimenting on an animal could gain manual dexterity when he came to operate on human beings?—I am quite sure that he could. I am quite certain on that point.

19039. (*Mr. Tomkinson.*) You claim that morphin and similar narcotics are an absolutely satisfactory preventive of sensation if properly administered?—Yes.

19040. Their action is first upon the sensory nerves?—Yes, the sensory cells.

19041. And afterwards upon the motor nerves?—Are you referring to morphin now?

19042. Yes?—In the case of morphin you want very large doses to affect the motor cells. I made a distinct difference between morphin and the alcohol anæsthetics. I said that there are two groups, the morphin group and the alcohol group.

19043. Is the first symptom something approaching stupefaction?—Yes.

19044. Stupefaction or very deep sleep?—Yes.

19045. Then you claim that the sensory nerves are rendered insensitive to pain?—The sensory cells become quite inactive.

19046. Although there might still be motion?—Although you can still get reflexes. The motor cells are intact.

19047. Then before the discovery of chloroform was it used?—Yes, it was to a limited extent.

19048. To any general extent?—I do not think it was used to any general extent.

19049. Was not the discovery of chloroform hailed as one of the most beneficent discoveries, and the first real alleviation of the horrors of the hospital?—Certainly, quite commonly operations were done without any anæsthetic previously.

19050. And it was generally accepted that an operation was a terrible ordeal; that amputations of limbs

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and other operations were so terrible that people could not survive them or live under them?—Yes.

19051. Morphia was not recognised then certainly as anything at all in the same class?—The point about morphia, you see, is that you dare not give it beyond a certain dose, because the respiratory centre gives out. In animals it does not matter if it does give out, because you can do artificial respiration.

19052. (*Dr. Gaskell.*) In those days was it not given by the mouth as laudanum? Did the hypodermic syringe exist in those days?—No; and then the morphin took a huge time to act. It was not morphin; it was opium.

19053. (*Mr. Tomkinson.*) The idea of instilling it through a vein or the skin was not known?—No.

19054. And that was the great difference?—That was one great difference.

19055. Then curare is an anæsthetic in a large dose. I understand you to say that to be effective it must not be given through the mouth in the way that you accidentally took it, but instilled by inoculation?—Yes.

19056. Then it becomes immediately effective?—Then it becomes immediately effective.

19057. I did not quite grasp what was the first step in the experiments in regard to wood poisoning by which animals were utilised. The men were suffering from a certain complaint?—Yes.

19058. Was a drop of their blood taken?—No, the wood was sent to us, the shuttle wood, in the form of sawdust, and the alkaloids were extracted from the wood; then we tried the action of the alkaloids on animals, and the effect on animals was exactly similar to the effects that were being produced on men.

19059. By inoculation?—Yes.

19060. That was not a painful experiment, nor did the animals suffer much?—No.

19061. But they showed the same symptoms that the men suffered from?—Yes.

19062. In regard to the causing of death by blowing air into the vein, would that be a very painful death to an animal not under anæsthesia? It stops the action of the heart, I understand?—Yes, immediately. It would be just as if you were shot, I take it. It is an immediate cessation of the heart's action. There might be a moment's pain. I could not say.

19063. Is that the common way of destroying animals before they recover consciousness?—I think it is the quickest and easiest way by which you can kill them. Sometimes I kill them before the class with chloroform to show them exactly what happens to the heart and respiration. I might sometimes kill them with morphin.

19064. When you speak of the brain being paralysed, does that mean insensibility to pain?—Yes.

19065. That is the form that the anæsthetic takes?—Yes.

19066. It paralyses the brain and arrests its receptivity?—Yes, it cannot receive any impulses from outside, and so the animal or man becomes unconscious.

19067. With regard to cocaine as a local anæsthetic, that is introduced through the skin?—Yes, subcutaneously.

19068. At the part which it is desired to anæsthetise?—Yes, it is injected under the skin.

19069. For a local operation?—Yes.

19070. How deep will it extend?—The anæsthesia extends to the muscles. The muscles are not very sensitive, of course. It would not extend far beyond the skin.

19071. We have had it stated here by one witness on the subject of veterinary operations on a farm, that he sometimes uses it in the case of castration, in your opinion would it be a tolerably effective anæsthetic in that case?—I think it would, because there is such a lot of cellular tissue there; there are no muscles, and it would very readily get at all the nerves.

19072. Is it introduced by injection?—Simply by injecting it into the scrotum.

19073. You spoke of injecting into the spinal cord. That would be rather an important operation, would it not?—No, it is quite painless. You simply, if it is a man, make him sit up and bend forward, and you take the needle and put it in; it has to go in quite a long way, a surprisingly long way, until you feel the bone or front part of the spinal cord, then you withdraw it a little, and you get the spinal fluid coming out. Then you fix your syringe on to the needle and squirt as much of the local anæsthetic as you require into the fluid of the spinal cord.

19074. (*Mr. Ram.*) Have you done that on man without anæsthetics?—Yes.

19075. And was there any pain?—No, no pain at all.

19076. Just the prick of a needle?—Just the prick of a needle.

19077. (*Mr. Tomkinson.*) You strongly deny and protest against the idea of demoralisation being caused by familiarity with vivisection experiments?—Certainly. Familiarity with pain would be quite a different matter in my opinion.

19078. (*Mr. Tomkinson.*) Have you any knowledge of the system carried on in Paris, I believe without anæsthetics?—No, I have no knowledge of it.

19079. Have you never heard of the Veterinary Surgical Establishment in Paris?—Are you referring to the Pasteur Institute?

19080. At Alfort, where experiments were carried out upon living horses in the Paris veterinary schools?—I am afraid I know nothing about it.

19081. With regard to the question of demoralisation resulting from familiarity with vivisection experiments, it has been stated by an English veterinary surgeon, who had gone over there to study, that the demoralisation of the French students and their apparent entire indifference to the torture of animals, was something too terrible for him to even stand?—I do not know the fact; I have not heard of it, but I can quite believe it. But, of course, there is no comparison. No student in England has ever seen pain in an animal experiment.

19082. Then your position is that you totally deny that under the system, as carried out in England, any pain is inflicted?—Certainly. My students, practically all of them, are graduates of the University, and most of them have taken an honours school previously. Surely one would not show a man of that stamp experiments involving pain.

19083. Not if they could help it?—They would be the first to protest if such a thing were possible even.

19084. You are aware, I suppose, that there is on the part of the public, rightly or wrongly, considerable doubt and scepticism on the point of there being no pain inflicted in vivisection experiments?—Of course.

19085. Would it, in your opinion, be satisfactory if there were to be a freer right of entry or permission of entry, to witness these experiments by independent, properly accredited witnesses, in order to reassure the public, if possible, on that point. We all know that certain people are prepared to observe what they wish to observe?—It is a perfectly common trait in all forms of life. If the people were responsible people, that is to say, if they had had a medical education, I should not object at all.

19086. I purposely said, I think, properly accredited people, not violent partisans who went there in order to make a case?—Precisely. Then I should be the last person to object; whenever I do an experiment before a class, I have perhaps 40 or 50 witnesses.

19087. (*Sir Mackenzie Chalmers.*) Do you have any of your advanced students at your research experiments?—Yes.

19088. Besides yourself, when you are doing a research experiment, there are a certain advanced students allowed to be present?—I have a considerable number of advanced students working in the laboratory doing these experiments.

19089. Under licences?—Yes, six or seven, I should think.

19090. They are licensees and qualified men?—Yes.

19091. What I meant was, when you are doing research experiments, are they always done absolutely in the presence only of yourself and a laboratory assistant, or are advanced students allowed to be present and see what goes on?—I never do them alone.

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When I do an experiment, it is such an educational advantage to other people to come in and see it that I should be the last person to prevent their coming in. Any one who wishes can always come in.

19092. (*Dr. Gaskell.*) The question is, do they?—Yes, they do, certainly.

19093. (*Sir Mackenzie Chalmers.*) They do, in fact, advanced students do come in and see the experiments? —Certainly.

19094. Then we have not only your word for it (I do not say it in any offensive sense), that these animals are properly anæsthetised, but we have got certain other qualified persons there who are also able to judge whether the animal is properly anæsthetised?—Yes, at practically every experiment.

19095. (*Dr. Wilson.*) You would still agree that experiments on animals should be regulated by Act of Parliament in this country?—I certainly think so. You might have all kinds of irresponsible people doing them otherwise.

19096. You also contend that certificates should be abolished?—Yes, they lead to continual complication and bother. A man might quite easily, unless you take the most stringent preventives, do experiments under the present complicated system which he was not entitled to do.

19097. You object to them on account of the delay which is sometimes entailed on application to the Home Office?—The delay and the complication.

19098. Now, as the General Medical Council is responsible for the education of medical students throughout the kingdom, would you be prepared to advocate that that Council should be held responsible for the issuing of licences rather than the Home Office? The Home Office has to see that the Act is carried out, but in difficult cases the inspector is consulted as to whether it is advisable to permit experiments or not?—Yes, I think it would be a very good plan that some responsible body of that description should have the power of issuing licences. I had not thought of it before.

19099. Are all the experiments carried on in your class room on animals painless experiments?—Yes, all painless.

19100. And it is perfectly understood by your class, I suppose, that they all must be painless experiments? —Yes, we always discuss it. I discuss with my class the anæsthetic which the animal is under and the men all come up and examine it; and they learn from that fact alone. And then we go on with the experiment. I think it is a most instructive thing.

19101. Would you object, then, to a notice being put up in the class room to the effect that "all experiments carried out on animals in this class room shall be painless," because I suppose a good many students do not know much about the Act?—I suppose that a good number of them do not, naturally.

19102. Would you object to such a notice?—I should have no inherent objection. I do not know whether I should approve of it.

19103. Would you be prepared to certify that, to the best of your belief and endeavour, for example, all experiments which you have been carrying out have been carried out painlessly, in reporting to the Home Office?—Yes.

19104. Would you be prepared to append a certificate to that effect?—I always do append a certificate to that effect. I think in two or three cases I put down that they had experienced "slight pain," or something of that kind. After a subcutaneous injection of some drug an animal feels ill after it, and you have to say that it feels pain.

19105. Now with regard to the experiments carried on for the standardisation of drugs, those experiments, I suppose, are carried out for the Pharmaceutical Society?—They have been. We have done them simply to try these special drugs. This standardisation of these drugs was a new thing, and we did it in order to show how variable these drugs were naturally.

19106. But certain manufacturing chemists, I suppose, employ experts to carry out experiments for them?—Yes, I believe they do.

19107. There are only, I suppose, a very few drugs about which there is any real doubt as to their physiological action?—As to their strength. The more important ones, far and away more important than any others, are, of course, digitalis and ergot and stropanthus.

19108. And so long as these drugs are used you contend so long must experiments be carried out on animals to standardise them?—If a time arose when we understood sufficiently of the chemistry to estimate the active bodies chemically, then the need for animal experiment would disappear at once.

19109. But until that time arrives you must continue to experiment on animals?—Yes, it is the only way.

19110. In experimenting upon man, ergot is generally given by the mouth, is it not?—Yes.

19111. Always?—No not always, it is sometimes injected. In fact, if you want to be sure of its action in these days, and you want a quick action you inject ernutin or ergotoxin; you then get its action quickly and certainly.

19112. And are these preparations as reliable as ergot itself?—They are standardised.

19113. With regard to those experiments on African boxwood, of course, the alkaloid was absorbed by the workmen during the process of manufacture by inhalation?—By inhalation.

19114. You did not try any experiments by using the sawdust on animals in that way?—I made beds of sawdust for the animals and they did not show any ill effects at all. But then these people work in a cloud of dust—you cannot see across the room I am told; it is an absolute cloud of dust; one could not be continually shaking up an animal's bed.

19115. But with the perspiration there might be some absorption through the skin?—I do not think so. Drugs are not absorbed through the skin except under very rare circumstances.

19116. (*Sir William Collins.*) I understand you to suggest the abandonment of certificates under the Act, but that you think it would be well to retain the practice of licensing under the Act?—Yes.

19117. May I ask whether that is in the interests of the public or of the animals or of those who practise vivisection?—I should say that it was on behalf of the public and those who practise vivisection. I do not think the animals come into it. I do not think they are affected. They would be the same in either case.

19118. The advantage then would be on behalf of the public and those who practise vivisection?—Certainly.

19119. What advantage do you think accrues to the public?—That you are able to do researches quickly and able to do them more efficiently, and therefore, as all the results are for the good of the public, they must benefit.

19120. But why are you able to do researches more quickly if you require licensing than if you do not?—The trouble at the present time is with the complication of certificates.

19121. You did not understand me. I am agreeing for the moment, for the sake of argument, with your suggestion that there should be no certificate. I understand that you suggest that there should be a licence under the Act?—Yes.

19122. What advantage is it to the public that there should be a licence under the Act as against having no licence at all?—As against having no licence at all, I beg your pardon, I misunderstood your question from the beginning.

19123. I thought so. Perhaps I had better put it from the beginning again?—Thank you.

19124. I understand that you suggest the abandonment of certificates, but that you think it well to retain the practice of licensing under Act of Parliament?—Yes.

19125. Is such licence in your opinion to the advantage of the public, or of the animals, or of those who practise vivisection?—It is to the advantage, I think, of everybody.

19126. Will you tell me in what way you think it advantageous to the animals?—Because you might have irresponsible persons—anyone could do it—making experiments. I do not think that irresponsible persons should be allowed to do that, and I do not think that experiments should be made except in proper places.

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19127. Do you think that there were irresponsible persons making these experiments in this country before the Act of 1876?—I think there is not the slightest doubt about it.

19128. (*Sir William Church.*) Before 1876?—Yes, before the present Act. I should say there is not the slightest doubt about it.

19129. (*Sir William Collins.*) It has been stated before the Commission that the Act was superfluous because there was no occasion for legislation?—It is a question that I have not looked up. I am not prepared to give you facts and figures, but I think so far as I remember my reading that there is no doubt that unnecessary experiments were done before that time.

19130. Do you think that the effect of the Act has been to reduce the amount of vivisection in this country?—Undoubtedly; especially directly after the Act; it absolutely stopped a long series of experiments that were going on. Very many people were unable to continue their experiments entirely owing to the difficulty of the Act, and researches were allowed to lapse for a time.

19131. Do you think that there has been less vivisection this year than there was in the last year before the Act of 1876—that is, in the year 1875—in this country?—No, much more. But you take into consideration the leaps and bounds that medicine has been making in this time. It is just the time that medicine has been making those great advances.

19132. Do you think that if the Act were repealed now that there would be danger of animals being unnecessarily employed for purposes of vivisection?—I think the medical profession would be strongly against the Act being repealed now, because, as I say, any medical student, any irresponsible person, could make all kinds of experiments if he wished to do so at his private house.

19133. We have had one physiologist here present who said, and I think he said his opinion is shared by others, that the Act had operated altogether antagonistically to the advance of physiology. Is that your opinion?—I do not agree with that.

19134. Then you think it would be unsafe to repeal the Act for fear of unnecessary and irresponsible vivisection?—I think so, certainly.

19135. Would you tell me what advantage you think the Act secures to the public?—I think it does, or at all events ought to, give them confidence that animals are not unnecessarily subjected to pain.

19136. And what advantage do you think it secures to those who practise vivisection?—To those who legitimately practise it, those responsible persons, I do not think it makes any difference, but they would be subjected to less abuse if it was known that there was an Act in existence than otherwise, of course.

19137. You think that the Act affords protection and security to those who practise vivisection?—Undoubtedly. We are not liable to be wantonly prosecuted; there is no doubt about it; it forms, of course, a very great protection.

19138. Then you would not agree with those medical witnesses who think that it would be a matter of indifference to those who practise vivisection if the Act were abolished altogether?—No. I should like to see some kind of Act still continue.

19139. You do not think it should be described as an Act that the vivisectionists are ashamed of?—No, certainly not.

19140-1. (*Mr. Ram.*) You spoke just now of certain experiments which were in course of being made when the present Act passed, and which had to be suspended or stopped in consequence of that Act. Can you tell us how did those experiments militate against the Act, so that the Act caused their suspension?—I have not followed the experiments. I was referring to Sir Lauder Brunton's experiments for one, on snake poisoning, and then there were some other experiments.

19142. Did they involve such suffering to animals as the Act forbade?—No, but as I understand it, there was very much more difficulty in obtaining certificates in those times than there has been in more recent times.

19143. Then the difficulty, I understand, so far as your recollection goes, is not that they were experiments which would now be illegal?—No.

19144. But they were experiments which, because the machinery had not got into operation, were delayed in consequence of the failure to obtain the necessary permission?—That is what I believe.

19145. (*Dr. Gaskell.*) Do you consider that before 1876 there were irresponsible persons who practised vivisection for mere curiosity, or because they took delight in cruelty? You speak of irresponsible persons who, you think, did vivisection before the Act was passed, but I am curious to know what class of person you think they were?—I should say they did it from curiosity.

19146. Do you think they were connected with the medical profession?—No, I think they were not necessarily connected with the medical profession. It is the medical profession who want the Act, in order to protect themselves.

19147. But is this remark, may I ask, that you have just made about the possibility of such persons existing simply a pious opinion of yours, or have you anything to go upon?—It is absolutely nothing that I know; it is simply a possibility.

19148. (*Sir William Collins.*) Would not that class of irresponsible persons that you have in your mind be amenable to the ordinary Act for the Prevention of Cruelty to Animals, if this Act had not been passed?—Yes, I think they would.

19149. So that for the prevention of that form of irresponsible vivisection, the ordinary law would have sufficed?—Yes, that is so.

19150. (*Sir William Church.*) I should like just to ask very much the same question that Dr. Gaskell has asked you. You meant that there was a possibility before the Act for irresponsible persons practising vivisection?—Yes.

19151. I understood you at first to say that you were of opinion that superfluous experiments were done before the Act?—No, I meant that there was a possibility; of course, there was no control.

19152. That is a very different thing, I might say I should like also to ask you a question in relation to what Dr. Wilson asked you. He asked you if you were in favour of the General Medical Council, rather than the Home Office, being the body which should grant licences and certificates, and you said that you would be in favour of a body. But he, I think, referred to the existing General Medical Council. You would not consider that, as now constituted, with the duties that it has thrown upon it by Act of Parliament, it would be a good body to refer to?—Not as it exists at the present time, because, of course, there are not necessarily the right people members of it, but there should be a reference to some responsible body, which should be composed, of course, of people who were, at least some of them, experts.

19153. Do you think that the Home Office is likely to get assistance from the Association for the Advance of Medicine by Research? You know that all applications for licences go before that body? Yes, certainly, because that body practically consists of the highest scientific opinions in the country.

19154. On the other hand, witnesses have stated before us here, that they are the worst possible body, because they are vivisectors, and so strongly in favour of vivisection themselves?—Of course, in a case of that kind, it is absolutely necessary that those giving the advice should be the people who know.

19155. That leads me on to what I want to ask you. Therefore you feel sure that it would cause very great dissatisfaction among all physiologists and researchers into all forms of experimental investigation by means of animals, if the question of granting a licence for special work was not referred to a body of what I may call experts in that subject?—To a body who are acquainted with the needs of the subject, certainly.

19156. (*Sir William Collins.*) Reverting for one moment to the question I asked you before, and putting aside the irresponsible persons with whom you have dealt, I should like to understand from you what

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advantage you think it is to the public to retain the licensing under the Act, in dealing with responsible vivisectors; do you think that the present inspection and machinery under the Act give any real security to the public?—Certainly.

19157. Have you known of instances in which inspectors have detected irregularities?—No, because I do not see how irregularities could occur in a licensed laboratory. If a man has got his licence, I do not see what he can do that is seriously wrong if he does not exceed his licence and certificate.

19158. Then what is the nature of the security that the public receive as the result of such inspection and machinery under the Act?—In what way do you mean?

19159. Do you think that the inspection, as carried out, of vivisection by responsible persons gives any security to the public?—Yes, it gives them a sense of security, I admit.

19160. Can you tell me of any case of irregularity which has been detected thereby?—No, there have been none in my laboratory.

19161. Do you think, as we have been told by other witnesses, that we might reasonably trust to the responsible vivisectors under the Act without inspection?—Certainly, I think so; but at the same time there is no objection to any number of inspectors. Practically whenever one does an experiment, although there are a great number of unofficial inspectors, it is perfectly true that there is not an official inspector. It would make no difference whether the inspector happened to have an official mark on him or not.

19162. Then what is the value of the official inspection to anybody?—I do not know that there is any special value as regards his inspections. I am referring entirely to the vivisection as I explained in the last answer.

19163. (*Sir William Church.*) When the inspector visits your laboratory he does a great deal more than merely walk into the laboratory, does he not?—Yes.

19164. Would you just mention in what the visit of the inspector generally consist?—The inspector, as it happens, has come in several times just when I have been doing an experiment. He has come in twice to my knowledge when I have been giving a demonstration—in the middle of a demonstration to the men. He always examines the animal with interest, and he waits until the demonstration is over, observes the result of the demonstration, and then examines the animal, and sees it killed. Those are exceptional occasions, because he does not usually hit off the time when there is an experiment on. Under ordinary conditions he comes and perhaps asks to see the laboratory book, and asks to see the experiments going on. He sees every worker in the laboratory if he is about. He inquires after the animals, sees every animal under experiment, and sees the condition of the cages in which they are kept; and he would, I presume, make objection if they were not properly housed, and so on.

19165. Therefore he is a security to the public that these animals are kept in a condition in which they suffer no pain before they are experimented upon, and that they are kept in such a condition as to be as healthy as they can be, apparently, under the circumstances after they are experimented on?—Yes, I suppose he is a security to the public in that way. It would make absolutely no difference to any conscientious man whether there was inspection or not.

19166. That is an important part of the inspector's duty, is it not, that he should inspect the condition of the laboratory and of the premises connected with it?—Yes, to see that the animals' cages are suitable ones and that kind of thing.

19167. (*Sir William Collins.*) After that explanation may I again repeat my question: What is the value of the official inspection to anybody?—I do not quite know what answer you require. The value of inspection is to give the public a sense of security that everything is being done according to the Act.

19168. But I understand you to add in one of your answers to Sir William Church that no one would make any difference if he had not an inspector?—No, naturally not, because the people who vivisect under the Act are not criminals nor have they criminal instincts.

19169. So that what is the value of the official inspection?—To give the public a sense of security.

19170. Is it superfluous so far as the vivisectors are concerned?—I think so. Of course, it is conceivable (anything is conceivable) that you might get an unscrupulous and brutal vivisector, as you might get an unscrupulous and brutal man in any profession.

19171. But you think that the Act gives valuable protection and security to those who practise vivisection?—Yes, I think that.

19172. Is that, do you think, one of the most important reasons for retaining it?—It is a reason for retaining it. And I gave my other reasons before, that some sort of Act ought, I certainly think, to be in existence, as I said, to prevent irresponsible persons, unsuitable persons, from obtaining a licence. There are lots of men to whom we would not give a licence.

19173. Who have applied for them?—Before they apply they would come to me and I should regard them as unsuitable men, and I would not have them in the laboratory, as not being the proper kind of men to do the work.

19174. Have many unsuitable persons applied to you for assistance to obtain a licence under the Act?—I would not say many.

19175. Have any unsuitable persons applied to you for assistance to obtain a licence under the Act?—They have approached me.

19176. How many?—I am not prepared to give any number.

19177. (*Dr. Wilson.*) Is it on account of their inhumanity, may I ask?—No, because the only men who do this kind of work are really what I call the very best men. We use the very best men.

19178. (*Sir William Collins.*) I should have liked to have known if you could have told me whether one, two, or many unsuitable persons had applied to you for assistance to obtain a licence?—People come and talk to you and say, I should like to do a piece of work on such and such a subject, and you do not say, "I am not going to have you in the laboratory"; you are not so foolish as that. You are tactful and suggest they should do something else.

19179. You would not like to tell me how many of those persons there are?—I could not tell you. I do not suppose I could remember.

19180. (*Mr. Ram.*) With respect to inspection you, as I understand, think that with regard to all the persons, and all the licensees you know, their conduct is such that they are not restrained from misconduct by the presence of the inspector?—That is my point.

19181. On the other hand, very many members of the public think that there is either danger that they might act illegally, or, at any rate, it is desirable that exactly what they do do should be known.?—Yes.

19182. In other words, that exactly what does take place should be known?—Yes.

19183. Does the presence of the inspector in your opinion give such security to the public?—Yes, I think it does. That was my point.

19184. Therefore, although in your opinion such security may not be necessary, for the sake of those who do not share your view, it is desirable as you think to retain inspection in order that such persons may receive the security which inspection may give them?—Certainly, that is my view.

19185. (*Sir Mackenzie Chalmers.*) Take, for instance, the inspection of factories. A well-conducted factory is inspected just as much as any other factory?—Yes.

19186. And it is desirable that all factories should be inspected, and, therefore, is it not desirable that all laboratories should be inspected?—Yes, I think it is for the same reason.

19187. (*Sir William Collins.*) Have you heard of any ill-conducted laboratory in this country?—I have not heard of any.

19188. (*Dr. Wilson.*) But, after all, in spite of any amount of inspection, the question of painless experiment depends entirely on the experimenter, does it not?—Entirely.

19189. That is to say, in some experiments, when artificial respiration is being carried on, the inspector really could not tell?—You mean with curare?

19190. No, with any anæsthetic, except on your own assurance that the animal was thoroughly under anæsthesia during the whole experiment?—Yes, certainly he could. The animal would have no reflexes; exactly the same as with man, you can tell if a man is under an anæsthetic.

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19191. But with curare it would be difficult?—With curare he would simply know that you had administered the anæsthetic, and he would be able to tell from the blood pressure. Those are the two points.

FORTY-SECOND DAY.

Tuesday, 10th December 1907.

PRESENT:

The Right Hon. The Viscount SELBY (*Chairman*).

Sir W. S. CHURCH, Bart., K.C.B., M.D.
Sir W. J. COLLINS, M.P., M.D., F.R.C.S.
Sir J. MCFADYEAN, M.B.
Sir M. D. CHALMERS, K.C.B., C.S.I.
Mr. A. J. RAM, K.C.
Mr. W. H. GASKELL, M.D., F.R.S.

Captain C. BIGHAM, C.M.G. (*Secretary*).

Mr. CHARLES R. J. A. SWAN, M.B., M.R.C.S., L.R.C.P., called in and Examined.

Mr. C. R. J. A. Swan, M.B., M.R.C.S., L.R.C.P.

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19192. (*Chairman.*) You live at 3, Chester Place, Hyde Park?—Yes.

19192A. Do you practise in London?—I have practised for the last fifteen or sixteen years.

19193. As a physician or surgeon?—I do not do any surgery; I know nothing about it, I am afraid.

19194. You are a Bachelor of Medicine, and also of Surgery of Oxford?—Yes.

19195. And you have various other qualifications?—I believe I was examined by one of the Commissioners, Sir William Church.

19196. You are a member of the Royal College of Surgeons, and a Licentiate of the Royal College of Physicians?—Yes.

19197. And Consulting Physician to the Consumption Home, Gloucester Place?—Yes, it is only a small hospital.

19198. You are appearing here at the request of the Canine Defence League?—Yes, they wrote to me and asked me if I would come.

19199. You have come to give us evidence in favour of the opinion that dogs ought not to be subjected to vivisection, whatever may be the case with other animals?—Yes. I do not want it to go out to the world in general that I am one of those people who say that vivisection must not be. I think it is a painful necessity, and we cannot blink that fact.

19200. You think that experiments upon live animals properly conducted are essential to the medical profession?—Yes, they are.

19201. I will bring you at once then to the point that you are going to speak on, viz., Why the dog should be exceptionally treated. The two main points appear to be, first, the question of ethics or sentiment, or whatever you like to call it, in the case of an animal so much the companion of man?—Yes, that one gets so attached to.

19202. And the other is as to whether or not the examination of the living dog is of special importance, having regard to its resemblance in its construction in some respects and its want of resemblance in others, to the construction of man?—Yes.

19203. As regards the first point, I understand from what you have said about your love of dogs that that influences you?—Yes. I admit it is to a great extent a sentimental feeling, and, if I may so, what I am anxious to know is, whether the Commission could not find some means of using other animals and obtaining the same beneficial results to mankind, letting the dogs alone.

19204. That comes rather to the other head that I was speaking of as to the special qualifications or usefulness of dogs for experiments. As regards your feeling about it, I do not think we need ask you more; we quite understand that view, and it has been put before us at considerable length very often. You have that feeling yourself very strongly?—I have.

19205. As regards the usefulness of dogs for the purpose of experiment, that depends a good deal, no doubt, upon how far the dog resembles man in its structure?—Yes.

19206. We have had a good many eminent witnesses before us who have pointed out that in certain parts of their structure dogs do resemble man very nearly, and that in others they differ from him, and therefore are not specially valuable in respect of certain experiments; would you give us your view about that?—Going back to one's early days, I have seen the old muscle and nerve experiments, what we call the classical muscle and nerve preparation experiment. We did them first of all on frogs, which was perfectly simple. You isolated your sciatic and stimulated it and made the whole experiment work. Then I remember at one time it was tried on a dog at Oxford and it was a very difficult matter to see; there was a mass of blood about, and if I had not known my procedure and learnt it from books and diagrams first, I do not think I should have been much the wiser. I remember then it struck me how much simpler it was to take a lowly-organised animal like a frog to do your simple experiment. I thought, when I get qualified one day, if ever I do, I will stick to that way and not use dogs for demonstration purposes before students if one can avoid it.

19207. You never have been licensed yourself?—No.

19208. Nor have you performed experiments of this kind?—Only on a frog. The laboratory arrangements at Oxford were that we had all our frogs, and pithed them or not, as we liked, and made muscle and nerve preparations with Burdon Sanderson.

19209. It was not what you would call a very serious training in physiological experiment?—It was all we had at the beginning.

19210. (*Sir Mackenzie Chalmers.*) How long ago are you speaking of; ten years ago?—In 1883. I began my scientific work at Oxford the same day that Burdon Sanderson came into the laboratory for his first lecture. Then there was another time when we were doing a pressure experiment, a manometer experiment on the jugular, that was done, I believe, on a dog. I know it was a warm-blooded animal, and I frankly confess that I may have been stupid or dense, but it was impossible to see the mercury rising or falling properly in the manometer.

19211. How many were present?—I should think about ten.

19212. And only the physiologist himself who was performing the experiment could see it. It may be difficult sometimes for the pupils to see?—That is a thing I could not be sure of. If I may state my own

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conviction about it, it is that errors in manipulation are so manifold and easy, that you are very likely to get a wrong result; then you repeat your experiment, and go on till you get your results to agree with what they ought to be.

19213. (*Mr. Ram.*) Have not things improved very much since those days?—I am not in a position to say, and I do not want to dogmatise on what I am not qualified to dogmatise upon.

19214. (*Chairman.*) That experiment that you were last speaking of, on the blood pressure, is an experiment for which the dog is said to be particularly fitted?—Yes, but my own feeling is that one now understands the whole thing, and I am perfectly willing to accept the fact that that experiment has been done—the vagus stimulated and the blood pressure made to fall—and for the purpose of teaching other students I should be anxious that it should be taught by the aid of diagrams.

19215. Of course, demonstration before students is a branch of the question only?—Yes.

19216. But do you consider now with your present experience that experimenting on dogs with reference to blood pressure, always under anaesthetics, of course is a desirable thing?—If it is done under anaesthetics I do not think one ought to be inclined to veto it.

19217. And that the results of such experiments on dogs are more useful, or more possible one ought to say perhaps, than results by examining rabbits and guinea-pigs?—I cannot see why rabbits should not be as useful. I have thought it over anatomically, and I do not see why the distribution of your vagus should not be equally effective in a rabbit. I may be wrong in my histology, my minute anatomy; but that is my idea. You can get at the jugular of the rabbit easily enough.

19218. Has not size something to do with it. The dog that would be used for this purpose would be larger than a rabbit?—You mean on account of a clot of blood spoiling the tube?

19219. I am not myself a medical man, and I would not like to express it in detail, but I am giving you the views which were given by some witnesses that we have had before us?—I cannot honestly say that I do not think you could do it as well with a rabbit. I should think that you could do it with a big rabbit. I have never seen it tried.

19220. What you say then is that the dog is an animal in which the blood pressure can be watched and tested as satisfactorily as in any other animal, but whether it could not be just as well done on a rabbit you are not prepared to say?—That is so. I am not trying to hedge in my evidence. I want you to understand exactly what I mean.

19221. That is just why I have summed up what I understood you to say, to see whether you assent to it. Then, as regards questions relating to the digestive and nutritive organs, is the dog suitable for those?—I should think so, but without wanting to be conceited about it, I have managed to estimate the condition of my own and other people's stomachs with perfect comfort without making a gastric fistula.

19222. Without making it on an animal, do you mean?—Yes. I estimate the condition of my own stomach for experimental purposes often.

19223. I was not aware that it was the practice of the profession to make gastric fistulæ for the purpose of examining what the state of a patient's stomach is?—I did not mean that. My idea is that there are cases which are quoted in lectures by those who wish to state what is going on in the stomach. There was an old experiment, as the member of my profession who is present will remember, in which you made a gastric fistula of a dog, and held a piece of fried bacon to the dog's nose to see the gastric juice spread to the fistula, and it struck me that you could just as well do that by syphoning. I have made a great hobby of the treatment of gastric disturbances, and I constantly sample the contents of my own stomach to see how it is going on.

19224. That is hardly what you would call a painful experiment?—No, but my idea is that you can arrive at the benefit on yourself and your friends just as easily without experimenting on a dog with a gastric fistula.

19225. Do you mean by that for the purpose of physiological study and research?—With a view to treating mankind. I thought that all research was really with the object of treating mankind in the end.

19226. I suppose that is the pathological side of it, but then there is the purely physiological side, is there not?—I only look at it from the chemical side.

19227. But not necessarily with a view to the immediate result of a particular experiment which shall give you a particular remedy, but that by greater knowledge of the working of the human frame and of its diseases, you can get knowledge which ultimately will prove useful. It does not follow that the physiologist, as I understand from his own account of it, is necessarily performing the experiment for the express and immediate purpose of the discovery of a remedy for some known disease, but because it is very important to know how the machine works. Is not that so?—My view is that you can ascertain just as well how the machine is working by syphoning the stomach at different intervals.

19228. (*Sir William Collins.*) Is your point that any light which may have been thrown upon the human digestion by experiment on dogs by way of gastric fistula could equally have been obtained by syphonage on man?—That is my view, whether right or wrong, and it is backed by my having experimented a great deal on myself, and my friends occasionally, by means of which I have been able to treat cases.

19229. (*Chairman.*) When you say that you investigate the state of the stomach by syphonage, does that include every stage of the condition of the stomach? You do not acquire all knowledge that is useful about the stomach from that, do you?—I do not see why you should not acquire all the knowledge that you would get from a fistula.

19230. You think that you can add nothing by experimentation on animals to the knowledge which you can obtain by use of the syphon?—I think that you would get as good results by the syphon.

19231. (*Sir William Collins.*) Is that limited to the stomach, or would your remarks go to the digestive process in the pyloric end of the stomach?—That is where the difficulty comes in, I know. You mean with regard to absorption?

19232. I mean with regard to absorption and secretion beyond the stomach?—Yes, I think you would have to make your fistula, but there, again, one would be anxious to take some animal which ate a mixed diet other than the dog.

19233. Such as——?—A pig is an excellent animal.

19234. (*Chairman.*) *Apropos* of that, is your reason for preferring a pig that you think it is not so intelligent as a dog, or so friendly?—Partly. I have no affection for a pig, and if you want to trace anything in a pig's liver, the member of my own profession who is here will allow that there is nothing so beautiful to examine under the microscope as a piece of pig's liver. Is not that so?

19235. (*Sir William Collins.*) I am afraid it is the privilege of a Commissioner to ask questions?—I beg your pardon; I am sorry. That is my view of the matter.

19236. (*Chairman.*) I gather from your *précis* of evidence that you consider that a dog has anticipation of pain?—I know that it has.

19237. What exactly do you mean by that; under what circumstances has it anticipation of pain?—I will give you an instance of a dog of mine which had cancer in its ears. We decided that we must curette them. We curetted one ear first of all without an anæsthetic.

19238. Will you explain to us what curetting is?—Scraping the diseased tissue away with a half sharp instrument, to get a clean space; it is much worse than cutting.

19239. Did you do that under an anæsthetic?—I could not, the dog was discharging so very much.

19240. This was not an experiment?—No, it was for the benefit of the dog; the dog was so bad. We did one ear, and then it had had enough of it. It was perfectly intelligent, and never tried to bite. Then, when the veterinary surgeon and I, about two days afterwards, tried to do the other ear, the dog simply shook, and I thought it was anticipating pain.

19241. That is when you came there in order to scrape the ear?—Yes.

19242. Did you scrape it?—No, we could not; we had to give it an anæsthetic. We got it down, and the dog shook and whined.

19243. And you had the knife ready for the purpose?—Yes.

19244. That I can quite understand. That is what you mean by being sure that a dog anticipates pain?—Yes.

19245. (*Sir William Collins.*) Based upon a reminiscence of suffering?—Yes.

19246. (*Chairman.*) Is experimentation on the kidneys of a dog a thing which you consider useful?—That is a thing which I should, perhaps, not have put in my *précis*, because what I have to say on the other side is purely clinical.

19247. I do not quite know what that means. Do you mean that clinical observation will teach you everything that can be learnt about the kidneys and their action?—I should think that you could learn two-thirds of it, and that you might do as well with the kidneys of other animals as with those of dogs—of the cat, for example.

19248. Do you think that in the sphere of clinical observation, which was the method of observation for, I suppose we may say, thousands of years, there is as much to be added to your clinical knowledge as there can be to your experimental knowledge?—I think that you must have experimental knowledge too, but I would exempt the dog if possible.

19249. You have not quite answered my question. Do you think that there is as large a field for new knowledge to be obtained by clinical observation as there is for new knowledge to be obtained by experiment?—There is a field in both.

19250. Is there as large a field for new clinical knowledge, considering that our clinical knowledge has been of such long standing, as there is for experimental knowledge?—Yes, but our clinical knowledge now starts on a very much higher level since we have had the knowledge obtained from experimentation to help us.

19251. Then one of the results of knowledge obtained by experimentation is that it enlarges your clinical observation?—Yes.

19252. And you know where to look?—Yes.

19253. And what to expect to find?—Yes, one cannot deny the benefits of experiment.

19254. Have you any suggestion to make as regards the present law; would you have it altered in any way?—I would urge, if possible, that all dogs should have their experiments conducted under anæsthesia, and if, for the sake of arriving at a conclusion, you want to prolong the animal's life after the experiment, you should keep it under morphia.

19255. So as to dull the pain?—Yes, and give it freedom from pain.

19256. Is that always practicable?—I should think it was a thing that might be tried, and that you should use other animals where it is not practicable at present, and then, of course, if it is found that mankind is suffering, or the knowledge of the world is suffering, the question must be reconsidered.

19257. I am speaking in entire ignorance, and I ask the question for my own information. After a dog had recovered from an operation would observation while it was under morphia be as useful as observation would be without morphia?—That would depend upon the organ upon which you were operating. If you were operating upon the kidneys it would be equally useful, but if you were going to do one of those operations where morphia would alter your results, you should try first and see whether you cannot use some other animal. One has to speak temperately on this subject because it is such a very important one.

19258. I rather gather from what you say that assuming that animals are not brought out of anæsthesia for future observation except in cases where it is strictly necessary, and if possible not using a dog, you do not object to such observation after operations?—On animals, no; but I would strain every nerve to leave the dog out.

19259. (*Sir William Collins.*) Was the handbook of Burden Sanderson, Klein, and Brunton used in the laboratory at Oxford when you were there?—Yes; but it was not a handbook; it was not bound; it was loose sheets.

19260. Was the matter contained in that book used before students in the laboratory?—At Oxford, I think it was what is called the handbook now; they were loose unbound sheets that Sanderson always issued and Gotch used to take round.

19261. Was that the material upon which the ordinary student or the advanced student worked?—That was what we all of us worked by—the thing that we saw. You had the experiment for the day, and the apparatus required, you know.

19262. If experiments on dogs for scientific purposes were always conducted under anæsthetics would all your objection to using dogs be met?—Yes.

19263. You have no objection to what has sometimes been called exploiting dogs, using them for that purpose provided that it is painless?—No, for this reason, that if you are going to benefit your race I do not think you have any right then to let your sentiment come in the way. I would say always, if possible, try to use some other animal. If you can assure me that you have a right to know more about these matters than any of us who are engaged in other work, and you say to me that you know that the dog is the only animal that will answer the purpose satisfactorily, and if you also say that the dog shall not suffer afterwards, then I say I am sorry that it has got to be so, but mankind must come first, and you must take it.

19264. Would you exempt the monkey as well as the dog?—I have not got the same affection for the monkey. I only had one.

19265. (*Mr. Ram.*) And you were glad to be rid of it?—It bit everybody; there was no affection about that animal.

19266. (*Sir William Collins.*) Your objection then is based upon affection and not upon the proximity to man by way of evolution?—That I do not know. I have some relatives who like monkeys, but one has not the same sentiment about a monkey that one has about one's dog.

19267. It is based then on affection, or sentiment?—Yes, I admitted that.

19268. You spoke of experiments on the kidneys and you suggested that clinical experience was rather against their value, I understand?—I did not mean against their value, but I was thinking of a case where we had cut down—at least I did not, but the veterinary surgeon did—upon the kidney, and we expected to find stone, whereas we found the whole kidney instead of having stone in the hilum was simply riddled. The case was a woman, and from watching how the kidney was working we took the trouble to catheterise through the bladder or ureter separately to see how much the other kidney was helping it. That seemed to me indirectly valuable information It was done for the patient's benefit, of course.

19269. In your *précis* you state: "Experiments on the kidneys objected to. Careful clinical work preferred"?—I do not mean altogether to say preferred; I mean what I said just now, that you can supplement your experimental work.

19270. Has the treatment of kidney disease by providing the capsule been the outcome of experiments upon animals?—Yes, it has.

19271. Is that a valuable mode of treatment?—A most valuable; one cannot get away from those facts.

19272. Do you find the views that you hold prevalent amongst the profession?—I was surprised to find how prevalent they were.

19273. They were or they are?—They are. There are many men, of course, who think the other way, and I feel great diffidence in coming here and opposing them, because so many of the men who think otherwise than I do are my intimate friends, and men for whom I have the highest regard. One of my dearest friends, for instance, is Victor Horsley, so that I feel a certain amount of diffidence over this; but my object in coming was to try and get that one animal exempted wherever it was possible—not to let it suffer.

19274. (*Sir Mackenzie Chalmers.*) Have you seen any experiments yourself lately—any research experiments at all?—I have not, and I explained that to the Canine Defence League.

19275. The last experiments that you have seen were lecture experiments at Oxford?—Yes.

19276. About twenty years ago?—In 1883 or 1884.

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19277. Rather more than twenty years ago?—Yes. My experiments were finished at the time that I had a talk with Sir William Church.

19278. Who were your examiners?—Sir William Church was one of them, Dr. Bristow the other.

19279. We should all like, of course, if we could, to restrict experiments on dogs, therefore I want to ask you one or two questions to see how far you go. You agree that superfluous dogs have to be destroyed, I suppose?—Certainly.

19280. And you are aware, as a matter of fact, that about 30,000 dogs a year have to be destroyed in London in the lethal chamber?—Yes.

19281. Have you any doubt yourself that a dog can be properly chloroformed and anæsthetised?—I have not the slightest doubt that a dog can be chloroformed, and I have had some little difficulty in persuading the gentlemen of the Canine Defence League to that effect. I will tell you why I think so. Any of you gentlemen who probably are used to dogs will know that taking out a dog's dew claws is most intense agony. Every dog that I have in the country—I dare not do it in London—has to part with its dew claws, because it would be a misery to itself and everyone else. I have tried everything, cocaine and ukaine, but a local anæsthetic does not do. When I chloroform a dog it will go absolutely to sleep. I took both dew claws off an Irish terrier; we did it in the morning, it did not have its food in the morning, it went dead asleep under the chloroform, and I know that by one o'clock it was growling over a bone, perfectly happy, and it never evinced any pain.

19282. Having ascertained your opinion on this point, I was going to ask you a further point. Seeing that a great many dogs have to be destroyed, if the animal is put under anæsthetics till death do you feel any objection to a dog being experimented upon during the process of dying?—No, I think you are justified.

19283. Therefore, experiments under a licence alone without a certificate you have no objection to?—I do not quite follow you.

19284. When a gentleman holds a licence under the Act, but does not hold any certificate, the condition is that every animal experimented upon must be put under anæsthetics, and he may perform what experiments he likes provided that the animal never recovers from the anæsthetic, but is killed during the time of anæsthesia. You would not object to that?—No, I do not think it would be right to say that you shall absolutely forbid it provided that the animal is not going to suffer.

19285. (*Mr. Ram.*) Provided that it dies under a lethal dose?—Yes. You should treat it kindly first. Do not knock it down if it is going to be a benefit to mankind. If it has to die, there is no reason why it should not be made use of in that way, provided that it will not know anything about it.

19286. (*Sir Mackenzie Chalmers.*) Have you any reason to believe that any of these animals are ill treated in any physiological laboratory; have you any knowledge of it?—No, of recent years I have not had any means of knowing.

19287. Can you quote to us any experiment that you know anything about in recent years which has been in your opinion objectionably conducted?—No, I am not in a position to state that during the last few years.

19288. Have you followed the course of experiment lately. Do you carefully read up the physiological experiments?—To a certain extent; but it is very hard; I read it by fits and starts. A man in full practice does not find it very easy.

19289. Coming to the first certificate, certificate A authorises the experimenter to experiment on an animal without anæsthetics, but does not authorise anything more than a skin-puncture, for instance, the prick of a hypodermic needle; but, of course, that involves the inoculation of the disease. What do you say about that?—I would exempt the dog from it until it can be shown that the dog and the dog alone will be of benefit to humanity.

19290. But if that can be shown, then you think that the dog must give way to humanity?—If that can be shown, then I do not see that one has any right to neglect to take means to save human life.

19291. Now I will come to the next point. Take the case of distemper. You know what a dog suffers from distemper, and what valuable dogs are lost through it?—Yes.

19292. Do you think it is justifiable, for the sake of discovering a remedy for distemper, to use experiments on dogs?—You mean to let one dog suffer in order that many dogs may escape?

19293. Yes?—Of two evils I should choose the less.

19294. Now, may I ask you which is the less. Do you think it is justifiable to experiment with dogs in the hope of curing or mitigating distemper some day?—It is in the dogs' interest, therefore you must do it.

19295. It is not in the interest of the particular dog that is experimented on?—No, but it is in the interest of dog-land.

19296. It is in the interest of the canine race?—Yes. It is no more than we do to human beings.

19297. You think that the particular dog must suffer for the rest?—Yes.

19298. Does not that argument carry you this far, that if we can do anything for the human race by sacrificing dogs, we are bound to do it?—I come back to the other point, if you can satisfy me that the dog and the dog alone can answer the purpose; but I should require to be satisfied on that point first.

19299. Have you read Professor Starling's evidence on that point?—No, I have not. I meant to have done so.

19300. I am rather sorry that you have not, because he gave us certain reasons why he said that it would be almost impossible for research to go on if dogs were excluded: that there were certain cases where the dog was the only appropriate animal. I should have liked your opinion on that?—I may say that I only had notice that I was to give evidence three days ago, and I have not had time to read it.

19301. Therefore you cannot give us any opinion upon it?—I cannot.

19302. Now I want to come to Certificate B. Under Certificate B, if any operation is to be performed on an animal, the animal must be kept under anæsthesia throughout the whole operation, including, of course, all the subsequent stages, but the animal may be allowed to recover. There, as I understand you, is where you would draw the line with dogs?—Yes.

19303. Is that because you think that there must always be subsequent pain?—I think there is always some subsequent pain.

19304. We have been told that in many cases there is nothing more than small discomfort?—I know, but I have my own personal experience the other way. I have had various things done to me when I have been damaged at boxing and fencing, and have had little bits of me removed, and I have always had pain afterwards.

19305. That is personal experience?—Yes, I had a fatty tumour removed from my neck and the surgeon told me afterwards that I had no pain. I told the surgeon that it was my neck not his, but he said I had had no pain at all.

19306. Then Certificate B is really the part of the Act to which you object?—It is.

19307. On the ground that during the period while it is necessary to watch the animal you think that in most cases there must be not only discomfort but a certain amount of pain?—There is pain.

19308. And that the experiment would be frustrated if the animal was killed as soon as bad symptoms of pain appeared?—Yes.

19309. But you admit that you have not watched the particular experiments?—I have not, and therefore I am not in a position to dogmatise, and that is why I felt that I was almost taking up the time of the Commission under false pretences.

19310. (*Mr. Ram.*) I have very little to trouble you with. Everything turns, I take it, in your opinion, with regard to the use of dogs, upon the presence or absence of suffering of the dogs either mental or physical?—Yes.

19311. If you were sure that no pain was caused either mental or physical, in the interest of science you would not preclude the use of dogs?—No.

19312. With regard to physical pain Sir Mackenzie Chalmers has asked you some questions. With regard to mental pain you gave us the instance of that dog whose ear you curetted, and who, next time you endeavoured to do it, showed great signs of fear. Did that dog show signs of fear on the first occasion?—It became nervous when I came to touch the tender part.

19313. But before you touched the tender part, when you produced the knife did it show any signs of fear?—No.

19314. Its terror on the second occasion then would be due to memory?—Yes, recollection.

19315. I want to put a question on your *précis* with regard to a statement that I am not clear about. Do you think that a dog can be said to have a greater anticipation of pain than any other animal if you eliminate all idea of memory?—No more than a child or a man. Human beings have no sensation of pain the first time. If you try to take a tooth out for the first time from a child it does not mind.

19316. Apart from memory, then, would you say that there is any anticipation of pain?—Perhaps I used the wrong word when I said anticipation of pain; my *précis* was drawn up in a hurry.

19317. There is another sentence in your *précis* that I want to put to you. You say with regard to the treatment of gastric troubles that you are entirely with the vivisector or anybody else—that he may vivisect himself or any of his friends or any dog if that is the only way by which he can arrive at the state of the stomach at a given moment, by a gastric fistula. That is your view?—Yes, because I believe with regard to the stomach that you can do anything you like with a tube.

19318. In that sentence, therefore, do you mean to go the length of allowing a fistula to be made in a dog to be kept perfectly open and the dog to recover from the anæsthesia?—No, I was putting a *reductio ad absurdum* knowing that you would not say that that was the only method or taking it for granted that you would not.

19319. Then I am not quite sure that I follow your view about this. Does it mean that if it be essential that a dog should be used and no other animal, then under anæsthesia it should have a fistulous opening, and be allowed to recover, the wound being treated aseptically?—No, I meant it the other way. I am so sure that you can get the same results by intubation of the stomach that if anybody could prove that it was absolutely essential to make a gastric fistula I should say, do it, but I am absolutely certain that you can do it by syphonage.

19320. I am glad I asked you the question, because that makes it perfectly clear. Your attitude of mind, as I understand, would be that if it could be proved that it was absolutely necessary——?—I say that the world would come to an end; it is absurd.

19321. You do not think it is absolutely necessary?—I am sure that it is not.

19322. In the mode that you adopted of examining the stomach you evacuated the stomach?—I took a little out of it. I often test my own stomach. I have tested the digestibility of different foods on myself. I wait a certain time, pass the tube down, and draw off what I want, and test it for acidity and parapetones and peptones.

19323. But you do not get pure gastric juice?—I could get pure gastric juice if I chose to wash the stomach down first.

19324. But do you by that method get pure gastric juice?—I never tried for pure gastric juice, but I should think I have.

19325. What you have tested would have been gastric juice, plus other contents of the stomach?—Yes. I do it to test the process of digestion.

19326. I think the summing up of your evidence is this, is it not, that you would like to narrow as much as possible any operation on dogs?—Yes.

19327. That you have no objection to an operation on a dog if it be a desirable animal, provided that it dies under the anæsthetic?—Yes.

19328. And that if it could be proved to the satisfaction of competent persons that it was necessary that dogs should be allowed to recover, if such proof were forthcoming, you would not object even to that?—Allowed to recover and suffer?

19329. Yes?—I could not go so far as that.

19330. I put it to you only in the event of such persons as the President of the Royal Society, the President of the Royal Society of Edinburgh, the Presidents of the Royal Colleges of Surgeons and Physicians, and such persons being satisfied that for the advantage of mankind it is necessary that a dog should be allowed to recover, being effectually treated?—I should want to know all the details of it.

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19331. (*Sir Mackenzie Chalmers.*) That would be *carte blanche*?—To give leave to do that would be a large order.

19332. (*Mr. Ram.*) There is no question of *carte blanche*. The person who certifies has to know what the operation is that is to be done, and he has to satisfy himself that the experiment will be necessarily frustrated unless it is performed on an animal similar in constitution or habits to a cat or dog, and no other animal is available for such experiment?—Then take a cat.

19333. You would take a cat in preference to a dog?—Yes.

19334. Supposing that there were no cat available?—You can always find a cat.

19335. You would sacrifice the cat to the dog?—Yes.

19336. Does your view depend upon the amount of suffering that is subsequently caused? Supposing that the amount of suffering upon recovery is very slight indeed, should you object even to that?—It would be a dangerous precedent.

19337. You have said that you would substitute every other animal for a dog, and you suggested a pig. You know that experiments can only be made in licensed places?—Yes.

19338. And that those licensed places are almost always laboratories attached to hospitals, medical schools, and so on?—Yes.

19339. A dog or cat can be kept without discomfort in such places, but a pig could hardly be kept under those circumstances, if at all. You realise, therefore, the difficulty of using pigs?—There were some experiments I believe on pigs in the Oxford Laboratory.

19340. (*Dr. Gaskell.*) I understand you to say that you have not seen any experiments under Certificate B which allows the animal to be kept alive under the experiment and to recover?—No, I have not.

19341. Therefore you are not competent in the slightest degree to consider really what the condition of the animal is afterwards?—No, I am not.

19342. And you have made no inquiries into the matter?—It has always struck me that inquiries are biassed on one side or the other. Unless you can see the operations you cannot judge.

19343. That is what I mean. You have never seen an operation yourself—it is very remarkable to see?—I stated at the beginning that all my experiments are 20 years old.

19344. Do not you think from your knowledge of physiological laboratories that the vivisectors themselves would agree with practically all you have told Mr. Ram just now?—I believe they would.

19345. Do not you think that they do as a matter of fact choose other animals rather than dogs if they can?—Yes, I think so, and I am very glad of it.

19346. And do you not consider that that is partly because they have the same feeling with respect to dogs as you have?—I am sure they have. I stated at the beginning that that is my difficulty in giving evidence, because so many of these men are my personal friends.

19347. In addition to that is it not the case that dogs are more expensive than other animals, so that the very lowest of motives, the motive of the pocket alone, would cause them to prefer other animals?—You can steal a dog anywhere.

19348. You do not mean to imply, do you, that the dogs used in physiological laboratories are stolen, and not paid for?—I have stolen dogs in my early days.

19349. Do you think that they are not paid for?—I thought honestly that the dogs are taken from the Dogs' Home for nothing. I was told so, and I said that I would accept the statement with salt.

(*Sir Mackenzie Chalmers.*) It is forbidden by the Statute.

19350. (*Dr. Gaskell.*) I do not know how much you

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are acquainted with other laboratories than that of Oxford?—That is the only one I know.

19351. Was it not a fact that all the dogs were paid for there?—I should think so.

19352. And that they were more expensive than other animals, so far as that goes. I am not saying that that is the sole motive?—Yes.

19353. (*Sir John McFadyean.*) I suppose you have sufficient acquaintance with dogs to know that they are the subjects of certain diseases quite peculiar to the canine species?—Yes, they are.

19354. And I daresay you also know that there is a great deal of ignorance with regard to some of those diseases?—Yes, there is.

19355. Therefore you will agree that it is advisable that they should be further investigated?—Certainly.

19356. You would also admit that if they have to be investigated by experimental methods only dogs would suffice since the disease is not transmissible to other animals?—Yes, but if you are going to carry that further you can only experiment on man if you want to improve the state of man.

19357. I did not mean that. In the case of a disease which is only transmissible to man you cannot experiment at all?—No.

19358. You have assented to the statement that certain diseases are peculiar to the dog?—Yes.

19359. I ask you further to assent to the view that if those diseases have to be experimentally investigated you must use the dog?—Yes.

19360. Now take such a disease as distemper. You are aware that it causes a tremendous amount of pain and suffering to dogs?—Yes.

19361. Do you think it is desirable that it should be experimentally investigated if there appears to be a reasonable prospect that you might thereby get a knowledge that would save future suffering to dogs?—My practical experience is that I have had many dogs with distemper, and if you keep them warm, and give them an occasional aperient you will not lose a dog from distemper; it will get well comfortably.

19362. Will you assume, please, that that is an exceptional experience, and that you can have an immense amount of evidence to show that it is a very deadly disease in special outbreaks, and that the dogs are undoubtedly very ill and uncomfortable?—I may have been lucky in my experience.

19363. You think it is desirable that it should be experimentally investigated if there is a reasonable prospect that you could get a remedy?—That is what I should prefer.

Mr. R. J. Cowen, L.R.C.P.I., L.R.C.S.I., called in; and Examined.

Mr R. J. Cowen, L.R.C.P.I., L.R.C.S.I.

19364. (*Chairman.*) You live at 15, Half Moon Street?—Yes.

19365. Do you practice there?—Yes.

19366. As a surgeon?—As a surgeon.

19367. I believe you are a Licentiate of the Royal College of Physicians, Ireland, and of the Royal College of Surgeons, Ireland?—Yes.

19368. Are you a Fellow of the Gynecological Society?—Yes, and several other societies.

19369. You attend to give evidence at the request of the Canine Defence League?—Yes.

19370. Are you a member of that Society?—No.

19371. I believe you desire to give evidence on the anatomical differences between dogs and men, with a view to suggesting that there is no utility in examining dogs in order to study their diseases, and how the remedies for those diseases act on the body of man?—That is so.

19372. Would you put it so high as to say that dogs are so different in their structure that no such information can be gained by examining their bodies with regard to man?—No. I would put it that information could be gained more readily, and the conclusions would be less liable to be erroneous, if the experiments were made upon animals more similar to human beings, mixed feeders, for instance, such as pigs, and especially animals that perspire.

19373. You look upon the pig as more allied to man?—I look upon the pig as having a closer resemblance to man, and being on that account more suitable for experiment.

19374. You say that the pig is a mixed feeder?—Yes.

19375. Is not the dog a mixed feeder?—The dog was not intended to be a mixed feeder. The teeth of the dog show plainly that he was intended to be a carnivorous animal entirely.

19376. What was man intended to be?—I always hold man to be a mixed feeder.

19377. I suppose that would depend upon where he lived, and how he could get his food?—Certainly; but his teeth vary much from the teeth of a dog and think we have considerable evidence in favour of the fact of his being intended always to be, and his being, a mixed feeder.

19378. A feeder on one substance in one place, and another in another, do you mean?—Or a feeder on two or three different substances in the same place.

19379. But supposing that a man was living as a naked savage on a barren plain, where there is but little vegetable food, would he not be then an animal feeder entirely?—The difference, I think, is that man can support life upon the fruits of the earth; a dog cannot. If a dog was only given uncooked vegetable food it would die; it could not live on it.

19380. You think that is the crucial difference?—It is an important difference.

19381. And one that you think makes experiments on dogs valueless?—Undoubtedly it does to a large extent.

19382. You go as far as that?—I go so far as to say that conclusions drawn from experiments made on the digestive organs of dogs must necessarily be erroneous.

19383. I do not know whether you have read the evidence that has been given before us?—I have read some of it.

19384. Then you are aware, probably, that a large number of very eminent physiologists, some of the most eminent in the kingdom, have given evidence that they consider experiments on dogs to be really valuable in many cases of research?—Yes, I am aware of that. I have gone very carefully over the experiments performed abroad and here where dogs are used; and I have not yet succeeded in finding one single experiment which could not have been performed quite as well, if not better—I hold better—upon some other animal.

19385. In books?—Both in books and personally in laboratories abroad and from reading the researches of others.

19386. Have you held a licence yourself?—No, I have not.

19387. Where have you studied—in what laboratories?—In Germany, in Bonn, in Leipsic, and in Paris with Pasteur. I cannot say that I have studied there, but I have spent time there, and gone over with them and gone into the work.

19388. Did you belong to the class of Dr. Pasteur?—No, I did not belong to the class.

19389. Or at Leipsic?—No, I was only a visitor.

19390. As such did you frequent the laboratories or did you visit them once or twice?—I suppose I visited each of them a dozen times or more.

19391. Is that the extent of your experience in operations on live animals?—That is the extent.

19392. Would you tell us what you consider to be the special points of difference between men and dogs? In the first place the teeth. We do not find in a dog the same shaped molars that you nearly always find in mixed feeders, or in vegetable feeders. The position of the canines also, the over-lapping canines, is a very important distinction.

19393. That is a distinction in structure; but it is not a distinction which bears much, or at all, upon the question of experiments upon dogs?—That is so; the canine teeth do not bear upon that because they are intended almost entirely as weapons of defence, but the shape of the molars is very distinctive.

19394. What I rather meant by internal structures are those examined in experiments. You do not examine a dog's teeth by vivisection?—I was going down from that. The next difference is the digestive

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tract in the dog, the dog bolts its food and as a consequence the stomach has to do the greater part of the digestion—the stomach and the intestines. As a result of this, relatively speaking, the digestive tract is very much shorter and the digestive juices are a great deal more concentrated and more powerful; they will digest more powerfully than the similar juices of the human being.

19395. Have you observed in the evidence of the witnesses before the Commission that that distinction has been pointed out to us by some of the gentlemen who still think that for many purposes experiments on dogs are extremely important?—No, I did not notice that. I have not had time to go over the whole of the evidence.

19396. What is the next distinction that you find?—The position of the heart and the character of its beat is a very important distinction. The intermittent pulsation of a dog's heart is very different from the steady regular beat of the human heart.

19397. Do you consider that an experiment to ascertain the blood pressure, on a dog, and the action of different drugs upon it is not a proper subject of research?—I cannot conceive how it is possible to draw conclusions as to human blood pressure from experiments on an intermittently pulsating dog's heart.

19398. Have you read the evidence which has been given on that point as regards experiments on dogs?—Yes, I have, and some of it I cannot agree with at all; with much of it I cannot agree.

19399. You differ from those gentlemen who have said that for purposes of blood pressure, experiments on dogs are valuable?—I differ very much.

19400. Exceptionally valuable?—I object to the term "exceptionally."

19401. You do not object to the term "valuable"?—It is just possible to conceive a condition under which an intermittent pulsating heart might bear close resemblance to a deceased human heart and consequently might have some value in that way, but otherwise it would be impossible.

19402. Is there any other point to which you wish to call attention as regards the difference between a dog and a man?—The brain is a curious thing. A careful analysis of the chemical composition of the brains of different animals and the relative amounts of phosphorus which they contain shows that the larger the proportion of phosphorus in a general way the greater are the signs of intelligence shown by the animal that owns the brain. In a dog's brain there is a very considerably larger proportion of phosphorus than in that of any other animal—next to it comes the horse—but the dog has a very much larger relative proportion of phosphorus in its brain. I think that is important.

19403. I see you say that one of the reasons why dogs are so often made the subject of vivisection is that they can be cheaply and readily procured?—That is so; it has always been so.

19404. I suppose that rabbits and guinea pigs can be more cheaply procured?—Not quite so easily, and perhaps not so cheaply.

19405. Have you ever bought guinea pigs or dogs for the purposes of experiment?—No, I have not, but I have gone into their prices more or less. I have ascertained what they cost, what they could be bought for, and how they could be obtained.

19406. But you yourself have not bought any?—No.

19407. We have been told that dogs are more expensive to experiment on than guinea-pigs or rabbits by those who do use them. I do not know whether you are prepared to dispute that evidence?—I can only dispute it on hearsay.

19408. What animals do you suggest should be used instead of dogs?—Almost any other animal that is a mixed feeder would be more suitable for experiments, if experiments are necessary.

19409. A cat, for example?—A cat is undoubtedly more suitable, although the cat is a domestic animal, and can hardly be called a mixed feeder.

19410. You have given us the reasons why you think that a dog is less suitable. Do you say that it is less suitable than a rabbit or a guinea-pig?—Undoubtedly. I think that is proved by the action of different drugs. If we compare the action of different drugs upon a dog and upon a human being, it is very different. I have noted down two or three here. For instance, camphor, while undoubtedly causing sleep in most animals, has no effect whatever upon dogs. Take, again, citric acid, the acid of lemons, which is a deadly poison to most animals.

19411. Do you think that a skilled physiologist, if he wanted to experiment with camphor or citric acid, would choose a dog for the purpose?—I am taking only a few out of a very large number of drugs.

19412. You are stating that there are a good many substances which do not affect a dog?—Yes.

19413. Do not you think that a skilled physiologist would be aware of those substances and would not use a dog for the purpose?—Yes, but I am taking the case of a new drug. Supposing that he is experimenting upon the action of quite a new drug, about which he knows nothing, he makes the experiment on a dog, and finds a definite result; he finds, for instance, in one case *cocculus indicus*, or say another drug such as woody night-shade, which is deadly poison to a dog, he finds that it kills the dog, and comes to the conclusion that it is a deadly poison necessarily to man, whereas it is quite harmless.

19414. Do you think that mistakes of that kind have been made? Have you ever seen any accidents occurring through such mistakes?—I have seen a very serious mistake made and carried out in many medical journals that calomel has no effect in increasing the bile flow in a dog, and, therefore, it must be so in a human being, whereas the medical profession for almost centuries have known the value of calomel. It was found that a mistake had been made.

19415. How was that discovery made—by experiment?—By experiment in the first instance that it would not produce bile, and it was then published that calomel was really of no good to cause an extra flow of bile. Gaurana is another drug. Gaurana intoxicates a dog, but is absolutely harmless to a human being.

19416. Do you consider that dogs can be fully anæsthetised and yet kept alive for the purpose of examination?—On account of the intermittent pulsation of a dog's heart, of which I have spoken, I think it is practically impossible to keep a dog fully anæsthetised for any length of time.

19417. I think you said that you yourself had not operated upon a dog?—No, I have not.

19418. Or anæsthetised dogs during an operation?—No, but I have watched it very carefully. I am wrong in saying that I have not anæsthetised a dog. I did it once in order to remove a little tumour from a pet dog of my own. I used every possible care in the case, and I am perfectly aware that the dog felt and could not help feeling it. It was a pet dog, and I did my best to avoid giving it pain. There will be an anæsthetic I think in the immediate future that can be used with animals to produce a more perfect anæsthesia.

19419. Do you consider that any struggle in an animal or man while under an anæsthetic is an indication that they must be suffering pain?—I do. In the case of a human being, when your patient is being put on the table he passes through the usual stages of delirium, excitement, and so on, and then sleep. So long as he is sleeping really perfectly anæsthetised and there is no movement he is absolutely painless, but then there comes a period when he is just recovering, a period which I have noticed you have had described in the evidence before you, when if you put an extra stitch in, the patient will give a cry and pull away from you. When the patient awakes in most cases he will say that he felt no pain whatever, but having forgotten the pain is no indication that it did not exist.

19420. Then do you draw a distinction between a person who comes to and is not conscious of having been hurt at all and a person being hurt who has actually felt pain; do you consider that there is any difference between the two?—I think that wherever there is any purposive movement, where there are any of those reflexes that one sees when the skin is touched with a knife, that animal, human or other, feels a pain, but in many cases of anæsthesia apparently they have forgotten the pain when they awake, but the pain was there.

19421. Do you attach importance to that?—Yes

19422. Do you consider that a person who has an operation and struggles, and whom you think is under pain, but when he comes to and is asked about it when the operation is over says that he has felt noth-

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ing, has suffered shock of any kind more than he otherwise would?—Undoubtedly. We find in the case of surgical operations which were performed before the introduction of chloroform, and even since then——

19423. I am speaking of since the introduction of chloroform. Supposing that a person is operated upon under apparently complete anæsthesia, and when he comes to, feels and remembers nothing, is he in any worse condition if he has shown some signs of struggling during the operation from a man who goes through exactly the same thing and has not struggled?—I think he is in a worse condition from two points of view, because pain increases the risk of surgical shock, and he has also felt the pain whether he has forgotten it or not.

19424. Can you point to any case in which a patient has suffered severely from shock in consequence of having felt pain during what was apparently complete anæsthesia?—If it was apparently complete anæsthesia, they would not struggle or give any evidences of pain whatever.

19425. The question is whether it is an evidence of pain?—I think the purposeless movements that one sees very often, reflex movements, and excitement, have nothing to do with pain, but I have many times seen persons under apparently complete anæsthesia, when you can touch the eyes and prick the skin and they will not be affected, but there is a certain very painful part of the body, and the moment that you touch this one part with a knife it is a sure test, because if the anæsthesia is incomplete the patient tries to struggle away; and with a little deeper anæsthesia these movements will be abolished, and the patient will lie perfectly still.

19426. You would exclude dogs from the operation of the Act?—I would exclude them from all experiments.

19427. And you think that science would not suffer at all in that case?—No. I cannot see why in the later Act stray dogs should be given privileges which kept dogs have not got. Stray dogs are expressly forbidden to be used for vivisection purposes.

FORTY-THIRD DAY.

Wednesday, 11*th December* 1907.

PRESENT:

The Right Hon. The Viscount SELBY (*Chairman*).

Sir W. S. CHURCH, Bart., K.C.B., M.D.	Mr. A. J. RAM, K.C.
Sir W. J. COLLINS, M.P., M.D., F.R.C.S.	Mr. W. H. GASKELL, M.D., F.R.S.
Sir J. MCFADYEAN, M.B.	Mr. G. WILSON, M.D., LL.D.
Sir M. D. CHALMERS, K.C.B., C.S.I.	

Captain C. BIGHAM, C.M.G. (*Secretary*).

Mr. L. E. SHORE, M.D., called in; and Examined.

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19428. (*Chairman.*) You are, I believe, a Fellow of St. John's College, Cambridge, and Doctor of Medicine?—Yes.

19429. And University Lecturer on Physiology?—Yes.

19430. Do you remember some experiments that were made by Professor Gaskell and yourself (you were assisting, I believe, in them) with a view to ascertaining what the method of action of chloroform on the heart was?—Yes, I remember them perfectly.

19431. Were there a series of experiments by you and Dr. Gaskell?—Yes, there was a series by Dr. Gaskell and myself which were carried out in the years 1890 and 1891.

19432. Do you remember Colonel Lawrie coming to Cambridge to see some experiments of the same kind?—Yes.

19433. Which, I believe, were experiments made for the purpose of showing them to him?—Yes.

19434. In what year was that?—That was in July, 1894.

19435. There were two experiments made in the presence of Colonel Lawrie, I understand?—Yes.

19436. Were those of the same nature as the experiments that you had conducted previously in 1891?—Yes, of the same nature with a slight improvement in the method.

19437. Will you just tell us what the experiment was; was it what you call an experiment by the method of cross circulation?—Yes.

19438. Will you just explain very shortly what it was?—The object of the experiment was to decide whether chloroform causes a fall of blood pressure by an action on the heart or an action on the vaso-motor centre in the medulla, which could be absolutely decided by taking an animal and so ligaturing the arteries of the brain that its own blood could not reach its own brain—the brain of the animal being supplied with blood from another animal. When that was done one could send chloroform either to the brain or to the heart of the first animal.

19439. Then two dogs were operated upon side by side at the same time?—Yes.

19440. And then the vessels of one which would lead to the brain, and bring blood to the brain, were fastened up, tied up with ligatures?—They were tied with ligatures and in some cases divided between the ligatures. The animal with the vessels divided to separate its blood from its brain we call the fed animal; the other animal we call the feeder; it delivers blood to the brain of the fed but to the brain of the fed only, and the blood is returned from the brain of the fed back to the feeder.

19441. What is the process by which you ascertain what the effect of chloroform on the heart directly is without the possibility of its coming through the brain? A record of the blood pressure of the fed animal is taken and a fall of that pressure can only be brought about by a change in the heart when chloroform is administered to it, because the chloroform administered to it cannot reach its brain, because the blood vessels leading to the brain of the fed animal are completely ligatured.

19442. The object being by giving chloroform to the fed dog and at the same time cutting off the supply of blood to the brain of the fed dog, to test what the direct action is of chloroform upon the heart without the possibility of the brain affecting it?—Yes.

19443. You carried out that operation, or practically the same operation, I understand, in 1891 and 1892?—1891.

19444. Were those operations successful?—Yes.

19445. Were they carried out successfully in the sense of ascertaining what you wanted to ascertain?—Yes, they satisfied us that we had definitely proved the point that chloroform does act directly upon the heart and that a fall of the blood pressure is due in the main to that.

19446. Then in 1894 you carried out practically the same experiment twice in the presence of Colonel Lawrie you told us?—Yes.

19447. Were those two operations both successfully carried out or was there any failure in either of them?—The first experiment was quite successful.

19448. Proving, to your satisfaction at least, what?—Proving the point that we wished to demonstrate to Colonel Lawrie.

19449. That there was a direct action on the heart?—Yes, but he criticised our experiment and thought that some chloroform was reaching the brain of the fed; so we offered after the death of the dog to inject some coloured solution, a blue solution was used, to see whether our ligatures were all perfectly tight and that no chloroform was reaching the brain. We found that a small amount of our blue colour did reach the brain.

19450. You forced it, I suppose?—It was forced by a

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syringe. There is no proof from that that any chloroform got up by the same path as the blue colour did because the conditions of the pressure in the brain were totally different during life and very probably the blood did not leak up past those ligatures, but even if it did it did not vitiate the experiment in the slightest degree.

19451. And the first experiment was not vitiated in any degree?—It was not to our mind. But we offered to do the experiment again in four days' time before Colonel Lawrie once more, taking particular care, and in that case we divided the blood vessels between the ligatures and took the greatest care that nothing whatever could leak up.

19452. Then Professor Gaskell, I understand, communicated what you had ascertained about the possible vitiation to a certain extent of the first experiment to Colonel Lawrie?—Yes, we offered to do that straight away and the result of it was that we offered to repeat it again?—But afterwards there was a letter written. Then when the first experiment had taken place, Professor Gaskell wrote and explained what had happened?—Yes.

19453-4. That is a letter that has been communicated to us by Colonel Lawrie?—Yes.

19455. And then this second experiment was held four days afterwards, and it went all right?—It was a perfectly satisfactory one; there was no leakage whatever in that case of any sort.

19456. That is the full account of what is referred to the Question 16972 and the following questions in the evidence?—Yes.

19457. Then there is another matter—I think it is probably an accidental error of Colonel Lawrie's. At Question 16869 he is asked this question: "You also say that you saw another physiologist perform prolonged vivisection experiments on dogs under a ten-drop dose of morphin solution, this dose being equal to one-twelfth of a grain of solid morphin. Had they no other anæsthetic of any sort or kind?" and he answers, "I believe not, until the observations began on the action of chloroform." Is it correct that this was one-twelfth of a grain?—No. I suppose Colonel Lawrie is referring to these two experiments that we did?

19458. It would look so?—In each of those two experiments we had two dogs. Of the two dogs, one dog received ten cubic centimetres of a 2 per cent. solution of hydrochloride of morphia, and the other dog six cubic centimetres of the same solution.

19459. Will you just explain what the difference is between that and the statement of Colonel Lawrie? —A 2 per cent. solution of morphia contains 30 grains in the 100. As the dog had received 10 cubic centimetres, it therefore had 3 grains of morphia, not one-twelfth.

19460. The dose was really 36 times stronger than what appears from Colonel Lawrie's statement in his *précis*?—Yes, and the other one which, as I have said received only six cubic centimetres of the solution, had received 1·8 grains.

19461. That may be accounted for, I suppose, by his having thought that it was a 10-drop dose of a different solution from that actually used?—Perhaps he made some mistake of that kind.

19462. However, it is inaccurate to the extent of 36 times?—Yes.

19463. And that dose that was given was a completely full and satisfactory dose?—Yes. I see in the statement that Colonel Lawrie implies that no chloroform was given until the test was purposely made with the chloroform. That is not the case. In addition to the morphia, chloroform was given to the animals before the skin was cut, and during the cutting operation the A C E mixture, just as it was required.

19464. Were the animals kept in a state of anæsthesia before the cutting began and up to the time of death?—Absolutely.

19465. (*Sir William Church.*) Others were present at these experiments besides Colonel Lawrie, yourself, and Dr. Gaskell?—Yes, at the first experiment there were two of Colonel Lawrie's students from India. I suppose they had been helping him in his own experiments in India, and they were present. And at the second experiment Sir Thomas Clifford Allbutt was present, and Dr. Anderson, as well as ourselves.

19466. And during the experiments the various steps in the operations were recorded as they took place?—They were written, nearly all of them, automatically, by the tracing of the animal and by clocks and other automatic arrangements.

19467. And the record, such as it was, was gone over by all those present at the end of the experiments and signed by them, was it not?—Colonel Lawrie did not actually sign the experiment. He was present at the time, and confessed that the experiment was a success, and that air was properly entering the animal, and other details that we asked him.

19468. Did the others who were present sign?—No. They did not actually write their names on the record.

19469. (*Dr. Gaskell.*) Surely they did?—No, we wrote down the statement that they were present on the record, but they did not actually sign it.

19470. My impression is that they did?—No.

19471. (*Sir William Church.*) Do you know what became of that record?—I have it in my possession.

19472. Was a copy of it sent to Colonel Lawrie, such as it was?—The tracing was analysed and a report of it was sent out to Colonel Lawrie to India, as was agreed, for him to sign there, and then for it to be published.

19473. (*Dr. Gaskell.*) That report that was sent out to him was signed by the other people present, if I remember rightly?—I do not remember.

19474. You signed it, and I signed it, and Sir Thomas Clifford Allbutt, and Dr. Anderson all signed that report if I remember rightly?—I do not remember their signing it. I signed it, and I think you got their signatures afterwards.

19475. My impression is that they all signed when it was sent out to Colonel Lawrie?—Yes, I know that we agreed to do that.

19476. Have you ever had that report back?—Colonel Lawrie has never sent that report back to us for publication.

19477. (*Sir William Church.*) Did he send any reply at all to the communication?—None whatever.

19478. (*Dr. Gaskell.*) There is another matter, but I think that the Commission will be interested in it. You have had chloroform as an anæsthetic yourself?—Yes, on two occasions.

19479. Could you tell the Commission whether on those two occasions you endeavoured to test the length of time after the anæsthetic had been administered before unconsciousness supervened?—On the two occasions that I had to undergo an operation, and was given an anæsthetic, I decided to see whether I could tell the length of time that intervened from the commencement of giving the drug to the period of unconsciousness, and I decided to do it by carefully counting my regular respirations. On the first occasion I was able to count ten respirations, and then I became absolutely unconscious. On the second occasion I could count twelve. That makes considerably less than a minute before unconsciousness supervened. But one ought to remember that in that case I was receiving as an anæsthetic nitrous oxide and ether, which are known to be fairly rapid. I have no experience as to whether chloroform alone is equally rapid, but we know that chloroform is a stronger anæsthetic, but its full effect may not come on so soon. The second point which those personal experiences led to is also interesting. In the middle of the operation I heard the surgeon make a certain definite remark about the operation, but I could feel no pain whatever. When the operation was completely over, and I recovered consciousness, some four hours later, I inquired of the surgeon, and he told me that the remark had been made when the operation had been in progress about half an hour. That experience, I think, points out that the sensation of pain is more easily abolished than such a sensation as hearing, and it is interesting to note that in local or peripheral anæsthesia, which is now very much used, we have the sensation of pain much more readily abolished than other sensations. Many years ago I made some observations with cocaine on the tongue, when I found that the different sensations of the tongue disappear in definite and precise order according to the dose of cocaine or the duration of the cocaine action; and the first of all sensations to disappear is the sensation of pain, then of sweet, and then bitter, then salt, and then acid, and last of all the sense of touch. It is quite striking to notice that

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when the tongue is touched with the needle you feel the touch, but although the needle is thrust into the tongue you feel no pain. So we see that also in local or peripheral anæsthesia painful sensation is the first, I think, to be abolished.

19480. May I ask was the operation in which you heard distinctly the surgeon make a remark, and correctly heard it, of the nature of a fairly severe operation. I mean was the anæsthesia that you had supposed to be full surgical anæsthesia?—The operation was certainly severe. I was put under the drug at 9 o'clock in the morning, and I did not recover consciousness till 1 o'clock in the middle of the day.

19481. Would a surgeon, do you think, with your experience, say that that anæsthesia was rightly described as light anæsthesia, that they were attempting to put you under, at all events? We have had a great deal of discussion here at this table about the meaning of light anæsthesia and deep anæsthesia, and it has been said that light anæsthesia is never given in the case of operations. Therefore I presume you were under comparatively deep anæsthesia; you were supposed to be fully anæsthetised?—I was supposed to be fully anæsthetised, but at the time when I heard the remark, no doubt the anæsthesia had been allowed to go light.

19482. But yet there was not the slightest sensation of pain?—Absolutely none.

19483. (*Dr. Wilson.*) You say that these animals that were experimented on were absolutely unconscious of pain during the whole time of the experiment?—Absolutely.

Mr. A. GUILLUM SCOTT and Sir FREDERICK BANBURY, Bart., M.P., called in; and Examined.

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19484. (*Chairman.*) (*To Mr. Scott.*) You are a Member, and, I believe, Chairman of the Royal Society for the Prevention of Cruelty to Animals?—I am.

19485. They have drawn up a *précis* of the evidence to be given before the Royal Commission on Vivisection, which they have printed, and sent to us?—That is so.

19486. I do not quite understand, seeing that there are two witnesses before me, whose *précis* that is. Is it a *précis* of the evidence which you propose to give, or which Sir Frederick Banbury proposes to give, or both?—It is evidence of the Society. It was decided upon at a meeting of the council that the representatives should give this evidence on behalf of the council and the Society.

19487. Then, perhaps, instead of calling it a *précis* of the evidence to be given, we had better call it a statement of the views which the Society have resolved to be their views?—Yes, that is so.

19488. And, therefore, to begin with, and probably to shorten matters, we will put it upon our notes as such?—If your Lordship pleases:—"The Royal Society for the Prevention of Cruelty to Animals was founded to protect animals generally, and not for the suppression of any kind of cruelty in particular. The members of the society are drawn from all classes, and there is a considerable difference of opinion amongst some of them upon various subjects, although on others they are practically agreed. On the question of vivisection, for instance, there is a wide diversity of opinion, many persons objecting to any kind of vivisection; while others object only to painful experiments; others again consider that vivisection is necessary for the advancement of science in the interests of mankind. The attitude taken up by the society, almost since its foundation, has been that it deprecates all experiments on animals which cause pain, but as regards experiments which cause no pain there is no ground for interference by the society, because the question of cruelty does not arise. The society, therefore, is of the opinion that all severely painful experiments should be carried out while the animal in question is completely under the influence of an anæsthetic, and that it should be destroyed before the effect of the anæsthetic has been removed. The society has, ever since the passing of the Cruelty to Animals Act, 1876, repeatedly pointed out that the supervision of experiments carried on under licences granted under that Act is totally inadequate. It may not be possible that every experiment should be performed in the presence of an inspector, but all those should be which cause pain, either in their initial stages or afterwards. All severely painful experiments should only be performed in a particular place or places, so as to facilitate inspection; and all painful experiments should be prohibited except in the presence of an inspector. The number of experiments has increased since 1876 from between three and four hundred in that year to over 46,000 in 1906, and the number of inspectors (originally two and now three) is entirely out of proportion to the increase in the number of experiments. The number of Inspectors is absolutely inadequate if a proper supervision is to be exercised over the licensees. The Inspectors occasionally witness experiments, but very few. The inspectors' reports are drawn up from the information culled from the reports made to them by the licensees. It is impossible for the Inspectors to exercise proper supervision while experiments are going on every day in all parts of the country. The society is strongly of opinion that additional inspectors, who shall not necessarily consist of medical men, should be appointed, and that such inspectors should visit all licensed places frequently, at various times, unknown to the licensees, and that all holders of licences should be required to declare that no experiments, except those duly reported in accordance with the terms of the licence, have been performed, and the licensees should be required to make a return of all animals acquired by them, and how obtained. With a view of securing and maintaining the confidence of the public, the necessity of obtaining the consent of the Home Secretary for the prosecution of a licensee for an alleged offence should be abolished, and it should be left to the discretion of the magistrates, who are empowered to refuse a process if they consider an application is vexatious. The period for taking proceedings should be extended, so as to be within six months of the publication, or the giving information of that which is impugned. The society, soon after the Act of 1876 came into force, successfully instituted a prosecution under Section 6. The society has not been interested in or promoted any other prosecutions."

19489. In the case of the Royal Society, where something of the same kind was done, we thought it necessary to call the President of the Royal Society, so that any questions might be asked which occurred to any members of the Commissions arising upon their views, and we are following the same course in calling you and, as the society wishes it, another well-known member of the society. I do not know for myself that I have any questions to ask you, except whether there is anything that you wish to add to this statement of the society?—I do not know whether I might say that we are as anxious as the most extreme persons to abolish unnecessary pain, and we differ from them only in not attempting impossible legislation, while trying to improve legislation on this subject. That is our effort. The anti-vivisection societies, of course, occupy a special position, and carry out a special work, which could not be undertaken by an institution organised to conduct general operations by various modes of action, jeopardising other objects of a kindred nature on a far more extensive scale. For instance, drinking fountains have a society, and the Dogs' Home another of a special character.

19490. Your society has a very large number of members?—A very large number of members.

19491. Can you say how many?—I cannot really tell you that, but I can tell you how many officers we have. We have about 160 inspectors all over the country.

19492. And you spend a considerable income?—We spend in every way about £60,000 a year.

19493. When was your society established?—In 1824 I think it was.

19494. And it has been in existence ever since?—Yes.

19495. And in full operation?—Yes, and it has been growing and growing in the most extraordinary manner. I may, perhaps, say that I am what is called a man of leisure and, therefore, give up a great deal of my time as Chairman to the society. There is not a day passes when I have not some communication. I mention that to show that there is a great deal of work.

19496. A man of leisure means an unpaid busy man?—It seems so, and in my case I think I may say it is so.

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19497. I think, perhaps, the best plan would be that we should hear Sir Frederick Banbury, and then if any of the members of the Commission wish, as I dare say some of them will, to ask questions first of you and then of Sir Frederick Banbury it will be better; but I think it is better to have first the general views of you both?—If you please.

19498. (*To Sir Frederick Banbury.*) Will you tell us what view you wish to speak to?—I, of course, only come here to represent the view of the society and not my own particular views upon the question of vivisection, but I think I can say that, speaking broadly, the view of the society is that while vivisection is permitted by law and animals are called upon to undergo, not for their own benefit, but for the benefit of man, operations which in their nature involve not merely the sacrifice of the life of the animal but the possibility of considerable suffering, every precaution should be taken to prevent pain to the animal creation. They are called upon to bear this burden for the general benefit of men; that is the idea, I believe.

19499. Of men and animals?—Men and animals; and it would seem to be an obvious principle of humanity and justice that man should concede to these dependent creatures all those safeguards against pain that he naturally demands for himself if he is undergoing a similar operation for his own benefit. Therefore, I think I may say (and Mr. Scott, I think, will agree with me) that the opinion of the society is that there are not a sufficient number of inspectors in order to see what actually is going on. Next, I think that, in the opinion of the society, it is not necessary that the Inspector should always be a medical man. I would not for a moment suggest that he should not be a medical man, but that that should not be an essential qualification, but that if there are other qualifications they should be considered perhaps before the qualification of being a medical man. Further, I think that another important point is that the Inspectors should give their whole time to the duties of their office and not hold any other appointment. In the Report of the Commission of 1876 I believe there is an observation that "publicity is the antidote of suspicion," and I am rather afraid that that observation of that Commission has not been carried out by the law, because I think there is a strong opinion amongst members of the society that the Inspectors obtain their reports very often, not from what they have seen themselves, but from what they are told by the people conducting the operations.

19500. Was that phrase, "publicity is the antidote of suspicion," used with reference to operations in laboratories, or was it used in the Report with reference to reporting cases brought into Court by the society?—I am afraid I cannot answer that. I have not referred myself to the Report; it has been given to me on, I think, good authority, that there is that statement in the Report, but whether it referred to experiments in laboratories or whether it referred to the case you have mentioned I cannot say.

19501. (*Sir William Collins.*) I have the passage here: "Publicity is the antidote of suspicion, and we look to the reasonable superintendence of constituted authority as affording the means of reconciling in the public mind the sentiment of humanity with the desire for scientific knowledge."

19502. (*Chairman.*) That looks as if it referred to laboratories?—Yes, I was not quite sure. I do not want to say anything that I am not absolutely certain about. I think that practically that is all I have to say. I do not know whether Mr. Scott bears me out.

(*Mr. Scott.*) Yes, I do. I think the inspection of returns should be the inspection of experiments; that is to say we want more inspectors. They seem to be able to inspect the returns, but they do not, I believe (in fact they could not) inspect all the experiments.

19503. You say that there were 46,000 experiments in 1906?—Yes, that is so.

19504. You are aware that the vast majority of those are inoculations?—Yes, quite painless.

19505. You do not mean that every one of those should be inspected?—No.

19506. Would you confine the inspection to any particular class of experiment. Some, of course, which are cutting operations are extremely slight?—Yes.

19507. And some are very serious?—Yes. What we say in our statement is that "All severely painful experiments should be carried out while the animal in question is completely under the influence of an anæsthetic."

19508. That answers my question?—Yes.

(*Sir Frederick Banbury.*) Might I say one word? I did not quite finish. I ought to have said that with a view of facilitating the work of inspectors, the society is of opinion that the number of registered places should be to some extent limited.

(*Mr. Scott.*) Yes, that is so. The words of the Statute are, "Cruelly ill-treating, abusing, or torturing animals." Where no cruelty and certainly where no pain is caused in an experiment interference is not called for any more than when animals are destroyed for food.

19509. You are referring to the original Cruelty to Animals Act?—Yes, the original 1849 Act.

19510. Not the 1876 Act?—No.

19511. That is your most important Act?—Yes, the improvement on Martin's Act.

19512. (*Sir William Church.*) In your statement you say, "The society, therefore, is of the opinion that all severely painful experiments should be carried out while the animal in question is completely under the influence of an anæsthetic." Might I ask you what you consider a painful experiment?—It is rather difficult to define. I cannot give you anything in the way of a professional opinion. I am sorry to say I cannot do that, but there are operations which are, of course, notoriously painful, and such a one, we think, should be under an anæsthetic and the animal should be destroyed before the effects of the anæsthetic had been removed.

19513. Is not that the present law?—Is that so? We have been reading in the papers about the brown dog at Battersea—I have seen something about that. That is a case in point.

19514. As you have referred to that, might I ask you what the circumstances are with regard to the brown dog at Battersea?—I do not know them. I only know that I read in the papers that the students, I am not surprised, were annoyed at some inscription.

19515. Before you advance that as an instance should you not know something of the circumstances?—The dog, I understand, was twice operated upon. I thought it was understood that it was brought round (I do not know it of my own knowledge, but I understand that it was the case) and operated upon again and suffered a great deal of pain; the forceps were in the dog.

19516. Might I ask for a little more particulars about the dog, as you have instanced the brown dog of Battersea. Do you know when it was first operated upon?—No, I do not know, I am sorry to say, but I understood that that was the case.

19517. Do you know what the character of the operation was?—No, I do not. I believe it was a painful one.

19518. Was the dog under anæsthetics at the time the operation was done?—I understand so.

19519. Do you know when the second operation was done upon it?—No.

19520. At what interval of time after the first?—Very soon I am told.

19521. You are not aware that between two and three months elapsed?—Is that so? It was about the third day, was it not?

19522. Are you aware of the condition the brown dog was in during the two or three months that elapsed between the first operation and the second?—No, I am not. I think there were three operations altogether. (*Sir Frederick Banbury.*) If I might answer the question, because I happen to have read the evidence at the trial, the evidence at the trial to the best of my recollection (I have not refreshed my memory with it to-day) was that there were three operations upon the brown dog: that the first operation took place some two or three weeks, or a month, before the second, I could not say which, but that at the end of the second operation the dog was handed over to another operator, the operator who performed the first operation quitted the building, leaving the dog in the hands of the operator for the third operation. That, to the best of my recollection, was not denied in Court.

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19523. Was the dog allowed to recover from the anæsthetic between the second and the third operations?—Is it possible for a dog to be under an anæsthetic for so many hours as that dog was without recovering consciousness?

19524. I suppose you will not take it from me that you can keep a dog under anæsthetics as long as you like almost, that is to say, up to six, eight, ten or twelve hours?—I am not an expert, but I am informed by experts that it is almost impossible.

19525. You have no knowledge of your own on the subject?—No, I am not an expert.

19526. Would you accept it from me that the dog was under anæsthetics at the end of the second operation, and was not allowed to recover from that anæsthesia until after the third operation was performed?—My desire is to accept everything that you say, but in this particular case I do not think I can.

19527. (*To Mr. Scott.*) I should like to go back to what is considered by your society a painful operation, because you say that you have no objection to inoculation experiments?—No, we have none, I think. They are classified, certainly, in the return to the Home Office under different heads.

19528. With regard to inoculation experiments you say apparently that your society does not regard them as painful?—We believe them not to be painful.

19529. But we have had a good many witnesses here who have objected to inoculation experiments very strongly, saying that they are extremely painful in some cases; not the inoculation itself, but the result of the inoculation. What is the view of your society about that?—That all painful experiments should be prohibited except in the presence of an inspector.

19530. That is just my point. I am speaking of these inoculation experiments. You say that they are not painful, but we have had a good many witnesses who have objected to them on the ground that their results are painful?—If the Inspector is present and pronounces an experiment not to be painful I suppose we should accept it.

19531. (*Chairman.*) I understood you to say when you spoke of 46,000 operations that you were aware that a very large majority of those were merely in the nature of inoculations, and that you never thought of there being any necessity for inspectors to be present at those?—No, I do not consider, speaking for myself, that those would be painful, and I think that is the view of the society, unless they produce pain, which we do not think they do.

19532. (*Sir William Church.*) But my question was this. I wished to know whether your society objected to inoculation experiments on the same grounds as many witnesses have, that the results may be painful?—Yes, in that case we should.

19533. But you cannot tell what your results will be?—We say, "All painful experiments should be prohibited except in the presence of an inspector"—he must be the judge.

19534. Do you know what these inoculation experiments are chiefly done for; are you not aware that the large bulk of them take place in laboratories connected with public health for the purpose of diagnosis of disease and preventing its spread?—Yes, I think that would be a very noble and proper thing.

19535. But that is what the large bulk of these inoculation experiments are done for?—Yes.

19536. And, of course, you cannot tell whether the result of an inoculation may cause discomfort to an animal or not until after the proper time has elapsed to see what has been the result of the inoculation?—Our view, as we have expressed it, is that it is not possible that every experiment should be performed in the presence of an inspector, but that all those should be which cause pain either in their initial stages or afterwards.

19537. Then am I to take it that your society does object to inoculation experiments for the purposes of diagnosis?—Anything that causes pain to the animal afterwards we should object to.

19538. The Inspector would have to be present then and give his consent or not according as he thought that the result of an injection experiment might or might not be painful. Is that what I am to take from you?—I think so.

19539. That, of course, would put a stop to the use of inoculation experiments, possibly for the purposes of public health?—I am sorry to say that not being a professional man I am not able to follow some of these questions.

19540. Might I put it in rather a different way. Would your society prosecute a man who overrode or overdrove a horse when going to the assistance of another man?—I think very likely if we had all the facts of the case before us we probably should not.

19541. But yet you would not allow an inoculation experiment which might or might not have the result of saving a great many human lives, I mean by preventing the spread of an epidemic or anything of that kind, unless the Inspector was assured that the result of the experiment could not be painful?—That is the line we take. We want to defend the animal as much as we can—that is our object—what the society exists for. I only give the general views. I am not giving my own opinions at all.

19542. I want to get the opinion of your society?—Yes, I am trying to give it you.

19543. I think you will admit that it would be very hard for the Inspector, unless he had some fixed rule whenever any material was going to be injected that might give rise to pain or discomfort in the animal, to say that it was not to be allowed; he would have great difficulty in knowing what injections he should permit and what he should refuse permission to?—We say, "All painful experiments should be prohibited except in the presence of an inspector."

19544. Might I ask what you would consider the qualifications for an inspector?—I should imagine that the man should know something of anatomy and surgery generally, and I suppose that really he would have to have some knowledge of hospitals and something of that sort. I suppose that would be necessary before the man could give an opinion.

19545. You think that they should not all be medical men?—Not necessarily medical men. There seems to be an idea that they might be men of some experience who were not medical men.

19546. Still, I should like to have some more definite idea what you think should be the qualifications for an inspector?—We have no objection, so far as I know, to the qualifications that exist now, only we say that there are too few of them.

19547. (*Mr. Ram.*) Too few inspectors?—Yes.

19548. (*Sir William Church.*) Hitherto all the inspectors have been, I think, medical men?—That I am not sure of. They are appointed by the Home Office. I should think they most likely were, but there are very few of them.

19549. (*Chairman.*) Do you understand that these inspectors are to be persons who should have authority to say to a professor of physiology in a university, who is going to perform an experiment under anæsthetics with a certificate from the College of Physicians, that the animals should be allowed to recover, and therefore feel pain, "In my opinion this will cause pain to the animal, and it must be stopped." Surely all he has to do is to report any breach of the law?—I should say yes.

19550. He would not have any preliminary authority to stop experiments which appeared to be in conformity with the Act, although they caused pain?—No. If a license has been granted for a painful experiment we say that it ought to be performed in the presence of an inspector.

19551. (*Sir William Church.*) You cannot, therefore, give the Commission any assistance with regard to what the qualifications of these inspectors should be?—I am afraid very little.

19552. Has Mr. Coleridge placed before your society his amended Bill for experimentation on animals?—I do not know whether he has or not. I rather think that he has very much taken up the lines of the Bill that we ourselves proposed.

19553. Have you sent us a copy of your proposed Bill for the prevention of cruelty to animals?—No, we have not. Nor have we one.

19554. (*Sir Mackenzie Chalmers.*) Have you not got one?—No. (*Sir Frederick Banbury.*) I introduced one in the House some two or three years ago.

19555. (*Sir William Church.*) But not by your society?—Well, with our approval. It was not drawn up by the society. It was really Mr. Stephen

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Coleridge's Bill amended by myself—his Bill of that date.

19556. (To *Mr. Scott.*) You are aware that in his amended Bill he proposes that before a man receives a licence for being allowed to experiment on animals he should get a certificate of humanity?—This is what you refer to, I think: "A certificate recommending that the application be granted on account of the applicant's reputation for humanity, which must be signed by two or more of the following persons, that is to say, a Justice of the Peace, a minister of any religious denomination in the United Kingdom."

19557. Yes, that is what I refer to. Is that approved by your society?—No, I cannot say that it is. We have not had it before us.

19558. Do you think that that certificate would be of any value?—I do not think it would be, really.

19559. Then you would also approve of its being in the power of anyone, without the consent of the Home Office, to institute a prosecution against anyone who is stated to be guilty of cruelty in experimentation?—Our view is, "With a view of securing, and maintaining, the confidence of the public, the necessity of obtaining the consent of the Home Secretary for the prosecution of a licensee for an alleged offence should be abolished, and it should be left to the discretion of the magistrates who are empowered to refuse a process if they consider an application is vexatious." That is what we prefer.

19560. At the present moment anyone can prosecute a person guilty of cruelty to animals under the Cruelty to Animals Act?—Yes.

19561. And your proposal would amount to a similar method of prosecution in the case of those who hold licenses for experimentation. That is the meaning of it?—We mean that it should go before a magistrate in the same way as anything else, and should not be left to the Home Secretary, which, of course, causes necessarily a great deal of delay, and very often no proceedings can be taken, because the period has passed when we get the information of anything like an abuse of the Act.

19562. You do not think that that might be a very vexatious clause to those who are engaged in this work in laboratories?—It did not occur to me that it would be so at all.

19563. Considering the statements that you must be aware are made about the experimentation that goes on in laboratories, do not you think that very often cases would be brought forward before magistrates who could not judge at all of the rights of the case, which would be vexatious?—No, I think the ordinary magistrate would be a man of common sense, who would grant a summons or not, as he thought right, and the process would not go on.

19564. In judging whether an experiment is cruel or not, is not there a considerable amount of what is generally considered physiological knowledge required?—I should say certainly.

19565. Is that possessed by the public and the majority of magistrates?—I should be afraid not.

19566. Therefore is it not only possible but probable that in perfectly innocent ignorance, a great many prosecutions might arise?—So far as the society is concerned every possible care is taken never to prosecute unless it is absolutely necessary. We meet and discuss these matters. No prosecution can take place without the consent of the parent body.

19567. You are speaking now of your society; I am speaking of the general public?—I beg your pardon; I did not follow you. It did not occur to me in that way. So fas as we are concerned, I am quite sure that we should not prosecute unless we had a very good case.

19568. Have your society any evidence whatever that unnecessary cruelty is practised in laboratories?—We have never had but one prosecution, and that was for advertising, under the Act of 1876. That is the only one we have had, and it is very curious that it should be in that connection. I have it here, if I may just read it to you. This is from our report for 1876: "First conviction under the Vivisection Act.—The Cruelty to Animals Act, 39 and 40 Vict., c. 77, came into operation on the 15th of August last. Three days later Dr. Abrath, of Sunderland, issued a large placard headed, 'The Balham Mystery,' announcing his intention to deliver a lecture at Sunderland on 'Antimony,' when he would perform experiments on animals to show the effects of poisons, and to demonstrate his theory that Mr. Bravo was not killed by that drug. The branch of the Royal Society for the Prevention of Cruelty to Animals at Sunderland immediately reported the matter to the secretary in London, and prompt measures were taken to prevent the learned gentleman from performing his experiments. According to the 6th Section of the Statute named above, an offence had already been committed by the announcement of a public exhibition of experiments on animals, and Dr. Abrath, having spoken contemptuously of the new Statute at his lecture, and provoked the derision of a portion of his audience against it, instead of apologising for his projected defiance of the Act, was summoned to appear yesterday before the Sunderland Borough Bench, at the instance of the R.S.P.C.A., when he was fined for publishing the illegal placard alluded to."

19569. Might I ask who Dr. Abrath was? Was he the holder of a licence at the time?—No.

19570. Then that case did not come under the Act?—Yes, it came under it in this way. This is the section: "Any exhibition to the general public, whether admitted on payment of money or gratuitously, of experiments on living animals calculated to give pain, shall be illegal."

19571. That is, illegal to the holder of the licence?—"And any person publishing any notice of any such intended exhibition by advertisement in a newspaper, placard, or otherwise shall be liable to a penalty not exceeding one pound." This man's offence was under that Act, of asking the public to come and see this experiment, and for advertising that he was going to perform it.

19572. But the gentleman in question was not the holder of a licence?—No.

19573. He did not propose that these experiments that he wished to show to the public should be done under the Act?—His offence against the Act was that he proposed that there should be a public exhibition; that is where the offence came in.

19574. That had nothing to do with the Act regulating experimentation on animals. In that Act there is a special clause that an experiment should not be performed in public?—That is just what I was saying. You were asking me about these matters. It is the only conviction we have ever had, and that was simply for advertising.

19575. He was not a licensee?—I am not able to say that, but his offence was advertising the performance of the operation.

19576. Then your society, which has, I understand, £60,000 annually at its disposal, has never been able to bring forward instances of unnecessary or wanton cruelties having been performed in laboratories?—No, I have no case.

19577. Might I not infer from that that the probability is that wanton and unnecessary cruelty does not exist in laboratories?—That is a difficult question for me to answer. We say that there are not enough inspectors, and that the Inspector inspects the returns but does not inspect the experiments. Only three inspectors cannot possibly do that.

(*Sir Frederick Banbury.*) If you give us leave to be present at an inspection we will answer the question.

19578. (*To Mr. Scott.*) Do you think it possible for an inspector to be present at every operation done in a laboratory?—I think at a great many they might be. At present it is not possible.

19579. Have you any idea of the number of laboratories that there are in this country?—I have a list of them here. I have not counted them.

19580. Do not you think it would be a great drawback to science if a man's work had to be stopped in order that a communication might be made with the Inspector that he was to be present at such and such a time, and then the Inspector said he could not be present at such and such a time?—Then I think they should arrange between them that the Inspector should be present, as it is the law that he should.

19581. How many inspectors do you propose there should be for the Kingdom?—It is impossible to say.

19582. What sort of remuneration would you give these inspectors?—Whatever they are satisfied with

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now I daresay others would be satisfied with. I daresay it would be very costly, but it would be money well expended, would it not?

19583. What grounds have you for thinking that an inspector would be more likely to tell the truth or to stop what should not go on than the gentlemen who at present are in charge of these laboratories and are responsible for what goes on in them?—I have the greatest admiration for your profession, I assure you——

19584. I beg your pardon, you are under a misapprehension. A large number of the heads of these laboratories in which experimentation on animals goes on are not members of my profession, or not necessarily members of my profession?—I thought you would hardly throw that back at me.

19585. But as a matter of fact some of the most distinguished experimenters on animals are not members of the medical profession, but men of science?—Yes, men of science certainly.

19586. What reason is there for thinking that the Inspector would be able to keep better—I will say order—in the laboratories than these gentlemen themselves?—It is the law that they should be inspected, and therefore I think that it should be carried out; that is all.

19587. Besides the experiments in laboratories, are you aware what other points the Inspectors direct their attention to?—No; I am not.

19588. You do not know that it is part of the duties of the Inspectors, which they always do, to examine into the condition of the laboratories?—I understood that they were licensed after being examined.

19589. The manner in which the animals are kept, both before and after experimentation?—Yes.

19590. You were aware of that?—Yes; I knew it.

19591. Although visits to laboratories are not very frequent now, do you consider that they are sufficiently frequent to enable the Inspector to form an idea whether every proper care is taken of the animals or not?—I should think not. I do not think that there are enough inspectors to be able to do that.

19592. How frequently do you think visits to a laboratory should be made by an inspector to see that the animals are properly taken care of?—I should think almost weekly—every day almost.

19593. Daily and weekly?—Yes; something of that sort. I do not see how a man can say that a thing is properly done unless he goes frequently to see that it is so.

19594. You do not have your dairies daily or weekly inspected?—No; but I inspected my own stable every day. I did not trust them, however good the men might be.

19595. Therefore it would be almost necessary to have an inspector for each laboratory?—I do not think so. I should think they would have districts, could they not? A man could pay a surprise visit on Monday to one, and on Wednesday to another, and so on. We do not think there are sufficient inspectors. If there are to be inspectors there should be a certain number of them.

19596. I wished to gather from you some idea of what number you thought would be necessary?—A sufficient number to look after these laboratories and other places and keep them up to the mark.

19597. If a laboratory has to be visited daily a man could only visit one or two if they happened to be near to one another on the same day?—More than that I should have thought.

19598. How many do you think?—I should imagine that if they are close to one another it would not be a difficult thing for a man to visit many in a day to see that the laboratory is properly conducted, and that the animals are being well looked after.

19599. And to be present at every operation?—Yes, he would have to make those arrangements to fill up his time. I mean to say he would have to arrange his time.

19600. Therefore, taking such a small place as London no operations could be done on several days in the week in some of the laboratories because he would be visiting others?—But "a small place like London" the laboratories, I should assume, and not the siz of the place.

19601. I mean small as regards its area for travel ling?—Yes.

19602. (*Sir William Collins.*) Your society gav evidence before the last Royal Commission in 1876 did it not?—Yes.

19603. Through Mr. Colam?—Yes, the secretary.

19604. And they put in a letter addressed by com mand of her late Majesty the Queen to the presiden of the society?—Yes, to Lord Harrowby.

19605. And there was a good deal of documentar evidence put in by Mr. Colam purporting to show th existence of painful experiments?—I believe so. I wa not there then.

19606. In the report of the Royal Commission o 1876 there is an allusion to your society. They stat "We were reminded by the secretary that the Roya Society for the Prevention of Cruelty to Animals is no the society established for the total abolition of ex periments. The Royal Society for the Prevention o Cruelty to Animals has prepared its Bill upon th supposition that experiments of a nature to cause pai are justifiable if they are performed when the anima has first been rendered wholly insensible to pain an is destroyed before the effect of the anæsthetic ceases. Is that still the position of your society?—Yes.

19607. Then another allusion is made to you society in the report of the previous Commission. The say, "The secretary of the Society for the Preventio of Cruelty to Animals when asked whether the genera tendency of the scientific world in this country is a variance with humanity, says he believes it to be ver different indeed from that of foreign physiologists and while giving it as the opinion of the society tha experiments are performed which are in their natur beyond any legitimate province of science, and tha the pain which they inflict is pain which it is n justifiable to inflict even for the scientific object i view, he readily acknowledges that he does not kno a single case of wanton cruelty, and that in gener the English physiologists have used anæsthetics whe they think they can do so with safety to the exper ment." Is the experience of your society still in th direction?—Personally, I say so certainly.

19608. You have told us of one prosecution institute by your society successfully?—Yes.

19609. Is that the only prosecution that they hav instituted?—That is the only one we have ever had.

19610. Has your society any reason to think th there is considerable suffering to animals under th operation of the existing Act?—It has been suggeste to us that there is.

19611. The Commission was told by Dr. Starling i answer to Question 3451: "Though I have been en gaged in the experimental pursuit of physiology fo the last 17 years, on no occasion have I ever seen pai inflicted in any experiment on a dog or cat, or, might add, a rabbit, in a physiological laboratory i this country, and my testimony would be borne out b that any one engaged in experimental work in th country." Have you any remark to make with regar to that?—I should think that is very strong eviden in favour of what you have read of Mr. Colam's ev dence; it is corroborated very much.

19612. We were told by Dr. Thane, the Inspecto under the Act, in answer to Question 457: "On th other hand it is certain that in some cases of th group, that is, experiments performed under Certificat A, the infection or injection is followed by great pai and suffering. I may mention the injection of tetan toxin and the infection with plague; also the insertio of certain drugs." Have you any remark to make i regard to that?—No, that is the experience of a su geon, is it?

19613. It is the Inspector?—I beg your pardon.

19614. What I want to gather is what would be th wish of your society in regard to such cases in whic after injection or infection there is great pain an suffering.

(*Sir Frederick Banbury.*) We think the animal ough to be destroyed.

19615. Then a question has been put as to th difficulty of determining in which cases of injection means so many inspectors according to the number infection considerable suffering may result. Do I co rectly understand your suggestion to be that in a cases in which such result may happen the animal should be kept constantly under the eye of the Inspector?—If there is any risk of serious suffering, yes, I think so.

19616. That would almost necessitate the residence of an Inspector on licensed premises, would it not?—No, I do not think so, because the Inspector would know that a certain operation was going to take place and the operator would know when the result would begin to show itself. When the result began to show itself then I think the Inspector should see, if there is pain, that the animal should be destroyed.

19617. But I am afraid that in the case of some of these injections or infections the date at which the operation of the particular injection begins to take effect may be doubtful even in the mind of the experimenter. What I am anxious to know is what suggestion the society would make with a view to secure as far as possible that no case of suffering should result in animals subjected to injection or infection?—I do not think that the society could take upon itself to suggest a law which should never make mistakes. It is one of those things that we should have to accept as inevitable. I think the opinion of the society is that if vivisection takes place pain must sometimes result. The wish of the society is to limit that pain as far as possible.

19618. I think I gathered that you stated that the society would wish the number of licensed places to be as few as possible?—I will not say as few as possible.

19619. Or reduced?—Yes, and grouped together more.

19620. Was that with a view to secure more frequent inspection?—Yes, certainly.

19621. Do you think that valuable results have attended the operations of the Inspectors in the past as regards preventing or detecting breaches of the Act?—I do not think that any result has happened at all. We cannot give an opinion upon that because so far as we know many of the Inspectors do not really ask what has taken place; they take the opinion of the operator. It may be all right, but it may not be. You have already instanced an Inspector who says that pain has taken place. Previously to that you instanced one eminent man of science (I believe not a member of the medical profession) who says that no pain ever does take place. I cannot say which is right. I have my own opinion.

19622. We have had evidence from witnesses in both directions, some setting great store upon inspection as being valuable, and others regarding the present inspections as superfluous. I was rather anxious to hear from either of you gentlemen what view you take of the present inspection so far as it goes. Is it superfluous or is it valuable?—I think it is better than nothing.

19623. Then I should like to know, if possible, what opinion the society has in regard to a question that has also been raised here by witnesses as to the effect of repealing the Act of 1876. Do you think that animals would be in a better position *qua* scientific experiment if the Act were repealed and they were left to the operation of the ordinary law, than if the present Act were maintained or modified or amended?—That is a legal question. I do not think I could answer it as to what power a society like our own would have under the ordinary Prevention of Cruelty to Animals Act. (*Mr. Scott.*) I am sorry to say that I cannot answer it.

19624. (To *Sir Frederick Banbury.*) I understand that you introduced a Bill into the House which had the approval of the society?—Yes.

19625. In what direction did that Bill go?—Unfortunately I live in the country, and have not got the Bill with me, but it more or less was Mr. Stephen Coleridge's Bill altered in a way that I thought would be more acceptable to the House of Commons. My recollection goes that it left out the clause with regard to the certificate for humanity, because I do not see how that is practicable; or the object of the Bill was to prevent experiments being made for the purposes of illustrating a lecture or to demonstrate facts which were already well known; experiments could only be made in the presence of the Inspector and in order to ascertain something which had not already been ascertained. That was, roughly speaking, the object of the Bill.

19626. Did it prohibit experiment on animals which would be painful?—No. All that it did was to say that they must be under anæsthetics and the animals must be destroyed, but it did not prohibit experiment, provided that the animal was under anæsthetics and was destroyed before it recovered, and provided that it was shown that it was in the interest of humanity to discover something which had not previously been known.

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19627. Would it have prohibited an injection experiment which might have resulted in painful disease?—I do not think it would; but the result would have been that when a painful result ensued, the animal would have been destroyed.

19628. (*Sir John McFadyean.*) (To *Mr. Scott.*) You have already explained that this statement of evidence has been drawn up by, or has been before, your council?—It was drawn up by the council.

19629. Does it contain anything that you are not prepared to subscribe to yourself?—I am only here as representing the council, you know.

19630. Does it contain anything that you are not prepared to subscribe to yourself?—I do not think that it does. (*Sir Frederick Banbury.*) I think I subscribe to everything.

19631. (To *Mr. Scott.*) I thought, very likely, you did subscribe to it. Your society was founded for the Prevention of Cruelty to Animals. Would you mind telling us what is the society's definition of cruelty?—"Cruelly ill-treating, abusing or torturing animals."

19632. Take cruelty by itself without the torturing. What do you understand by cruelty?—I see a good many forms of it. I think cruelty would be to over-bit a horse, if you want an extreme case.

19633. I can get what I want better in this way. The term "cruelty" is not used as if it were synonymous with the expression "pain," is it—for preventing pain to animals?—It would be certainly for preventing pain.

19634. Do you think you would quite express the views of your society if you were to substitute "pain" for "cruelty?"—I think so. I think to starve an animal would be painful. There are so many forms of cruelty that I do not wish to commit myself to the fact that it must necessarily cause pain. We call ourselves the Society for the Prevention of Cruelty to Animals, and I have no wish to alter the word at all.

19635. But the point is really this: Do you think that your society recognise that it is ever justifiable to cause pain to an animal for some other purpose than that individual animal's own benefit?—In all painful experiments we consider that the animal ought to be protected by being under anæsthetics.

19636. That is to say, it should be prevented from suffering any pain?—Yes.

19637. That seems to be an affirmative answer to my question?—Yes, pain should be prevented.

19638. Then your society denies the right of man to cause pain to an animal except for that individual animal's own good?—Oh, no! I do not say that, under an anæsthetic, we say.

19639. But that would not cause pain?—It might.

19640. You mean that it must be an efficient administration of anæsthetics?—Yes.

19641. In which case there would be no pain?—No. (*Sir Frederick Banbury.*) It depends upon the amount of the pain.

19642. If you do not mind, I will deal with your Chairman in the first instance. (To *Mr. Scott.*) Do you think it would perhaps better express the objects of your society if, instead of substituting the word pain for cruelty, that is to say, "for the prevention of pain to animals," you were to put in "for the prevention of unnecessary pain." Is that what you mean by cruelty—unnecessary pain?—That would answer the purpose very well. It is unnecessary pain that we desire to prevent. A man spurs a horse unnecessarily very often to get it over a bit of timber, and so on.

19643. In its operations does your society find it necessary to discriminate between necessary and unnecessary pain?—No, but we do discriminate between a man ignorant very often of what he is doing, and a man who is not ignorant, but does it of set purpose and knows what he is doing. You may have an ignorant man inflicting pain from ignorance.

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19644. But all pain inflicted by an individual who was conscious that what he did would cause pain you would regard as cruelty?—Yes.

19645. But I thought you had already told us that your society would not proceed against a man who rode a horse to death on the chance of saving a human life?—It would depend altogether upon the case. We should consider that matter. If the circumstances were such it would be a matter of very grave consideration whether we should prosecute in such a case. There might be a difference of opinion.

19646. But if the case came before your council, you think it would really cause them some difficulty to decide whether a man who had ridden furiously to save a human life, and had saved it, but had caused the horse's death, ought to be prosecuted?—It is a question whether the same object might not have been attained without causing the pain.

19647. I will ask you to assume, please, that there was no evidence to show that the human life could have been saved in any other way?—That means to say that the horse could not be galloped fast enough without spurs, do you mean?

19648. No, I mean that if this particular horse had not been ridden to death the human life would have been lost?—I say that if we knew that the man knew that he was going to kill that horse we should prosecute him.

19649. Supposing that there were a possibility that he might, by riding the horse furiously, save a human life without causing the horse's death, you will admit that he would cause the horse very great distress and pain?—Yes.

19650. Would your society prosecute then?—If it was unnecessary pain—for instance, if the man rode the horse with spurs and could get the same pace out of the horse without spurs.

19651. It is not a question of spurs at all; it is a question of causing pain?—But if the man could have attained the object without killing or ill-treating the horse we should then prosecute him, because the same object could have been attained without cruelty.

19652. But I ask you to assume that the object could not have been attained in any other way?—Then I should put it before the council.

19653. You think possibly that the council might consider his action justifiable?—Possibly.

19654. On the principle that the life of a man is more valuable than the life of a horse?—Yes, I agree "of more value than many sparrows." I believe that certainly.

19655. Can you tell me how you discriminate between a man's action in that case and the action of a person engaged in research who causes a certain amount of pain to an animal in the confident belief that he can thereby gain knowledge that will afterwards be useful for the saving of human life and alleviating human suffering, and, also, perhaps, alleviating suffering among animals too. The case is this: you justify the sacrificing of the horse's life to save a human being in the particular circumstances that I put to you?—I did not quite agree to that.

19656. Then I will put the question again. I asked how do you discriminate between that and another case in which an experimenter would sacrifice a horse's life to extend knowledge which might be useful, and which he believed would be serviceable for saving human life. I asked you to discriminate between the two, and you said that possibly your council would not proceed against the individual who rode a horse to death to save a human life, but I understand from you that they would object to any pain being caused to a horse to obtain knowledge that would be useful for saving human life?—That is so.

19657. What is the principle that enables you to discriminate between the two cases?—One is certainly a chance and the other is a fact. We say: "The society if of the opinion that all severely painful experiments should be carried out while the animal in question is completely under the influence of an anæsthetic"; and if in the case in point the animal is under an anæsthetic we say that we should agree to it, but that it should be destroyed before the effect of the anæsthetic has been removed.

19657A. That is no answer to my question at all. The hypothetical case that I put to you was one in which the animal would be caused a certain amount of pain in an experiment?—We object to that.

19658. When you advise us that there ought to be inspection in all cases in which the experiment is of a severely painful character, does that cover also those cases in which the licensee has to have the animal thoroughly under the influence of an anæsthetic all the time. Do you call that a severely painful experiment?—I should imagine it would be.

16959. Although the animal is under an anæsthetic all the time?—Yes. That is the protection which we think the animal ought to have.

19660. But you only mean that it would be severely painful if the animal were not anæsthetised?—Yes, that is what I understood you to mean.

19661. What is the purpose of having an inspector present; is it to judge as to whether the anæsthesia is efficient or not?—I assume that that is one of the reasons.

19662. What other reason would there be for his presence?—Because by law he has to be there to see that the thing is properly carried out.

19663. You think that under the existing law the Inspector must be present?—Yes, that I believe to be the law.

19664. Have you read the Act?—No.

19665. Do you think you could find any passage in it which lays that down?—We look upon it in this light. I have already said that we think the Inspector refers to the returns and not to the experiments—that the Inspector guarantees the returns, but that he has never seen the experiments.

19666. I do not think we have ever had it suggested that the Act lays it down that the Inspector is to be present at every experiment or even at every painful experiment?—We consider that all painful experiments should be prohibited except in the presence of an inspector.

19667. Does that mean that you think that that is the state of the existing law, or that the law should be amended in that way?—If it is not so we think it ought to be so.

19668. I ask you your view?—You see how we refer to that: "It may not be possible that every experiment should be performed in the presence of an inspector, but all those should be which cause pain, either in their initial stages or afterwards." That is the question you asked me, I think?

19669. No. I asked you to give us the reasons why you thought it necessary to have an inspector present. Is there any reason beyond his being there to judge of the efficiency of the anæsthesia?—That is what we mean.

19670. Was there any other reason?—Yes, to see that the experiment was properly carried out, and that it was under the particular certificate that is granted.

19671. You have expressed already a high admiration for medical men in general?—Yes, I have, and have reason to believe so.

19672. In spite of Sir William Church's partial disclaimer I am afraid that we must admit that the great majority of those who experiment are members of the medical profession?—I thought so.

19673. Have you any reason to think that at the present time any one of the licensed experimenters would willingly violate the law. For instance, do you think that there is any licensed experimenter who would deliberately or even carelessly neglect to administer an anæsthetic in sufficient quantity in those cases in which the certificate prescribes the use of anæsthetics?—I do not know—it is quite possible.

19674. I did not ask you whether it is possible; I asked you whether you think it is at all likely?—I repeat what I have already said. I believe the medical profession to be very highly honourable men and men who from a high sense of honour would do the best they could to carry out what they undertook to do. But we know that in all professions there are men who are not what we wish them to be. We cannot help that.

19675. Have you read the Act?—No, I have not.

19676. Are you aware that the application for a licence has to be accompanied by a recommendation?—Yes, I know that. I have it here.

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19677. Does not that appear to you to be a sufficient guarantee that the individual will be at least an average member of the profession?—Yes, certainly, I put him a bit above that.

19678. Then unless you have some very strong misgiving as to whether the Act is violated or not in such a way as to cause cruelty to animals, does it appear worth while to go to the great expense that will be involved in having every experiment witnessed?—We classify them here by saying that all "severely painful experiments should only be performed in a particular place or places so as to facilitate inspection; and all painful experiments should be prohibited except in the presence of an inspector." That is our view.

19679. I suggest, though you may not have calculated it out, that that would be very expensive?—Yes.

19680. And I further suggest that unless you have a very strong misgiving as to the way in which experimentation is carried on at the present time it does not appear to be worth while to incur that expense?—That has always been our line, I think, ever since the Act passed. I think we have always taken that view that there were not enough inspectors.

19681. (*Sir Mackenzie Chalmers.*) You told us that your society had had one prosecution?—Yes.

19682. That was about 1877?—I think it was in 1876, soon after the Act came in.

19683. And it was perfectly effective?—Yes.

19684. It stopped once and for all any attempt at a public demonstration?—I think it did; we have never had another case.

19685. That was a successful piece of work?—Yes.

19686. I take it that the object of your society generally is to prevent cruelty and not pain—cruelty to animals, that is to say, pain which is not justifiably caused?—Yes.

19687. You do not disapprove of sport, do you?—Personally, I do not.

19688. I mean that necessarily there is a great amount of pain caused to animals in sport which might be saved; there are a great number of wounded animals?—Yes, I am afraid it must be so always.

19689. But that is not cruelty; the pain is not caused intentionally?—No.

19690. The intention is to kill; therefore it is outside the scope of your society?—Yes. If a man overrode a horse out hunting that would be cruelty; or sometimes we have had cases of cruelty brought to our notice with carted deer.

19691. But it would not be cruelty if a man overrode a fox?—I wish we could take the fellow up for it, but you cannot.

19692. (*Chairman.*) You have prosecuted for over-spurring in a race?—Yes, we have.

19693. (*Sir Mackenzie Chalmers.*) And for over-flogging in a race?—Yes, for over-flogging too.

19694. I suppose you have never considered whether the amount of pain that is caused in the whole of the laboratories in this Kingdom would be equal to the pain caused on one day's rabbit shooting?—No, I have never considered it in that light.

19695. Your society again would not, I imagine, touch commercial operations, would it? For instance, every horse, as we known in London, is gelded?—That we have never taken up, of course.

19696. That is for commercial purposes?—Yes, it has never been taken up. (*Sir Frederick Banbury.*) We have taken up docking and we should take up gelding if it was done by an unskilful person and done without the necessary precautions.

19697. (To *Mr. Scott.*) As a matter of fact it is very seldom done under anæsthetics, is it. Perhaps you have not gone into it?—I have never had one done.

19698. It would be outside the scope of the society?—Yes. Of course it is mutilation in a sense, but it is considered a necessity.

19699. Under the Vivisection Act no such operation would be allowed to be done without anæsthetics and under the aseptic condition?—I believe as a rule it is done very young with a horse, and in later life when it has been done I believe it is done under chloroform.

19700. We have had some evidence on the subject, but I need not trouble you with it if you have not considered it?—I once had a stallion I used as a hack, but it is very rarely this is done.

19701. I want to get at your ideas about inspection. I quite understand that the public would feel more confidence if there was more inspection?—That is our view.

19702. But I want rather to try and see what would satisfy the bulk of your society, because there are some difficulties to be considered. Last year 20,795 inoculations were made on mice for the Cancer Research Fund, and I want you to take that as an example. The object of the inoculation is to produce cancer for the purpose of watching it and seeing whether any alleviation can be obtained for a most horrible human malady. Do you suggest that the Inspector should be constantly watching those mice as the tumour may develop?—There are difficulties of detail like that, I confess. I see what your point is, of course.

19703. 20,000 experiments are half the experiments performed in the whole year. I will give you the exact figure: 20,795 inoculations on mice out of 21,082 inoculations made for the Cancer Research Fund and 1,200 on other small animals. You have considered that?—I have not considered that the Inspector should watch every one of those. I do not think it would be reasonable.

19704. Within the principle that you have laid down the initial operation would be painless as being inoculation, but afterwards more or less painful symptoms would develop in those mice. Cancer is sometimes painless and sometimes not?—Yes.

19705. Then there is another point which shows the difficulty. 2,144 experiments were to produce tubercle, and tubercle produced in an animal may or may not give rise to a considerable amount of pain. Would there not be a difficulty there if you laid down your principle?—There would, of course, be considerable difficulty. I quite admit all that.

19706. We all agree, I think, that some extra inspection would give extra confidence?—That is really what we want to secure—the confidence of the public.

19707. But you agree that there are difficulties if you state a proposition in an Act of Parliament which would be unworkable?—Yes, I quite see all these difficulties, and I am very glad that you have pointed them out.

19708. Then 6,000 experiments were performed for Government Departments, County Councils, Municipal Corporations, and other public authorities. Those would be chiefly in testing food. There again there would be somewhat the same difficulty?—No doubt.

19709. Now I should like to ask you about what most people call vivisection—namely, cutting experiments. As I understand your evidence, you have no objection to experiments done under a licence alone. You know the difference between a licence and a certificate?—I think so.

19710. May I put it to you, and then we shall see how far we agree. Under a licence alone, which is granted by the Secretary of State, an animal that is experimented on must be put under an anæsthetic, and any operation may be done upon it, but the animal must never recover from the anæsthetic. The animal must be put under an anæsthetic, and there is no limit to the time, provided that the animal never recovers from the anæsthesia until death puts an end to it. Have you any suggestion to make as to that?—That is just what we say.

19711. That is, under a licence alone?—Yes.

19712. Then you know if any departure is to be made from that a special certificate, signed by two eminent authorities, has to be obtained?—Yes.

19713. Under Certificate B, if any cutting experiment is to be performed more than the prick of a hypodermic needle, the animal has to be under anæsthetics the whole time, but then the animal is allowed to recover, and may be kept alive until the object of the experiment is obtained. Have you any objection to that?—That is where we do object. We say that the animal should be destroyed before the effect of the anæsthetic has been removed.

19714. Then that would render impossible any physiological experiment, would it not? Have you any comment to make upon what the Inspector says in his last report, where he is referring to Certificate

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B: "The operations are required to be performed antiseptically, so that the healing of the wounds shall, as far as possible, take place without pain. If the antiseptic precautions fail, and suppuration occurs, the animal is required to be killed." Does that go far enough to meet you?—(*Sir Frederick Banbury.*) If that is correct it would more or less meet our views.

19715. That is the report for last year?—But everybody makes mistakes, even doctors and inspectors sometimes. Our only desire as a society is that if pain ensues the animal should be destroyed, and we do not for a moment say that it is possible to make a state of affairs under which pain can always be stopped. Accidents will occur we all know; but, so far as possible, pain should be stopped.

19716. I wanted rather to separate cutting operations under Certificate B from mere inoculations under Certificate A, and to ascertain the opinion of the society on these two points?—Certificate A is what we desire, so far as we can have it, but Certificate B we object to unless it is absolutely modified in the direction laid down by us. If what you have read is correct, and there is no pain, then we do not object to it.

19717. I am afraid we are not quite understanding each other. So far as I know, great pain may be caused under Certificate A?—But the animal is under anæsthetics, and is destroyed.

19718. No; the quotation from Professor Thane, that Sir William Collins read, referred to Certificate A, and Certificate A deals with inoculation of disease. For instance, very recently what appeared to be very necessary experiments were carried out with plague—that is to say, animals were inoculated with plague for the purpose of testing the mode of transmission of plague. Do you object to that?—If the animal is allowed, after the inoculation has taken place, to remain alive and suffer pain, if that is the question.

19719. It may be necessary for the disease to develop to a certain extent, or death would come on very quickly. I am asking whether you would object to that?—I think we should object to the animal being kept alive when the operator knew that it was suffering pain.

19720. Even though thousands of animals are dying all round from the same disease?—Yes, I think so.

19721. To take another instance. Supposing that some work is undertaken with reference to finding a cure for distemper, which is a very distressing disease to animals, it may be necessary to inoculate an animal with distemper and to try various modes of treatment in order to find out a successful mode of treatment. Must not that animal necessarily be kept alive until the treatment can be applied, and the treatment may fail?—(*Mr. Scott.*) Yes; in the same way that we keep a dog alive, supposing that it is mad, and watch the results. (*Sir Frederick Banbury.*) I have a considerable knowledge of distemper in dogs, and I should myself have said that distemper is not painful. The animal gets very weak, and I have had, unfortunately, dogs die of distemper, but I do not think there is actually any pain. They have been weak and they will not eat their food, and they fall away. I think those are the chief symptoms.

19722. But when you come to legislate in the sense that you propose you admit that there is a good deal of difficulty?—There is no Act of Parliament that has ever been passed—I think your Chairman will bear me out—through which you cannot drive a coach and four. A perfect Act of Parliament will never be passed.

19723. I was rather putting it the other way—that you would pass an Act which would hit all the cases that you did not mean to hit?—There are means of getting round that, I think.

19724. (*To Mr. Scott.*) I was not quite sure from your evidence whether you wanted this present Act of 1876 amended, or whether you wanted further inspection to enforce it. Do you think the Act requires any amendment?—I think personally what we say in our statement: "The Society has, ever since the passing of the Cruelty to Animals Act, 1876, repeatedly pointed out that the supervision of experiments carried on under licences granted under that Act is totally inadequate"—that is our position. We want more inspectors.

19725. Are you aware that the Inspectors continually see animals under experiment, though not the initial experiment?—I can quite believe it.

19726. Have you ever yourself been over any of these laboratories?—No, I have not. I have studied the matter a little in a very small way. I was once a student at the college at Cirencester, and I read privately with a veterinary surgeon, so that I have some little knowledge, but I should not have presumed to mention it except that I thought you asked me whether I had ever had anything to do with it.

19727. Have you ever asked to see over any of these laboratories?—No, I have never done so.

19728. Do not you think it would be a good thing to do so?—We would go with pleasure if some of you gentlemen would do us the honour to ask us.

19729. We have had several witnesses here from laboratories who have said that if any person with a sense of real responsibility and not from mere curiosity desired it they would be perfectly willing to show them round?—We should think is a great privilege to be allowed to go.

19730. But, as a matter of fact, up to date, you have not yourselves asked to see any of these experiments?—No, neither of us.

19731. And you have no specific cases about which your society knows anything, or enough to bring to the attention of the Commission?—I do not think so.

19732. I do not want to go into the case of the brown dog, but I want to put this question to you. Supposing that an animal was experimented upon, and then was allowed to recover for the purpose of some observation—I am putting a hypothetical case—you know that under the law, as it stands, at the end of the observation that animal must be destroyed painlessly?—Yes.

19733. Since that animal has to be destroyed, do you see any objection, after the anæsthetic has been administered to that animal, to its being again the subject of experiment for demonstration purposes. It has to be destroyed in the case I am putting?—(*Sir Frederick Banbury.*) While it is still under the anæsthetic?

19734. Certainly?—That is a very difficult question to answer, because my view (I do not know whether Mr. Scott agrees with me) is that if one were certain that it is possible to keep an animal under an anæsthetic, and provided that the experiment is carried out at once, our point is met. (*Mr. Scott.*) I should say so. (*Sir Frederick Banbury.*) But then the question arises whether it is possible.

19735. That is outside my question. I am assuming that the animal can be kept under an anæsthetic, and does not recover. Do you see any objection?—I do not think so. Our objection is entirely to pain. (*Mr. Scott.*) And it should be destroyed before the effect of the anæsthetic has been removed.

19736. Even though an incidental effect is that it has two operations; that is to say, that it saves two animals being operated upon, instead of one?—(*Sir Frederick Banbury.*) As long as pain does not ensue is all I can say.

19737. (*Dr. Wilson.*) The question, I understand, is this. The animal is operated on for a certain physiological experiment, and is allowed to recover, say, after taking out a portion of the kidney or the pancreas; then after a time, when the results of the experiment have been sufficiently studied, that animal has to be destroyed; it can be destroyed, of course, painlessly, by administering chloroform. Do you think that that animal should be handed over for another experiment?—No, decidedly not. I did not understand the question. I thought that the case put to me was that of an animal which is brought into the room and put under anæsthetics, an operation takes place, and then another operation takes place immediately. To that I should not object, because I presume that pain does not ensue; but I do object to an animal being operated upon and being allowed to recover, and then being brought forward again and operated upon. I think we both agree as to that. (*Mr. Scott.*) Yes.

19738. (*Sir Mackenzie Chalmers.*) I am not quite sure now whether we have got the point. The animal has been operated upon, and is allowed to recover. That animal has to be killed, and has to be killed painlessly. Do you object to that animal being put under chloroform before death occurs, and another operation being performed upon it?—Yes, certainly.

19739. You would rather that a fresh animal was sacrificed?—Certainly.

19740. On what ground, if the animal suffers no pain?—On this ground. My contention is that the animal ought to have been killed after the first operation, and that if that sort of thing were allowed as it is difficult always to get animals, the tendency would be to keep one wretched animal for many months in that sort of state, and your argument would always hold good, because you have broken the first contention—that after an operation the animal should be killed, and you might multiply it indefinitely.

19741. Then your objection is not to an animal being experimented upon when it has to be killed, but to the animal being kept alive after the first operation?—I object to the animal being operated upon if pain ensues, except under anæsthetics. But I object to the animal being experimented on and kept alive, and then experimented on again and kept alive again, and experimented on again, and so on.

19742. That is not the question I put to you?—But it might result.

19743. (*Mr. Ram.*) I want to ask you a few questions, if I may, because I am very anxious to get the views of your society, which we value greatly on certain points. First of all, with regard to your definition of cruelty under your Act, I want to clear up that point, if I can. I take it that by cruelty you mean—you will tell me whether I am right—the infliction of unnecessary pain?—Yes.

19744. Not necessarily the infliction of any pain?—No, the infliction of unnecessary pain. (*Sir Frederick Banbury.*) I do not admit that.

19745. Then I must address myself to you gentlemen individually, and I will take Mr. Scott first, if I may. (*To Mr. Scott.*) I put the question, do you by cruelty mean the infliction of any pain in any circumstances on any animal, or do you limit it to unnecessary pain?—The wording is "Cruelly ill-treating, abusing, or torturing animals."

19746. I want to get at a definition of cruelty, if I can. Let me put an instance to you. A cab horse is drawing a cab, it may be a lazy horse, or it may not, at any rate, that horse is every now and again struck by the cabman with a whip, not excessively, but probably every time it is struck the horse feels some pain, and without that pain it would not go at all. Then there is a second case, in which the horse is made to draw an excessive load, or is whipped with barbarity and cruelty to make it do that which it is very painful for it to do. Do you view those two cases as coming exactly under the same definition of cruelty, or do you draw any distinction between them?—I draw a distinction between them.

19747. The first case would not, in your opinion, be a case of cruelty?—No.

19748. The first case is simply whipping the horse, perhaps a lazy horse, enough to make it do a task which is within its power?—Yes.

19749. You would not call that cruelty?—No.

19750. That is, however, the infliction of pain upon the animal?—But it is necessary pain, like when one used to get the cane at school.

19751. Yes, we had necessary and very beneficial pain?—Exactly.

19752. Then I take it that you at any rate will agree with the definition that I suggested of cruelty, namely, the infliction of unnecessary pain?—I think so.

19753. (*To Sir Frederick Banbury.*) Might I ask why you do not agree with that definition?—Because it depends upon the extent of the cruelty.

19754. Shall we say rather the extent of pain?—Yes. I beg your pardon. I should not admit that a cabman had a right if he has a lazy horse to thrash it severely in order to make it go. If you ride a horse at a fence sometimes you may get a horse that will not jump, and I think you are justified in inflicting a certain amount of pain by using spurs, but I do not think that you have a right to have five or six fellows with hunting crops, slashing and hitting at the horse, although the result that you may desire may ensue. I maintain that you must do it by gentle means.

19755. Perhaps I may get you to take my view if I suggest the addition of one word to my definition of cruelty and say that it means the infliction of unnecessary and excessive pain?—I should say excessive pain and leave out the "unnecessary."

19756. (*To Mr. Scott.*) Has this Act of 1876, which we are dealing with now, ever been pronounced upon as an Act by your society?—We have it in that book of ours as one of the Prevention of Cruelty to Animals Acts. (*Sir Frederick Banbury.*) You mean, have we ever given an opinion upon it?

19757. Yes?—I do not think that we have.

19578. (*To Mr. Scott.*) I understand, I think, that neither of you gentlemen has read the Act?—Only portions of it. I was reading our book only this morning. (*Sir Frederick Banbury.*) I have read the Act, but it is some little time since I read it, and I could not stand a cross-examination upon it if you have the Act there.

19759. I am not going to attempt to administer one. (*To Mr. Scott.*) I think I gathered from your answer to Sir Mackenzie Chalmers that you would not abolish the present Act?—I do not think so.

19760. Your society would not wish to abolish it?—I do not think that they have ever considered it. I do not think they have any reason for wishing it.

19761. You have suggested that the Act should be amended by increasing the number and perhaps making some new regulation with regard to the Inspectors?—Yes.

19762. Can you help the Commission at all by making any other suggestion as to the way in which, in the opinion of your society, the Act might be amended, or may I take it that that is the only one matter in which you think it requires amendment?—That is the only one that occurs to me, at all events.

19763. (*To Sir Frederick Banbury.*) I think you began your evidence by saying that throughout the claim of your society has been that animals should be given by man the same protection as regards the infliction of pain as man asks for himself?—Yes, I said so.

19764. That is to say with regard to anæsthetics and with regard to aseptic treatment in case of recovery from wounds?—Yes.

19765. Do you know that that is the protection which is now given to animals under the Act?—Yes, the Act provides that.

19766. With regard to anæsthetics, the animals have the pull over mankind in this respect, have they not, that whatever the subsequent pain of an operation may be to a man he can only be given such an amount of anæsthesia as implies his subsequent recovery, though it may be accompanied by much suffering?—I suppose so.

19767. In all cases of operations upon men it is intended that they should recover, however much they may suffer subsequently?—Yes.

19768. With regard to experiments on animals in a vast number of cases, at any rate where the experiment is a painful one, the animals are under an anæsthetic from which they never recover. To the extent to which that takes place your society, I gather, has no objection to such experiments?—No.

19769. Again, with regard to the advantage that an animal would have, if an animal is operated on under anæsthetics and is allowed to recover and is treated aseptically or antiseptically, should it be found that the treatment is insufficient or that it does not effect its object—in other words, if the animal suffers, that animal can be, and in the vast majority of cases would be, destroyed at once?—Yes.

19770. That is not so with regard to man. Man, of course, when he recovers from the effects of the anæsthetic must suffer pain, whatever pain it is necessary he should suffer in the course of recovery?—Yes.

19771. Now, with regard to inspection, I am anxious to have your views. So far as the animal is operated upon and never allowed to recover you have already told me that your society would not object, and the presence of the Inspector has no particular effect one way or the other there; it ensures that the animal is under anæsthetics and is destroyed before it recovers?

19772. What I want to put to you as a matter which strikes me as being much more important is this:

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Would it meet the views of your society if, in cases where the animals are allowed to recover, there should be an enactment that those animals should be visited by the Inspector, say, shortly after the operation, and again at a short period, in order to see whether those animals that were allowed to come out from a state of anæsthesia were in a state of suffering or not?—I do not think it would. That is a question to which I would rather not give an answer at such short notice, but I do not think it would. I can only repeat our statement, which I think is clear, that when pain ensues then the animal must be destroyed.

19773. I am sure you recognise that I am anxious to have your assistance so that we may get something workable?—Certainly.

19774. Sir Mackenzie Chalmers has pointed out to you, and I think you both accepted it as I should expect, the difficulty of acting stiffly with regard to the Inspector seeing every experiment, viz., that you will either stop everything or you must have an army of inspectors; therefore, I want to see whether you can help us to make it workable. Would it *pro tanto* meet the views of your society if it were enacted that all animals which by reason of coming out of anæsthesia may be liable to suffering, should be inspected by the Inspector at a certain period shortly after recovery and again after a short interval?—(*Mr. Scott.*) It would be a step decidedly. I should not like to commit the society with regard to that; it is a very important question. It seems a very reasonable suggestion indeed, but, speaking for the council, I do not think I could commit them without further consideration.

19775. I think there is only one other matter that I need trouble you with. I am not going into the case of the brown dog, but are you aware that the brown dog was the subject of the action of *Bayliss v. Coleridge*?—Yes.

19776. Have you read the report of that case?—Yes.

19777. And you know what was stated in that case. It was tried before the Lord Chief Justice and a special jury at great length and the jury found for the plaintiff for £2,000?—Yes.

19778. (*To Sir Frederick Banbury.*) In your statement, you say that all severely painful experiments should only be performed in a particular place or places, so as to facilitate inspection. Are you aware that with regard to all experiments for scientific purposes, whether painful or not, under the Act they can only be performed in a particular place or places?—I think our idea there is that there are too many particular places licensed, and that we would rather restrict the number of places licensed or restrict the area, so that inspection may be easy.

19779. (*Dr. Gaskell.*) You mean that you would restrict it simply for the purpose of making inspection easy?—Yes.

19780. Not because you think there are any signs, in consequence of the increased number of places, of violation of the Act?—No, we do not know anything about that. (*Mr. Scott.*) It is not for that reason.

19781. It is to facilitate inspection?—Yes.

19782. But you would not advocate that if you thought that by so doing you were going to hamper research?—(*Sir Frederick Banbury.*) I do not think that our society takes the line that we are to sacrifice everything to the advance of research. It is a very difficult question for us to answer, but, personally, I do not think that we should hamper science. We might perhaps inflict a little more trouble upon the scientific gentlemen, but we should not object to that. (*Mr. Scott.*) No, we should not.

19783. (*Dr. Gaskell.*) (*To Mr. Scott.*) I think you told us that you looked upon it as a noble and proper thing for experiments to be made by means of inoculation for the purpose of curing diseases; you consider that a thing which ought to be done?—Yes.

19784. And therefore the people who do that, you believe, should be recognised as benefactors to humanity?—Yes, I think so.

19785. Would you say the same if it was a question of inoculation, not for the purpose of curing an actual disease, but for advancing knowledge; that if the experiment was for advancing knowledge it would still be a noble and proper thing to do?—Yes, I think so.

19786. Therefore, you would also say that men who performed such inoculation experiments are actuated by high principles of humanity for the purpose of helping on the human race and the animal race?—I think we may go so far as that. I do not wish to impute anything but proper motives.

19787. The experimenters in physiological laboratories, you will agree with me, are actuated by high motives?—I would say, probably. I would not for a moment suggest that they were not. That would be a reflection upon a body of gentlemen which I have no wish to make.

19788. The object of their experimentation would always be to get the best results, would it not?—I think that is the assumption.

19789. Therefore, if the results were better obtained by the animal being kept quiet under an anæsthetic, the tendency would be to take care that the anæsthetic was thoroughly given, for their own sake and for the sake of the experimentation?—Yes.

19790. And also in the other cases you would agree, would you not, that when the animal is kept alive after the operation it would be for the benefit of the experiment that that animal should be well cared for, should be well looked after, and should be kept free from suppuration?—Yes.

19791. So that the object of the experiment necessitates in the minds of these high-minded people the absence of pain as far as possible?—Yes.

19792. Then what I want to understand is in what way you consider the present inspection deficient. I am not concerned with inoculations here—we have already talked about them—but in a physiological laboratory there are two kinds of experiments. The first is an experiment under a licence, in which the animal is anæsthetised and is killed before it recovers from the anæsthetic. If that is properly administered there would be no pain there?—No.

19793. Therefore, if you have evidence, or if you can believe, that the anæsthetic is properly administered, you would not require a large increase of inspectors for that purpose, would you?—No.

19794. The second class of experiments consists in an operation for the removal of an organ, or the removal of a nerve, say, with the purpose of keeping an animal alive in order to see what happens to its economy in consequence of that removal?—Yes.

19795. Those are operations under Certificate B?—Yes.

19796. The first part of such an operation is perfectly painless, because the animal is anæsthetised. The only place where pain may come in afterwards is when the animal has recovered in the laboratory; there it is possible for some pain to occur?—Yes.

19797. Do I correctly understand that it is for that particular part of the work done in physiological experiments that you would require more inspectors?—Yes, we should.

19798. Then, may I ask, do you know what the Inspector at present does; do you know what the present Inspector does?—No; I know what he is supposed to do. I understand that he has to be present at these experiments, but I do not see how it is possible that he can be.

19799. But the experiment in question, which is the one for which you have just allowed that you want more inspectors, is not a cutting operation, but it is followed possibly by pain in the animal which has recovered from the first operation, but is in, so to speak, the ward of the hospital—that is, it is in the laboratory, and is alive?—Yes.

19800. That animal can be inspected, can it not, by any inspector who comes at any time, because the experiment, if you call it so, is going on all the time, is it not?—Yes.

19801. Then I want to know when the present Inspector comes now into a laboratory, does he not see all those animals which have been operated upon under Certificate B, and which are in the laboratory at the time?—I really do not know.

19802. But the present Inspector has told us that he does; it is one of the main duties of the Inspector. Does he not go round and see the animal house, and see how the animals are kept, and see every animal

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that has been operated upon?—That is what I think he should do, but I did not think that there were anything like enough men to do it.

19803. But it only requires one man?—Then I have misunderstood some previous question. It would only require one man for that particular place, but if this experimentation is going on in many places you want many men.

19804. No, pardon me; supposing that an experimenter makes an experiment on a cat to-day, and makes an experiment on another cat to-morrow, and makes an experiment on a third cat the next day, and each of those cats is kept alive, if the Inspector comes on the fourth day, he can see all those three cats?—That is perfectly right if he does.

19805. But he does, does he not?—I understood a Commissioner at the further end of the table to say that if you had inspectors to inspect these matters we should have such an army of them that it would be a very great expense.

19806. I want to draw a distinction, if I can, between animals which are being operated upon and animals which are kept in the laboratory after an operation. All those animals which are kept in a laboratory after an operation can be seen, and very often are, by one inspector at one moment, can they not?—Yes, I should say so.

19807. And he comes at unknown times and sees all these animals?—Yes.

19808. Is there any advantage in having another inspector coming on another day to see those same animals?—No. If that man can see those animals, as you say, that is what we want.

19809. That is exactly what is done now, do you not understand?—I understand what you say, but I understood that it was impossible, that there are not enough inspectors for the work that is done. If you say that there are enough inspectors, that shakes me very much, because I read of so many experiments and I cannot understand how the three Inspectors that there are can look after them all.

19810. I will put the question in this way: A surgeon in a hospital has two duties, has he not; one is to operate upon the patients, and the other is to go into the wards and inspect those patients upon whom he has operated?—Yes, he does that every day.

19811. And he does that, as you say, every day. The Inspector comes into a laboratory where he sees the operation that is being performed; that is one duty?—Yes.

19812. He also goes round the wards and sees all the animals which have been operated upon?—Yes, that is right.

19813. Those are two duties?—Yes.

19814. You yourself say that the latter animals are those in which pain is likely to exist?—Yes.

19815. So that those animals in which pain is most likely to exist he sees most thoroughly whenever he comes to the laboratory, do you understand?—Yes.

19816. He is like a surgeon going into the wards; he sees all the animals in the wards and inspects every one of them with the same care that a surgeon would inspect his patients with?—Yes, I quite follow you.

19817. Is not that inspection in that respect efficient inspection?—I should say that it was very good inspection.

19818. I want to understand from you why it is necessary to have an army of inspectors to do that. If a man comes down at any moment into a laboratory just as a surgeon comes to the ward of a hospital at any moment, and inspects all the patients there, do you think he is likely to find any great difference if he comes another day?—If that is done that is what we mean—if you can say that it is done.

19819. That is exactly the point. I thought you were under a misconception?—You said every day, did you not?

19820. No, I said if he comes down and inspects on one day, is there any reason why he should find anything different on another day. If you were to go into the ward of a hospital——?—Which I have often done.

19821. For the purpose of judging how that hospital is getting on, and you went round the surgical wards one day, would you consider it necessary, or that there would be any point in it, to go round those surgical wards again the next day and the day after?—You are putting to me the case of a surgeon going round.

19822. No, I say supposing you as the Inspector went round?—As you say, I should pay these chance visits, but I cannot see how these men can do it when there are such a number of places and so many experiments.

19823. What I mean is this: in each laboratory there are at any one moment of time a number of animals which have been operated upon, and when the Inspector pays his chance visits he investigates the condition of all these animals?—That is what we wish.

19824. But he does?—If you say so I admit it.

19825. The Inspectors have told us so themselves, as you know if you have read their evidence?—Yes, they have told you so.

19826. What I do not see is how you, or anybody else, would get more advantage than that by increasing the number of Inspectors if the Inspector comes a certain number of times by chance and sees all these animals?—If he comes often enough.

19827. Why do you say that?—Because fresh animals come in and go out.

19828. Why should there be any difference in the arrangements of the animals and the nature of the animals from one time to another?—From Monday to Wednesday, do you mean?

19829. No, I mean at any one moment of time there may be six animals which have been operated upon in the laboratory?—I do not quite follow what you mean. Do you mean that a man might come on Monday and again on Wednesday or on Friday or at an interval of a month?

19830. I mean at any time you like, whenever he chooses, casually. Why should you suppose that there should be any difference in the condition of things in the laboratory from any one day to any other day?—Only that there may be fresh patients.

19831. So there are in a hospital?—Yes, and there, of course, the surgeon visits very frequently. That is what I mean, whether the Inspectors make these visits frequently.

19832. Another point is that the animals should be well looked after?—Yes.

19833. Would you not again consider it sufficient if the Inspector on a chance visit does tell us that the animals are well looked after?—Yes, that would be a very great thing.

19834. That would be sufficient?—It would go a long way towards efficiency.

19835. You see, it is to aid the experimenter that the animals should be well looked after, because otherwise, the experiment would fail?—Yes, I quite see that.

19836. It is his interest to look after that?—Yes.

19837. Then there is one other point I want to put to you. At the present time the law says that the animals must be kept in a licensed place, and those licensed places are, as a rule, in towns. Do you not think it would be better that the wards of hospitals for animals after they have been operated upon should be out in the country so that they should have every opportunity of the best air and the best food?—Yes.

19838. Would it not be better, instead of their being obliged to be kept in licensed laboratories in towns, where they are now, that they should be free to go out into the country?—It seems very reasonable, indeed.

19839. At present the law does not allow that, as you know?—I think it would be a very good thing. It is hardly, perhaps, a proper illustration, but I may say that in the case of our dogs' home we started a country branch, but unfortunately we cannot make it answer at all. People do not seem to care to go to the country. In addition to our home at Battersea we took a place at Hackbridge, because it is much better for the dogs, but it has not been a success. As I say, I do not know whether I ought to have mentioned that, but it seems the same kind of thing. And

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your suggestion would seem a very reasonable and right thing, in the same way as we have convalescent homes.

19840. (*Dr. Wilson.*) I gathered from what Sir Frederick Banbury stated that objection is taken by your society to experiments on animals being carried out for teaching purposes in the presence of students. Do your society take up that position?—Yes, they do very much.

19841. They object to all experiments for teaching purposes?—There is an objection on the part, I think, of a good many members on that ground.

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19842. (*Chairman.*) I understand that you wish to say something about what has taken place in reference to Professor Gotch's evidence?—Yes, but also the night before last I had placed in my hands Sir Victor Horsley's evidence, and with your kind permission I should certainly like to say a few words on the subjects that he has dealt with, not at all with respect to any things which the Commission may think are answers to anything that I said, but I wish to deal with the accusations of a personal nature made against me as a man and a gentleman, which I think certainly need my reply as they will be published.

19843. In that case before we allow that, I think you must tell us precisely what the points of fact are, because we could not hear anything in the nature of a speech in reply to Sir Victor Horsley?—No, of course not.

19844. But if Sir Victor Horsley has made a statement of fact relating to you personally which is untrue, or which requires explanation, that explanation, confining it to the matter of fact, we might think you entitled to give, but it must be confined to that, because this enquiry is a very long one; you have yourself given evidence for three days, and in the course of that evidence you have felt it your duty to attack a good many people?—I never said a word against Sir Victor Horsley.

19845. I am not speaking of that. I am only saying that we could not have everybody who considered that an attack had been made upon him called back again to give evidence, because something must be left to us—when we have heard both sides—to say whether we think it is of importance, and what importance we attach to the statements on each side; and it is only when there is a clear misstatement of fact which is capable of correction relating to yourself that we ought to allow the matter to be gone into. Otherwise many witnesses would say, "I wish to make a reply on certain branches of the case that Sir Victor Horsley touched upon." We must avoid anything of that kind?—Well, my Lord, I have been accused by him of "deception," "constructive misstatement" (Q. 15728), "gross misrepresentation" (Q. 15905), and "dishonesty" (Q. 16052), while my methods of controversy, it is insinuated, are ungentlemanly (Q. 15729, 16011), and have been subjected to "exposure"(Q. 15978). Those I took out as things which he said against me personally. They involve material of this kind. They involve the question of my *bonâ fides* about the publication of the catalogue of instruments for vivisection, upon which I wish to defend myself of course; they involve a statement that I made a perfectly false statement to you about the meeting at the Mansion House for the purpose of instituting a Pasteur Institute in England, a statement on the subject accompanied with considerable personal imputations.

19846. You do not complain of that, Mr. Coleridge, do you? Many witnesses here, yourself included, have not hesitated, when they thought that a thing deserved strong language, to use it?—Certainly.

19847. Therefore, it is a question of fact, and not of a comment on the fact?—Yes. He is wrong in his facts, and I am prepared to show that he is wrong in his facts. Then also he is wrong on the question of muzzling. I can show you that my Society has never said a word against muzzling; on the contrary, I have always regarded the muzzle as the opposite treatment to Pasteurism, and have supported it. I supported Mr. Long in his muzzling order, so that Sir Victor Horsley is utterly wrong, and I can show it.

19848. He speaks of your Society?—Yes, he says that we actually got up meetings against the muzzle. It is absolutely false, we did nothing of the kind. He has mistaken our society for another. I do not attribute anything more than that.

19849. But we had a witness only the other day who spoke very strongly against muzzling?—Yes, from the Canine Defence League.

19850. Who said that his Society originated the opposition to the muzzle?—Yes, that is so. Sir Victor Horsley has mistaken us for the Canine Defence League.

19851. I am only pointing out there that it would be very much shorter for you to say, "This is quite a mistake. Our Society has always been in favour of muzzling, and it is evidently a mistake for the Canine Defence League?—Yes, that is all I want to say. I have got eight headings, and they will not take very long; they are all quite short and all of that nature.

19852. We will deal first with Professor Gotch's evidence, and as to that we could not hear you to discuss the value of the whole of the evidence; it is only if there is something about yourself personally that we can hear you?—That is all I wish. I do not wish to prolong the thing at all. I am very reluctant to come back again. The first matter is at Question 13668. You, my Lord, said: "Some questions were asked as to what Mr. Coleridge knew, and of course he said he knew nothing about it personally, but that these gentlemen had given him this information, and he had no doubt that they were speaking the truth." I do not think that I ever said I had no doubt that they were speaking the truth.

19853. You did not use those words, but that was no attack upon you. I was not in saying that making anything in the nature of an attack upon you at all?—No, I am only raising it because as a matter of fact I never said it.

19854. But you said something rather like it?—No, my Lord, I did not.

19855. You said something to the effect that they were credible persons?—I sent their statement down to Dr. Gotch, that is what I did with it.

19856. I should have thought that that was not a matter which you need have brought forward?—I had some doubt because I sent it down to Dr. Gotch. If I had had no doubt I should not have done that. I did not accept their statement without further explanation. I read out the letters, and the comment upon them begins at Question 11455.

19857. You say at Question 11444 that you received a letter, and you say, "I should like on this to show you the kind of evidence that is constantly brought before me"?—Yes.

19858. Then you go on to say at Question 11446, "I am only showing the sort of evidence that comes before me. I did the obvious thing that a gentleman ought to do. I wrote at once to the head of the laboratory, told him the information that I had received, and asked for his explanation; and he gives the lie to the six gentlemen at once. All I can say is that in a court of justice the six witnesses would go against the one?"—Yes.

19859. That surely is saying that Professor Gotch's evidence is not to be believed as against that of those six gentlemen?—I do not think that.

19860. Surely it was not a very unfair representation for me to put it shortly as I did?—Surely, it is a question of the weight of evidence.

19861. I think we should hardly have recalled you as to that?—Then at Question 13668 you, my Lord, say: "The language probably which he (that is, Mr. Sharpe) did not when he wrote it intend to be read out to the Commission or anybody else"; you are alluding there to Mr. Sharpe's letter to me, in which he says: "Professor Gotch is a liar." When your Lordship drew that inference——

19862. This is really replying. This is not a question of fact. I did not think so, but after that there was a letter from Mr. Sharpe, who said that he did give you leave, or leave was given to you in a second letter to read it out to the Commission, but he did not think it would go into the Press?—I want to defend myself upon this very strongly, because I sent a letter down to this young gentleman by messenger. I sent him the following letter because I was coming before the Commission in a day or two afterwards: "Dear Sir,—I am forwarding you by special messenger copy of a letter which Mr. Coleridge wrote to Professor Gotch, and his reply. Owing to the nature of his reply, do you think you could send me any fresh information which would explain all the discrepancies between the accounts before Wednesday, as Mr. Coleridge is to give evidence before the Royal Commission on Vivisection there?" So that it was in answer to a deliberate invitation to him to give me more evidence that I might put it before this Commission that he wrote the letter, which I am now vilipended for producing here.

19863. I do not see that you are vilipended. I said what seemed extremely natural from the evidence which I had before me?—Further on I am attacked equally by Dr. Gotch. He says at Question 13700 that he is bound to say he cannot acquit me. Then, further, there is a letter from the Warden of Keble, who actually writes in these terms——

19864. (*Mr. Ram.*) Will you give the reference to it, please?—It is Question 13668, near the bottom: "They" (that is, the undergraduates) "made some attempt to make inquiries, but the attempt was obviously inadequate, and I think that Mr. Coleridge treated them a little badly in accepting and making public the second letter without further investigation on his own part." This young man knew, therefore, that this letter would be read out to the Commission. I asked him for a letter to read here.

19865. (*Chairman.*) Are you complaining of what the Warden wrote?—Yes, I am, very bitterly; most decidedly I am. He cannot have known, I presume, that this young man had an invitation from me to write that letter that I might produce it here or he would not have accused me thereafter of producing it here. What else could I do.

19866. I do not see anything further than that the letter is one which shows that the Warden thought you ought to have made investigations not merely by asking these young men, but by finding out what really happened at the laboratory?—How could I do that? I have no warrant to enter the laboratory.

19867. You had as much power of finding out as they had?—Quite so.

19868. What do you expect us to do? You are now making a speech upon the Warden's letter?—No, my Lord, not a speech.

19869. You are not contradicting any question of fact; you are making observations upon the Warden's letter as not being fair to you?—Quite so; it is not; it is extremely unfair to me. Now I go further, and I will say this: I wish to point out that when these various people announced that these gentlemen had withdrawn and apologised, the fact is that they have never withdrawn and apologised for the only thing that I think is of any value at all in their original letter. Dr. Warren, the Vice-Chancellor, says that he has had placed in his hands "a complete apology by these undergraduates to Professor Gotch, a recognition by them of the correctness of Professor Gotch's statements, an explicit and unqualified withdrawal of the letters and statements sent and made by them to Mr. Coleridge." I say that no such withdrawal has ever been made. They contend, and they have never withdrawn it, that they heard the howls of dogs in agony.

19870. Is not this a speech upon the evidence?—No, my Lord.

19871. Surely it is. Here are the letters before us, and you are saying that those are not an unqualified apology. We have before us the fact; we have what the letters are, and we have the statement which you have just made, that this was an incomplete apology. We must judge of that. We cannot call upon you as counsel to make a speech?—I have got a letter here from Mr. Sharpe, in which he says that he does not apologise.

19872. Has that letter been read here?—No. I have another letter from him to myself.

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19873. All I can say is that if Mr. Sharpe writes a letter apologising, and then writes another saying that he has not apologised, we should not pay much attention to anything that he said?—No, he does not say that. He has only withdrawn the statement made to him by the laboratory attendant that such and such things took place in the laboratory. He has not withdrawn that he heard the howls of dogs.

19874. He says that in a letter to Mr. Tomkinson, which was read?—Yes.

19875. If you say that the only thing of importance is whether the dogs howled, although they were not being vivisected——?—That is all I referred to.

19876. We must judge of that?—That is the only evidence that had the slightest weight with me. What the laboratory attendant said to him I do not care.

19877. I venture to think that there are other things in the matter which will have weight with us?—Very well. Now, my Lord, I want to speak about the catalogue.

19878. Which question is that?—This is in the evidence of Sir Victor Horsley at Question 15729, in the middle of the first column, where he says that this catalogue was corrected in 1902. He said that 50,000 copies had been sent out, and then he said: "It was corrected, as I said before, in 1902, and Mr. Coleridge stated to this Commission, at Question 10559, that where he was shown anything which was wrong he liked to correct it. I may say, in parenthesis, that he has never corrected anything which he has been shown, repeatedly was incorrect, nor has he ever apologised." My comment upon that is that it is perfectly true that a Dr. Grünbaum, of Liverpool, wrote to me, and said that certain items in the translation of the catalogue were, in his opinion, incorrect, but he said that they were not of any very great moment. I therefore sent his letter on to the gentleman who translated the catalogue for me, and asked him what his view of it was. He wrote back to me that he did not agree with Dr. Grünbaum's suggested corrections. And as the person who translated it for me was appointed by the librarian of the British Museum, at my request, I presumed that he was an impartial man. I had never even seen him, and I did not see why I should correct my catalogue at the instance of a licensed vivisector, but I printed his letter and published it. Therefore I do not see why I should accept that correction. I did what I think anybody would do—I laid the correction before the original translator, who refused to adopt it, and there I left the matter. I do not see how I am blameworthy there at all.

19879. What is the next point?—Then a little further down on this subject, those being the facts, you will find at Question 15978 Sir Victor Horsley speaks of "the exposure of the catalogue of instruments, and Mr. Coleridge's action"; that is the way he speaks of it. All I say is that I did what any gentleman ought to have done, and left undone nothing that a gentleman ought to have done. I do not want to go into the general question, unless you wish it, of the verity of that catalogue, but I can hand in copies of it if anybody likes to see it. I have got 13 copies here.

19880. No; confine yourself, please, to the specific contradiction?—Then the next point is that I stated in one of my opening statements that I prevented a public meeting being held at the Mansion House for founding a Pasteur Institute. With regard to that Sir Victor Horsley, at Question 15594, said: "In the first place, Mr. Coleridge informs the Commission that that meeting was prevented by his society from taking place. It is not so. The meeting took place at the Mansion House," and so forth. Then he says: "It was not called for the purpose of establishing a Pasteur Institute in England; it was called" for other purposes. Therefore he gives me the lie there. My position is this. It was originally intended to be a public meeting, and I and my friends were determined that if it was to be a public meeting we would take care that the opposition was properly represented. Accordingly, I went myself to the Lord Mayor, and told him that if it was a public meeting the Lord Chief Justice of England and Cardinal Manning proposed to speak in opposition to the resolutions, whatever they were, upon which the Lord Mayor said: "It is not a public meeting; it is a private one," and I said, "Very well, if it is a private meeting the Lord Chief Justice and Cardinal Manning can have no part or lot in it. You can have as many

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private persons as you like, and pass any resolutions that you like, and there will be no opposition from anybody." From that moment it was changed from a public meeting to a private meeting. I can prove this by the "Daily News," of July 1st, page 7, which is a perfectly impartial thing. There is the heading—"To-day.",

19881. I am glad to hear of a paper that is impartial upon such questions. I should hardly have classed the "Daily News" as such?—It was only an advertisement of the meeting, and it was 20 years ago, and they were not anti-vivisectionists then.

19882. But does the Lord Mayor take the chair at a private meeting to discuss a question of this sort? I never heard of the Lord Mayor taking the chair at a meeting of this kind, which was private?—That is exactly what it was. That is exactly what we turned it into. We enforced that upon them by my visit to the Lord Mayor.

19883. How was it advertised—as a private meeting?—Yes.

19884. Would you kindly read the advertisement?—This appeared in "The Times": "The Lord Mayor's secretary, replying to a letter from G. Candy and Co." (I think that should be G. Candy, Q.C.), "informs him that at the meeting which is called at the Mansion House on Monday next to discuss the subject of the prevention of hydrophobia, no one will be admitted who has not received an invitation." That proves that it was a ticket meeting.

19885. In that sense I see what you mean. I thought it was not a private meeting?—It was a ticket meeting. I mean that nobody was admitted without a ticket. Then, as I say, in the "Daily News" you have the heading "To-day, Meetings, Proposed London Pasteur Institute—Mansion House, 3."

19886. Is there not a possible explanation? Supposing that somebody gives notice that there will be a very strong hostile party——?—That is what I did.

19887. Which you did. The Lord Mayor would particularly dislike presiding over a meeting at which there were likely to be signs of disturbances?—No doubt.

19888. Therefore nobody would be admitted without a ticket?—That is exactly the history of it. It was turned from a public meeting into a private one, and my statement to you was that there was no public meeting held—that I stopped a public meeting being held, and I did so. Then the second statement that I made was that the meeting was originally intended for the purpose of founding a Pasteur Institute for London, and I say there is the "Daily News," which actually heads the meeting: "Proposed London Pasteur Institute."

19889. You are not going to cite an announcement of a meeting in the "Daily News" on a question so very controversial as being positive proof of what the object was?—No, it is only evidence of my *bonâ fides* in making this statement. And the "Times" had the same.

19890. If I wanted evidence I should not go to a paper that took strong views?—I think it was against us then, but I will not be sure. The "Times" has always been against us.

19891. (*Mr. Ram.*) Is not this really a very minor point?—Yes.

19892. There was a meeting?—Yes.

19893. You have shown that it was a ticket meeting, apparently in consequence of what you said?—Yes.

19894. That was not known to the witness. He thought that there had been a public meeting?—No doubt.

19895. (*Chairman.*) And certain resolutions were passed?—Yes, but Sir Victor Horsley also said that there was no intention of founding a Pasteur Institute. I said that there was originally, until we intervened.

19896. Sir Victor Horsley must be the person who knows best what the object of the meeting was?—I quite agree that when the meeting did assemble they had made up their minds not to push a Pasteur Institute for England, and no such proposal was placed before the meeting.

19897. That is not quite what Sir Victor Horsley said. You had said: "The intention was to hold a meeting the object of which was to start a fund," and so forth; and Sir Victor Horsley said: "The object was not as he states it; it was not called for the purpose of establishing a Pasteur Institute in England, it was called," and then he quotes, "for the purpose of hearing statements from Sir James Paget and other representatives of scientific and medical opinion with regard to the recent increase of rabies in this country, and as to the efficiency of the treatment discovered by M. Pasteur for the prevention of hydrophobia." That is his version of it?—Yes.

19898. Surely it is a very small point whichever way one considers it. You say that it was meant for the purpose of starting a Pasteur Institute?—No, my Lord, that is not my point. I say that the original intention, until the anti-vivisection party intervened, was to have a public meeting to promote a Pasteur Institute for England, and by our intervention it was changed into a private meeting, with a perfectly harmless resolution, saying that Monsieur Pasteur was a wonderful man, and they would like to subscribe and give him a testimonial, to all of which we had not the slightest objection.

19899. (*Sir William Collins.*) At any rate, was the meeting that was held one to which the public had access?—No, they were excluded.

19900. (*Sir Mackenzie Chalmers.*) It was like all those meetings that we have every day from which suffragettes are excluded?—I have not been to any of those, but I thought they always got in.

19901. (*Chairman.*) I quite understand you. It is quite easy for you to point out that it was not a public meeting in the ordinary sense, but one to which admission was by ticket?—Yes, that is all my point. Then Sir Victor Horsley says that we opposed the muzzle.

19902. What question is that?—It is Question 15597. He says: "Immediately this point of universal muzzling was raised the anti-vivisection party, Mr. Coleridge's society in particular, entered upon a virulent campaign against us," and so forth. I say that he has mistaken us altogether for the Canine Defence League. We did nothing of the kind, and he ought to have been more careful in making these accusations, because he holds a very prominent position, and it is rather careless for a man to come and make that kind of statement without any ground whatever. The slightest inquiry would have shown him that he was mistaking my society for some other with which we have no connection at all.

19903. Has your society never opposed the muzzle?—Never.

19904. Has it never put forward the view that rabies was not a disease?—We are against Pasteurism; we regard the muzzle as an excellent antidote to Pasteurism, and we think we have proved it to be successful.

19905. (*Mr. Ram.*) And to rabies?—Yes, it has kept both Pasteurism and rabies out of the country. We are entirely in favour of universal muzzling. It has nothing to do with any cruelty, and is an admirable method of stamping out an abominable disease. Then the next point is what Sir Victor Horsley says about the Anti-Vivisection Hospital, at Question 15592. He says: "I have placed upon my *précis* a very brief reference to the Anti-Vivisection Hospital, because the money for that hospital was originally collected many years ago, by Mr. Coleridge's society in part, on the idea that medical practice could be conducted without any reference to vivisection." There is nothing unkind in that, and I do not in the least find fault with it. I merely want to point out that it was true from February 7th to October 6th only in the year 1897, and on October 6th, I proposed the following resolution with success to the council of my society: "That inasmuch as the object of this society is to maintain a constant fight against existing vivisection and not to erect institutions from which the practice shall be excluded, the council declare that its committee should not actively participate in the project of founding and maintaining an anti-vivisection hospital." So that really almost as soon as I had any authority with the society at all I passed this resolution, saying that we would have nothing to do with the promotion of an anti-vivisection hospital; therefore it is a little unfair to put it upon us in that way, not that I do not feel an extremely friendly interest in the Anti-Vivisection Hospital, but I did not think that it was a proper thing for our society to have it actively participating in it.

19906. (*Chairman.*) Sir Victor Horsley only says that you did in part collect the money?—Yes.

19907. And for a time you did?—Yes, for a few months only.

19908. It was hardly worth while making that correction?—I only wanted to put that straight. Then at Question 15738 Sir Victor Horsley said: "The Home Office had not given me, to use Mr. Coleridge's words, unrestricted leave to do experiments when I liked, where I liked, and what I liked." I never used those words in my life, or anything like them.

19909. Yes, I remember he did put those as your own words, and it struck me at the time that before us, at any rate, you did not use those words?—I never did.

19910. You did say that he was allowed powers which he ought not to have been allowed?—I only quoted what I found in the yearly report, which was accurate. I found that he had permission to vivisect where he chose. I also deduced that he could not be inspected under the Act in an unregistered place, and, therefore, if he chose to vivisect in an unregistered place he could not be inspected. I think that statement is absolutely correct, and I am sure that Sir Mackenzie Chalmers will say it is so. It was in the yearly report for seven years, I think.

19911. (*Sir Mackenzie Chalmers.*) You accept Sir Victor Horsley's account of what the restrictions were now that you have read it?—Yes; the restrictions put upon his licence to vivisect where he liked have nothing to do with the point. My point was that he was allowed to vivisect where he could not be inspected.

19912. (*Chairman.*) There is power, you know, under the Act, to allow vivisecting in other places than laboratories?—Yes, I did not say that there was not. I did not say that what was done was illegal. I said that I objected to what was done because it placed him outside the possibility of inspection. That was my objection.

19913. Sir Victor in his evidence gives here a specific explanation of why he was given that certificate. The point is that it was absolutely necessary, if the experiment was to be done at all, that it should be done in a hospital, and by the bedside. That is his explanation?—Yes; I am defending myself from an attack upon me; that is my position. I still say that what I said was perfectly true—namely, that he could vivisect where he chose, and that he could not be inspected. And I further go on to say that what I said was perfectly true about his leave being withdrawn from him just at a time which synchronised with the appointment of this Commission.

19914. I think your suggestion was that he was receiving an extraordinary favour, and that the Home Office was going outside its duties, if not its powers, in doing what it did?—I protested against it, and I had protested two years before in vain, but the appointment of this Royal Commission synchronised with the withdrawal of that permission.

19915. I must say that although I do not think you did use the very words before us which Sir Victor Horsley puts into your mouth, his evidence, to my mind, did very much alter the complexion of the matter as stated by you?—I do not see that, my Lord. I stand to every word that I said, and I have said nothing which is not carried out by the published document from the Home Office. If you look at what I said, it is absolutely carried out in the Home Office document, which I have here, and can show you. I decline to admit for one moment that I said anything which I could not stand to in the letter as well as the spirit. I adhere to every word of it.

19916. Is there any fact which Sir Victor Horsley states about that operation that you say is incorrect? —My Lord, he is accusing me of inaccuracy. I am here defending my accuracy. I am not here to attack Sir Victor Horsley.

19917. You do not want to correct any statement of fact except the fact of your using those words?—It is a matter of indifference to me what was done under those extraordinary provisions. I did not attack Sir Victor Horsley. I attacked the Home Office for giving him permission.

19918. (*Sir William Collins.*) Did you ever say that Sir Victor Horsley had been given power to vivisect when he liked, where he liked, and what he liked?—No; I did not.

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19919. (*Chairman.*) What is the next thing?—Here is what I consider an extremely offensive answer at Question 16011. This is with regard to Sir Victor Horsley's evidence at the trial of *Bayliss v. Coleridge*. Your Lordship asked him, "Was it your evidence he was quoting from?"—that is, I was quoting from, and Sir Victor Horsley says, "It is my evidence he professes to be quoting from, but from my experience of Mr. Coleridge, and especially after what he has admitted to this Commission, I accept nothing that he produces in the way of evidence of what I have said, unless I have the context. . . . Mr. Coleridge's was quite a false rendering of my evidence in the way he presented it to the Commission." I have brought the evidence here to present it to the Commission, that they may see for themselves that I was absolutely right. I should like first of all to know what he means when he says, "especially after what he has admitted to this Commission." What have I admitted derogatory to my accuracy or to my honour to this Commission. I want to know? I think it is a most offensive phrase. Now, as to the question itself. This is the question and answer, Question 834. "(*Mr. Rufus Isaacs.*) Would it be possible to perform the experiment or the demonstration which we know took place if the dog had been conscious?—No, absolutely impossible unless the dog had been fixed up with all manner of apparatus to absolutely fix every bone. You can fix an animal with apparatus of that sort (*pointing to the operating board*)." That is the evidence I quoted. I quoted it absolutely verbatim, and I really fail to see what he means. If Sir Victor Horsley does not like the evidence I cannot help that. Those are his words.

19920. Are you reading from the shorthand notes of the trial?—Here are the actual shorthand notes of the trial. I consider that I am entitled to quote the evidence given at the trial by Sir Victor Horsley without having this sort of innuendo.

19921. Are those words in the shorthand note—"absolutely fix every bone"?—Yes, there is a split infinitive, I cannot help that; the words are "had been fixed up with all manner of apparatus to absolutely fix every bone."

19922. That is what you were reading from the shorthand note?—Yes.

19923. That is what we have in Sir Victor's evidence?—Yes, you will see that I quote it absolutely verbatim.

19924. He goes on to say, "I showed the Lord Chief Justice what I meant by fixing every bone with a rod passing through the bones. Of course, it is an absurd position, you observe. It is impossible to fix every bone in an animal"?—There is no report of that demonstration to the Lord Chief Justice in this report.

19925. "I said, 'You can fix an animal with apparatus of that sort (*pointing to the operating board*). That is the answer to that—you can fix an animal on that board, and he pointed to it; they had it in Court'"?—That is right.

19926. You quoted that?—"Pointing to the operating board." I then say, I suppose, "That is my answer to that."

19927. Are not those words, "You can so fix an animal," what he says that he said, following on?—"You can so fix an animal with apparatus of that sort (*pointing to the operating board*)"—that is in the report.

19928. Does he go on to say, "That is the answer to that"?—No, those are my words.

19929. Then the inverted commas are put wrong?—Yes, that is what I said to you.

19930. Then it is a mistake in inverted commas being put here as if those words were also part of his evidence?—Yes, that is what I said. That is all I have to say about that. I fail to see any justification for that kind of language. Now I come to Dr. Crile's experiments, and the letters to the surgeons, Question 15712. The question is, "He says, 'We are told in the control experiments, as well as in this, the dog was not under full anæsthesia.' Do you know by whom he is told?" and Sir Victor Horsley says, "No, that is Mr. Coleridge." It is not, it is Dr. Crile. Here is the book; I will read it out of the book. "In the control

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experiments, as well as in this, the dog was not under full anæsthesia. In the former the animal struggled on application of the flame; after the injection of cocaine he did not. There was apparently blocking of the sensory impulses from the paw." That is the quotation.

19931. What is the page of the book?—In the big book it is pages 118 and 119 in "Surgical Shock." Sir Victor Horsley attributes to me what is found in the book, so that that was a mistake of his.

19932. If I follow you, you say that those precise words at Question 15712 were taken by you from the book?—Yes, absolutely from the book, and the book is here. Then the next point is at Question 15728—this is the most important thing of all. "In 1899 Mr. Coleridge attacked attacked Dr. Crile at the Home Office, and, after he had been fully informed by the Home Office as to the meaning of this term 'complete anæsthesia,' he then wrote to several surgeons and asked them a question about incomplete anæsthesia, and he informed this Commission that he wrote exactly in the terms in which the Home Office had written to him. As a matter of fact, he did not write exactly in the terms of the letter of the Home Office, because he omitted the all-important sentence which defined this term 'incomplete anæsthesia.' And the result was that he deceived these surgeons into thinking that it was suggested that operations were done upon human beings in hospitals without adequate narcosis." If you can follow me I can show you exactly how wrong he is about this. The letter received from the Home Office had these words: "But short of this is the condition termed by Dr. Crile 'incomplete anæsthesia' in which the creature is quite insensible to pain, although the corneal and other reflexes can still be obtained." That statement was a statement given to me by the Home Office as a statement given to them by their advisers, and it contained in it the whole question at issue between me and the Home Office. It stated that the condition of the corneal and other reflexes being obtained was a condition of insensibility to pain. That was the crux that I wished to ascertain; therefore, when I wrote to the surgeons I naturally did not tell them that the Home Office thought that it was a condition of insensibility to pain, but I asked them what their opinion was about the condition of a human being when corneal and other reflexes can still be obtained; and it was in answer to that question that they wrote me those letters. The suggestion that I deceived them is wholly false. I deceived them in no way whatever. I asked them practically whether they agreed with the Home Office in saying that that was a condition in which pain was absent, and their answers were tantamount to saying that they disagreed with the Home Office. That is the whole of that. The only alternative to my mind is that, if it be suggested that my letter deceived the doctors, it must be suggested that I deceived them into telling the truth—that I tricked them into telling the truth, which they would not have told if I had not tricked them, which I think is a strange thing to suggest about the heads of a great profession. I do not make the suggestion. I think their answers to me were perfectly true when they told me that they did not commence operations upon patients in hospitals until the corneal and other reflexes were entirely abolished. I believed them. I believe them now. I do not believe for one moment that any surgeon would commence an operation until the corneal and other reflexes were abolished; and that is what they told me, and there is no deceiving in the matter at all. I asked them a perfectly plain question, and the surgeons gave me a perfectly plain reply. My motive in asking the question and their motive in making the reply have no bearing whatever upon the question. Their answer to the question was either a true answer or an untrue answer. I maintain that it was a true answer, and the allegation of deceit against me is wholly unfounded. I do not think I need say any more, except this:—At Question 15588 Sir Victor Horsley says: "Mr. Coleridge's society, to take that alone, have collected from the public £85,000, and have not added one item of knowledge to us for the relief of suffering or the prevention of disease." We are not a society for the collection of knowledge; we do not spend our money in that way. And let me say in conclusion, before I leave this Commission, that if, as the result of this Commission, inspectors are appointed who will look without protest at live dogs struggling while they are burnt with flames, the certain result will be that I for one, and, I believe, all the humane people of this country, will unite utterly to abolish what we regard as an abominable wickedness.

19933. (*Sir Mackenzie Chalmers.*) There are only two questions that I would like to ask you. I want to understand now, do you accept Professor Gotch's statement to us that during last year no experiments were made on dogs in his laboratory when the undergraduates heard this howling?—Was he there all the time himself? I should want to know whether he was there all the time.

19934. I am only asking you whether you accept his statement?—I accept his statement that, so far as he is aware, no operations were performed there, of course, but unless he is there all the time he does not know what happens in his absence. The students might operate there in his absence.

19935. Do you really suggest to us that any operation was performed there?—I do not suggest anything. You ask me whether I accept a statement of a man as to whether a certain thing is done or not done at a place where he may not have been. I will accept his statement that it did not happen when he was there, certainly; but I am not prepared to accept that he knows what happens in the laboratory when he is not there. No one would accept that.

19936. (*Chairman.*) It is not merely a person who was not there. He is the person who is the head of the laboratory, and there could hardly have been any vivisection of dogs without his knowing it. And, mind you, the statement in the letter is that this happens every day?—The howls of dogs, yes.

19937. And "in consequence of vivisection," the letter goes on to say?—Yes.

19938. Do you mean to say that Professor Gotch is not to be believed?—I have not said that. I carefully do not say that.

19939. You are carefully refraining from saying that he is to be believed?—I am not indeed. I believe his statement so far as he has a right to make it.

19940. Then has not the head of the laboratory a right to say, "There was no vivisection in my laboratory"?—No, no more than a magistrate has the right to say that no crime happens in his district.

19941. If you think those two things are the same?—But many things may happen in a laboratory when he is not there.

19942. (*Sir Mackenzie Chalmers.*) May I put it in this way? Without more hypotheses, are you satisfied yourself that no experiment took place upon a dog in that laboratory during that year?—Sir Mackenzie, you ask me that question. Am I not entitled to ask how Professor Gotch explains the fact that six young gentlemen say that they heard these howls? He does not suggest an explanation. I should have liked him to be asked that. No one asked him how he explained that these six young gentlemen, with no motive of any kind except that of humanity, should have said that they heard day after day the howls of animals in agony.

19943. (*Chairman.*) He does explain that?—How?

19944. Anyhow, he explains it in a sense. He says that the only explanation he can offer is that there were puppies there, and these puppies were very noisy, and that these gentlemen who were so ready to attribute illegal vivisection to him were perhaps a little predisposed to think that they were the howls of dogs in pain.

19945. (*Sir Mackenzie Chalmers.*) I only want to know, is your mind quite relieved on the subject of what happened there or not after having heard Professor Gotch's explanation?—No, it is not quite relieved. I am quite ready to accept Professor Gotch's statement that, so far as he knows, no vivisection has taken place there, and I am sure that he did not do it himself; but the explanation of the puppies I do not think sounds to me a very adequate explanation. These young gentlemen say that they know well enough what the cries of puppies are, and they say that these were not the same; and it is half a dozen young men, with no other motive, so far as I know, except that of humanity.

19946. (*Dr. Wilson.*) But they apologised?—No, they have never apologised for saying that they heard

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the howls of dogs. They maintain still that they did hear the howls of dogs, and they have not withdrawn it; they decline to withdraw it.

19947. (*Sir Mackenzie Chalmers.*) Do they decline to withdraw the statement that the howls of dogs which they heard were the cries of dogs in pain?—Yes, absolutely; they still say that they heard the howls and they still think that they were the howls of dogs in agony. What they have withdrawn is that they had been told by a certain official that vivisection had been going on. This certain official says that he did not say so, and they accept that.

19948. (*Mr. Ram.*) At what date did you first learn that they had made the withdrawal which they have in fact made; was it before you gave evidence here or afterwards?—The whole thing was afterwards—long after I was here, of course.

19949. (*Chairman.*) The first letter in the Oxford paper was in October?—Yes. I had no withdrawal when I came here.

19950. And your evidence was given in June or July?—Yes. I had heard no whisper of withdrawal when I was before you.

19951. (*Mr. Ram.*) Did you receive a letter from these young gentlemen in which they did make such withdrawal as they did make; because I see they state that they sent you a letter?—Yes, they told me that they had to withdraw.

19952. "An explicit and unqualified withdrawal of the letters and statements sent and made by them to Mr. Coleridge." The date of that is October 19th, 1907?—I have the letter here, because I wrote at once in reply—I want to point that out—and said, "Am I to understand from you that you withdraw the fact and you now say that you did not hear the howls of these dogs?" and they wrote back, "Oh, dear me, no; we do not withdraw that."

19953. What they did withdraw was this; it is in a letter to Mr. Tomkinson of the 19th of October, in which one undergraduate writes, "I am sorry to say I have no more evidence to give. I have found that on closer investigation the facts which were told me as true—viz., that a dog and rabbits had been cut up, and had been yelling—were entirely false. I felt therefore compelled to withdraw these statements, and to apologise to Professor Gotch for my language concerning him when I heard he denied this." That is what they withdrew?—That is all they ever did withdraw.

19954. Since you received that letter in which they withdrew that statement have you published anything which has incorporated any facts concerning this undergraduate case either in your evidence or otherwise?—I do not think so. I have not dealt with it at all until I came here.

19955. I think in one of the papers which some one has been good enough to send to me there is an intimation that the evidence which you have given before us here was either published or about to be published?—Yes, it has been published by the Commission.

19956. But I mean otherwise published?—I have reprinted in a large type for our own use just a facsimile, and we have had an index put to it.

19957. Has that been circulated?—Not sold.

19958. (*Sir Mackenzie Chalmers.*) How many copies have been circulated?—Oh, a lot.

19959. Some thousands?—Yes, it has been circulated widely.

19960. Will it be circulated further?—Yes, I presume so. We have got about 200 or 300 copies left.

19961. (*Mr. Ram.*) Is it your intention with regard to either a further circulation of this particular book or the further circulation of your evidence, to give the withdrawal as given in this letter to Mr. Tomkinson of the 19th of October?—I propose to incorporate my evidence of to-day with it.

19962. And the evidence of to-day will, therefore, incorporate what I have just put to you as to the withdrawal which these gentlemen have made?—Yes, every bit of it.

19963. (*Chairman.*) But you are not proposing to incorporate in your book the whole of Professor Gotch's evidence on this point?—No, I shall not do that. At least I might do it. I had not really considered that.

19964. (*Sir Mackenzie Chalmers.*) Would it not be rather more fair to include Professor Gotch's evidence?—I only want to do what is fair. I will consider whether I will not incorporate everything that has to do with it.

19965. (*Chairman.*) It should be made sufficiently clear that the statement is contradicted by Professor Gotch and withdrawn to the extent that it is withdrawn by these gentlemen and in their own words?—Yes. My Lord, if it would give you any satisfaction I should be very pleased to submit what I propose to incorporate to the Commission, so that they may see that it is fair.

19966. I think I will not take that responsibility upon myself, but I really think it is a responsibility that you ought to undertake yourself, not only as being fair to Professor Gotch but because I really doubt whether you might not be made answerable at law for publishing anything without it?—Now that you mention it, I will consider whether it would not be proper to put in what Sir Victor Horsley said against me before I put in my reply to him.

19967. (*Mr. Ram.*) One question, please, with regard to the matter which you have just dealt with as to Dr. Crile's experiments and the question as to whether they were under incomplete anæsthesia or otherwise. What was put against you was this: "Mr. Coleridge attacked Dr. Crile at the Home Office and after he had been fully informed by the Home Office as to the meaning of this term 'incomplete anæsthesia.' He then wrote to several surgeons and asked them a question about incomplete anæsthesia, and he informed this Commission that he wrote exactly in the terms in which the Home Office had written to him. As a matter of fact, he did not write exactly in the terms of the letter of the Home Office, because he omitted the all-important sentence which defined this term 'incomplete anæsthesia.'" The question I want to ask you is, did you receive a letter from the Home Office in which there was a sentence defining the term "incomplete anæsthesia"?—Yes, I did. "One of the last of these reflex movements to disappear is the blinking of the eyelids when the surface of the eyeball is touched, then called the 'corneal' or 'conjunctival reflex.' But short of this is the condition termed by Dr. Crile 'incomplete anæsthesia,' in which the creature is quite insensible to pain, although the corneal and other reflexes can still be obtained."

19968. That is in the letter from the Home Office?—Yes.

19969. Did you in writing to the surgeons omit any sentence of that letter?—Yes, I left out that sentence that that was a condition in which they were insensible to pain. I did not call that a definition.

19970. Then the allegation that you omitted a sentence of that letter, which sentence defined the term "incomplete anæsthesia," is correct?—No, I do not say so. I do not consider that that is part of the definition. That is an assertion that an animal when its corneal and other reflexes are not abolished is insensible to pain. I do not agree to that assertion, and I wrote to the surgeons for the purpose of ascertaining whether it was true or false.

19971. May we take it that a sentence in that letter was left out in your letter to the surgeons?—Yes.

19972. And it was a sentence which Sir Victor Horsley considered did define the term "incomplete anæsthesia"?—Yes, I quite admit that Sir Victor Horsley said that it defined it.

19973. And that sentence which he said defined the term "incomplete anæsthesia" did appear in the letter of the Home Office, but was not quoted by you in the letter which you sent on to those surgeons?—Yes. Those words were left out certainly, because they were the issue between us. I never said in the letter to the surgeons that I was quoting the whole of the letter from the Home Office. I asked them one definite point, did they perform operations on patients when the conjunctival reflex was still obtainable.

19974. I was only dealing, of course, with the allegation?—Quite so.

19975. Then there is only one last matter I should like to mention. You have been good enough to send me a letter, dated the 24th of October, in reference to your qualification of the word "pain" in your previous evidence. In reply I wrote to you, and said that I would take care that that letter should come before the Commission, and that your correction should appear?—That is so.

19976. I think, perhaps, the best plan will be, as we have the advantage of your being here to-day, to give

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that correction now, so that it may appear on the notes as part of your evidence?—If you please. Will you read my letter then?

19977. Yes. Then the letter will appear on the notes, and you will say whether that is your evidence. You say: "Dear Mr. Ram,—On reading over my evidence and considering it with due care, I cannot but feel that it would be unwise to qualify the word 'pain' with any adjectives in my Bill. I write to you to say this because under examination by you, at Questions 11265, 11266, and 11267 I said I would agree to add the word 'serious.' This, I think, was a somewhat hasty concession. The addition of the word 'serious' or 'severe' before the word 'pain' would, I sincerely fear, lead to the legal permission of pain to an amount I could never think morally justifiable.—Very faithfully yours, Stephen Coleridge"?—I am very much obliged to you for putting that letter on the notes.

19978. That is the correction that you wish to make?—If you please.

19979. (*Dr. Gaskell.*) With respect to Question 15712, I should like to get clear what the words of Dr. Crile are. You say: "We are told in the control experiments, as well as in this, that the dog was not under full anæsthesia"?—Yes.

19980. I have not got in my own mind what Dr. Crile's words were. It is in Experiment CXXXIII.?—These are the words: "In the control experiments, as well as in this, the dog was not under full anæsthesia. In the former the animal struggled on application of the flame; after the injection of cocaine he did not. There was apparently blocking of the sensory impulses from the paw."

19981. That is all right. I wanted to know where those words actually occurred?—Yes, they are in the book.

19982. (*Chairman.*) Of course, Mr. Coleridge, when I suggested that it would be advisable to put some statement as to Professor Gotch's evidence in whatever you intend to publish you will understand that I was not myself suggesting at all the publication of any evidence taken before the Commission?—Quite so.

19983. I say that simply because it is not a matter for the Commission; it is a matter, as you know, for the Stationery Office?—Certainly. We have only done it because the official report is in such small type, and we wrote to the Stationery Office about it.

Note.—Mr. Coleridge subsequently requested the Commission to allow the following extract from an article by Mr. Edwin Wooton* in the Dublin Journal of Medical Science for 1885 (Vol. 80, p. 290) to be added here, as he had intended to call the attention of the Commission to it while dealing with the question of the attitude of some vivisectors. The extract is as follows: "I, some time since, began the study of experimental phenomena in the brute world in relation with tuberculosis and other major diseases, and have continued it until the time of writing. It is a pursuit full of difficulties—legal, social, and scientific. The investigator is hampered by absurd anti-vivisection laws, to evade and defy which is his simple duty; and he is annoyed by the sentimentalism of weak-minded neighbours or acquaintances who may become aware that he is engaged in research, and who regard him as a nineteenth century 'six hundred three score and six.'"

FORTY-FOURTH DAY.

Tuesday, 17th December, 1907.

PRESENT:

The Right. Hon. The Viscount SELBY (*Chairman*).

Colonel The Right Hon. A. M. LOCKWOOD, C.V.O., M.P.	Sir M. D. CHALMERS, K.C.B., C.S.I.
Sir W. S. CHURCH, Bart., K.C.B., M.D.	Mr. A. J. RAM, K.C.
Sir W. J. COLLINS, M.P., M.D., F.R.C.S.	Mr. W. H. GASKELL, M.D., F.R.S.
Sir J. MCFADYEAN, M.B.	Mr. G. WILSON, M.D., LL.D.

Captain C. BIGHAM, C.M.G. (*Secretary*).

Mr. D. J. HAMILTON, called in; and Examined.

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19984. (*Chairman.*) You are Professor of Pathology at the University of Aberdeen?—Yes.

19985. And you have been nominated by the University of Aberdeen to give evidence before us?—Yes.

19986. I believe the University of Aberdeen, like some other universities, have not been desirous to give evidence on the general question, after having read the evidence which has been given by other physiologists?—Quite so; they deem that sufficient evidence has been given on that line already, and that there is not any further necessity for it.

19987. And they only now desire you to come forward with a view to your giving evidence about some special inquiries which you have made with regard to diseases in sheep, in the course of which you had recourse to experiments on animals?—That is so.

19988. I believe the experiments which you are going to tell us of were first of all carried on privately?—Yes.

19989. And then afterwards, at the suggestion of the Highland and Agricultural Society, who desired you to continue them?—Yes.

19990. And then you were Chairman of a Departmental Committee which was appointed by the Board of Agriculture in 1901?—That is so.

19991. And who issued their Report in 1906?—Yes.

19992. That Report is contained in three parts presented to Parliament, numbered Cd. 2932, 2933, and 2934?—Yes.

19993. And in that Report there is a detailed account of the researches which have been spread over four years?—That is so.

19994. I will take you at once then to your evidence on that subject. The Committee was appointed as you told us, in 1901, by the late Mr. Hanbury, then President of the Board of Agriculture?—Yes.

19995. And the object was to investigate the two diseases of sheep known as louping-ill and braxy?—Yes.

19996. Would you explain to us what the extent of the mischief was?—These two diseases of sheep that we were appointed to inquire into are extremely disastrous, and the cause of enormous mortality all over Great Britain more or less, especially in Scotland, the northern counties of England, and also in Ireland; they prevail in all three divisions of the British Isles, and the mortality from them is something terrible, so much so that in certain districts where they prevail sheep farming as a profitable industry is so hampered that it is threatened with extinction, if that has not already occurred in many places.

19997. Has that been going on for many years?—It has been going on for a great many years—as far back as we have any records, apparently.

19998. Has it been worse of late years?—Yes, I think so.

19999. One has not regarded sheep farming as being an industry that was in danger; the general public were not aware of the extent of the mischief, at any rate, until recently?—Some parts of Scotland have become depopulated on account of the enormous loss. As much as a quarter of a million of money, it is said, is lost from braxy alone, and that is probably an under-estimate of the total.

* *This gentleman's name does not appear in the Medical Register or Directory for 1885.*

20000. A quarter of a million of money altogether, do you mean?—Annually.

20001. It affects particular areas, you say?—Yes.

20002. But it spreads?—Yes, it has been endemic more or less all over the west of Scotland for as far back as anyone can remember, at any rate as far back as the sheep industry goes. The sheep industry in Scotland on a large scale commenced only at the beginning of the last century. Previously to that the country was all laid out in cattle; about the beginning of the last century the sheep industry on a big scale commenced, and the disease has prevailed on the west coast of Scotland ever since; in fact, long before that apparently. There are records of its being a very old disease.

20003. You say it is very destructive. Up to what percentage has it been destructive in the farms most affected?—In the case of braxy I have known from my own experience, 10 to 15, 25, 50, 80, and 100 per cent. of mortality over certain districts in certain farms. From louping-ill the mortality is usually not so high, but sometimes comes up to 15, and 20 to 25 per cent.

20004. There is a great loss from mortality, and, I suppose, great interference with business in not being able to transfer sheep?—Not only in the transference of sheep, but in the whole farming operations. The mortality interferes with the regular routine of the sheep farm.

20005. I think that you propose first to deal with louping-ill which is what you first dealt with in the Committee?—Yes.

20006. Will you tell us what steps you took in investigating that disease?—Our first experience of the disease was gained in the valley of the north Tyne, the Duke of Northumberland's property. His Grace very kindly furnished us with a station there and fitted it up for us, so that we could undertake the investigation.

20007. There are large sheep farms there?—Yes, very large sheep farms. We lived in the district for two seasons of something like two or three months each, and most of my experiments were carried out in this neighbourhood on that particular disease.

20008. Will you tell us what the symptoms of the disease are?—The first thing you notice probably is that the sheep will appear duller than its mates, and separate itself from other sheep in the same run, and it shows then more or less pronounced toxic symptoms, looks almost as if it was suffering from alcoholic intoxication sometimes, a reeling gait, giddiness, and a tendency to lean up against any support. Then perhaps in 48 to 60 hours after these symptoms have shown themselves, the animal is unable to stand, and falls over on its side. It now passes into the second stage of the disease in which it is convulsed, the limbs are spasmodically contracted, and so on. The animal's intelligence does not seem affected, it recognises objects perfectly well; but after these phenomena have lasted, say, from four to five days, in the second stage, the animal passes into the third stage in which it becomes more or less paralysed in all its extremities.

20009. Does it fall to the ground?—Yes, after it once falls over on its side it never gets up again; it is quite incapable of standing, and the limbs hang listlessly down when it is held up by the fleece.

20010. After lying in that condition for some time it dies, I suppose?—It usually goes on from bad to worse. Sometimes it may linger. I have known them linger as long as five weeks in some very chronic cases, but usually within a matter of a week to ten days after the commencement of the disease the animal is dead.

20011. Does the disease always terminate in death? —My own experience favours the view that the great majority of cases terminate fatally. Shepherds will tell you that in some cases recovery takes place; they will point out individual sheep to you which have suffered from the disease in the previous season; but my own experience leads me to doubt whether these were cases of louping-ill at all.

20012. How did you proceed to investigate the cause of the disease?—We began, of course, by studying the phenomena of the disease so far as we could make them out by mere visual and other external means of examination, and an examination of the carcases of animals that had died.

20013. What did you find?—We found so very little that was tangible in this particular disease that we for a long time were puzzled.

20014. You mean that the organs appeared to be healthy?—The organs appeared to be healthy. In the vast majority of cases, if you see the animal immediately after its death, or if you slaughter it in the height of the disease, the organs appear to be practically healthy; a few punctiform hæmorrhages along the intestine are perhaps the only residue you will find; the brain is apparently quite healthy, and the spinal cord also so far as one can make out by mere naked eye examination; the membranes of the brain and the spinal cord are healthy.

20015. And the blood?—I could never find anything wrong with the blood. I examined it over and over again. I must have examined, I daresay, thousands of samples of blood.

20016. Then having learnt nothing as to the cause from what you call your *post-mortem* examinations, did you proceed to experiment on living animals?—We did. We found it absolutely impossible to get any further before we tested whether there was any materia peccans, anything pathological in the liquids or the tissues of the body.

20017. And will you tell us what you found by experiments on living animals?—We found that we produced no result by inoculating the blood.

20018. And the cerebro spinal liquid?—Yes, the same; it had no effect, the blood and cerebro spinal liquid were negative.

20019. You found it had no effect deleterious or otherwise upon the sheep, did you?—No apparent effect whatever.

20020. Did you then proceed to investigate the abdominal cavity when you found these punctiform hæmorrhages in the intestine?—Yes. As a last resource we investigated the liquid in the peritoneal cavity, the abdominal cavity. We noticed that very often it presented a turbid appearance, especially a few hours after death, and on inoculating this subcutaneously we killed the animal practically in every case.

20021. The inoculated animal?—Yes, the inoculated animal.

20022. Did you say what you found in the peritoneum?—The bacillus, a long rod-shaped organism, was present in the peritoneal liquid; where we were perfectly certain about the nature of the disease it was found practically in every case; it had peculiar characters and the same characters in each case.

20023. Did you find it in the intestines also?—Yes, both in the abdominal cavity and in the intestine. In the intestine sometimes the bacilli were in tremendous numbers.

20024. (*Colonel Lockwood.*) Was it like the anthrax bacillus?—It is something like. It resembles the anthrax bacillus in so far as it is rod-shaped, but it has morphological differences, and grows differently from the anthrax bacillus, although probably there is a certain relationship between them. I have always had a notion that they may belong to the same family.

20025. (*Chairman.*) You injected, I understand, the liquid containing these bacilli into the sheep?—Yes.

20026. With the result as you have told us that you killed them?—Yes.

20027. You killed them more quickly than the natural disease did?—Usually much more quickly. When one inoculated under the skin, the animal usually died in from 36 to 48 hours. Some cases, however, were protracted.

20028. (*Mr. Ram.*) Did the inoculated animals show all the usual symptoms?—That depends apparently upon the time that the animal lives after inoculation has been performed. In most cases the animal dies very rapidly from the toxic poisoning; it is dead within a few hours afterwards; but in those cases where the animal has lived over, say, four or five days, I have seen the most perfect reproduction of the disease—not only in the sheep, but also in the rabbit —the tremblings and spasms, and so on, and paralysis of the limbs, all exactly as in the natural disease.

20029. (*Chairman.*) Would you tell us the conclusions that you came to form these experiments?—The conclusions that we came to were that the disease was caused by a specific bacillus inhabitating the intestine, and under certain circumstances getting over into the peritoneal cavity.

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20030. Did you come to any conclusion as to the nature of the propagation of the disease which I believe up to that time you had not ascertained?—Yes, we did; we came to the conclusion that the disease is spread through the dejecta of the animal through the manure, and also through the carelessness of farmers in leaving the carcases about everywhere.

20031. You mean that these bacilli would in that way get into the mouths of the feeding sheep?—The spores will lie in the soil for a long time apparently, and they are taken up by the second host, the second sheep, and in that way the disease is reproduced.

20032. That was the result of your investigation of louping-ill?—Yes, so far.

20033. Now as to braxy, will you tell us what your investigation was, what the course of it was?—Braxy is due to an organism of the same nature, although not identical with that of louping-ill. It was discovered in Norway originally.

20034. Was it discovered by Nielsen?—Yes.

20035. And he discovered the bacillus that you believed to be the cause of the disease?—Yes, he did, and our investigations corroborated it.

20036. Do you know how he discovered it?—By looking for it and by experimenting with it. No one thought it worth their while to look for it before.

20037. Did he experiment on living animals for the purpose?—Yes.

20038. That was how he discovered it?—Yes.

20039. And what was the habitat of that bacillus?—We discovered that the habitat is the same as that in louping-ill—that the intestine is full of it practically, absolutely swarming with the organism.

20040. The symptoms of the disease are quite different, are they not?—Yes.

20041. Is it a very short disease?—It gets the name of braxy, I believe, from an old Scandinavian derivation, signifying the suddenness of the disease—the suddenness of the fatality of the disease. Bradsot it is called in Scandanavia and our word braxy is probably a lineal descendant of the same.

20042. Does "bradsot" mean quick or sudden?—Yes, quick disease.

20043. Had it also been ascertained by Nielsen that sheep might be inoculated with the bacillus, and so take the disease?—Yes; that is so.

20044. Accordingly you began your investigations in braxy with rather more knowledge; you were more advanced in your knowledge than you were in the case of louping-ill?—Yes.

20045. But so far you seem to have satisfied yourselves in both cases; in the one by your own investigations and in the other by the investigations of Nielsen, that you have ascertained what the cause of the disease is?—Yes; certainly.

20046. And its bacillus?—Yes.

20047. Did you accept Nielsen's experiment, or did you experiment yourselves for that purpose?—We made a great many experiments.

20048. To find this bacillus?—To find this bacillus, and in the way of prevention afterwards.

20049. I am coming to that. I was dealing with the question of finding the cause?—Yes.

20050. As to remedial measures for both these diseases, would you tell us what you had to look to. Was there any remedy that you could discover when the animal once got the disease?—No; there was no remedy that we could discover. There were any number of popular remedies, but nothing that was really founded on any scientific basis, and I think they were all equally bad, so far as I know.

20051. You were obliged to look to prevention, therefore?—Yes. When once an animal is attacked with either of these diseases anything in the way of treatment, from my experience, has very little effect upon it.

20052. Could anything be done in the way of prevention by merely segregating the animals that were attacked with it?—That has been tried over and over again, shifting them from one pasturage to another. It is practised by farmers only as a last resource; shifting them, say, from hill pasture down to low pasture, and so on. My own experience is, and I have seen it done frequently, that they go from bad to worse.

20053. I rather meant as soon as the animal showed any signs of the disease, separating it so that its manure after it was diseased did not affect the pasture of others?—I do not think that has the slightest effect—the disease is so widespread.

20054. And the object, I suppose, you had was to immunise the animal?—Yes.

20055. Was that the only remedy that suggested itself to you?—It was the only feasible remedy so far. The animal dies within a few hours after it is seized by braxy, and there is no time to apply any remedy.

20056. Did you experiment with that view of obtaining immunisation?—Yes.

20057. Will you tell us about your experiments and the result of them?—We thought first that the most likely method of producing immunity in an animal would be that which is frequently practised upon animals—namely, that of the introduction into the system of the animal of the organism in an attenuated condition; in a weakened condition, that is to say, when the full virulence of the organism is lessened by certain procedures. We found that that method, although it occasioned apparently a certain amount of immunity, was a dangerous one.

20058. Why?—By no means infrequently we killed the animal, and, moreover, the carrying out of this method on a large scale in sheep-farming districts is impracticable; it entails the use of a delicate instrument in unskilled hands; and hence we gave it up altogether.

20059. In looking for some method of immunisation, had you any guide from the manner and time at which braxy attacked the animals?—Yes, we had. Braxy is essentially a disease of hogs.

20060. That is first year sheep?—Yes. Farmers look upon their two-year-olds as practically safe. Sporadically a case occurs now and again, but the great mass of their sheep after the second summer are safe. That suggested to us that immunity, if it is an acquired immunity, must be acquired through some agency residing in the soil, and manifestly the idea struck us that the spores of the organism getting into the alimentary canal, are Nature's agent in inducing this immunity.

20061. Are sheep more liable to take braxy at one time of the year than another?—Yes; there are certain months of the year in which they are more susceptible. This is just about the height of the braxy season at present; it begins usually in September—or at any rate, what farmers call braxy—and goes on to about the middle or end of February.

20062. Is it your suggestion that the sheep which took these spores into their system, and were only immunised by so doing, and not killed, took them in at a time when braxy was not prevalent, or that they were exceptionally strong sheep?—Most of them had very likely become immunised in their infancy, whilst still lambs; that is my strong impression.

20063. You mean that they would not be so susceptible?—They would not be so susceptible. Others probably enjoy a partial immunity, and are more resistant. But my strong impression is—and I am not alone in this, I have heard it from sheep farmers—that all the animals pass through a mild attack of the disease.

20064. How did you proceed to deal with it then, when you had all these facts before you?—The method of dealing with it was that we got the organism in a pure state first of all, and isolated it in a pure condition, and after that grew it artificially. We grew it from the peritoneal liquid when it was very abundant, the abdominal liquid, we may call it, and then this was administered as a drench.

20065. That means as a liquid?—Yes, poured over the throat of the animal.

20066. Administered by the mouth, you mean?—Yes.

20067. And at any particular time of the year did you do that?—Yes, at the time when sheep are practically immune from the disease, in the month of August.

20068. Have you carried out your experiments on those lines for any length of time—that is to say, having discovered the bacillus, having ascertained

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that the only mode of prevention was by administering a drench of this culture, did you experiment with this drench?—That is so; we have experimented for several years progressively—experimentally I mean—in so far as we might modify the method and study what the effect would be over a whole braxy season.

20069. And ascertaining what would be an effective dose, I suppose?—Yes, ascertaining what would be an effective dose, and in what form the bacillus ought to be administered, and so on; all these things we had to work out experimentally.

20070. At present do you consider that you have arrived at success?—Fair success, but by no means what we anticipate in the way of still further progress.

20071. I suppose all this was done by experiments upon living animals?—Yes.

20072. What animals?—Sheep, and other animals as well; rabbits and guinea-pigs, but chiefly sheep; almost entirely sheep.

20073. Do you consider that you have arrived at a true immunising remedy?—There is very good evidence to show that we have. What is defective still is apparently the method of preparing it. I think we can prepare it in the spore stage instead of the bacillary stage, and if so, there will be a much better chance of its immunising the animal.

20074. How many sheep have you administered this preventive to in the course of last year?—During August we treated, I think, some 13,000, and in the springtime, in February of this year, I think some 4,000; something like 17,000 odd have been treated this year.

20075. Are you speaking of braxy, or both braxy and louping-ill?—Both braxy and louping-ill.

20076. You treated them both in the same way?—In one line of treatment we did. A very important point was to make out experimentally whether one could immunise an animal to braxy and louping-ill at the same time, as that would mean that one could treat the animals at the same time and save the farmer gathering his sheep twice or three times a year; and these experiments are still going on.

20077. You say that you administered this drench to 17,000 sheep. I suppose you rather look upon it as medical treatment of them?—Yes, that is the view we take of it.

20078. But for the purpose of obtaining the right dose, and seeing what the effectiveness was, and so forth, you experimented upon certain sheep in the first instance?—Yes, individual sheep in small numbers.

20079. About how many?—I do not know that I could give you the figures exactly from memory.

20079A. Was it 50 or 100?—Sometimes we have used 50 or 100 sheep, or more, I dare say.

20080. Were any of those killed by the experiment?—That depended upon the nature of the experiment a good deal. In the case of subcutaneous inoculation we certainly killed a good many. There is no doubt we did.

20081. That was a line which failed?—That was a line which failed, but by administration by the mouth, if given at the proper time of the year, I do not think I can recall any death that we had from that method.

20082. Did the animals seem to take the disease to a certain extent; did they seem to be made ill by it?—Yes, sometimes they would remain ill for a day or two in the case of louping-ill. One proprietor described it as if they were all intoxicated. He thought they were all going to die, but he said, most remarkably, they all recovered, and he did not lose a single one of them.

20083. After you took to this method of administering the preventive by a drench instead of subcutaneously, there were practically none lost, I understand?—I do not think so. I do not think I can trace one. At any rate, in this present year's experiments I have had no complaints of that kind whatever.

20084. Now to what extent can you say that you have ascertained any benefit to the flocks—that is to say, to what extent, if any, do you consider that you have suppressed this disease?—Perhaps it will serve your purpose if I give you an individual instance.

20085. Yes?—A proprietor on the west coast of Scotland, who was determined that he would see the experiment carried out properly, took the very greatest pains. I may tell you that it is very difficult to get these experiments carried out as one would wish among sheep-farming people; but, being a man of education, and determined to see the thing out, he made the crucial experiment of drenching 400 sheep.

20086. Were those the whole of his first year sheep?—He had 20 others, and that comprised his whole stock of 420. They were put under exactly the same circumstances, the first lot treated against braxy, and the second lot in its natural state. Of the 400 drenched sheep he lost 30 up to the end of May, many of them, it was said, from louping-ill. I examined the peritoneal liquid from the carcases of 11 of these, and could not discover that there were any deaths from braxy, or only two, which were doubtful.

20087. Do you mean he lost 30 from all causes?—Thirty out of 400 he lost from all causes; whereas in the case of the 20 undrenched sheep he lost 19.

20088. (*Mr. Ram.*) Were they examined after they were dead?—Yes, I examined a large number—not the animals, but the liquid from the abdomen. I got the liquid from the abdomen, and had it very carefully examined.

20089. What did it show?—It showed that they died from several other diseases, and very few, if any, from braxy.

20090. Of the 30?—I was speaking of the 30. Of the others there is no doubt there was any amount of it. That has been my invariable experience. If one has a chance of comparing two lots like these, in the one case the number of braxy cases you get is something appalling.

20091. Can you tell us what proportion of the 20 were proved after death to have died of braxy?—I am sorry to say I have not got my figures here, and perhaps I should not enter too fully into that matter as yet, as I shall have to report to the Board of Agriculture first about my year's experience.

20092. Did you examine them yourself?—I did.

20093. And did you find that a considerable number of them showed symptoms of braxy?—Yes.

20094. (*Chairman.*) Do I correctly understand—I want to have it clear—that you examined the 11 that died out of the 400 that were drenched?—Yes.

20095. And you also examined the whole of those that died that had not been drenched?—No.

20096. Take first the 11, please. The 11 you did examine?—I examined as many as possible. I provided the shepherds with means of sending in the liquid, but a shepherd, as I daresay you know, is rather lax, and it is very difficult to get him to pay attention to a thing of this sort. I got as many as I could.

20097. You examined most of the 30?—Eleven of them.

20098. I did not quite understand. Did you find any trace of braxy in any of the 11 that you examined?—I think there were two cases, so far as I remember, I do not remember more, and even these were doubtful.

20099. And I think you said the whole of those that were undrenched died?—Well, 19 of them.

20100. Of those, when you say the whole died, about how many were there?—Twenty.

20101. How many of these 20 did you examine?—I really cannot tell you.

20102. Did you examine half of them, or a quarter of them?—I should say something like a quarter of them.

20103. What did you find in those cases?—In those cases I found braxy bacillus constantly occurring in the liquid sent in.

20104. What do you mean by constantly—that you found a great many bacilli in each, or you found some free of bacilli and others apparently killed by them?—Some free from the bacilli of braxy, others containing it.

20105. You mean in many instances amongst those you examined?—Yes. It is a very complex question, and I see you are a little puzzled about it. I should tell you that there are some braxy-like diseases which are continually called braxy among farming people, but these differ from braxy in the scientific sense of the term, and a certain proportion in each case, both of the 400 and of the 20 died from these braxy-like diseases, with which we had nothing to do.

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20106. It is a complicated matter, and I agree that the distinction between braxy and braxy-like diseases are too complicated for me. I want to know, did you ascertain to your satisfaction whether three dozen or so sheep out of the 20 that you seem to have examined died from the disease which your drench was intended to prevent?—Certainly a large proportion had died from braxy in the proper sense of the term.

20107. You say a large proportion of the 12?—No, of the 20.

20108. But you only examined one-quarter of the 20?—Yes; a large proportion of them did not come under my own observation. I could only take the word of the shepherds of the total mortality.

20109. As regards the remainder of the 20 that you did not actually make a *post-mortem* examination, did you see any symptoms that that was the disease that they died of?—I did not. It was a long distance off, in a wild district.

20110. Then I must ask you generally, you have given us a particular instance which, I suppose, was as good an instance as you could get for the purpose of ascertaining?—Yes, a very good one.

20111. What conclusion did you come to as to whether or not you were on the right track?—We came to the conclusion from the returns that we got, the evidence we had heard, and the examination of the peritoneal liquid, that, with certain exceptions, the mortality in those cases treated by us was considerably lower than in those which were not treated.

20112. When you say with certain exceptions, what do you mean; do you mean on certain farms?—On certain farms where, apparently, the disease that they suffered from was not braxy at all—it was very often a disease known as blackquarter, but which they call braxy very often in a loose sense of the term.

20113. Was your prophylactic intended to meet the case of blackquarter?—No, not in these particular instances that I am referring to.

20114. Now as to the experiments which you performed; were the experiments made on the farms?—They were made on the farms and also made in licensed premises that I have in Aberdeen.

20115. The others on the farms?—On the farms a large proportion—the majority, in fact.

20116. You were acting then for the Board of Agriculture, I understand?—I was.

20117. And had you a licence for performing them on a farm?—I had.

20118. I think we have heard so much evidence about the amount of pain that is caused—if it be called pain—by mere inoculation experiments, that we need not trouble you about that; but as regards the consequences when you were giving the disease to the animals as a preventive, did they seem to suffer at all from it?—Not the slightest when administered by the mouth, with the exception of their being a little upset for a day or two, and giddy occasionally, but only occasionally.

20119. You have told us roughly the number of sheep that you had to operate on in these experiments; can you tell us what the number of sheep was that died from braxy and louping-ill, on an average, in the years that your inquiry was going on—say in 1901, 1902, and 1903?—It is very difficult, of course, to say; it varies in different years, and it varies on different farms, but as a general statement you may say, as I think I mentioned before, from braxy 15, 20, 25, 50, and 80 per cent., or even total annihilation.

20120. I was not asking you the percentages; you gave us those; but is there any return made to the Board of Agriculture of the actual number of sheep—the thousands of sheep that died from these two diseases, say in the years 1901, 1902, and 1903. Did 500 or 5,000 die in a year. I do not know what 25 per cent. of the sheep in Scotland is. Sir John McFadyean tells me there are no statistics?—If you mean statistical returns, I do not think there are any.

20121. But the number of sheep fed upon these parts where braxy prevails, I suppose, amounts to many thousands?—Yes; it must be enormous.

20122. So that if your experiments were successful, even to the extent of reducing it 5 per cent., that would mean a great many more lives saved than were lost by your experiments?—Certainly; enormously so.

20123. (*Colonel Lockwood.*) All these experiments, of which you have conducted a very large number, were conducted under licence?—Yes.

20124. Were the 13,000 sheep treated experimentally or not?—No.

20125. Do you claim that before you began the experiments on the sheep you had tried any other means of investigation?—In what way? I do not understand exactly what you mean by investigation.

20126. I mean by watching their symptoms or trying to discover by any other means except experiment on a living animal?—The first thing we set ourselves was to examine the symptoms and note those with the very greatest care, the *post-mortem* changes, and so on, but as I have explained in evidence already, it was always too late, we could not get any further with the inquiry, and it was necessary to resort to experiment to elicit further knowledge.

20127. Do you claim that by those experiments you have obtained any satisfactory results?—I think we have got extremely satisfactory results. Were it only that we now know something definitely about the nature of the diseases which stimulated inquiry, it would be an enormous result. But I think we have got more than that, I think we are on the lines calculated to prevent the diseases.

20128. But you do not claim yet to have discovered any cure?—I do not think, as I explained before, that you can cure these diseases. I have never seen anything that did the animal any good when once it was attacked by braxy.

20129. Do you claim that these experiments of yours have produced a preventive?—I do.

20130. (*Sir William Church.*) Is anything yet known of the life of these bacilli outside the body?—There is a great deal known about them.

20131. I meant whether any experiments had been made with soil or any form of vegetation to see whether they can maintain their life?—That has all been worked out. One of the original ideas, I remember, so far back as 1882, when I began working at these diseases, was that there was something contained in the soil or the nature of herbage, which was the cause of them. The Highland and Agricultural Society appointed a Committee to investigate that among other things, with the result that nothing of the kind which you refer to has been discovered from the herbage point of view.

20132. I meant that at present you do not know whether this bacillus continues to live and grow, and probably go through the state of sporification in the soil?—It is voided in the soil; the spores are voided into the soil through the manure, and through the carcases of the animals, in enormous numbers, and I know this, so far by experiment, that I have kept such spores in their natural habitat, in the natural liquid of the body, for from five to six years in full virulence. I might illustrate this by an experiment which we performed over and over again, and which bears upon the question you have asked me, that of taking a sheep from what we call a "clean" farm, where there is no disease known, louping-ill let us say, where they are perfectly free from it, and bringing these sheep on to a louping-ill field or a braxy field, you will find that two-thirds of them will be dead—if it is the proper season of the year—within ten days afterwards.

20133. And that, of course, is your reason for thinking that it is impracticable by clearing the ground for a certain time of sheep to get rid of the disease?—I will not say that. I will not go so far as that. If you could put cattle on for something like a matter of 50 years or 100 years, I should not wonder (Sir John McFadyean will, perhaps, bear me out) that the ground might be cleared under those circumstances.

20134. I should like to be quite clear about your experiments—not as to the cases treated medicinally, but your experiments. I understand that of those that were drenched 30 died, and some of those you examined and verified yourself, by finding the bacillus, that they died of braxy?—In the undrenched ones.

20135. Out of the number all of which died, you personally only examined a few of the bodies?—A few of the animals. The drenched ones were sent in to me especially, because I had given instructions that they were to be sent in; the others, the undrenched ones, were sent in more at the will and wish of the proprietor.

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20136. But in the case of the 20, 19 of which died, those who were present and observing them during their illness thought it was braxy?—Yes.

20137. And by saying that you do not know that they died of braxy, you simply mean that you had not an opportunity of verifying it by finding the organism?—That is so; they were quite of opinion that they died of braxy.

20138. (*Sir William Collins.*) May I take it that the evidence that you have been so good as to give the Commission this morning is offered by the University of Aberdeen as an example of the valuable results which may be obtained by experiments upon living animals?—That is so; the Medical Faculty thought that perhaps you had not had so much evidence of this kind as bearing upon the brute creation directly, and that possibly it might be acceptable to you on that ground.

20139. Did I correctly understand from you that there are several contagious diseases of sheep, between which it is necessary to discriminate?—Yes, that is so.

20140. How did you find the organism in the case of braxy; was it by microscopical examination of the intra-peritoneal fluid?—You find it in braxy all over the body, especially in the peritoneal cavity. If there is any part of the body where you find it, it is in the peritoneal cavity.

20141. By microscopical examination?—Yes.

20142. Did you cultivate it on various media?—Yes.

20143. Are the other diseases from which you consider it necessary to discriminate braxy, those of blackquarter, malignant œdema, and louping-ill?—That is so, these are among them.

20144. May I take it that you have the means by bacteriological investigation of discriminating accurately between those four diseases?—I think so. They belong to a family, doubtless, and they have certain points in common, but by the method of culture and one thing and another, we can distinguish them with great readiness. I will not go so far as to say that there do not occur exceptional cases where I should be in doubt, but usually by means of prolonged culture and inoculation more particularly, they can be differentiated.

20145. Should I be right in saying that an expert ought in any case to be able to discriminate accurately between louping-ill, braxy, blackquarter, and malignant œdema?—I think so.

20146. Did I correctly understand you to suggest some possible relationship between braxy and anthrax?—I should not wonder if they belong to the same family. I will not say that it has anything to do with anthrax, or any anthracoid diseases; but in the manner of growth, and one thing and another, it has in some respects a resemblance to anthrax.

20147. If these be four separate diseases, should I be right in thinking that it would be necessary to employ different methods of protection against each of them?—I have tried protecting against the whole of them simultaneously.

20148. Has it been successful?—I do not know yet; it is being tried this year.

20149. Do you mean by administering some polyvalent dose?—Yes.

20150. Would that be mixing the preventive fluids against the four diseases?—Yes.

20151. But would a preventive fluid that was good against braxy be good against blackquarter?—No.

20152. Or *vice versâ*?—No. I do not think so, at any rate, my experience leads me to suppose that it would not be.

20153. You think that a prophylactic which was found to be good against blackquarter, could not be relied upon as protective against any of the other three?—I think so.

20154. There has been some confusion, has there not, in the minds of some investigators, as to these four diseases which you regard as distinct?—There is a very great deal of confusion. I think the matter wants clearing up.

20155. Has your attention been called to an outbreak that occurred on Romney Marsh?—Yes.

20156. What was the nature of that disease?—It was what that call "struck."

20157. Was that a case of braxy?—No, not what I saw of it, at any rate; the cases I saw of it were not braxy certainly. Everything else points to its not being braxy.

20158. Was it blackquarter?—I do not know; I have not had sufficient experience of it. I have not seen a sufficient number of cases. I went down to Romney Marsh and investigated it there as well as I could, but I have not had sufficient experience to say whether it is blackquarter or not.

20159. I see on page 9 of Part I. of your Report it is stated: "The disease has been pronounced by Principal McFadyean to be blackquarter"?—I do not know whether Sir John is of the same opinion still; but it is quite possible that it is blackquarter. I do not know, I would not like to say that it is not, possibly it is; only it did not look to me as if it was blackquarter.

20160. After considering that suggestion, I see on page 10 of your Report the Committee say: "The question of louping-ill being blackquarter in the sheep may, therefore, be put out of court; the two diseases are opposed to each other in all essential respects"?—Yes, I think the view is quite untenable.

20161. Then I see on page 27 of the same Report you say, "We must sound a note of warning, however, as to the result to be anticipated"; that is in regard to preventive treatment?—Yes.

20162. "Let it be clearly understood that our treatment is projected against louping-ill and nothing else. One of the first objects to be ascertained is whether the mortality upon a particular farm is caused by louping-ill or by one or more of the many other contagious diseases of the sheep which are so often associated with louping-ill and mistaken for it. Thus, we have found that malignant œdema prevails in many districts, and has been confounded with louping-ill over and over again, even by the most experienced sheep-farmers and shepherds. This is quite excusable, seeing that the symptoms of malignant œdema might easily be mistaken for those of acute louping-ill. Then stray cases of braxy and of the braxy-like diseases occur among sheep during the louping-ill season, and particularly among those which have been wintered on the east coast of Scotland, and which have returned in the spring to their native pastures in the West Highlands. The confounding of these with cases of louping-ill, of course, tends to vitiate any results obtained by our method of prevention." They think it is necessary to draw attention to that?—Yes, we discovered that frequently.

20163. Then I see you go on to say, "The problem of the prevention of the contagious diseases of the sheep is a large one, and will most likely require years of patient observation and experiment before it will reach perfection. Those which are most destructive in this country are closely related, and hence there is good reason for believing that they may be combated upon the same principles as those which have proved so effectual in the case of louping-ill. It may turn out that the sheep can be immunised to several of them at the same time." Those are the experiments you were referring to just now, I suppose?—Yes.

20164. You say, "In the meantime, however, we would beg those interested to exert a little patience, and not to draw conclusions, at present, from what may seem to them, in certain instances, a disappointing result. The pathology and prevention of this class of diseases of the sheep is far more intricate than the ordinary layman may suppose, but we feel assured that we are on the path which will ultimately lead to a successful issue"?—Yes, that is so.

20165. Am I right in gathering from that that you regard the matter as still *sub judice*?—Certainly, the matter of treatment or prevention.

20166. I think you called attention in your second report to the curious bactericidal power of sheep's blood?—Yes, that is so.

20167. Which I think you say varies in different seasons of the year?—Yes.

20168. That, I suppose, would introduce a rather complicating factor in any preventive treatment?—Most complicating.

20169. You state, I see, in the second part of your report, on page 239, among your conclusions, "The blood of the sheep at certain seasons of the year is eminently bactericidal to certain disease microbes which are the causes of most of its contagious diseases, while at other seasons it loses this property, more or

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less completely, and becomes an excellent medium for their propagation?—Yes.

20170. That must be rather a confusing factor?—It is a most confusing element; it vitiates experiments; experiments may be negative or positive, according to the season at which they are performed.

20171. Then I see on pages 201 and 202 of your second part you say, "This, however, seems fairly clear, namely, that animals in good condition are more liable to braxy than those poorly nourished. But even in this respect there are endless sources of fallacy. Is it, strictly speaking, braxy that these well-fed animals die from, or is it something else which has been mistaken for braxy? Is it so-called 'blackquarter,' for instance, or is it the disease known as 'struck' or, finally, is it a disease hitherto unrecognised? To commit one's self to such untested statements would be productive only of further confusion where confusion is already rife." Should I be right in thinking from that that it may turn out, after all, that blackquarter and braxy are the same?—No. The term "braxy" in the bucholic mind is one applied with a very wide margin; a shepherd will tell you that there are all kinds of braxies, red braxy, white braxy, water braxy, and so on, these all representing different diseases.

20172. I see you proceed on the same page to say, "All these crude and untutored notions seemed, in our estimation, quite inadequate to account for the seasonal character of the contagious diseases of the sheep. We felt there must be something deeper, probably something peculiar in the constitution of the sheep, in all likelihood something reaching far back in the animal's ancestral history which was at the foundation of this seasonal peculiarity"?—That is so.

20173. Some constitutional peculiarities, I apprehend, irrespective of any microbic invasion?—Yes, quite.

20174. Have you formed any opinion as to the method of communication of the disease from sheep to sheep, whether by tick or otherwise?—I have.

20175. Would you be so good as to state it?—We made a great many experiments with the tick to see whether we could communicate the disease through the bite of the tick.

20176. (*Chairman.*) Which disease are you speaking of?—Louping-ill. There was a prevalent notion that louping-ill might be conveyed by the bite of the tick, probably from the fact that the tick appears parasitically in the sheep at the same time as louping-ill, both in the spring months, and accordingly we had to see what truth there was in that opinion. I may say that in some of the most pronounced cases of louping-ill that we saw the sheep had not a single tick upon it, nor a single tick bite. Others were covered with ticks, but never did we see any evidence of the virus being inoculated from the point where the tick had bitten. We showed experimentally that if you tattooed the skin of the sheep with a little of the virus of louping-ill you could produce louping-ill, and you produced a local lesion, a lesion very much like that found in blackquarter sometimes; but never in any instance did we see the disease spread from a tick bite as in the case of this superficial scarification.

20177. (*Sir William Collins.*) You think the tick may be dismissed then as the medium of conveyance of the disease?—No, there is one method by which I think it may under rare circumstances (but so rare that it may almost be put out of account), become the means of conveyance, namely, when the tick is swallowed. The tick crawls all over the soiled parts of the fleece, and becomes smeared with the fæcal matter and so on, from the moist parts of the skin, and under the circumstances I can quite well conceive that it might convey the disease when it is swallowed by the sheep.

20178. Then the evidence points to the fact that the disease is communicated by ingestion rather than by inoculation?—Certainly.

20179. In your earlier observations did you find that the slaughter of the animal in the earlier stages of the disease was the best mode of finding the cause or by allowing the animal to die and examining the carcase?—By allowing the animal to die; but never in any instance when I examined the peritoneal liquid in an animal killed during the height of the disease, did I fail to recover from the peritoneal liquid by incubation the same bacillus as that which I found so constantly.

20180. I see on page 51 of the second part of your Report you say, "We discovered, however, as time went on, that the killing of the animal was an entirely mistaken procedure"?—Yes, if you want to investigate the disease.

20181. May I ask what was the earliest date of the administration of drenching?—I think there were some preliminary experiments which we made (I am open to correction) in the years 1903 and 1904. 1903, I think it would be, when we begun.

20182. When did you first try it on a large scale?—We went on increasing the number year by year up to the present time.

20183. Have you formed any idea as to the duration of the alleged immunity?—In braxy it seems to last the life-time in the majority of cases. As in the case of small-pox, you will find exceptions here and there; but in the case of braxy I think it will last the natural life, or the commercial life of the animal, at any rate—quite sufficiently long for commercial purposes. I am not so sure with louping-ill whether that is so. Two-year-old sheep take the disease, and it is said sometimes take it twice. I do not think, in the case of louping-ill, it is quite so lasting, although it tides the animal over the louping-ill season, and that is the great point.

20184. When you speak of the duration of a lifetime, what period have you in your mind?—From two to two and a half years, that is to say, in the case of an animal for slaughter.

20185. Referring to the particular case of the 420, when the 400 were drenched and 20 were not drenched, has it been the subject of an official report?—Not yet; we are getting the report ready. It has been a most arduous matter to procure all the details of such a report, and we are trying to render it as accurate as possible, to sift out the kernel of truth from the many statements sent into us; it will be presented to the Board of Agriculture before many weeks. I have it nearly ready.

20186. I was wondering whether it was in a form in which you could favour the Commission with it?—I do not think so. I must present it to the Board of Agriculture first.

20187. I did not quite gather your evidence in regard to the 30 out of the 400 that died. I understood you to say that they died of various things, including accidents?—Yes, various things.

20188. In the case of how many of those 30 was the peritoneal fluid presented to you for examination?—I have not got my note-book with me bearing upon that particular subject; but I think I made up the number when I got the details returned to me to be eleven.

20189. Including those cases by drowning?—I do not know. If you take the total mortality you must always leave a margin for drowning—and all sorts of accidents.

20190. But I understood that the 30 included all these casualties?—Yes.

20191. I was wondering how many of the 30 were submitted to you for examination?—Eleven.

20192. Were there any casualties among the 30?—I cannot tell you definitely. I would not like to commit myself upon a matter of that kind. I cannot remember at present.

20193. Do you think that you examined one fourth, as I understood you to say to the noble Chairman, of the 30 that were undrenched and died?—I would not like to say. I could not commit myself to any figures definitely. I am only speaking from memory.

20194. You prefer not to commit yourself to any precise figures?—If you please. I must present them to the Board of Agriculture first. I would like to have my figures before me before I say anything further about it.

20195. (*Sir John McFadyean.*) These investigations of yours have been followed with great interest, I understand, by the agricultural community of Scotland?—They have.

20196. And so far as you know the line of investigation that has been pursued has had their approval?—Yes, it has had their approval with certain failures in certain districts.

20197. I was not referring to that. I mean that they do not disapprove of the fact that you have attempted to solve the causation and method of prevention of these diseases by resorting to experiments?—Very far from it.

20198. Is there an Anti-Vivisection Society located in Scotland, do you know?—I think there are some ladies inclined that way.

20199. Have they formed themselves or been actually constituted into a society?—I do not think so.

20200. So that no opposition that you are aware of has been raised to your experiments?—No combined opposition. I think many persons hold very strong views upon the subject, and they may have individually complained. I do not think there is any society of that kind, so far as I know.

20201. It has been suggested here by various witnesses that experiments on animals with a view to extend knowledge with regard to human or animal disease ought to be absolutely prohibited by law. What do you think would be the general feeling of agriculturists in Scotland with regard to such a proposal?—I would not like to speak for the agricultural community, but from conversation and wide experience that I have had with intelligent farmers, I think it would be perfectly disastrous; it would raise a *furore* among the intelligent agricultural population were you to attempt anything of the kind. That is my impression.

20202. You are pretty well in touch with agriculturists?—Yes, I have seen a great deal of those interested in sheep farming at any rate.

20203. (*Sir Mackenzie Chalmers.*) Do I correctly understand that you in giving evidence to-day represent the University of Aberdeen or the Medical Faculty of Aberdeen?—Through the Medical Faculty you may say I represent the Aberdeen University. I do not think the matter came before the Senatus, but we had it up at the Faculty and discussed it there on two separate occasions.

20204. May I ask if the Faculty are unanimous or not on the subject that experiments on living animals are necessary?—Perfectly unanimous.

20205. I take it as regards these experiments that you performed that your licence enabled you to experiment on unspecified farms, that is to say, where the disease was, as well as in a laboratory?—Yes, what is called a travelling licence.

20206. Without a travelling licence could you have effectively experimented?—I could not; I should have been breaking the law had I done so.

20207. When you are dealing with animal diseases spread about over the country, is it essential that the experiments should be performed where the disease is found?—Absolutly essential. I would not trust to laboratory experiments exclusively on a matter of that kind. My experience is dead against that line of inquiry being final.

20208. Would there be this further danger that you would have to bring infected animals from a long distance to the laboratory, which would in itself be a danger?—We would not be allowed to transport the animal.

20209. I may take it that your opinion generally, and the opinion of your Faculty, is that animal experimentation is a necessity if any progress is to be made?—Most certainly; I think that is their unanimous opinion, so far as I am able to speak for the Faculty.

20210. Is there any opposition in Aberdeen itself to animal experimentation?—Very little.

20211. Is that within the University or outside?—Even outside.

20212. (*Mr. Ram.*) You have told us that you discovered the nature and the cause of these two diseases that you were experimenting for by administering to living animals a portion of the fluid from the intestinal tracts of diseased animals?—Yes, and from the abdominal cavity also.

20213. You had, before you tried that, been endeavouring for a considerable time, I believe, to ascertain the nature and cause of the diseases?—That is so.

20214. And so far as investigation of dead animals went, had you failed to discover them?—Certainly.

20215. In your opinion would it have been possible that you could have discovered what you have otherwise than by making use of living animals in the way you have described?—I do not think so.

20216. At any rate you had for years been endeavouring to ascertain the cause without success?—I will not say for years. Two sets of observations were going on relatively, observation of symptoms and *post-mortem* phenomena, and so on.

20217. And for how long have you been endeavouring to deal with the disease?—It is very hard to specify any time, but in the case of louping-ill you may say for three months—I do not know now I could express it to give you an idea. The two things were going on very much at the same time, although experimentation came on after we had discovered that there was something to work with.

20218. Then it was not, as I imagined your evidence to be, that you had been searching for years, but you had been searching for a considerable time?—I would not say for years.

20219. I think you told us that you were first put upon the idea of immunising the animal from finding in the case of braxy that a sheep which had survived the first year was practically free from the disease?—Yes.

20220. Therefore in seeking to immunise the animals you were following the law that nature gave you in that respect?—Yes, we were.

20221. Is there any official return of the number of sheep kept in Scotland year by year?—I really do not know; I do not think so.

20222. There is none that you know of?—No.

20223. Is there anything by which you can say that the disease has been diminished greatly, either louping-ill or braxy, throughout Scotland in the last year or so?—Nothing whatever, so far as I know, except mere local evidence—nothing on a general scale affecting the whole of Scotland.

20224. You gave us an account of 350 or 400 sheep, some of which were drenched and some were not. Were there other experiments made in other parts of the country besides that one that you told us of?—Yes.

20225. Have you had any result of such other experiments?—We have.

20226. Do they generally give the same result that you told us attended that 400?—With certain exceptions here and there on particular farms and in particular districts, the returns, so far as braxy is concerned, have shown that the mortality in the drenched is very considerably lower than in the undrenched.

20227. I gather, then, that you think you have yet much to learn?—We have a very great deal to learn, I consider.

20228. Do you think that what you have learned in consequence of your investigations has been on the right lines?—Yes.

20229. And you hope that you may yet learn so much more as to effectively combat the disease generally, and what I may call commercially?—I hope so. I am thoroughly confident that we will do so.

20230. Do you find that farmers generally willingly lend themselves to this attempt to immunise their sheep by drenching?—I have not had the slightest difficulty. I have had every encouragement from farmers. I say it to their credit that they behaved extremely well under the circumstances.

20231. Is the method of administering the immunising material by drenching a process which can be done by a less skilled person than would be necessary for inoculation?—Yes, a shepherd could do it perfectly well.

20232. A shepherd, of course, is continually practising drenching?—Yes.

20233. Lastly, may I ask you this: When you were carrying on your examination to test the nature of the disease, did all the animals that showed symptoms of being contaminated with the disease show precisely the symptoms of the disease that you expected to find from the toxin you administered?—Yes, with this qualification—that in the case of louping-ill if you inoculate subcutaneously, usually the virus is so active that it kills the animal before the chronic symptoms come on; but should the animal live over a matter of four or five days, then it presents a picture of the disease. And in the case of braxy all the points are brought out.

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20234. What I particularly wish to arrive at is this: Was the disease the symptoms of which they exhibited the specific disease that you expected to find, and not an analogous disease or a disease that might be mistaken for it?—Not only was the disease the same, but we recovered the same organism out of the tissues.

20235. (*Dr. Gaskell.*) I should like to be quite clear about this matter. I understand that your method of immunisation is to put into the alimenary canal the spores that are cultivated artificially?—We have not worked with the spores yet—with the bacilli.

20236. Is that the same thing as putting into the alimentary canal the same bacillus as the animal itself would take by feeding?—That is so.

20237. There is no attenuation caused by the method of cultivation?—Not to any extent. In the natural disease exposure to weather may have the effect of attenuating the organism to a certain extent.

20238. Then your method, I presume, is to give a mild form of the disease?—Yes.

20239. And so to cause immunity to it?—To give it at the time of year at which the animal is not subject to it.

20240. That is the whole point, is it, to give it at a time of year when the animal is not naturally susceptible to the disease?—Yes.

20241. And so to cause immunity?—Yes.

20242. If you gave it at other times of the year it would not be efficacious?—You would kill them in great numbers.

20243. You say that you tried inoculation experiments?—Yes.

20244. And you tried the method of attenuating in those inoculation experiments?—Yes.

20245. Did you at that time know the difference of the times of the year?—We knew that the particular diseases occurred in the sheep at different times of the year, but we had none of the specific knowledge that we gained afterwards through further experimentation.

20246. May I gather that those inoculations were not given definitely at the time of year when the animal was less susceptible?—Some were and some were not.

20247. Can you say from any records that you have whether there was any difference at all?—I remember that the first inoculations made with louping-ill were administered at the end of the louping-ill season—let us say towards the middle of June; but always afterwards, so far as I remember, we endeavoured to make the experiments during the time that the animal was not subject to the disease, with this exception—that I remember one instance, now that you ask the question, in which we made a subcutaneous inoculation of braxy during the month of November—in the middle of November—with the result that we killed nearly half of the animals, I think, that were experimented on.

20248. Do you suppose, then, that the failure of the inoculation experiments and the success of the feeding experiments were due to the activity of the mucous membrane in the alimentary canal, or something of that kind?—That is one of the deepest questions, I think, in the whole of biology. How these particular organisms act is a matter so complex that I really prefer not to enter upon it.

20249. What I want to be clear about is—are we to understand that the failure of your inoculation experiments and the success of the feeding experiments tend in any way against the value of inoculation experiments in other diseases?—Oh, no. I think you quite mistake what I said about the failure of inoculation experiments. They confer undoubtedly a certain immunity—there is no doubt of that; we proved that; but the difficulty was in the administration of the system in unskilled hands. It means that you would have to provide a skilled person to make the inoculations, that you could not trust shepherds to do this, and in that respect (a very formidable respect, of course) the method of subcutaneous inoculation naturally fell into disuse.

20250. There is just one other question I should like to ask you with respect to your University. Are you authorised to say whether the University desire the existing Act to continue, or whether they would like to see it modified in any way?—I cannot answer for the University as a whole, but I can so far answer for the Medical Faculty. It is my opinion, and I think it is the opinion of the Medical Faculty, that they would desire the existing Act to continue. I think, so far as I know, we are satisfied with the existing Act. I would not like to speak specifically for my colleagues, but I know that those with whom I have talked are quite well satisfied with the present Act and its administration, and we would have no objection to go on under the same circumstances.

(*After a short adjournment.*)

20250A. (*Dr. Wilson.*) With regard to all these allied diseases, the diseases that you provisionally designate A and B, as well as braxy and louping-ill, you would call them, I suppose, all bacterial infectious diseases?—Contagious diseases.

20251. Would you call them acute febrile diseases? Have you taken the temperature?—Yes, they are acute febrile diseases.

20252. So that, as regards the febrile symptoms, braxy especially bears some resemblance to anthrax?—In some respects it does.

20253. As regards temperature?—Yes, the temperature goes up very high sometimes. It may go up to 105, 107, and 108 sometimes.

20254. And during life have you ever been able to discover what I call the braxy bacillus in the blood?—We found it immediately after death.

20255. But during life?—No.

20256. Then in that respect it differs very materially from anthrax?—Yes, entirely; it is not primarily a blood disease.

20257. With respect to anthrax, is it not very essential that a specimen of the blood should be taken immediately after death has taken place; because it is contended that other bacteria invade the carcase?—If you mean in the case of anthrax, in my opinion, the blood should be taken immediately after death, for several reasons.

20258. Otherwise you could not depend upon the bacteriological diagnosis of the case?—The organism is apt to disappear some time after death. I should not say very apt, but it sometimes vanishes from the blood.

20259. Now, with regard to this bacillus in the intraperitoneal fluid, I take it from your Report that you never find it when the fluid is clear?—You do not find it microscopically in the case of louping-ill; usually if you kill the animal you have great difficulty in identifying the bacillus by mere microscopical examination; but if you incubate the liquid, I have never failed to recover it within a few hours after the incubation has commenced.

20260. Whether the liquid has been taken from a living animal or from a dead animal?—Yes.

20261. But you state also, I think, that you always find it microscopically when the peritoneal fluid presents an opaque appearance?—I do not know that I have ever failed to find it in such cases; and that has been strengthened by later evidence during the last 18 months or so.

20262. But would you not say that even during life, when the intra peritoneal fluid assumes that milky or opaque appearance, pyæmic changes had set up?—No, there is no evidence of pyæmia in the slightest degree.

20263. But when you get the peritoneal fluid of a milky or opaque appearance in ordinary cases of disease, would you not say that pyæmic changes had set up?—Oh, no, not necessarily.

20264. Not if you had febrile symptoms along with the appearance of the disease?—Not necessarily. It might be so. If you had a case of septicæmia to deal with you would very likely find a septic organism growing in the peritoneal fluid after death.

20265. But during life?—Yes, and even during life—in septic peritonitis or peritonitis from any cause.

20266. Then you get a milky or opaque appearance?—Yes, but that of itself would not be sufficient to diagnose what the milky or opaque appearance was due to, it may be due to difference causes.

20267. May I ask what interval of time usually elapses before you receive any of these specimens?—It varies very much; we try to get them as soon after death as possible. A great many cases were received, as I have stated in the case of louping-ill, immediately after death, and, in the cases of braxy also, but with braxy it is more difficult. I think about the longest interval that I remember intervening between the death of the animal and our examination of the carcase was something like a couple of days; I do not think more than that. But I have received braxy cases just as they dropped down dead—within a few minutes after the animal had died.

20268. You admit that all these similar diseases from clinical symptoms or ordinary observations are more or less allied, and it is very difficult to distinguish them?—Sometimes it is extremely difficult. I have known experts in the veterinary profession and among farmers, who have been deceived in cases which they thought they were perfectly certain about.

20269. Does it not sometimes happen that in passing the virus from animal to animal, with the specific bacillus, say, the appearance of that bacillus is very much modified?—I do not think so—not in the case of any of these diseases, at any rate, that we are dealing with at present? It comes out exactly the same as you introduce it. You will find in the peritoneal liquid or in the serus liquid of the inoculated lamb, or in the blood, the same bacillus as that which you inoculated, giving all the reactions of the bacillus that you have inoculated, and more vigorous usually than it was previously.

20270. So that from the appearance of the bacillus, microscopically and culturally, you contend that these diseases are essentially distinct diseases, notwithstanding their similarity?—Yes, and by inoculation.

20271. But in inoculating you state that very often with braxy the animal dies very suddenly?—Yes, it dies very suddenly.

20272. In those cases of sudden death, supposing that a rabbit has been used, could you distinguish that from death caused by, for example, in testing for ptomaines?—I do not know that the two cases are exactly parallel. A ptomaine is a very different thing; it is not a living poison. I do not think you could exactly draw a comparison.

20273. Except that there might be a sudden death of the animal in both cases?—Both may kill the animal very rapidly; but you can introduce the pure spores of any of these diseases mentioned in our Report, and you get the same organism out again in the animal which you inoculate and in a pure condition.

20274. But I am referring to other symptoms. If you inoculate them with the organisms the tissues of the animal will make a cultivating medium always?—Not always, very far from it sometimes.

20275. I mean in dead animals?—Yes, but in louping-ill sometimes you will find the result is the very opposite to what you suppose.

20276. They vary from different conditions of the blood, and so on?—Yes.

20277. It all depends upon that?—Yes.

20278. But you do find, as a rule, that if you inoculate into the peritoneum the bacilli thrive as in a medium culture?—Not always, sometimes. It depends upon the nature of the experiment a great deal and the nature of the organism that you introduce. I will undertake to say that at certain times of the year you can introduce almost any of the organisms in question subcutaneously into sheep, and will hardly kill the animal in a single instance.

20279. Referring again to the instance of louping-ill, that is jumping-ill?—Yes, louping is an old Saxon word: *chorea paralytica ovis.*

20280. You state that so long as the lambs are being suckled as a rule they do not take the disease?—Sometimes they do, but as a rule they do not. I have seen both mother and lamb affected with the same disease.

20281. Does that apply more to braxy? Do lambs suffer from braxy in the suckling period?—Braxy occurs at the time of the year when they have ceased to be suckled.

20282. That would show that lambs are more or less immune, would it not?—Oh, no. Hogs become more or less immune, but the lambs are very far from being immune.

20283. But they do not take it, you mean to say?—They do take it. *Mr. D. J. Hamilton.*

20284. What, braxy?—Yes, they take braxy. Not while suckling; they are away from their mothers at that time, weaned. 17 Dec. 1907.

20285. Would not that be the time to administer your prophylactic? Have you tried feeding lambs with your prophylactic to see what the result may be?—Yes, August is the best month. We have tried all the months of the year, but August we find the best.

20286. At that time, before the lambs are weaned?—Weaning time is most convenient for another reason, because you have to consider the convenience of the farmer. August is most suitable, because he is weaning them, and he then gets them together.

20287. With regard to that lamb to which you refer in your Report, in whose *post-mortem*, if I may say so, you first discovered the habitat of the bacillus, was that a weaned lamb?—No, it was not; it was at the time of year when it was still following the mother.

20288. After the louping-ill?—Yes.

20289. Was it in an emaciated condition?—No.

20290. So that you could not say that it was improperly fed in any way?—Not in the very slightest degree.

20291. Do you think that these diseases are influenced at all by the amount of food which the sheep may be able to obtain at times?—In the case of braxy the better fed they are the more liable they are to it. I do not think that is the case in louping-ill, but in the case of braxy it appears to be the case. It is always the best member of the flock that is affected.

20292. Is there any relation between outbreaks of the disease and very severe wintry weather, for example?—There appears to be a certain relationship, but I do not know whether it is a direct one. It is always said that on a frosty morning animals are more liable to die from braxy. I expect that is more from the deteriorating influence the frosty weather has upon the general condition of the animal. Any change in the weather is said to favour braxy. I have seen the very worst outbreaks, however, in the most beautiful weather; but I think it may be admitted that any sudden change in the weather increases the mortality.

20293. Have you any idea of the incubation period of the disease?—It is almost impossible to say in the case of braxy. From the fact that you very seldom see the animal before it is advanced in the disease, or, at any rate, for some hours after the disease has commenced. But in the case of inoculation experiments the usual period is something like 24 or 25 hours. I expect in natural braxy it may be a little longer.

20294. In the Western Highlands, the South of Scotland, and the North of England, where the disease is more or less prevalent, they have these old sheepfolds, have they not?—Yes.

20295. Do you think that they influence the spread of the disease at all?—Practically on an infected farm they have the disease everywhere.

20296. But these folds are never changed?—Very seldom.

20297. Most of them are made of stone walls?—Yes, they are.

20298. So that during very severe and stormy weather, when the sheep are huddled up together in these pens or folds, you would call those conditions more or less insanitary?—But I do not think they huddle them up in severe weather at all. That is the very time that they do not huddle them up.

20299. But sheep must take shelter in the course of a storm?—No, they do not.

20300. What are the folds for, then?—For gathering in dipping time, mostly.

20301. Not for shelter?—No, the sheep lives out in the open unless it is buried in the snow, and then they dig it out—it is only in dipping time that they are brought in, or for castration, or anything of that sort.

20302. You make some reference in your *précis* to the influence of the Gulf Stream, but are you still of opinion that it can have any influence in the spread of these diseases?—It is quite likely that it may be a fostering influence.

20303. But braxy is also very prevalent in Iceland?—Yes.

20304. And in Norway?—Yes.

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20305. And in certain parts of New Zealand?—I believe so, although I have no direct evidence bearing on the allegation.

20306. So that the Gulf Stream influence is not of general application?—I do not know; it comes pretty close to it. The waters of the Gulf Stream, as I understand, pervade directly or indirectly all round these shores.

20307. Do you think it is felt as far north as Iceland and the coast of Norway?—Indirectly, at any rate.

20308. You also state, I think, that even although the bacilli of these diseases may be found in the intestines, yet when the sheep are moved down into the lowlands, to be fed, say, on turnips or pasture, they never fall with braxy or louping-ill?—I would not say never.

20309. Well, hardly ever, I was going to say?—Much less often.

20310. It is seldom that cases of braxy or louping-ill crop up amongst the sheep when they are being fed on turnips?—Comparatively seldom, but I have seen it.

20311. Of course, you have found that inoculating with the bacilli, the vaccine, in braxy or louping-ill is attended with a considerable amount of risk?—Yes, it is.

20312. On account of the mortality?—Yes.

20313. But that in administering these bacilli by their mouth there is little or no risk at all?—If it is done at the proper time of the year.

20314. Have you made extensive experiments, say, during and before weaning amongst lambs, to ascertain whether that would have any influence upon it then?—That is the very time when it ought to be done, any time up to weaning time. That is the time in which the sheep is immune from practically all these diseases.

20315. I am referring to lambs. Do you feed the lambs with the prophylactic before weaning?—No, we cannot; they are too young, and they cannot be gathered.

20316. Does this prophylactic that you call drenching, scour or produce diarrhœa?—I have seen it produce a little diarrhœa sometimes, but not as a rule.

20317. In the natural disease, in louping-ill, or in braxy, do the sheep suffer from diarrhœa?—Sometimes they do.

20318. Then you would expect a considerable increase of bacilli, such as the coli bacilli, in the intestines if the sheep was suffering from diarrhœa?—Quite likely. It would depend greatly upon what the diarrhœa was due to, of course. The sheep is remarkable in this respect, that its intestine contains so few bacteria of any kind.

20319. I have seen it stated that in the intestines of animals in the Arctic regions no bacilli are discoverable; is that your experience?—I have no experience of this. I do not know. I know from examination of healthy sheep that the amount of bacteria found in the intestine is comparatively slight.

20320. Is not this prophylactic mode of yours unique in the bacteriological treatment of disease. I mean you do not know any other bacteriological disease, or bacterial disease, for which a prophylactic is used in this way?—The old method of drenching is not a new one by any means. It has been applied in veterinary practice for long.

20321. I mean to drench with a preparation or culture of bacillus?—The drenching has usually been performed, so far as I know, with some of the liquids of the body of the animal suffering from the disease, as in pleuro-pneumonia, for instance.

20322. Such method has not been successful?—The method of drenching has been employed for the prevention of pleuro-pneumonia of cattle, for instance, but I do not know that it has been applied to these diseases of sheep, nor in the particular way in which we employed it, by the method that I have described.

20323. Is not this drenching method applied in similar diseases in other animals?—I will not say that drenching has not been a method in use, but it has been applied empirically.

20324. But specific drenching?—I do not think so, but I would not like to be positive on the matter. Of late the alimentary canal has been looked upon as the source from which one can treat several contagious diseases and produce immunity, and much more so than formerly.

20325. In cholera, as of course you have heard, the cholera bacilli can be swallowed with impunity, and have been swallowed with impunity?—Yes; but I would not like to try it.

20326. I think you make reference to the system of preventive treatment followed in bye-gone days of drenching with the dung of pigs fed on the manure of sheep?—No, fed on the same pasture as the sheep.

20327. But do you not state that pigs may feed on the manure?—No, the pasture.

20328. Does that mean pasture covered with the manure of sheep?—Yes.

20329. Was your treatment suggested by that, or was it an induction from your discovery of the habitat of the bacillus in the digestive tract?—We supposed that this pigs' manure treatment of these diseases (which is a thoroughly empirical one), had nothing in it at first; however, the farmers were so positive about its virtues, that we made further inquiry into it, and after we discovered the intestinal habitat of the organisms we were dealing with, the explanation was afforded of how the pigs' manure might act as a prophylactic.

20330. Just because it might contain the bacillus?—That is so; the pig is always put out to feed upon the grass over which the sheep are grazing, the manure is collected after it has been upon the grass. The manure is filled with spores of some kind, most likely the spores which are the cause of the disease in the sheep, and if you administer the manure to the sheep you confer an immunity upon the animal. It seemed quite rational after that explanation was available.

20331. Now the bacillus, you say, was first discovered by Professor Nielsen in Norway?—The bacillus of braxy.

20332. Did he discover it in the peritoneal fluid?—No, not in the peritoneal fluid.

20333. Where?—In other parts of the body. You find it in the blood in some cases.

20334. But according to your experience you find it very seldom in life in the blood of living animals?—You find it pretty often in the blood of the living animal immediately after death.

20335. Did Nielsen also find it in the intestines?—Yes, he did, in the stomach, not in the peritoneal liquid. I do not think he ever examined that.

20336. Have your experiments been repeated by any Norwegian bacteriologists?—I do not know whether they have been repeated, but the Norwegian Government sent a representative over here from their Agricultural Department to interview me, and learn about them. Professor Jensen, also, from Copenhagen, is very much interested in the matter, and I have had a good deal of correspondence with him about it.

20337. But you cannot say that your experiments have been yet confirmed by other investigators?—I do not know.

20338. Have they been disputed?—I have had no evidence of their being disputed or confirmed, either one or the other.

20339. Of course, your inquiry is incomplete still?—Yes.

20340. Now as to the results, I suppose the farmer would look upon what I may call the total general mortality as the index of the healthiness of his flock without differentiating?—Quite so, they take a general view of the total mortality.

20341. In your statistics I see, for example, with regard to braxy, in your first series of 34, you have no deaths at all?—Yes, I think so.

20342. But you have deaths from other diseases—3 or 9 per cent.?—Yes.

20343. These other diseases, including the allied diseases to which you refer as A and B, which you have provisionally so named?—Yes.

20344. As well as malignant œdema?—Yes.

20345. Then in your second series of experiments you drenched 1,545, I think?—I think that is so.

20346. And the deaths from braxy amongst those amounted to 9, or 0.6 per cent., but there were 144 deaths from the other allied diseases or 9.3 per cent.—Yes.

20347. So that there is, even excluding the braxy, still a considerable mortality amongst the flocks?—There was at that time. That was the second year we commenced our operations.

20348. So that from statistics it is sometimes very difficult to obtain what you might call reliable results?—I confess that is the case. One requires either one way or the other to examine the data upon which your statistics are made up with the utmost care, and to repeat the experiment over and over again in order to be perfectly certain.

20349. I think you have also stated that your experiments have not been equally successful on the various farms on which they have been tried; the results have not been equally satisfactory, on some farms they have been satisfactory, and on others they have failed?—On certain farms—on the majority of farms—they have succeeded wonderfully well; on other farms they have been a failure; the mortality has beer fairly high—not exceptionlly high, but fairly high—and apparently for a very good reason. The sheep were treated for one disease and they died from something else. We did not know what disease was present on the farm. I now always try to get an inkling of what diseases they have to begin with. Rationally, of course, one would consider that sort of basis at the beginning of the treatment on the farm.

20350. Now, with regard to the prophylactic itself, is it prepared by the Highland Society or by the Board of Agriculture?—It is prepared by myself.

20351. Is it distributed to the farmers free?—We have usually distributed it free until this last year, and, I think, part of the foregoing year (1907 and part of 1906), when we had to make a small charge for it. The applications we had were so numerous that the preparation of it was attended with a fair amount of expense, and we made a small charge for it, which they willingly paid.

20352. Then, I think, you say that these braxy carcases are still eaten with impunity in Scotland?—Yes.

20353. Do you think there is any risk yourself in eating that food?—There is no other animal except the sheep that takes braxy. I do not know that I would like to eat braxy mutton myself, but other people do. They eat it and it does them no harm, apparently. Dogs and other animals eat the carcases with impunity.

20354. But you would not think that there would be the same immunity attaching to eating meat from a tuberculous animal?—I would not like to give an opinion on tht subject at all. That is a matter *sub judice* at present.

20355. It would depend upon the amount of the disease, I suppose?—I do not know. I really would not like to express an opinion upon the subject.

20356. But you yourself have carried out investigations with regard to tuberculosis?—I have.

20357. Do you believe in tuberculin, as a diagnostic agent, as a very reliable agent?—Which tuberculin do you refer to? There are so many nowadays.

20358. The tuberculin used by veterinary surgeons?—You mean in cattle, as a diagnostic in cattle?

20359. Yes?—Certainly.

20360. Did they not find at Aberdeen that when using this tuberculin on tuberculous cows there was no reaction?—There are several circumstances that influence it. If an animal is far advanced in tuberculosis there may be no reaction, whereas a single tubercular gland sometimes will give a marked reaction.

20361. Have you ever tried it on young calves?—In what respect do you mean?

20362. In testing?—I do not know that I have.

20363. Would you expect a young calf to react?—I would expect a young calf to react. Certainly with an animal just out of the calf stage it is done often enough, and it reacts there.

20364. But the calves are free from the disease; they are not, as a rule, tuberculous?—Usually.

20365. Would you expect tuberculin to have a reaction in stall-fed cattle, cattle ready for the butcher?—I do not know that stall-feeding has anything to do with reaction.

20366. But supposing it were tried?—I do not exactly see the point of the question.

20367. Tuberculin I mean to say is exploited as a test for the disease. We know that it cannot be used in fairs or on cattle landed from America at Birkenhead, because they are in an unstable condition, although they may have no tuberculosis, and yet they will react. I want to know whether the experiment has ever been tried of testing stall-fed cattle with tuberculin, which are presumed free from the disease, to see whether those will react?—It is used for stall-fed cattle; they are tested with tuberculin.

20368. I mean cattle fed up for the butcher?—Do you mean a milk cow?

20369. No, any sort of animal, any sort of cow or heifer fed up for the butcher which is presumed not to be tuberculous?—I think so. I do not know why it should not. I have known it react.

20370. Even if the animal is not tuberculous?—No.

20371. So that it is not a very reliable test?—One of the most important points, with American cattle at any rate, is to know whether they have not been tested with tuberculin before you got hold of them. That is a very important point.

20372. (*Sir John McFadyean.*) Dr. Wilson understood you to say that such an animal might react, although it was not tuberculous; is that so?—No. I understood the question to be whether a stall-fed tubercular animal tested with tuberculin would not react. I have seen cases that failed to react, but what the cause of it was I do not know. I have seen a milk cow very far advanced in disease that did not give any reaction at all, or only the very slightest reaction; nothing you could swear by.

20373. (*Dr. Wilson.*) Do you know anything about sheep-pox?—Very little. It is a disease I have not had any experience of.

20374. There has been no prophylactic discovered for that yet?—I do not know.

20375. I am going to read you a passage from the evidence submitted by the late Sir John Simon, who, you know, was Medical Officer to the Local Government Board. This evidence was submitted by him before the last Royal Commission in 1876. Referring to sheep-pox, he says:—"By these experiments on sheep it has been made quite clear that the contagion of sheep-pox is something of which the habits can be studied as the habits of a fern or a moss can be studied, and we look forward to opportunities of thus studying the contagion outside the body which it infects." Has any advance been made since that evidence was given?—Do you mean in relation to sheep-pox?

20376. Yes?—I cannot tell you. I would not like to commit myself to any statement in the matter.

20377. (*Sir William Collins.*) Is there any organism recognised as the cause of sheep-pox now?—I do not know. I do not think so.

20378. (*Dr. Wilson.*) No more than there is in small-pox?—No.

FORTY-FIFTH DAY.

Wednesday, 18th December 1907.

PRESENT:

The Right Hon. The Viscount SELBY (*Chairman*).

Sir W. S. CHURCH, Bart., K.C.B., M.D.
Sir W. J. COLLINS, M.P., M.D., F.R.C.S.
Sir J. MCFADYEAN, M.B.
Sir MACKENZIE CHALMERS, K.C.B., C.S.I.
Mr. A. J. RAM, K.C.
Mr. W. H. GASKELL, M.D., F.R.S.
Mr. G. WILSON, M.D., LL.D.

Captain C. BIGHAM, C.M.G. (*Secretary*).

Sir GEORGE KEKEWICH, K.C.B., M.P., called in; and Examined.

Sir G. Kekewich, K.C.B., M.P.

18 Dec. 1907.

20380. (*Chairman.*) I believe you are a Member of Parliament, and Honorary Secretary of the Parliamentary Association for the Abolition of Vivisection?—I am.

20381. And you come here to represent the views of that Association at their request?—I do.

20382. I believe you do not profess to have any expert knowledge of either medical or surgical matters? —I have only the knowledge that an ordinary man of common-sense, I suppose, can obtain from reading. I have no medical knowledge.

20383. That is what I meant to convey by the word expert?—Quite so.

20384. (*Chairman.*) I shall ask Sir William Collins to be good enough to ask you the questions which arise upon your *précis* which you have sent in; he has been good enough to say that he will do so.

20385. (*Sir William Collins.*) At the request of the noble Chairman, I propose to take you through the *précis* with which you have furnished the Commission. I am sure that I shall not do it so effectively as his Lordship, but I will endeavour to obey the direction of the Chair. May I ask what is the object of the Association on whose behalf you appear?—The object of the Association is the total abolition of vivisection.

20386. What is the name of the Association?—The Incorporated Parliamentary Association for the Abolition of Vivisection. It is called the Parliamentary Association because its vice-presidents consist exclusively of Members of Parliament of the two Houses, and they include a great number.

20387. On what ground do they advocate the abolition of experiments on animals?—They advocate it on the ground that, first of all, they regard them as inhuman, immoral, and degrading, and a disgrace to civilisation; and, further, they consider that the experiments which have been made under the present Act, and which have been carried out at the expense of so much torture and so much animal life, have been practically unproductive of any beneficial result to the human race.

20388. Would not the latter statement involve some medical knowledge?—From a certain point of view it will; but I think that any man of ordinary powers of observation and common-sense can judge what results have been obtained from the experiments and researches which have been carried out.

20389. Have you considered the mode in which the Cruelty to Animals Act, 1876, was passed?—Yes. Its motive power was the Report of the Royal Commission on Vivisection, which sat in 1875, and was appointed in consequence of a strong public agitation on the subject. Horrible cruelties were reported to have taken place on the Continent, and vivisection was also in a less degree in vogue in this country. The fact that M. Magnan, a French physiologist, had performed the experiment of injecting alcohol and absinthe into the veins of dogs before a medical congress at Norwich showed that there was danger that the medical profession in England might become generally permeated by the vivisection propaganda. M. Magnan was prosecuted under the then existing law, and left the country.

20390. What was the date of that prosecution?—I think it was 1874.

20391. And under what Act then was the prosecution?—Under the Prevention of Cruelty to Animals Act, an Act that was known as Martin's Act.

20392. What was the result of the prosecution?—The result of the prosecution was nothing, because he left the country, so that I believe no conviction was obtained, but I have not been able to find out exactly what happened.

20393. (*Mr. Ram.*) Did he stand his trial?—I think not.

20394. (*Sir William Collins.*) It was a threatened prosecution only?—I cannot quite make out from the records on the subject what happened, but, anyhow, he left the country, and so no decision was arrived at.

20395. Then as the result of the deliberations and Report of the Royal Commission of 1875, was a Bill introduced into Parliament?—A Bill (now the Cruelty to Animals Act, 1876) was introduced in 1876 by Lord Carnarvon into the House of Lords, and read a second time, after debate, on May 22nd. In the course of his speech he said that under the then existing law only domestic animals were protected, that as to vivisection the protection afforded to those animals was insufficient, and that for animals which were not domestic there was no protection at all. I wish to call the attention of the Commission to the fact that Lord Carnarvon introduced this Bill as a Bill to afford further protection to animals. He said further that the Bill was intended to prohibit in express terms and as a general principle the practice of vivisection, but it allowed exceptions, for certain purposes of teaching, with anæsthetics, and for purposes of research, even without anæsthetics, but only in the most rare cases. Those were his words.

20396. Were any representations made to the Government by the medical profession at that time?—Yes; the Committee stage of the Bill was deferred until June 20th, in consequence of the action taken by the medical profession. Some hundreds of medical men attended a deputation at the Home Office in order to present a memorial with some 3,000 signatures, and the consequence was that, so far as the Bill was amended in passing through Parliament, it was altered in accordance with their views. I have been unable to discover from Hansard, which I have looked at, at what particular point in the Bill the amendments took place. There was one amendment that was introduced in the House of Commons, which, I think, was in the interests of the medical profession, to the effect that no prosecution should be instituted without the consent of the Secretary of State.

20397. The Bill was brought into the House of Commons in August, was it not?—Yes, the Bill was brought into the House on August 10th; it passed through all its stages by August 15th, on which day it received the Royal assent, and Parliament was prorogued. I wish to call the attention of the Commission to the fact that the whole consideration given to this Bill in the House of Commons was five days; it passed through all its stages in five days. And I think there is no Member of Parliament who will not tell the Commission that that was a totally inadequate consideration for a Bill of this importance and character.

20398. Did the discussion occupy any considerable portion of those five days?—No, it did not, so far as I remember. There was a debate on Second Reading; there was a little said on the Committee stage, and the last stage passed through without any discussion at all; the fact being, of course, that it was a wearied House at the end of the Session; and as you, Sir William, know very well, members are very glad to get rid of all these measures as fast as they can in order to get away for the prorogation. What I wanted to point out was that a measure so passed through the House of Commons has not really, although it has technically, the same authority as a measure which has received full consideration from the House, and has been fully debated throughout all its stages.

20399. Perhaps you will state the objections that your society entertains to the Act?—We object to it because the principle of the Act, namely, the prevention of pain and cruelty, which is contained, I think, in Section 1, is entirely nullified by the exceptions allowed on certificates given by the Home Secretary.

20400. Is the certificate given by the Home Secretary? We have been told, I think, by previous witnesses, that it is open to the Secretary of State to disallow a certificate, but the certificate is actually signed by other authorities?—I have no doubt that it is so. From my own experience in the Civil Service I know that that kind of routine work is always delegated by the Minister to other people.

20401. Is it a matter of delegation to those within his own office, or is it that he recognises the action of other persons, such as Royal Societies, in granting a certificate, and under the Act the power is reserved to the Home Secretary of disallowing that certificate if he sees reason?—I believe that is so.

20402. Will you proceed with your objections to the process of certification under the Act?—We think that the Act has been practically powerless to stop either pain or cruelty. The Act provides that the animal experimented on must be anæsthetised, and that it must be killed before recovering from the anæthetic. But if the Home Secretary gives a certificate anæsthetics need not be used, nor need the animal be killed before recovery. The "most rare cases" referred to by Lord Carnarvon (that was cases for purposes of research even without anæsthetics) developed into 379 certificates in 1906, and there were also issued 88 certificates for performing experiments on cats and dogs. It may be added that no less than 256 certificates were held by medical men, dispensing with the obligation to kill the animal before recovery, and 118 certificates dispensing with the obligation to kill cats and dogs before recovery.

20403. Are the 88 certificates for performing experiments on cats and dogs included in the 379 certificates?—I understand that they are not.

20404. They are in addition, are they?—Yes, in addition.

20405. Do you suggest that these were all painful experiments?—I do not quite understand how any experiment can be other than a painful experiment, either *in initio* or in its subsequent results.

20406. Would that remark apply to cases of experiments conducted under anæsthetics in which the animal is destroyed before it recovers from the anæsthetic?—I should answer to that that we have no guarantee that in all cases the anæsthesia is perfect. It may be that the animal is completely anæsthetised in many cases, and consequently the animal does not feel any pain during the experiment. On the other hand we have no guarantee that the process of anæsthetisation is complete.

20407. Possibly you may be asked questions upon that in cross-examination. Will you proceed to state your objections to the Act in the matter of experiments performed in illustration of lectures?—The Act lays down that experiments are not to be performed as an illustration of lectures, and then proceeds to nullify this provision by further provisos. I should like to say here that the form of the Act is what I should call absurd. It seems perfectly ridiculous first of all to insert a clause saying one thing, and then to nullify the whole of that clause by provisos; for I think that every one of the restrictions in Section 3 is practically nullified by the provisos attached to that section. I have never seen any other Act of Parliament in that form myself.

20408. I think you have already stated that you object to the provision whereby a prosecution against a licensed person shall not be instituted, except with the consent in writing of the Secretary of State?—We think that bears out our contention that this Act instead of being an Act for the protection of the animals, is an Act for the protection of the medical men who carry out the experiments.

20409. Did the subsequent passing of the Public Authorities Act, 1898, affect the question of prosecutions under the Act of 1876?—Yes, Section 1 (A) of the Public Authorities Act, 1898, renders it illegal for a prosecution to be instituted more than six months after the offence, and therefore prosecutions have become impossible. The only means of discovering illegal proceedings under the Act is the Report of the Home Office Inspector, and his reports are never issued until the matter reported on is at least six months old. No doubt the framers of the Public Authorities Act had not this result in view (I do not think they had this particular Act in view at all), but such is the effect of the provision. In answer to a question I asked the Home Secretary on the subject in the House of Commons on November 13th, 1906, he said that he could not issue the Report quarterly (it is now issued half-yearly) because of the immense mass of documents that had to be received and examined, and because it would very greatly increase the labour involved, with little or no advantage. He added that contraventions of the Act were reported to the Home Secretary as soon as they were discovered, so that the institution of proceedings for performance of illegal experiments was not dependent on the date of the Report. That would be well enough if the Home Secretary would institute prosecutions himself, or give to others the information enabling them to do so. But I am not aware of any case in which he has adopted either course.

20410. Do you think that the Act has operated in the direction of the restriction of vivisection?—No; I think it has acted as a great stimulus to vivisection. The proof of that is the number of experiments, which has risen from 481 in 1878, which was two years after the passing of the Act, to 45,287 in 1906.

20410A. Our attention has been called to the fact that a considerable number of such experiments are of the nature of inoculations, involving merely the prick of a needle. Have you anything to say in regard to that?—I am aware that that is the case, but I am not aware that inoculation does not lead to painful and undesirable consequences. I have often heard that if you inoculate with a bit of pus, say under the tongue, you may produce very great pain and a great deal of trouble. An instance, which I deal with later, is the inoculation for plague with Haffkine's serum.

20411. Have your Association embodied their views in a Bill?—They have, but I believe that Bill has not been furnished to the Commission.

20412. I think we have not had it?—No; you have not had it. The reason being because we find it extremely difficult to draft a Bill, and therefore I should like to eliminate that, if I may, from my *précis* altogether.

20413. Do you desire, on behalf of the Parliamentary Association for the Abolition of Vivisection, to give us any specific recommendation as to the form of a Bill for carrying out their object?—We could do that later. But we are rather of opinion that the time is premature for framing a Bill. We think we had

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better delay it until after this Commission has issued its final report, and then we shall introduce a Bill into Parliament expressing our own views on the subject.

20414. So that at the present moment the views of the Association are not embodied in any Bill?—We have not yet embodied them in any Bill.

20415. Do you desire to call attention to certain omissions and discrepancies which, in your opinion, demand the attention of the Commission?—Yes; to some which have characterised the evidence before the Royal Commission. I want to point out that such evidence, far from relieving the position, on the contrary greatly aggravates the widespread and daily-increasing disquietude in the public mind, as regards the suffering inflicted upon animals, and the hopeless confusion and anomalies of present-day medical science. The Commission will understand that I am representing experts although I am not an expert myself.

20416. Is that the opinion of the Parliamentary Association?—It is the opinion of the Parliamentary Association.

20417. (*Sir Mackenzie Chalmers.*) I do not quite understand, do you represent the experts in your Association?—So far as my *précis* goes I represent the opinions of persons who are experts in the sense that they belong to the medical profession. I do not say that they are experts in the sense that they have taken part in experimental research or vivisection, except, perhaps, in their student days in hospitals.

20418. (*Sir William Collins.*) You desire to suggest certain reasons for alleging this increasing public disquietude, I understand?—We wish to say that in consequence of the increasing public disquietude we have been enabled to add recently, since the publication of the Blue Books detailing the evidence supplied to this Commission, no less than 29 members of Parliament to the list of our vice-presidents, and we have received from a number of other members such expressions of interested sympathy as experience has shown are prefatory to their becoming staunch opponents of the practice of vivisection.

20419. Then while not volunteering medical opinions yourself, do you desire to make some statements in regard to medical science?—I wish to confine my remarks to the important subjects of the infliction of pain, anæsthesia and anæsthetics, vivisections and demonstrations before students. These, as I consider, being subjects with which any man of common intelligence is qualified to deal, and, moreover, subjects which are of the most vital interest to the members of the Association which I represent, and to the general public, bearing as they do upon the systematic infliction by men in high and responsible positions of pain and considerable suffering upon helpless creatures, and also (I think this is a most important point) upon the demoralisation of the young men and women who are the future physicians and surgeons at whose hands the community will presently benefit or suffer.

20420. Do you su—est that such practices have a detrimental effect upon the humane sense and morality?—Yes. I would remind the Commissioners that the development in man of the humane sense has been the slow growth and higher moral awakening of many millions of years of evolution, and that, should they favour or further facilitate such demonstrations before students they will, by rendering them callous to the sight of pain and mutilation inflicted upon living, intelligent creatures, incur serious risk of blighting that humane sense which is the very highest characteristic of civilised men, and is, beyond all others, the faculty most needed in those who will one day have the lives and welfare of their fellows in their trust.

20421. It has been suggested to the Commission that the practice of vivisection under the Act would have no more demoralising effect upon students than witnessing surgical operations under anæsthesia. What do you say with regard to that?—I think undoubtedly it would. I think the witnessing of a series of experiments by students upon living animals undoubtedly renders them more callous to suffering than if they witnessed an operation or operations which are done for the benefit of the person undergoing the operation.

20422. Would your objection and your fears in regard to the lowering of the moral sense be met if you could be assured that vivisection was conducted entirely painlessly?—No, I do not think it would. I think the whole sight is degrading and immoral—the sight of the animals strapped down and cut to pieces I do not know that I have anything more to say on that point.

20423. Do you suggest that the same advantageous results which have been claimed for vivisection could have been obtained by some other mode?—By clinical research.

20424. Do you desire to call attention to what you mention in your *précis* as to the barren state of medical knowledge?—Yes; I think that it is obvious that the present state of medical knowledge is barren. We see disease and degeneracy on all sides, against which it is equally obvious that medical art is powerless, or they would cease to exist. To take a single instance, since the year 1729 influenza in various forms and of varying degrees of severity has been an ever-recurring scourge. Now it is perennial, even endemic. Yet our medical scientists cannot give us one satisfactory explanation of its origin or of its prevention; nor, if we judge by its constitutional sequelæ and its mortality, can we suppose that they know much about its treatment. Yet they have been supplied with abundant material, not only animal, but also human, for observations with a view to the discovery of means to rid us of this scourge. We are, however, apparently no whit nearer ridding ourselves of it than we ever have been. Here is a practical, everyday test, and surely, also a convincing evidence of the barren state of present medical knowledge.

20425. Do you suggest that medical science has remained stationary during the last 30 years?—I should say that it has not made any great advance. I have seen it stated that we know little more than we did in the days of Galen and Hippocrates as regards medical science. I do not go so far as that.

20426. You say that you have seen it stated. May we take it that the view which you have put forward is that entertained by the Parliamentary Association?

20427. (*Sir Mackenzie Chalmers.*) And its experts?—Yes. I was going to say (I happen to know something about it from home knowledge) that I have heard medical men say that so far as remedies are concerned there are constantly new remedies, such as antipyrin and sulphonal, which were at one time ushered in with a great flourish of trumpets, but which, I think, are not any longer in general use; and, as a matter of fact, many medical men, I think, are really limiting themselves to a very small number of efficient remedies.

20428. (*Sir William Collins.*) I understand that you desire to call attention to certain points in the evidence which has been laid before the Commission. Of course the Commission has an opportunity of reading that evidence, but you may desire to direct attention to one or two particular points?—Yes. As regards the infliction of pain, inasmuch as obviously the opponents of the practice are not in a position to obtain experiences, it is necessary to rely upon the admissions and representations of vivisectors themselves. For this reason, their contradictions and admissions are most valuable, as warning the Commissioners and the public that the evidence as to the pain suffered by animals in process of mutilation is wholly untrustworthy and unreliable, either because of the inaccuracy of statement or the inaccuracy of observation of the operators, or because, under the Act as at present carried out one operator actually experiments without pain while another confesses that he has done painful experiments, and indeed, adds that he is not ashamed of the fact. Professor Starling (Question 3451, First Report) commits himself to the bold and sweeping statement: "Though I have been engaged in the experimental pursuit of physiology for the last 17 years, on no occasion have I ever seen pain inflicted in any experiment on a dog or cat; or, I might add, a rabbit, in a physiological laboratory in this country, and my testimony would be borne out by that of anyone engaged in experimental work in this country." Professor Cushney says (Question 4932, Second Report): "I am quite satisfied that no animal suffered pain under my experiments." (Question 4962) "I should say that the statement that cruelty occurs in laboratories in England at the present time is grounded on misconception, and is false."

20429. Have you any reason for disputing these statements?—As a mere question of pain I do not see that there is anything illegal in the causation of pain because the operator may hold a certificate enabling him to dispense with anæsthetics.

20430. (*Chairman.*) That is not the statement that you make. The statement that you object to of Professor Starling's I understand is that he has not caused pain to animals in his experiments?—Yes.

20431. I understood Sir William Collins to ask you whether you dispute that statement?—Yes.

20432. (*Sir William Collins.*) And that the testimony would be borne out by anyone engaged in experimental work in this country?—He says so.

20433. Do you desire to suggest any reason for doubting that?—I do not know enough about the persons engaged in experimental work, but I should rather doubt whether they could honestly say that. Dr. Thane, an inspector under the Act (Question 450, First Report), says: "I have not seen nor have I ever heard of any operation—a procedure that was seriously painful—being performed without anæsthesia." I do not know whether Dr. Thane's experience is large. He is an inspector under the Act, but I understand that last year he was only present at 15 experiments, and I understand that the whole of those experiments were in Sir Victor Horsley's laboratory; he did not go any further than that.

20434. Do you suggest that Dr. Thane did not inspect last year any vivisections under the Act except those performed by Sir Victor Horsley?—Yes, I understand that to be the case.

20435. What is the source of that information?—I understand that Dr. Thane said that he had inspected 15 experiments, and that Sir Victor Horsley stated that Dr. Thane had inspected his laboratory 15 times.

20436. (*Chairman.*) What question is that?—I have not the number, but I understand that in Dr. Thane's own evidence he said that he had only witnessed 15 experiments altogether, so that if he had inspected Sir Victor Horsley's laboratory 15 times and had only witnessed 15 experiments altogether it is obvious that all those 15 experiments were in Sir Victor Horsley's laboratory and nowhere else.

20437. (*Sir William Collins.*) Would it not be possible for an inspector to pay an inspection visit to a laboratory and not to witness an experiment in the course of his procedure?—That is so, of course, but I should have thought that those visits were of very little use.

20438. That is the ground for the statement you have made?—That is the ground.

20439. Is there any other point in Dr. Thane's evidence to which you desire to call attention?—Yes, he later admits that, on the other hand, "In some cases of this group (that is, experiments performed under Certificate A) the infection or injection is followed by great pain and suffering. I may mention the injection of tetanus toxin and the infection with plague; also the insertion of certain drugs." "The injection of tetanus toxin," he states, "will produce tetanus, which . . . is manifested by convulsions which are of a painful nature." "Injection with plague makes the animals very ill and makes them look miserable." "It is obviously impossible to say to what extent mice, in which such tumours (implanted cancer) are growing, are suffering pain, but observation of their behaviour and habits does not justify the assumption that, so long, at all events, as the tumour is of moderate size, they are suffering acutely or severely." Nevertheless, the facts and the Inspector's evasive way of stating them justify the assumption that these creatures may suffer acutely or severely according to their own humble powers of bearing pain, whether or not the tumour remain of moderate size, and we are not told of means taken to restrict the growth. When asked, may the result of the operation be painful, although the operation itself is performed under an anæsthetic? (that is in the case of renewed operations) "That comes on," he replies, "two paragraphs further on;" and later says, "When in operations under these certificates the operation has been performed and the wound has healed, it does not follow that the animal remains in a state of pain or suffering." Nobody, of course, would suppose that after the healing of the wound it necessarily follows that the animal remains in a state of pain or suffering. But he carefully ignores the truth that in extensive cutting operations with excision of organs there must necessarily, upon waking from anæsthesia, be very severe and prolonged pain, until, that is, the severed tissues heal. He reiterates that the operation is conducted aseptically, which may hasten the closing of the wound, but cannot certainly diminish sensation and the degree of suffering which is inseparable from extensive cutting operations. Then Professor Pembrey has made some admissions.

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20440. Do you desire to call attention to them?—The freemasonry of reserve and evasion as to the fact and the amount of suffering inflicted is candidly broken by the very frank and damning admissions of Professor Pembrey, Professor of Physiology at Guy's Hospital. He says (Question 14084):—"I confess that I have done painful experiments, and I am not ashamed of admitting it. They are absolutely necessary. I want to show that pain is part of the scheme of nature, and that we must recognise its existence." He believes (Question 14093) "that a recognised physiologist should be given a licence to cover all experiments." He was asked whether that licence should be given for experiments without anæsthetics, and he said, "Without anæsthetics or with anæsthetics and without certificates." He believes that the Vivisection Act is entirely opposed to the advancement of physiology. Further, he expresses the opinion that the infliction of suffering is a meritorious act. Then, at Question 14147: "I consider," he says, "that it is perfectly right to inflict pain upon animals." Then there was a question asked: "Not unnecessarily?" and he said, "No." In answer to another question, he says: "I will be perfectly straightforward. I say that you should not inflict pain if you can obtain the knowledge in any other way, but I say that even where there is an operation the pain there is of a protective nature; it may produce syncope, and therefore less sensation of pain." I presume that a knock on the head will do the same thing. This suggests that such an extreme degree of suffering should be inflicted as to cause the unfortunate creature under operation to become insensible, in order that while it is in this condition of insensibility a further operation may be done. "Further," he continues, "I say that the introduction of an anæsthetic may produce complications, so that more experiments may be necessary than if the experiment were done on the animal in a condition of syncope. . . . If you give them anæsthetics you are introducing a complication which you could remove, and therefore without anæsthetics you actually save life and actually diminish the infliction of pain." That seemed to us to be a very extraordinary doctrine. By these admissions we get at the truth. Professor Pembrey has, at all events, the courage of his convictions. He has done "painful experiments, and is not ashamed to admit it." He considers it "perfectly right to inflict pain upon animals." "Painful experiments," he adds with conviction, "are absolutely necessary." "Pain is part of the scheme of nature." It is "protective" and "beneficent." He is right, no doubt, in that pain is protective when it calls the sufferer's attention to the presence of an injury, and also in rousing the reparative processes to make good that injury. But it is difficult to trace the protective character of pain inflicted under vivisection, as it will certainly not protect a creature, clamped powerless upon a board, from further investigations by an operator. And surely only the strangest perversion of mind can find anything "beneficent" in the infliction of suffering by intelligent man upon a helpless creature of lesser intelligence.

20441. If I remember rightly, some of the painful experiments admitted in this country by Professor Pembrey under licence had to deal with the destruction of rats by sulphur dioxide with a view to disinfect ships. Do you suggest that it is undesirable that experiments of that character upon rats should be performed with a view to prevent the propagation of plague?—I do not quite understand why painful experiments should be performed under those circumstances. I have not sufficient medical knowledge to know whether it is possible to carry out such an experiment as that without pain.

20442. I suppose that would be dealing with two different respirable gases, the anæsthetic on the one hand and the sulphur dioxide on the other?

20443. (*Chairman.*) If you are going to kill rats in a ship you could not first catch the rats and anæsthetise them, and then proceed to poison them with gas. You must do it at once?—This, I imagine, was a question of the suffocation of rats, was it not?

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20444. (*Sir William Collins.*) Yes. May I ask if the alleged fact that sulphur dioxide is a good mode of destroying rats with a view to prevent the propagation of plague had been discovered by other than vivisectional means, you would object to its use for that purpose?—I am met by the fact that I do not understand how suffocation comes within vivisection. I thought vivisection was cutting.

20445. You think that these experiments might be performed without being brought under the Act at all?—I should imagine so.

20446. The term vivisection is not, I think, defined in the Act?—I do not think it is, but I was speaking really of its obvious meaning—vivisection is the cutting of living animals.

20447. Does your Association distinguish between experiments upon living animals of a cutting character and painful experiments upon living animals such as the administration of suffocating gases?—I think we should oppose both, but, on the other hand, I do not know that anything except cutting experiments comes under this Act. The words of the Act are: "A person shall not perform on any living animal an experiment calculated to give pain." I do not know whether this particular experiment you speak of does give pain.

(*Sir Mackenzie Chalmers.*) Undoubtedly, if you have read his evidence.

20448. (*Sir William Collins.*) I thought you were alluding to Dr. Pembrey's evidence to show that he had conducted painful experiments?—I am not dealing with his experiments specifically; I am dealing with his general statements, where he says that painful experiments are absolutely necessary, that pain is part of the scheme of nature, that it is protective and beneficent, and all that kind of thing.

20449. I understand that you desire to draw attention to what you conceive to be a discrepancy between the evidence of Professor Pembrey and Professor Starling?—Professor Pembrey, in direct contradiction to Professor Starling and others, tells us that he does painful operations, and that, moreover, "they are absolutely necessary." Thereupon we are confronted by the question: Were those other gentlemen, then, imposing upon our credulity, or were they the victims of their own? Can extensive mutilations under anæsthesia administered, as Professor Starling confessed, by the laboratory boy, be wholly and invariably painless? Painful experiments, he says, are "absolutely necessary." This, then, is the candid truth of the matter. Vivisection is necessarily, that is, unavoidably, a painful—possibly a gruesomely painful—process. Some notion of the degree of that pain, and of the callousness of the operator, is indicated by Professor Pembrey's recommendation that anæsthetics should be dispensed with, and that the animal should be subjected to suffering so severe as to cause insensibility in order to avoid the "complications" attending the administration of an anæsthetic. That, as I understand, means that he advocates the infliction of pain of such severity as to make the animal collapse under the pain. That hardly seems consistent with the principle of the Act that anæsthetics should be administered. Professor J. N. Langley, F.R.S., says (Question 15251), "The degree of pain is, of course, always a relative thing. . . . I have indicated the difference, so far as I could, by saying that some of my experiments were clearly not painful, and some of them were of the nature of a severe operation in a hospital, and those I should think, possibly, at times do cause pain, as they do in a human being." I should have thought that they always did.

20450. We have frequently had our attention directed to Professor Klein's evidence before the last Commission?—No doubt. When he gave evidence before the last Royal Commission in 1875 he was asked at Question 3539, "Then for your own purposes you disregard entirely the question of the suffering of the animal in performing a painful experiment." His answer was, "I do," and he is still employed under the Local Government Board. Further indications of the blunting effects of vivisection upon the humanity of the operator are supplied by Professor Pembrey's observations upon the relief of human pain by anæsthetics. At Question 14119 he is asked, "Did I rightly gather that in your opinion anæsthetics are unnecessary," and he said, "Yes. I am convinced that they are often unnecessary, not only in the case of animals, but of men." And at Question 14120 he is asked, "For vivisection and for surgical operations?" and he answered, "For vivisection and for surgical operations, and I mentioned the case of midwifery." He condemns anæsthesia for women in childbirth (the suffering during which, it is acknowledged, is atrocious), and himself circumcises children without anæsthetics. "If they cry," he says, "it does not matter; it is much better that a child should cry and moan than that its life should be lost; the introduction of anæsthetics has done a considerable amount of harm." Professor Pembrey tells us that he has a class of 70 or 80 fresh students every year. We may judge for ourselves the moral effects upon these students of their professor's doctrines, and of the sight of the painful operations to which he confesses.

20451. Are you not rather ignoring the extent to which we are told that anæsthetics are employed in cases of experiments upon living animals?—As I understand, the point here is that you have a man in a very high position in a hospital who actually advocates the dispensing with anæsthetics altogether.

20452. I thought you were going to give us some reason why you doubted the assertion that anæsthetics are so largely employed in the case of experiments on living animals?—We have no absolute proof that anæsthetics are ever used, because no anti-vivisection doctor has ever been allowed to witness any experiment.

20453. (*Chairman.*) Then nobody is to be believed on his oath except an anti-vivisection doctor. We have had plenty of people, not speaking on their oath no doubt, but do you say that nobody's word is to be taken for a fact of that description unless he is an anti-vivisection doctor?—No.

20454. That is what you stated just now; "we have no absolute proof." That is to say that if 20 people came and said that they were present at an operation and that anæsthetics were used, and that the animal was perfectly quiet and still, and showed no sign of pain, and was not strapped down even, nevertheless you would disbelieve them because there was not an anti-vivisection doctor among them?—We should say that we want to send a competent witness from our own side.

20455. (*Mr. Ram.*) To be present on every occasion?—I do not know that on any occasion——

20456. On every occasion, I said?—But I do say that up to the present time, so far as I am aware, there has been no occasion on which any man of that kind has been allowed to be present—a person like Dr. Hadwen, for instance.

20457. (*Sir William Collins.*) Did not Professor Starling offer to admit other persons to his laboratory than those who are at present authorised to attend?—Yes, and I should like to say, partly in corroboration of what I have said, that anæsthetics are not always used in the case of painful operations when they might be, that I notice that in some of the evidence that I saw, given before this Commission, Colonel Lawrie said that he had seen the painful operation of dissecting the neck in order to establish cross circulation done without any anæsthetic whatever, but I observe that in his evidence he did not say where it was done, and he did not say whether the operator held a certificate enabling him to dispense with anæsthetics.

20458. (*Dr. Gaskell.*) Do you say that he saw it done without any anæsthetic whatever?—Yes.

20459. (*Mr. Ram.*) Can you give us the reference?—Yes, I have the reference here.

20460. (*Sir William Collins.*) Was not morphia administered in those cases?—Yes, but I understand that morphia is not an anæsthetic.

(*Chairman.*) It might have been worth mentioning that it was given.

20461. (*Dr. Gaskell.*) When you say without any anæsthetic whatever you mean with morphia, and that you do not consider morphia to be an anæsthetic. If that is what you mean, it is hardly the same thing as saying without any anæsthetic?—Yes, but Colonel Lawrie said that the operation was performed with a small dose of morphia only; he does not say with a fatal dose of morphia.

20462. What was the operation?—It was the operation of tracheotomy. He was asked, at Question 16816: "What was done; was tracheotomy performed without chloroform?—(A.) The whole dissection was performed without chloroform." Then he was asked: "Do you say it was performed without anything in the way of an anæsthetic?" And he answered: "It was performed with a small dose of morphia." He was asked: "What was the operation?—(A.) What is called a cross-circulation experiment. The necks of two animals are dissected and the large blood vessels cross-connected." Then he was asked, What animals they were, and he said dogs. Then he was asked: "But it was under an anæsthetic?" And he answered: "I do not call morphia an anæsthetic." I think that has been supported by other evidence, I think by Professor Hobday, if I recollect rightly, of the Veterinary College.

20463. (*Sir William Collins.*) Do you desire on behalf of your Association to make any suggestions as to the admission of persons representing anti-vivisection views into physiological laboratories?—Dr. Starling said that he would admit any Member of Parliament or layman, but not "any" doctor.

20464. (*Mr. Ram.*) It was not that he would not admit "any" doctor, but not every doctor?—I understand that no doctor holding views opposed to vivisection has ever been admitted to witness these experiments. And then further on Professor Starling said that no person would be admitted to a physiology class who came merely from curiosity. I do not know whether he would admit me.

20465. (*Sir William Collins.*) Seeing that your Association advocates the entire abolition of the practice I suppose it is idle to ask you whether an increase of inspectors would in any way meet your objections?—No, I do not think that any increase of inspectors would meet our objections.

20466. Even anti-vivisection inspectors?—Even anti-vivisection inspectors, because we are opposed to the practice altogether. The only object, so far as I understand it, of the appointment of what you would and what we would call anti-vivisection inspectors would be to see that the operations were carried out in accordance with the certificates, that is to say, that under a certificate allowing experiments on animals with anæsthetics, the anæsthetics were used and that complete insensibility was effected; and, further, that if the holder of the certificate was enabled to dispense with anæsthetics altogether to see (I do not know what he would see then) that the experiment was carried out in accordance with the certificate. But we are opposed on the ground of its inhumanity and inutility to the whole practice of vivisection. That is our position.

20467. If the Act were not abolished but modifications were made would your Parliamentary Association desire that the number of inspectors should be increased, and that provision should be made for the representation of those who are sceptical on the subject of vivisection on the inspectorate?—I do not quite understand what you mean by their being sceptical on the subject of vivisection. The only object, as I said before, would be to see that the experiments were painless. Of course our Association would prefer that to the existing practice, but we hold that the Act should be repealed and another Act passed in its place, abolishing the whole practice of vivisection of animals.

20468. Does your Association think that the result of the entire abolition of the practice in this country would be to reduce the totality of animals submitted to vivisection. It has been suggested to us that if abolition were decreed either the practice might be carried on surreptitiously or that those who were desirous of practising it would be driven abroad where there would be no, or fewer, restrictions upon their conduct?—I do not say that I am not concerned with what takes place abroad. I view it with as great horror as the English practices, but, after all, what takes place abroad is not our business; we have to look after our own country. And as for a surreptitious performance of vivisection, under those circumstances I apprehend that, if the penalties were large enough, there would not be much surreptitious practice of vivisection.

20469. (*Sir Mackenzie Chalmers.*) You would prohibit it under penalty; you would not leave it to the ordinary law of cruelty?—No, I should not be satisfied, for the reasons that Lord Carnarvon gave in introducing this Bill, with a mere repeal of this Act. I would not be satisfied without the passing of a fresh measure prohibiting the practice of vivisection altogether.

20470. (*Sir William Collins.*) Would you put further restrictions upon the submission of living animals to experiment for scientific purposes than exist in regard to cruelty to animals arising from other than scientific investigations?—No, I think I would bring them all under one and the same Act. For instance, now, I believe that if a horse has lockjaw, and it is not killed immediately, that is punishable under the Cruelty to Animals Act, and is dealt with in the ordinary way by the magistrates, and the horse is directed to be killed. If an experimenter produced artificial lockjaw on a horse, I should inflict precisely the same penalty upon him, but if he, as they do now, produced artificial lockjaw in a horse, and kept that horse alive as long as he possibly could for observation, I should inflict a heavier penalty in proportion to the degree of cruelty. May I add that Miss Lind-af-Hageby, who is the only anti-vivisectionist who has actually attended advanced classes in England (so far as I am aware), affirms that "the most unsatisfactory and obviously unreliable methods of anæsthetising are in vogue in the vivisectional laboratories." She makes that statement on page 159 of a book she calls (not very complimentarily) "The Shambles of Science."

20471. I understand that you desire to draw attention to experiments upon living animals, with a view to the prevention of plague?—In the examination of Mr. C. J. Martin, Director of the Lister Institute of Preventive Medicine, and a member of the Advisory Committee for the Investigation of Plague in India, he was asked by you whether he thought Haffkine's vaccine, or any other vaccine, in India is having a valuable influence in checking the spread of plague, and he replied, at Question 12300, "Very valuable." I should like, in passing, to call attention to the word "vaccine." I do not understand that the plague serum is vaccine at all; it does not come from the cow or calf. It is mere plague serum, and induces simply a modified attack of plague. You then asked him whether plague has ever been more prevalent in India in the memory of man than during the past year, and he answered, "Not that I am aware of." I wish to lay this matter somewhat more fully before you as a grave national responsibility. The point which brings the question of the plague in India under the consideration of the Parliamentary Association is the manner in which the serum used for the inoculations is prepared and tested; that is to say, the fact that it is prepared by inoculation of animals. W. Symmers, of Cairo, gives a record of experiments made to determine the therapeutic efficacy of plague serum (Central b.f. Bakt., April 15th, 1899) carried out at the Serum Institute of Abbasieh, in Egypt. Cultures were made from the bacilli of bubonic plague obtained from Bombay, and experiments (that is to say, inocculations) tried on guinea-pigs, white rats and mice, and horses. Cultures on agar or on bouillon were found to possess little virulence, and it was necessary to pass the bacilli through white mice—in other words, to inoculate them with plague—before sufficient virulence could be obtained; the bacilli obtained from these mice were then used to make cultures in bouillon, and the new bacilli thus obtained were injected into horses, generally under the skin of the neck. Local swelling, and inflammation of the glands, and slight fever followed these injections. Sometimes these injections were repeated as many as fifteen times on the same horse; then the jugular vein was opened, and from the blood thus withdrawn the serum was obtained, which was then injected into the abdominal cavity of white rats. As the plague serum thus obtained was not very virulent, it was suggested that more virulent bacilli should be employed, and that the injections should be carried out in large quantities, and for longer times on horses. The details of the preparation of plague serum, no doubt, vary very much; each manufacturer will have his own process; but in all there is the same system of injecting these filthy products of disease into healthy animals, so as to give them the plague, of finally injecting horses with the disease, and, when they do not die of it, bleeding them to obtain the serum, which is to be used for human inoculations. Monkeys have been used in the preparation of the testing of plague serum, as well as cats, guinea-pigs, mice, and rats. Dogs, we are told, are most refractory to the infection. "Lawson could not infect pigs, either by alimentation or by subcutaneous injection. Experimenting on sucking pigs he succeeded in causing tumefaction and œdema at the site of the inoculation, but could not discover the bacillus in the ganglia." That is out of "Bubonic Plague," page 16, by Montenegro, who wrote on plague

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at Oporto. The same author, on page 13 of his book, gives this case: "Batzaroff received some broth, the virulence of which had decreased so much that a large dose inoculated in a rat did not kill it. . . . Depositing a certain quantity of the cultivation in the nostril of another guinea-pig he succeeded in producing plague pneumonia, and killed the animal, but in eight days." "Simond (page 20) made a monkey breathe infected flour, but did not succeed in producing the pneumonia of plague." "Simond made a rat eat pieces of diseased spleen without any result" (page 21). Plague pneumonia is described (page 59) by the same author: "At the end of thirty hours fever begins, which rises very high; the animal appears to be miserable, depressed, breathes with difficulty, coughs up a foamy liquid. From its nose there flows a serosity which irritates the mucous covering of the snout; the inflammation . . . causes serious mischief in the eye. Meanwhile the exhaustion of the animal increases, respiration becomes strident, there is a distinct wheezing . . . and finally in twenty-four or thirty-six hours the animal succumbs." Thousands of animals have been used for these inoculations, and an enormous amount of suffering caused in the preparation and testing of the various serums. When Mr. C. J. Martin speaks of the "very valuable" influence of inoculations on the plague in India, he is probably not aware that the present epidemic has lasted eleven years, increasing in violence with every year, whereas the normal duration of an epidemic of plague is seven months; nor is he aware that during those eleven years (or, to be exact, ten years and seven months, from September, 1896, to the end of April, 1907) there have been 5,326,000 deaths from plague in India. Will he dare to say that without Haffkine's and Yersin's serums there would have been a greater number of deaths than five and a quarter millions? It is, to say the least of it, a significant coincidence that never within the memory of man has an epidemic of plague been of such immensely long duration, and never within the memory of man has plague been treated by inoculations of plague serum. When the plague broke out in Bombay in the end of 1896, all the experts were unanimous in declaring that the ordinary duration of the epidemic was seven months. In the "British Medical Journal" of February 6th, 1897, it is said: "As regards the epidemic, no better news is forthcoming, nor, we fear, is any great abatement of the disease in Bombay, or wherever else it may appear, to be anticipated with any confidence until six or seven months after the commencement of the outbreak. Bombay, it may be hoped, will be free of the epidemic in the month of April, that is seven months after its appearance in September, 1896." On February 13th, 1897, the same authority repeats: "We do not hope to hear that Bombay is free from plague before April, but we do rely on a distinct improvement by the end of that month." On February 27th, 1897, "the six or seven months of its wonted period of activity are over." On this same date occurs this ominous note: "Dr. Yersin is on his way to Bombay with a stock of serum." With the arrival of Dr. Yersin's serum, and other serums, all hope of the epidemic keeping to "its wonted period of activity" was at an end. A manufactory of plague was established, which has been hard at work ever since, producing its five and a quarter million deaths. These figures, 5,326,000 deaths from plague from September, 1896, to the end of April, 1907, were given by Mr. Morley in the House of Commons on June 6th, 1907, on the occasion of a debate on this subject, when Dr. Rutherford said that the plague was one of the chief causes of the unrest in India, that it was a preventible disease, that it had been brought under in most countries, and that only in India had it such awful effects ("British Medical Journal," June 15th, 1907). In the months of January, February, and March of this year the deaths from plague in India reached the enormous number of 58,438, 98,397, and 171,522 respectively.

20472. Do I correctly understand you to suggest that the use of these sera of Haffkine and Yersin has been the means of propagating the plague?—I do.

20473. Or that the use of them has diverted attention from other means in regard to plague prevention? —I do.

20474. Do you make both those suggestions, or which?—I suggest both. I suggest that in the first place the use of these sera was an actual propagation of the plague. These sera are not at all analogous to vaccine which is used for the prevention of small-pox. They are plague itself, and they give the plague. I think there is no proof whatever that a man who is inoculated with Yersin's or Haffkine's serum is not just as much a centre of plague as a man who has caught the plague in the ordinary way.

20475. (*Chairman.*) Are you speaking now from your own knowledge or from something that you have read? —I am speaking from my reading, not of my own knowledge. I have fortunately not been in these centres of plague, so I cannot speak from personal knowledge, but it seems to me that the inoculation with these sera for plague is very analogous to the old system of inoculation for small-pox, which is not only discredited, but is actually forbidden by law, I believe, at present.

20476. (*Sir William Collins.*) Are you not aware that it is claimed by some investigators that vaccine can be made out of small-pox virus?—I have heard so, but most assuredly if you inoculate a person from the cow pox virus you do not get a case of small-pox. I have heard it, but I find it extremely difficult to believe.

20477. Have you not also heard that it is claimed that by the use of small-pox virus the vaccine may be obtained by inoculating a calf or cow with human small pox?—No, I do not know that. I do not know whether it is proved or not.

20478. (*Sir John McFadyean.*) Might I ask why the material from this cow disease, cow-pox, is not regarded as "filthy materials" by your Association?—Our Association is not prepared to offer any evidence as regards the question of vaccination, because vaccination is a subject of special Acts of Parliament, one of which was passed as lately as last year, and we did not understand, and we do not understand, that it comes within the function of this Commission to inquire into it.

20479. That was not my question. I did not wish you to express an opinion as to the efficacy of ordinary vaccination, but you class sera and other products of disease as "filthy materials" when used with the object of curing or immunising human beings. I want to know, do you place the so-called cow-pox lymph in the same class, and, if not, why not?—I have already said that I do not come here prepared to give any evidence on the subject; as representing the Association, I offer no evidence whatever on the subject of cow pox. If the Commission would like to know my personal views on the subject of vaccination I shall be delighted to give them.

20480. (*Sir William Collins.*) Have you ever seen a case of natural cow-pox in a cow?—Natural cow-pox in a calf?

20481. In a cow?—No, I have never seen it.

20482. (*Sir John McFadyean.*) But I understood that it was you yourself that introduced the subject of cow-pox vaccination and contrasted the material used there, or the benefit of the operation with the use of serum?—I introduced the subject of inoculation.

20483. Of human beings?—The inoculation of small pox.

20484. I beg your pardon.

20485. (*Sir William Collins.*) Will you proceed to state the grounds on which you think that the practice of the use of sera has diverted attention from other means which you regard as more satisfactory?—As the counter proof that this treatment of the plague by serum inoculations has aggravated instead of "stamping out" the epidemic of plague, I will give the history of the outbreak in Alexandria in 1899. Before that time a Commission was sent by the Egyptian Government to inquire into the treatment of plague in Bombay. The "British Medical Journal" for August 14th, 1897, gives the report of this Commission formed by Dr. J. G. Rogers, Director General of the Sanitary Department, Egypt, Dr. Bitter, and Dr. Ibrahim Pasha Hassan. "The Commission agreed that improved sanitation is the only method of staying the inroads of the disease. The members declare that prophylactic inoculation is a method, the efficacy of which is not yet established, and contend that to render a whole nation immune on the outbreak of an epidemic is a practical impossibility." In May, 1899, the Commissioners had an opportunity of proving the truth of their report, when plague broke out in Alexandria. It was "dealt with on lines directly opposed to Mr. Haffkine's views," as Dr. J. G. Rogers reports in the following letter to the "Times" of August 4th, 1899: "Sir,—M. Haffkine's address on the subject of preventive inoculations against plague delivered before the Royal Society, and the leading article in the 'Times' on the same subject, will excite considerable interest amongst practical sanitarians all over the world, and nowhere possibly more so than in Egypt, where plague is now being dealt with on lines directly opposed to M. Haffkine's views. In considering those views it must be remembered that M. Haffkine is not a medical man, and that therefore in all probability he looks on disease from the laboratory point of view. It is otherwise difficult to explain his persistent attitude of opposition to the work of the practical sanitarian, opposition which, in the opinion of many, has had a dangerous, if not a pernicious, influence in India. It must never be forgotten that, at a moment when, though somewhat late, practical proposals were put forward by a committee of practical medical men of long Indian experience for checking the spread of plague in Bombay, M. Haffkine alone dissented, and by throwing the full weight of his reputation into the scales against any really useful sanitary measures being applied, was mainly instrumental in these measures being rejected. Plague has been known to exist in Egypt since May 4th, and no doubt the infection was introduced before that date. By applying the sanitary measures so despised by M. Haffkine and denounced by him before the Royal Society as useless in the prevention of the saprophytic forms of disease such as plague, results not altogether unsatisfactory have been attained, and the disease has at least been kept well under control. The total number of cases has never risen above 13 in the week. In the weeks ending June 28th, July 5th, 12th, and 19th, they were respectively 12, 11, 9, and 6 cases of the disease. In 100 houses in which plague cases or suspicious cases occurred, and which were efficiently disinfected, in not one instance has a second case been noted. Had practical measures been abandoned and protective inoculations substituted, would these results have been attained? I venture to think not.—I am, Sir, yours truly, J. G. Rogers, Director-General, Sanitary Department, Egypt."

20486. Did Dr. Haffkine reply to that letter?—I have no reply. I do not know.

20487. Did he not on August the 12th in the "Times" decline to discuss details with Sir J. G. Rogers?—I do not know; but the consequence was that the plague was stamped out in Alexandria within seven months.

20488. Has Dr. Haffkine not recently returned to India to resume work under the Government?—I believe he has. At all events this epidemic at Alexandria in consequence of the application of sanitary measures without plague serum lasted only seven months.

20489. (*Chairman.*) In consequnce of what do you say?—Apparently in consequence of complete sanitary measures having been adopted, and no plague serum having been used, the epidemic was stamped out or ceased in seven months, which is the usual time for the duration of an epidemic of plague in any particular place. And on the 22nd October, 1899, the Minister for Foreign Affairs received a telegram from Lord Cromer stating that it was then three weeks since a case of plague had occurred at Alexandria. The "British Medical Journal" congratulated Sir John Rogers and Dr. Ruffer, "on the success which has attended their carefully devised scheme for stamping out the disease in Alexandria."

20490. (*Sir William Collins.*) Did not they employ sera in any form?—I understand not. I understand that no serum was employed; on the contrary, they declined to employ it.

20491. (*Chairman.*) When you say you understand so I do not always follow the source of the information. Do you mean from newspapers or books?—No, I understand from persons with whom I have conversed who are qualified to speak about it.

20492. Conversation?—Yes. But we have the testimony of Mr. Rogers to that effect.

20493. That you have read, I think?—Yes, who was Director-General. He at all events was of opinion that the sanitary measures were the cause of the cessation of plague.

20494. (*Sir William Collins.*) Is there any further question to which you wish to direct attention?—I now wish to call attention to the commercial aspect of the present system of sera and the present system of vivisection. Dr. George Wilson, in his Presidential address in the section of State medicine at the annual meeting of the British Medical Association in August, 1899, used these words: "The whole bacteriological theory and practice is steeped in commercial interests." This is at the back of all the arguments in favour of experimenting upon animals; that it constitutes in itself a well paid occupation.

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20495. (*Dr. Wilson.*) You are not quoting now from me?—No, only those few words. This is mine. The inventors of sera and antitoxins receives royalties upon the sales; the manufacturers of sera and antitoxins charge for them as for any other manufactured article is charged for. Mr. Power, the Medical Officer of the Local Government Board, explained in his evidence (Question 4885) that the vivisectors who do their pathological work receive a sum which pays for the animals used, pays for laboratory expenses, and includes their own personal remuneration.

20496. (*Chairman.*) What question do you say that is?—Question 4885.

20497. You must have got a wrong reference?—That is the reference that was given to me.

20498. You have not looked at it yourself?—No; you can easily imagine that I cannot verify every reference.

20499. Mr. Power's evidence begins at Question 4281.

20500. (*Sir Mackenzie Chalmers.*) You said you could not quite verify every reference, but this evidence is your own evidence?—Yes, only the remark in inverted commas is Dr. George Wilson's statement.

20501. (*Chairman.*) When you gave us just now the statement about payment to inventors and the manufacturers of sera, was that your own statement, or was it from some other source? Were you quoting?—Yes, that was quoted from Mr. Power's evidence.

20502. That is the point. I dare say it is quite correct, but we have not got the reference?—I can get the proper reference and insert it in the proof.* Then there is a quotation from Dr. Luteand to Dr. Bantock, stating that the Pasteur Institute is the property of a limited company——

(*After some discussion.*)

(*Chairman.*) I do not think we can have evidence of one doctor on matters of medical skill, simply through a letter written to another doctor.

20503. (*Sir William Collins.*) You desire, I understand, to draw attention to Messrs. Burroughs and Wellcome's price list?—I do. It is very evident that it would not be worth their while to keep up large laboratories and horse farms for the production of sera if the sale was not profitable. I desire to put their price list in simply to show that these are expensive sera; it shows the commercial aspect and the commercial value of these things.

20504. (*Chairman.*) I do not see what the distinction is between selling serum at a price and selling medicine at a price. Chemists do not sell medicine at a price that does not pay them. Is there anything immoral in selling serum at a price that pays any more than in selling castor oil or anything else?—There is nothing immoral about it any more than there is in any tradesman pushing the sale of his goods, but all I want to prove here is that there must be a motive in pushing the sale of these goods. I imagine that they are pushed just the same as the sale of any other article is pushed.

20505. Do you think that, in the case of a general practitioner in the country who made up his own medicines and sold them, it would be a reflection on his evidence about the value of the serum which you put to him in cross-examination to show that he was probably not speaking the truth?—I do not say that he is not speaking the truth.

20506. Or doing anything discreditable. I do not quite see what the discredit is. If people invent a serum which costs money to make I do not see why they should not sell it, and sell it at a profit?—I do not say that they should not sell it, and sell it at a profit. All I say is that when you sell things at a profit, and the profit comes to you, there is always an inducement to push the sale.

20507. (*Sir William Collins.*) Have you suggested that there is anything immoral in the sale of these

* *The reference is in Mr. Power's evidence—Questions* 4409 *and* 4411.—G. K.

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sera?—I have not suggested that there is anything immoral in it.

20508. (*Chairman.*) Anything to discredit?—What I suggest is that there is an inducement to push the sale.

20509. (*Sir William Collins.*) May it be that the sale of these goods is so extensive because they supply a felt want?—I do not say that it is not possible.

20510. (*Sir John McFadyean.*) I should like to put another question to you. Do you wish to put that list in as evidence, that the inventor in this country of any serum shares in the profits of Messrs. Burroughs and Wellcome?—That I have no means of knowing.

20511. What is the motive of putting in Messrs. Burroughs and Wellcome's price list if you do not suggest that any British inventor of a serum or any medical man who recommends it is getting any of the profits?—I suggest that the inventors of sera receive royalties on the sale.

20512. What is your source of information with regard to that?—At this moment I cannot tell you, but I will furnish the Commission with the information.

20513. Might I ask that you will furnish the Commission, in the proof of your evidence, with evidence that some British inventor of a serum is sharing in the profits obtained by the sale of it?—Yes, I will.

20514. (*Chairman.*) I think we should not admit any evidence of a personal kind of that nature unless it is really first-hand evidence; it must be something that would be really evidence in a court of justice?—I quite agree, My Lord. I think there ought to be something to support that statement that the inventors of sera and antitoxins received royalties upon the sales.*

20515. (*Sir John McFadyean.*) It is particularly British inventors that I wish you to notice?—The whole object of what I suggest here is not to prove that anybody has done anything disgraceful, but merely that there is an ordinary commercial aspect of the question and a monetary stimulus to pushing the sale as in any other case.

20516. My impression is that you have not produced any evidence to show that the inventors are commercially interested. That is what prompted my question?—I will endeavour to do so when I get proof.

20517. (*Sir William Collins.*) Do you desire to call attention to the question of vivisection as a means of training medical students?—The Parliamentary Association does not consider that there is any evidence to show that vivisection is necessary for the training of medical students. We consider that there is strong evidence to the contrary. Mr. Morris, the President of the Royal College of Surgeons, has reached that eminent position, according to his evidence before the Commission, without ever having performed an experiment on an animal. He also stated that there are many other practising surgeons who have never practised vivisection (Question 7786). Sir H. R. Swanzey, F.R.C.S., President of the Royal College of Physicians of Ireland, speaking of operations on the eye, declares (Question 9896-7) that he does not consider it necessary for a student to operate on an anæsthetised animal in order to acquire skill in such operations, but that he might obtain that skill by operating on the eyes of dead persons (I think his expression was dead persons). Mr. H. Morris, F.R.C.S., President of the Royal College of Surgeons, England (Questions 7751 and 7752) approves of some demonstrations before students for teaching, but not for operative purposes, and he gives the following evidence: (Question 7783) "You made your name, I think, which is a very good one, by experiments on the kidney, was it not? (A.) I never made an experiment in my life." (Question 7786) "Were those operations, which you have conducted so skillfully and fortunately, discovered, and were you made to do them by experiments on living animals? (A.) Personally, not at all. The operation that I did in the first place, and was the first to do, was done quite irrespective of any experiment on animals; that is, the cutting of a stone out of a kidney." (Question 7828) "I gathered from one answer . . . that you rather objected to the use of vivisection for the purpose of obtaining manual skill? (A.) Those are my feelings, I do not think it is necessary." (Question 7831) "Shall I be right in thinking that there are many practising surgeons who have never practised vivisection? (A.) Surely yes."

20518. Of course, all this evidence is before the Commission. Is there any special point that you desire to make upon it?—Merely this, that if the President of the Royal College of Surgeons, England, can obtain such eminence without having ever made an experiment in his life, that seems to be a refutation of the claim that operations on living animals are necessary for the training of students in manual skill.

20519. You also desire to refer to certain answers given to questions by Sir H. R. Swanzey. Perhaps if you cite the number of the questions it will be sufficient?—Questions 9896 and 9897. I think I have already stated the effect of those questions. Then there is Sir W. Thornley Stoker, Inspector for Ireland, who says (Question 761): 'I believe that such demonstrations during lectures are used more for the purpose of interesting and attracting a class than for any direct teaching value they possess. I am strongly of opinion that in any legislation which may be undertaken with a view to the revision of the Act 39 and 40 Vict. c. 77, experiments on living animals should be forbidden in illustration of lectures, on the ground of their uselessness, and perhaps cruelty also. . . . Such demonstrations cannot but be demoralising to the young men and women who witness their performance. They seem to me to be an offence against humanity." Then he was asked at Question 904: "But the fact that the experiment can be carried out without pain would not modify your attitude in the least?" and he said "It would not." That is the end of what I should suggest as to the evidence. I have been asked by the Committee of the Parliamentary Association to make an explanation with regard to an oversight in Mrs. Cook's evidence if the Commission will hear it.

20520. (*Chairman.*) I do not see how, when we have had a witness before us who has given evidence and been examined, we can ask another witness to give an explanation of what she said. It must stand for what it is worth?—She also represented the Committee of the Parliamentary Association.

20521. But I do not think that makes any difference?—Then that will be all I have to say.

20522. (*Sir William Collins.*) Does that conclude all that you desire to say in examination-in-chief?—That concludes all I desire to say if the Commission rule this explanation inadmissible.

20523. (*Chairman.*) There are just a few questions that I should like to ask you. I observe that you say on page 2 of your *précis*, at the bottom of the third paragraph, that the Government of the day were no doubt glad to get rid of so unsavoury and controversial a subject so speedily and so easily. The reference is to getting that Bill through in the last four days of the sittings of the House?—Yes.

20524. You say that it was a controversial subject; but is it not the very strongest possible evidence that it was not controversial that it was allowed to pass through the House in all its stages in the last four days? Could you have stronger evidence that the Bill was not a controversial Bill?—I think that a Bill, for the purposes of the House of Commons, if it is being pushed through in the last four days loses a great deal of its controversial character for practical purposes.

20525. Do you not know very well that the Government are always asked towards the end of a Session what Bills they propose to press, and they say: "We propose to press such-and-such Bills, but not if they are controversial"? Unless they are really absolutely essential Bills they do not at that stage bring in a new Bill and pass it through unless both sides of the House are prepared to let it go?—It is a controversial subject. I am not prepared to say whether in those days it was regarded as a controversial Bill. Of course, as you know yourself, there are many degrees of the controversiality of Bills.

20526. When you spoke of it as controversial, and at the same time as having passed through the House in the last four or five days of the Session in all its stages, it struck me as a very strong piece of evidence that it was not a controversial Bill, but was generally

* "*Behring has patented his diphtheria anti-toxin serum on the Continent; Koch for years has made a princely royalty out of his tuberculin.*" *From the Presidential Address in the section of State Medicine, at the Annual Meeting of the B.M.A., August,* 1899. *By George Wilson, M.A., M.D., LL.D.*

assented to?—What I rather referred to was that it was, at all events, controversial in the country, if it was not controversial at that time in Parliament. Of course at that time, between the 10th and the 15th of August (and particularly in 1876, I think it was), I do not suppose the attendance of members was as it is now. They had mostly gone away for the grouse shooting, and so forth, before the 15th.

20527. I should have thought, after some experience of the House of Commons, that it was very strong evidence that it was not considered controversial?—I bow to your greater experience.

20528. You speak of there being 43,000 experiments in 1906. Do you know at all how many of those were inoculations?—No, I do not.

20529. The vast majority?—I do not know whether there is any means of discovering that.

20530. Are you aware that a large number of the 43,000 were experiments ordered and conducted on behalf of the Board of Agriculture, the Board of Trade, and the Local Government Board?—Yes, I believe that a great many of them were.

20531. I am told that about 6,000 were operations under that head, and about 20,000 in cancer research?—Those 20,000 in cancer research were inoculations.

20532. They were for what we may call a semi-public inquiry; it was not on the same footing as a Government inquiry?—No.

20533. Then I see you rather suggest that Dr. Starling's evidence on the question of pain is inconsistent with the views of other physiologists; I am leaving out Dr. Pembrey for the moment, but inconsistent with other statements. You say, for example, in the middle of page 7 of your *précis*, that he has said, and you seem disposed to doubt whether it is true, that he had on no occasion ever seen pain inflicted in any experiment on a dog or cat?—Yes.

20534. If you read the whole context I think you will see that it is quite plain that Dr. Starling is speaking about experiments under anæsthetics. He says that he always administers anæsthetics and that he administers them efficiently, and it is with reference to that that he is speaking when he says that he has never seen any pain inflicted in any experiment. You speak afterwards as if he was guilty of want of candour in saying that, because it is admitted that after animals have recovered (in cases where they are allowed to recover), they feel pain from the consequences of the operation. You observe that Professor Starling has never said the contrary of that. He says that in an experiment he has never inflicted pain, because he always used anæsthetics. He says that there may be pain afterwards in cases where the animal afterwards recovers. There is surely no want of candour there. You accuse him very strongly of want of candour. Is there any want of candour in those two statements?—I cannot find the second statement.

20535. You say on page 10 of your *précis*: "Professor Pembrey, in direct contradiction to Professor Starling and others, tells us that he does painful operations, and that, moreover, 'they are absolutely necessary.' Thereupon we are confronted by the question: Were those other gentlemen then imposing upon our credulity, or were they the victims of their own? Can extensive mutilations under anæsthesia administered, as Professor Starling confessed, by the laboratory boy, be wholly and invariably painless? Painful experiments, he says, are 'absolutely necessary.' This, then, is the candid truth of the matter." Are you aware that Dr. Starling's evidence on that point, as to pain not being suffered under anæsthesia, and as to a certain amount of pain being suffered afterwards, is the evidence of, I may say, a dozen of the most eminent physiologists and representatives of universities who have come here to give evidence before us?—I am perfectly well aware of that, but, as I understand, the anæsthetic, which I suppose was chloroform, was administered in the case of Dr. Starling's experiments by the laboratory boy. I think somewhere in his evidence, if I am not mistaken, there is some statement that when the animal began to move the boy increased the dose of chloroform.

20536. I do not know whether he said "boy"?—The laboratory boy.

20537. But that is hardly the point. The question here is that you are contrasting Professor Pembrey, who says that he does painful experiments, and that they are absolutely necessary, with the evidence of Professor Starling and others; and you speak of it as a question of candour, because you say that Professor Pembrey is a man who speaks the candid truth in the matter, implying that the others are not candid?—We certainly do imply that; and it seems to me obvious that if the anæsthetic is administered by the laboratory boy, and if the laboratory boy is entrusted with the duty of putting on more chloroform if the animal moves, those experiments cannot be absolutely painless.

20538. You will see that many questions have been asked on that subject, of how it is administered and by whom, and so forth, throughout the evidence, which I cannot take you through; but what I am pointing out now is this. I understand you to say that, although these 12 gentlemen practically agree in their statements about anæsthesia and its being complete, and about there being no pain suffered at the time by the animal, but that some pain is suffered afterwards, they are not candid witnesses, because they say that and because Dr. Pembrey says the reverse. You accept him as a candid witness, and the other 12 you do not?—He certainly was candid. He said "painful experiments are absolutely necessary."

20539. But you contrast him with their evidence. Do you suggest that these other 12 gentlemen were uncandid witnesses in what they said?—They have——

20540. I should have thought it would not be difficult to answer that with yes or no?—I always presume that these gentlemen have a different idea of pain from the idea that we ordinary laymen have.

20541. Then it was not so much a want of candour, as that they were under a delusion?—You may put it in that way if you like.

20542. Which do you prefer to put it as: that it was delusion or want of candour?—My Association think it is want of candour. I am somewhat doubtful whether it was from want of candour or a mistaken idea—whether it came really from a certain callousness imbued in them by their profession.

20543. Your society's suggestion then is, that all these very eminent members of their profession have come here and, as you say, have been "imposing upon our credulity"?—Well, we are told by another of them that pain is caused—that is "absolutely necessary."

20544. But do you say so. Never mind about the evidence of others at present. You say, "Were those gentlemen imposing upon our credulity or were they the victims of their own"? You say that your society (whatever you may think) do not think that they were the victims of their own credulity. Then we must adopt the other alternative, that they were imposing on our credulity. Is that your society's view?—I said: "Were these gentlemen imposing on our credulity?" In that case it would make them out as making an immoral statement. "Or were they the victims of their own?" That means, were they under a delusion? We ask the question.

20545. But you said that your society's view was that they were not under a delusion?—I should say that that is the view of the society.

20546. Then it is a want of candour?—Yes.

20547. Then we are to understand that these 12 gentlemen came here for the purpose of deceiving us, and Dr. Pembrey is the one honest man?—I do not say that he is the one honest man, but he seems to have been honest.

20548. You are aware that Dr. Pembrey's views on the whole subject differ very largely from those of his confrères?—Yes, I know they do.

20549. His view seems to have been that pain is a blessing and that anæsthetics are a curse?—Yes.

20550. (*Mr. Ram.*) To human creatures and to animals?—Yes.

20551. (*Chairman.*) And that the Vivisection Act is entirely opposed to the advancement of physiology in every part?—Yes.

20552. You are aware probably, if you have read the evidence, or have had it read for you, that I think I might say almost all the witnesses who have been called as physiologists are not opposed at all to the present restrictions as to anæsthesia; they are content with the restrictions imposed, with the obligation to use anæsthetics?—I believe those exceptions and restrictions in the original Act were really concessions

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to the medical profession, and therefore it is to be supposed that they are satisfied with them.

20553. I am putting the question, not for the purpose of arguing whether they were right in saying so, but as showing that their views and Dr. Pembrey's views are quite different on the main points in dispute, that they take quite a different view?—I was going to say that on the Second Reading in the House of Commons, Dr. Ward, one of the representatives of the medical profession, moved the rejection of the Bill, and after explanations from the Government, who professed great sympathy with the medical profession, he withdrew his opposition to the Bill.

20554. What Bill was that?—That was this Bill that is at present operative, the Cruelty to Animals Act, 1876; thereby admitting that the Bill was in conformity with the views and desires of the medical profession.

20555. There are such things as compromises, but I am putting it that it was in conformity with the present views as regards anæsthesia?—I may say that the society would not admit that there was any compromise about the Cruelty to Animals Bill.

20556. I do not know how the fact was in the least. I am merely suggesting that the mere fact that a person withdraws his opposition to a second reading does not mean that he agrees with everything in the Bill always?—No, that is so.

20557. (*Sir William Church.*) Referring to the bottom of the fifth page of your *précis*, do you think that the community suffers at the present moment from a want of humanity in the medical profession?—I think it does to a certain extent. Of course, you are asking me a question which I can only answer from my personal experience, and from what I have heard, and so forth. I think that there are a certain number of uncommonly rough physicians and surgeons in the profession.

20558. And you think that that number has increased since 1876?—In consequence you mean of what?

20559. I do not say what it is in consequence of; I ask you whether you think that the number of rough physicians and surgeons now existing is proportionately greater than it was before the Act of 1876?—That I cannot tell; it is impossible to tell. I was not acquainted with the circumstances before the Act.

20560. Then you have no evidence whatever that the introduction of the Act and the working of the Act have led to the demoralisation of those who enter the medical profession?—I think the medical student is a good deal rougher than he used to be.

20561. Is that the generally received opinion?—That is the general view, I should think.

20562. Do you mean to tell the Commission that it is the generally received opinion now in the world that the medical student is a more uproarious and rougher individual than he is described in books written some 50, 60, or 70 years ago?—I am not prepared to say that, but I think he is rough enough, at all events.

20563. That is not my point. I wish to know what evidence you can give us of the statement that the medical profession is rougher now, and I suppose you mean by that is less regardful of the feelings of those who come under their treatment, than the profession was 30 or 40 years ago?—The only evidence that one can give of that is hearsay. I do not for a moment suggest that the great mass of the medical profession are not exceedingly humane. I think they are. Personally I have never come across a doctor who has not been humane. But at the same time I can only imagine, speaking for oneself, that seeing these experiments carried out day by day all the horrors of it would have the effect of blunting one's sense of sympathy.

20564. Therefore if I might so say, it is only what you imagine to be the case. You have not any evidence to bring before us that it is the case, but it is what you imagine might be the case?—It is just the same thing, or it is very much the same thing as that a man who shoots a great deal is utterly insensible to the sufferings of the animals that he wounds, and the more he shoots the more insensible he becomes to it. I cannot think that it is improving the humane sense in them.

20565. So that we are all degenerating—our sportsmen have degenerated, too, because they kill more pheasants in a day now than they used to do 60 or 70 years ago?—I do not know that our sportsmen have degenerated, but I know that I myself gave up shooting because of the horrors of it. I used to shoot a great deal, and in consequence of the horrors that I saw I gave it up altogether..

20566. (*Mr. Ram.*) Then it did not have the normal effect upon you, Sir George?—What do you mean?

20567. You are putting it that the effect of much shooting is to inhumanise a man. It had the opposite effect upon you, I gather?—As a general rule I mean, but sometimes it turns the mind in the other direction, and it did so with me.

20568. (*Sir William Church.*) Then with regard to what you say on page 6, has clinical research been neglected, do you think since demonstrations have been shown in teaching medical students?—Of course, I am not an expert, and I am not qualified to answer these questions thoroughly, but I have heard from certain doctors whom I have come across in my position as honorary secretary of this Association, and whom you would call anti-vivisection doctors, that clinical research is to a great extent subordinated in these days to experimental research.

20569. Have you any knowledge of the present curriculum which is required of a medical student?—No.

20570. Then you have no knowledge, I suppose, of the medical curriculum that was required before; say 40 or 50 years ago?—No.

20571. You are not aware that now it is incumbent upon a medical student to attend for a fixed number of months upon clinical teaching and clinical observation?—I am not acquainted with the course of study of medical students.

20572. But you stated in your evidence that there are other means, that medical scientists are not driven to use experiments only, and you said, in answer to Sir William Collins, I think, that you meant by that that clinical research might be carried further?—Those are the views of my Association, and I think they are the views of a certain portion of the medical profession.

20573. But I rather gathered, I may, of course, have misinterpreted you, that you considered that the barren state of our knowledge was because clinical investigation and clinical research were not carried out, and were neglected for experimental research?—No, I have not said so. What we have said is that in all these years during which vivisection has been practised it has not had any great effect in either curing disease or preventing disease.

20574. Have the other means of research which have been in use for ages had any great effect?—I think at all events it produced all the knowledge that we have.

20575. Is it not the case that the great incentive for scientific research in physiology and in pathology is in the hope of supplementing the knowledge that we get from mere observation of disease?—I presume that that was the reason of the introduction of vivisection, but it does not seem to have had any result up to the present time. That is what we contend.

20576. I would beg leave to differ from you, because surely the foundations almost of medical knowledge arise from experiments made upon animals two thousand years ago, when the first cases that we have information of began. Do you think that no knowledge was obtained by the experiments of Galen?—I am not acquainted with the experiments on animals at so distant a date; but all that we affirm is that no real knowledge has been obtained as to curing disease or preventing disease from the vivisection experiments under the Cruelty to Animals Act. That is what my Association are prepared to contend.

20577. You mean that your Association are unable to find any direct practical application of any group of experiments since the Act of 1876 was passed?—I do not mean any practical application, because there has been plenty of practical application, but no beneficial application.

20578. Therefore you do not consider that any benefit whatever has been derived from the use of any what I will call sera, in the treatment of disease?—No, on the contrary, we should be inclined to hold that more lives have been sacrified by the introduction of sera than if they had not been used. We have an instance that such is the case in Koch's tuberculin, which I believe led to the loss of a considerable number of lives, and has now been exploded.

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20579. I beg to differ from you there altogether, it has not been exploded. But that is your position, that really no benefit has been proved yet to have resulted either to men or animals from the use of sera?—We should go further and say that harm has resulted, as in the case of plague.

20580. But you say you have no medical knowledge, and therefore I will not go into any actual specific cases in which I think you would find a difficulty in maintaining your view?—You quite understand that I am not a medical expert. I am merely dealing with this question as an outsider, with it is to be hoped a certain amount of common intelligence and sense; that is all.

20581. I think you told us that you are in the habit of reading books on medical subjects?—I read some that are germane to this particular subject.

20582. I suppose your reading is chiefly directed to works which are written by those who hold the same views as yourself?—No, I do not say that. One question which I have been greatly interested in, on public grounds of course, is the question that I have dealt with in my evidence at some length, namely, the question of bubonic plague in India. That is a question on which I have read.

20583. I should like to ask you a few questions on that point. You gave us as an instance of a disease in which no increase of knowledge had taken place, influenza. What makes you take 1729 as the year that you start from; is there any reason for taking it?—That is our first record of the invasion of influenza, so far as I know.

20584. I think if you look you will find that influenza epidemics were described long before that time. The first one that is described goes back to the fifteenth century. I should have thought that the argument that you make use of with regard to influenza is just an argument the other way. Perhaps you are not aware that experimentation in bacteriology has rendered it probable that we are acquainted now with the organism with which is always associated, if it is not the one which causes influenza; so that we have advanced somewhat in our knowledge?—I did not know that a particular bacillus had been discovered; but, of course, I am aware of the theory that the bacillus is not the *causa causans* of zymotic diseases generally.

20585. You do not think that the bacillus that is stated to be at all events always found with influenza is the *causa causans*?—Of course, I am always obliged to guard myself by saying that I am not a medical expert; but as I understand the two theories, one is that the bacillus is the cause, and the other is that there is a poison surrounding the bacillus on which really the bacillus feeds.

20586. If the bacillus produces the poison?—I did not say that. I said on which the bacillus actually feeds, in which it lives, and that the bacillus is really a cleansing and curative agent.

20587. Then I will not pursue that any further?—But all I say is that I am aware of those two theories. I am not medical expert enough to be able to adopt one side or the other.

20588. I will not pursue that any further: but I think that, in answer to a question by Sir William Collins, you mentioned the drugs of antipyrin and sulphonal, which you said had had great repute, and were now exploded or were not in favour. What leads you to say that?—They are not used now.

20589. Would you think I am wrong if I tell you that these two drugs and several others which are similar in composition are used daily, and I might say hourly, throughout certainly this country—I should think almost throughout the world?—I am informed that antipyrin is very seldom prescribed now because of its effect on the heart, and that sulphonal is also not prescribed generally because of its cumulative effects.

20590. May I ask you who was your informant? Where did you get that information?—I got it from a member of the medical profession.

20591. Did he tell you that he did not use them, but that he probably used bodies closely resembling them, which have also been introduced of late years?—That I do not know; but I do know also that in my domestic pharmacopœia sulphonal and antipyrin once played a very considerable part, and now they are not prescribed.

20592. I will not say the contrary?—That is all.

20593. Then on page 7 you say that "the evidence as to the pain suffered by animals in process of mutilation is wholly untrustworthy and unreliable, either because of the inaccuracy of statement or the inaccuracy of observation of the operators, or because under the Act as at present carried out one operator actually experiments without pain, while another confesses that he has done painful experiments." I should wish to ask you whether you are alluding to experiments carried out with the same object. Do you mean to say that when two experimenters carry out the same object, in one case the experiments are done with pain and in another without?—Do you mean similar experiments?

20594. I do not mean similar; I mean the same experiment done by two experimenters—an experiment directed to the same end?—That it may be carried out with pain in one case and not in the other.

20595. Yes?—I imagine that that is quite so, according to whether the anæsthesia is complete or not. Of course we do not presume to say that complete anæsthesia does not prevent pain. Of course it does.

20596. I did not gather that. What you mean, then, is that the difference in the result may be due to one experimenter having his animal under an anæsthetic and the other having his not under an anæsthetic?—Or, rather, one having the animal completely anæsthetised and the other having it imperfectly anæsthetised.

20597. You do not give the experimenter (who is a man whose whole attention has been directed to these subjects) the credit of being able to know whether he is performing experiments under one condition or the other?—In many cases, as I understand, for the practical purposes of the experiment the anæsthesia is kept as light as possible. I do not mean that it is not complete, but it is not very heavy anæsthesia.

20598. (*Mr. Ram.*) Do you mean that it admits of suffering?—I mean that in that case there may be intervals of waking, and therefore intervals of consciousness, during which pain is caused.

20599. (*Sir William Church.*) Then I gather from that that you would not grant that any experiments are valuable for the acquirement of knowledge, but that, if they were, you think they ought to be done without any anæsthetics at all?—No. I say that they ought not to be done at all, with or without anæsthetics—at least, that is the view of our Association.

20600. Then you rather complain on page 8 with regard to the mice that have tumours. You say: "We are not told of means taken to restrict the growth." Have you ever made any inquiries as to what is done with mice when the tumours become large? Has your society ever asked for any particulars?—I do not know, but I confess that as a mere ordinary layman I do not understand the advantage of implanting a piece of cancer in a mouse, and making it a tumour, because that is not the way in which cancer comes in a human body.

20601. I will not go into that; but, as a matter of fact, your Association has never either written or asked the Cancer Research Fund anything about the mice, and never asked to be allowed to see them, or anything of that sort?—I do not know that they have.

20602. They are not aware that if a tumour is likely to be produced, say in a mouse, which in any way produces symptoms of pain, that mouse is killed, the object having been probably obtained?—I have no doubt that it is killed. I take it from you that it is. But of course I do not know that we have made any inquiries as to what happens. I suppose it is killed.

20603. Of course, no method is taken to restrict the growth, because the growth is what is required. It is the only way by which material for experimentation can be obtained, by keeping up the growth in a succession of animals; but no questions have ever been asked as to whether the animals are kept in pain or not?—I should imagine that during the growth of the tumour they must feel pain, judging from the analogy of human beings, at all events. I am not prepared to say more.

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20604. It is a very natural thing to imagine, but I merely mean that your Parliamentary Association has not made any inquiries into the subject?—That I cannot say.

20605. What is the reason that leads you to state on the same page, "There must necessarily, upon waking up from anæsthesia," (that is after operations on man) "be very severe and prolonged pain until the severed tissues heal"? Is that the result of your reading or your information?—That is in the extensive cutting operations, with excision of organs. I should have thought that any ordinary layman would know that.

20606. Then that is not the result of any inquiries that you have made of persons who have undergone severe operations which have healed by first intention? —This *précis* has been prepared by members of the medical profession, and I imagine that they, at all events, know from experience that such is the case, although I have no experience of the kind.

20607. Might I ask by whom this *précis* has been prepared?—It was prepared by the Parliamentary Association generally and their executive, and there are in it members of the medical profession.

20608. Mr. Stephen Smith?—There are several. I do not wish to mention their names.

20609. Is he one?—He is not one.

20610. Miss Arabella Kenealy?—Yes, she is one.

20611. (*Mr. Ram.*) You told us that you represented experts?—Yes, to a certain extent. There are a certain number of experts connected with our Association. When I say experts, I mean fully qualified medical experts.

20612. Who assisted in the preparation of your *précis*?—Yes.

20613. Will you give us the names of any of those who assisted in the preparation of your *précis*?—I have already said Miss Kenealy.

20614. Any other?—Dr. Bourchier, and, I think, some others.

20615. (*Sir William Church.*) Your Association has not inquired into that question, I gather then, from those associated with the large general hospitals, and who perform many operations both in hospitals and in private?—I understand that the drift of the question is to show that with extensive operations with excision of organs, after the operation has taken place and the anæsthesia is over, there is no pain. That is what you would suggest?

20616. Not severe and prolonged pain. I suggest certainly that it is not necessary that that should take place?—I have not had many operations performed upon myself. I have had some little ones, and I have always found that any cutting out of anything, even the cutting of a fishhook out of a finger, is followed by pain. I have had a salmon-fly through my finger, and had to have it cut out. I have always found that that is followed by considerable pain, even that small operation. I cannot for a moment imagine it possible that after an operation such as the excision of organs there can be no pain until the tissues heal.

20617. First of all you say that it is in accordance with what you think yourself would be the case, without inquiry, and it is founded upon information that you have received from those people whom you call experts, whom the Parliamentary Association have consulted?—That is to say, it is a statement with which I, simply as an outside observer and an ordinary layman of some experience and a little intelligence, agree; that is all.

20618. Would you tell me where Dr. Pembrey says that he himself circumcises children without anæsthetics; I do not read his answer quite in that way?—The reference is not given.

20619. What he states is that circumcision when performed as a religious rite is not done under anæsthetics, and he does not say that he always does it himself without anæsthetics. What he states at Question 14,161 is: "I have circumcised children without anæsthetics." He does not say that he habitually does it. "If they cry it does not matter; it is much better that a child should cry and moan than that its life should be lost. That circumcision is not such a serious operation is shown by the fact that the ordinary medical man does circumcision under anæsthetics and the Jewish non-medical man does it without anæsthetics." And he himself, apparently having seen a death in a child from anæsthetics during the operation of circumcision, does not always use it for circumcision. But he does not state that he never uses anæsthetics.

20620. (*Sir Mackenzie Chalmers.*) I understand that he says that Jews never use it, and they circumcise largely?—He says that he has done it without anæsthetics, but he does not say that he habitually does it. That is the point, is it not?

(*Sir William Church.*) Yes.

20621. (*Sir William Church.*) I should like if I might to ask you a few questions about the plague in India. I gather from what you have said in answer to questions, and also from what you say on page 15 of your *précis*, where you say "A manufactory of plague was established," that was by the use of serum, "which has been hard at work ever since producing its five and a quarter million deaths," that the view of your Association is that the use of Haffkine's prophylactic has actually caused a spread of the disease?—The view that we hold is that the use of Haffkine's so-called prophylactic has not only not acted as a prophylactic and not reduced the number of cases, but that it has actually increased them; that it has created centres of plague where none existed before, and that it has as a matter of fact kept up the epidemic. I do not know whether it is all due to Haffkine. I think Yersin's serum is also answerable for some.

20622. But at all events the use of sera has caused an actual spread of the disease directly by its use?—Yes, that is our view.

20622A. Not because that has caused the Government to cease taking other precautions; that is what I want to get at?—I understand that the sanitary method of combating the disease is not taken to a very large extent in India.

20623. That I gather. You contrasted the sanitary steps which have been taken in India with those which were taken in Egypt?—I understand that sanitary steps have not been taken to the extent, at all events, that they have been taken in Egypt.

20624. In your opinion, therefore, the Government of India has been rather remiss in not taking more active sanitary steps in India in combating the plague?—I think so, and I think also that the Government is very much to blame, with all these facts before it, for not having taken active steps to ascertain whether Haffkine's serum did actually propagate plague or not.

20625. I will go to that in a short time. Are the circumstances of Egypt and India quite comparable. I mean with regard to taking what are called ordinary sanitary precautions?—I take it that it is a great deal more difficult to effect complete sanitation in India than it is in a country like Egypt, which is what I may call more closely governed.

20626. Therefore there would be greater difficulties in carrying out in India measures such as were adopted in Egypt?—No doubt.

20627. You allude, I see, to the state of unrest in India, and you say that the plague was one of the chief causes of unrest in India?—So we read—that plague, famine, and so forth, are the chief causes of unrest in India, or among the chief causes.

20628. In what way did this unrest in India arise in connection with the plague?—I think the people, whatever the Government may think, are gradually becoming converted to the same view as I am taking to-day, that the inoculation is actually causing plague amongst them.

20629. You think that it was the introduction of inoculation that caused the unrest?—I will not say the introduction of inoculation, but the continuance of the inoculation, and the fact that the plague has not diminished at all by the inoculation, but rather increased.

20630. You do not think that it is the attempt to put the rules of sanitation in force that has caused the unrest?—I have not heard so.

20631. And you would prefer not to give any opinion, I suppose, whether you think it is probable that that is the cause of the unrest?—Of course, if we were to go into the causes of unrest in India, we should be getting into quite political matters, if we deal with that very much. But the causes of unrest are of various kinds.

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(*Chairman.*) Do not let us go into that question.

20632. (*Sir William Church.*) But, as a matter of fact, are there not reasons why it is very much more difficult for the Government to put laws of sanitation in force in India, than it is in Egypt?—There are reasons, of course, connected with the religious creeds of the people.

20633. In fact, you cannot move the population about, you cannot move the population from a village to another spot without making arrangements for the different castes being separate, can you?—That is so. But I was not arguing merely that if they had taken measures of sanitation in India to the same extent that they have in Alexandria they would have stamped out the plague. I was rather arguing that if they had left things alone without the introduction of Haffkine's serum, the plague would, so to speak, have burnt itself out.

20634. That is to say the actual use of Haffkine's and Yersin's sera have disseminated the plague?—That is our view.

20635. And how have you arrived at that view?—No epidemic of plague, I believe, before this one has ever lasted more than about seven months, and the only condition in which this epidemic of plague differs from others is in the introduction of these sera. That is one reason.

20636. Has the present epidemic of plague that is going on in India been equally severe over all parts of the country?—That I cannot answer you.

20637. India is a very large country?—Certainly.

20638. You are not aware at all of the way in which the mortality has varied in different parts of India, according to the Government Returns?—No, but I can well understand that from natural causes it would vary in that way.

20639. I have the returns before me for five years, from 1901 to 1906. Your Association have not looked at those to see how the mortality has varied in different spots at different times during those five years?—Probably not.

20640. Have your Association read any of the reports of what I might call the experimental use of Haffkine's or Yersin's sera?—I myself have a book here which is very interesting on the subject of these sera, by Professor Montenegro.

20641. That was before the present epidemic, was it not? Was not his book prior to the present epidemic in India?—This book was written in 1900, during the epidemic.

20642. At the commencement of it?—Yes, and it gives sundry particulars about Haffkine's lymph and Yersin's lymph, and Lustig's vaccine. I observe that one of your witnesses before the Commission, who was an Indian medical man, Colonel Lawrie, said that when Haffkine's serum came to him, as a general rule it was putrid.

20643. Do you know the results of the use of the serum at the Byculla gaol?—No.

20644. Would you accept it as correct that of the prisoners living exactly under the same conditions so far as regards lodging and food and possible communication with the outside, 172 were not inoculated, and among those there were twelve cases of plague, six of which proved fatal, and on the other side of the jail, where there were 147 inoculated, there were two cases and no deaths. If the serum spreads the disease, how was it that there were not more cases on the inoculated side of the jail than on the uninoculated side?—As I understand, the whole of one side of the jail was inoculated.

20645. 172 were not inoculated, and 147 were?—Were they all mixing together?

20646. No, they were not?—I thought you said that they were on different sides of the jail?

20647. We will leave that out, as I am not positive about it. But it is the case that 172 were non-inoculated and there were twelve cases of plague and six deaths, and of the 147 cases inoculated there were two cases and no deaths?—But everyone of the inoculated cases had plague.

20648. No, this was for prophylactic purposes?—With Haffkine's serum?

20649. Yes?—But I read here that Haffkine's serum whenever it is given always induces a mild attack of plague.

20650. I beg your pardon. This was for a prophylaxis purpose against the plague?—I quite understand that, but the prophylaxis consists in the inoculated persons having a mild attack of plague.

20651. I will not go into that question, but they were healthy persons. It was not done for cure; it was done for prevention?—Yes, I understand that; but my point was that inasmuch as all these persons who were inoculated had a mild attack of plague, they could not possibly disseminate it—they all had plague, although it was mild. The two cases, I suppose, that died were cases of virulent plague.

(*Sir Mackenzie Chalmers.*) The two cases did not die.

20652. (*Sir William Church.*) Two cases had it?—Yes, then they were cases of virulent plague.

20653. Two cases apparently, according to you, had a second attack, one when they were inoculated and subsequently another attack?—Or had it virulently. But it is admitted, as I understand, that Haffkine's serum causes a mild attack of plague.

20654. (*Sir John McFadyean.*) Do you mean admitted here before the Commission?—I understand that it is admitted by the medical profession, and by Haffkine himself.

20655. (*Sir William Church.*) At all events, you are not prepared to contest those figures that I gave you?—No, only I should distinctly contest the deductions from them.

20656. I only wanted to know whether your Association have really taken the trouble to have any of the returns, which could easily be got now, before them and investigated them?—That I cannot tell you. But I do not think I should judge on such a return as that. There is no reliance whatever to be placed upon it, because although the return may be perfectly correct, it is vitiated by the fact that all cases inoculated with Haffkine's serum have a mild attack of plague; that is, they have a mild attack of plague, and they are centres of infection themselves wherever they go, and, as I understand, also they are only immune for a very short time, I forget how long.

20657. I quite appreciate your position now, and I will not ask you anything further about it, as it is a scientific and medical question. You say the action of the serum so far as plague goes is similar to that of inoculation in small-pox formerly?—Yes.

20658. I did not understand that that was your position before. Does that also apply to the use of other sera besides plague sera, that when you make use of sera you give either the animal or men the disease that you are trying to give it immunity from?—I take it that it does in some cases. I presume that you give it a modified form of the disease, otherwise no immunity would be conferred.

20659. Have your Association taken any trouble to inform themselves on that question?—I have no doubt that they have.

20660. But surely you must know whether they have; you come to represent them?—Of course, I cannot possibly tell everything that my Association have done.

20661. It is one of the most important and most difficult subjects, and one that you seem to have had a great deal of advice upon?—I cannot tell exactly what my Association have read. There are a good many members of it.

20662. So that really I may take it, I do not wish to say anything offensive, that personally you have brought forward the plague in India as an instance before us without having any personal knowledge of the subject, and apparently without the Association having taken very great pains or labour to investigate the subject?—I do not admit that at all. I admit that I have not had any experience from the point of view of a medical expert. I have not been in India, and I have not seen the plague; but I do say that I have read a good deal about it, and that I know as much (as I have indicated originally in offering my evidence to the Commission) as any ordinary intelligent educated man would know about such a subject, and I think I am entitled as one of the general public to draw my deductions from the facts that are reported to me.

20663. But your deductions were a most serious charge against the Government. It comes to this, that not only have they neglected enforcing what are called the ordinary sanitary laws, but that they actually spread plague by the means they have taken?—

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It is a very serious charge, but undoubtedly it is a charge that the natives of India are beginning to make, as well as some people at home.

20664. I will not pursue it?—If I might supplement my answer to one question as regards the effects of Haffkine's serum, I will read a passage from Montenegro, at page 73: "The inoculation causes a mild attack of plague. In six hours the patient suffers from malaise, headache, depression, and fever, and this condition continues a couple of days, at the end of which there is usually vomiting and diarrhœa. The symptoms disappear slowly. The site of the inoculation is much congested, the skin desquamates, sometimes eruptions appear." That is suggested here by this Professor Jose Verdes Montenegro, a distinguished medical man in Spain.

20665. (*Chairman.*) Do you know that he is a distinguished physician?—He is ex-Interne of the Central University of Medicine, Associate Physician of the Hospital de la Princesa, and Professor at the Municipal Micographical Laboratory, Madrid. He is a distinguished man.

20666. If this is to show that the fever which is given by inoculation is of a serious kind, I think that we are all agreed that the plague is a very serious disease?—It is to show that the inoculation actually causes plague.

20667. That is the question I asked you. Is this a description of the disease which is imparted by inoculation?—Yes.

20668. (*Sir Mackenzie Chalmers.*) Did you read Dr. Martin's evidence, who came here, who had inoculated himself twice or three times?—No.

20669. He gave us his personal experience of inoculation, which does not quite accord with Professor Montenegro's.

20670. (*Sir William Church.*) I think I can throw light upon it. I think I am right in stating that Dr. Montenegro's work was published in the last century; that is to say, about 1899, or some such date as that?—1900.

20671. And is it possible that knowledge with regard to these bodies has advanced since then?—I am not prepared to say. I do not know whether the lymph is differently obtained now, or what happens.

(*Dr. Wilson.*) But your contention is, I think, that immunity cannot be conferred in the case of any of these diseases for which vaccines or sera are used as preventives unless a mild attack of the disease is induced.

(*Chairman.*) Is not that a medical criticism? I do not think the witness really professes to lay down general rules about medical effects.

(*Witness.*) No; only that immunity is stated to be conferred by Haffkine's lymph, and it continues for six months, but it does not continue for longer.

20672. I understand your view to be in this case that Haffkine's remedy given as a prophylactic gives the disease itself in a mild form?—That is so.

20673. And that the disease in a mild form is capable of being communicated to others in a virulent form?—Yes.

20674. And therefore it spreads it?—Yes.

20675. But, of course, you are here speaking from information that you have gathered from books and conversations, and not from your own personal knowledge?—Of course.

20676. (*Sir John McFadyean.*) I think you said that you were so impressed with the pain caused to animals in shooting that you had given it up?—Yes, that is so.

20677. Could you tell us how long it is since you gave it up?—About 15 years.

20678. I understood you to say that, while disclaiming any expert medical knowledge, you felt quite competent to give evidence before this Commission on such questions as anæsthesia, and the results of experimentation on animals, in virtue of the fact that you were a person endowed with ordinary powers of observation and common sense?—Quite so.

20679. To what extent have you exercised your power of observation in judging as to the possibility of anæsthetising animals sufficiently? One of the questions you have given evidence on is anæsthetics?—Yes.

20680. To what extent have you exercised your powers of observation with regard to the controversial matters relating to that?—I am afraid that I do not understand the purport of the question.

20681. I do not want to misquote you, but I think you said that ordinary powers of observation and common sense would enable one to judge with regard to such questions as anæsthesia, and the value of the results obtained by experimentation on animals?—I do not mean observation in the sense that I have witnessed experiments on animals, for I have not.

20682. Then what did you mean by observation, may I ask?—I was rather referring to observations that one had of pain in animals; and you yourself referred to one thing, one's observation in the shooting field.

20683. But it was no question of anæsthesia that came in there?—No, it does not come in.

20684. I think the question with regard to which you made the statement was whether experimentation on animals had advanced medical knowledge or not, and I took it down that you said that ordinary powers of observation and common sense enabled one to judge with regard to that question?—Observation in that sense means that one observes that the results, at all events of medical knowledge, are no greater than they used to be so far as one can see.

20685. But can you really claim that your observations with regard to that point ought to be allowed to have any great value with this Commission?—That is for the Commission to judge.

20686. Would you not admit that, in so far as your evidence has any value on that and other subjects, it is derived mainly from the fact that you have weighed the evidence bearing on the question?—To a certain extent, yes.

20687. Is it not mainly that as regards the question of anæsthesia, for instance?—I suppose that most men get their knowledge from reading unless they are engaged in actual experiments, and I admit that I have not been so engaged, and therefore my knowledge must come from reading.

20688. And, as we are all aware, many of the points are controversial, and therefore when you formed your opinion as the result of your reading it must have been because you weighed the evidence; is that not so?—You know how one forms opinions. One considers the credibility of the evidence.

20689. That is weighing the evidence?—I suppose that is weighing it, after a fashion.

20690. Now I want to put this to you. We have had here the President of the Royal College of Surgeons and the President of the Royal College of Physicians, and we have had representatives of most of the Universities, and a great many other men who are medical men, and I think I may say that they have been almost unanimous in their opinion that great advances have been made in medicine within the last 55 years?—In medicine, did you say?

20691. In medicine and surgery; and most of them hold that a great deal of that advance is due to experimentation on animals. Now the Commission has to weigh against that your evidence and the evidence of a smaller number of persons, most of them non-medical, and do you think that we would be violating the principles of common sense if we accepted the opinion of the majority in this case?—I think I may divide it into two things. First of all, I think that one distinguished surgeon who was examined before you said that he had rejected remedy after remedy until his remedies had been reduced to a number that he could count on the fingers of his two hands.

20692. Who was that?—Dr. Bantock.

20693. He is one of the minority?—I have no doubt he is one of the minority. And another answer that I might make to you is that the fact that the majority is in favour of a thing is absolutely no proof that their opinion is right. That has been shown in medicine more than in almost any other line—more even than in politics.

20694. (*Chairman.*) I do not think you need argue that to us; if I thought that that was the meaning of Sir John McFadyean's question I think we should all of us say we are not to be bound by the mere counting of heads of witnesses?—But Sir John spoke of the majority—would I put my opinion and that minority against the opinion of the majority.

20695. (*Sir John McFadyean.*) No, I beg your pardon, the important part of my statement was that the majority was made up of men of great eminence in medicine, and presumably men qualified to form an opinion, as well qualified as the minority; and I therefore asked you whether you thought we should be violating the principles of common sense if we accept the opinion of the majority, not because they were the majority?—It is entirely for you to consider whether you will accept the opinion of the majority or of the minority, it seems to me; but what I was going to point out was that there was just as much common consensus of opinion in the medical profession years ago in favour of Lord Lister's antiseptic treatment of wounds; it was thousands to one in favour of it, I suppose; and as I understand at the present day Lord Lister himself has given up his antiseptic treatment.

20696. Have you taken the trouble to read the evidence which has been given to the Commission on that point?—Yes, I have. And I understand that he has given up his antiseptic treatment, and its place has been taken by the aseptic treatment.

20697. I might put the same question to you with regard to that. We have had evidence from distinguished surgeons here, who have scouted the view that you have just put before us, and we have really had no respectable evidence to show that what you have said is true.

20698. (*Chairman.*) If you only read the evidence on one side, that would be the conclusion that you would come to; but if you read the evidence on both sides, I cannot help thinking that you would come to the conclusion that it was not so?—But the evidence that I read was that of Lord Lister himself, I believe, in some speech that he made at Edinburgh.

(*Sir John McFadyean.*) Is that something that appears in the proceedings of this Commission?

(*Chairman.*) Yes, it has been referred to, and used on one side and explained on the other. There has been a good deal of evidence about it.

(*Witness.*) I think I could give some other instances if I may be permitted.

20699. (*Sir John McFadyean.*) Surely?—In former days the medical profession was uniformly in favour of blood-letting and strong purgative treatment by calomel. All that is gone. Inoculation for smallpox is another instance, and all that is gone, and yet it was supported by the great majority of the medical profession. My only point is that the majority are not always right, neither are they often right.

20700. I never suggested that they were, but may I ask whether that is the strongest reason that you can urge why in this case the Commission should accept the opinion of the minority rather than that of the majority?—As I said before, I have nothing to say as to the Commission accepting one view or the other. All my business is to place before the Commission our views, and the Commission will adopt which view they think the most consistent with common sense.

(*At this point Sir William Church took the Chair.*)

20701. (*Sir John McFadyean.*) On page 1 of your *précis*, in the second paragraph, you say that "The Association desires the total abolition of experiments on animals, believing the existing law which permits them, to be inhumane, immoral, and degrading, and a disgrace to civilisation. Further, the Association considers that the experiments which have been made under the present Act and which have been carried out at the expense of so much torture and so much animal life, have been practically unproductive of any beneficial results to the human race." When you spoke about experimentation upon animals as being degrading, I understood that you had in view principally the effect that it had on students. Is that so?—Yes, partly.

20702. Are you aware that under the Act, it is illegal to experiment on animals for the instruction of students, unless anæsthetics are used efficiently?—I think that anæsthetics must be used.

20703. And that the animal must not be allowed to recover; that in fact the experiment must be painless under the Act. (*The witness referred to the Act.*) I should have thought with regard to such an important point as that that you would not have had to refer to the Act?—There are six clauses and there are five provisos, I think. What the Act says is: "An experiment shall not be performed as an illustration of lectures in medical schools, hospitals, colleges, or elsewhere"; but then it goes on to modify that by saying: "Experiments may be performed under the foregoing provisions as to the use of anæsthetics by a person giving illustrations of lectures in medical schools, hospitals, or colleges, or elsewhere, on such certificate being given as in this Act mentioned," and so forth. There is nothing laid down as to the animal being killed.

20704. Do you suggest that it is possible under the existing law and under the Act to experiment on animals for the purpose of instructing students without the use of anæsthetics, throughout the whole course of the experiment and without killing the animal at the end of the experiment?—Without killing the animal at the end of the experiment. I do not see in this Act any provision of that kind.

20705. Will you take it from me that that is the state of the law; that this Commission is, I think, satisfied that that is the state of the law?—I am afraid I am not satisfied myself.

20706. Assuming that to be the state of the law, would you explain to the Commission wherein the experiments are calculated to demoralise and degrade the students?—I think that any body of young men witnessing a series of experiments involving horrible cuttings of animals, and practices of that kind would become degraded. I cannot conceive it to be otherwise. I should not like to put one of my own boys through it; that is all I can tell you.

20707. And you would say that, even if you were satisfied that the experiments involved absolutely no pain?—But as regards the killing of the animals afterwards——

20708. Would you please answer the question. I ask would you say the same if you were satisfied that the experiments involved absolutely no pain?—You mean if the animal continued under the anæsthetic the whole time and was killed at the end of it?

20709. I mean if the Act is complied with?—I am not prepared to say that the Act lays down that the animals under those circumstances should be killed.

20710. I did not ask you to give evidence on that; I think I may say that the Commission is entirely satisfied on that point. I ask you to assume that it is possible to experiment on animals for instruction without causing them any pain, and I ask you then whether you would give the same answer, namely, that such experimentation is calculated to degrade and demoralise the students?—Yes, I should.

20711. Would you explain why?—I have already explained why. I cannot imagine these horrible cruelties——

20712. Did you say cruelties?—You may say it is not cruelty because it does not give pain, but I do not at all agree. I think that if a number of surgeons were to tie me on a table, a perfectly healthy body, and to proceed to cut me up under the influence of anæsthetics, I should consider that was cruelty. And I consider that it is equally cruelty in the case of animals.

20713. Equally?—Yes.

20714. Observe that your argument seems to imply that man has no right to kill one of the lower animals?—I have very great doubts whether man has the right to kill one of the lower animals.

20715. I am quite content with that. I understand that another point about which you are in doubt is whether it is really possible to efficiently anæsthetise an animal under an experiment?—No, I do not say that it is not possible to anæsthetise an animal. I know perfectly well that it is possible to anæsthetise an animal under experiment. What I am in doubt about is whether as a uniform practice complete anæsthesia is obtained. I think there is a great deal of evidence that one has read in different places to the effect that complete anæsthesia is not obtained in these cases; that the animal wakes up, and so forth. I remember seeing a series of lectures by a man called Professor Crile, I think, in the course of which it was shown by his own admissions that the animal did wake up and move its legs and its body, and so forth.

20716. It has been given in evidence before this Commission that in most cases the experimenter has absolutely no motive for not anæsthetising the animal efficiently; that, on the contrary, it is greatly to his advantage as a demonstrator that the animal should be efficiently anæsthetised. What do you say to that

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argument?—I have no doubt that in a good many cases the animal is completely anæsthetised. But all I say is that there is evidence in a very considerable number of cases that the animals are not fully anæsthetised, and, more than that, that the anæsthetising is left to very incompetent hands.

20717. Does that mean that there is some difficulty in causing continuous efficient anæsthesia to animals like dogs, or is it that there is a deliberate evasion of the Act?—I take it that there are cases of both kinds. I take it that there are cases where there is a deliberate evasion of the Act. I am not prepared to say that 43,000 experiments take place without some cases of deliberate evasion of the Act. Surgeons are only human after all.

20718. Do you want to give any evidence on that, other than hearsay or supposition? You said that there were many cases you believed in which the animal was not sufficiently anæsthetised. Will you cite a case in point or half a dozen, let us say. You said many?—I cannot carry these cases in my mind, but I certainly have read of a good many cases in which the anæsthesia was not complete. The sort of evidence that I would put before the Commission is what I have put in my *précis*.

20719. Which *précis* is singularly lacking in detail. I was trying to help you to supplement it. Perhaps it is unfair to ask you for these cases?—I did not care to inflict all the cases upon the Commission.

20720. Will you kindly add six cases to your evidence when you revise the proof?—I know that this lady is not a lady who carries much weight with vivisectors, but Miss Lind-af-Hageby affirms that "the most unsatisfactory and obviously unreliable methods of anæsthetising are in vogue in the vivisectional laboratories." That is the kind of evidence.

The following cases were subsequently forwarded :—

1. *In the "British Medical Journal" for November 10, 1906, Dr. W. G. McCullum, recalling previous experiments, writes :—"It will suffice to recall to your memory the fact that in dogs, cats, rabbits, monkeys, and many other animals, complete parathyroidectomy is followed within a few days by the condition commonly described as tetany, in which convulsions, spasms, and rigidity of the muscles of all parts of the body render the animal almost helpless. Respiration becomes exceedingly rapid and laboured, profuse salivation occurs, and death supervenes in the attack, although occasionally the violent symptoms gradually give place to a stuporous condition which may last several days, terminating in death.*

2. *In Kiske's (Halliburton's) Physiology, 15th edition, p. 287, it is stated :—"Another experiment, originally performed by Salathe, can be demonstrated on a 'hutch rabbit.' If the animal is held by its ears with its legs hanging down it soon becomes unconscious; and, if left in that position for about half an hour, it will die."*

3. *In Dr. Crile's experiments upon "shock" the foot of a fox-terrier was crushed under "incomplete anæsthesia." Dr. Crile states also in his book that when he applied flame to a dog's paw "the animal struggled on the application of the flame."*

4. *Professor Schäfer experimented in drowning and resuscitating 36 dogs (2 without anæsthetics at all).*

5. *In a paper read at a meeting of the British Medical Association, at Toronto, Professor Cushney described a number of severe abdominal sections which he had performed upon dogs, cats, and rabbits, to which no anæsthetic, but merely morphia, urethane and paraldehyd, had been given.*

6. *In "The Journal of Physiology" for August, 1906, in a paper on "The Functions of the Thyroid and Parathyroid Glands," Professor Swale Vincent and Mr. W. A. Jolly, Assistant to the Professor of Physiology in the University of Edinburgh, record a number of experiments on monkeys, cats, dogs, prairie-wolves, badgers and rats from which the thyroid and parathyroid glands had been completely removed. They state: "Dogs, cats, foxes, and prairie-wolves frequently suffer severely and die."*

20721. That is the kind of evidence that weighs with you?—But I am not for a moment saying that the absence of anæsthetics is against the law.

20722. I have not asked any questions about that. I understood you to contend that the Act is being violated in many cases, inasmuch as animals are being experimented upon for the instruction of students without being under the influence of an anæsthetic all the time?—I did not speak of experiments for the instruction of students. I spoke of the general experiments.

20723. You will forgive me for saying that my question was limited to that?—I have not the smallest doubt that the same thing applies to the case of animals which are experimented upon for the instruction of students.

20724. Then it need not be qualified?—I have not the smallest doubt that there are many cases in which the anæsthesia is not complete.

20725. With regard to the ease with which animals may be anæsthetised with certainty, might I ask whether you have read the evidence given before this Commission by Mr. Hobday?—No, I just looked at it.

20726. You quoted his opinion, and with approval, I noticed, with regard to morphia?—I just looked at it.

20727. But you do think that he is a man whose opinion is entitled to a good deal of weight?—I do.

20728. Perhaps when you have time you would refer to his evidence given before this Commission, in which he told us that he had himself anæsthetised 1,200 consecutive cases, and that when he was asked whether he thought there was any difficulty in keeping an animal under complete anæsthesia for a long period he said no. He was also asked whether he thought there was any difficulty in deciding whether the anæsthesia was sufficient or not, and he also said no. And finally he was asked whether he administers the anæsthetic himself, and he said no, that his dog nurse does it—the attendant at the laboratory. I would like also to call your attention to the evidence which he gave with regard to the use of morphia. What was the source of your information with regard to his opinion concerning morphia?—He said that morphia was not an anæsthetic, I think.

20729. Where did he say that?—I understand that he said so.

20730. Have you read his evidence here?—I have looked at it. I have not read it. I cannot say that I have read it carefully.

20731. Then you must have seen the part that I have just been giving you the substance of with regard to anæsthetics. Do you attach much importance to that?—With regard to morphia not being an anæsthetic do you mean?

20732. No, to the evidence which I have just summarised with regard to the ease with which animals may be anæsthetised and kept under anæsthetics for a long period?—Yes, I am bound to believe what he says, that they can be.

20733. At Question 16462 he was asked : "Would you consider that morphia is a narcotic to a dog? (A.) Yes, I do consider it a narcotic. (Q.) Do you use it in your practice in the same way as medical men do in their practice with human beings for allaying pain? (A.) I use it largely for stopping pain. (Q.) And you find no difficulty? (A.) I find no difficulty in the dosage." Does that modify the opinion that you had when you came here with regard to the value of morphia for stopping pain?—I believe that morphia deadens pain. I am not sufficiently an expert to be able to understand the exact effects of morphia and the exact effects of anæsthetics generally, but I have always understood that morphia is used to deaden pain, but that it does not act as an absolute anæsthetic or stop pain unless indeed it is given in an excessive dose so as to induce insensibility.

20734. I point out to you it was you yourself who introduced Mr. Hobday's name to convince us that morphia was not of any use?—I do not know whether there is anything more in his evidence.

20735. Oh, there is a great deal more; but I think that you will find it very difficult to discover anything that seriously qualifies that?—If you will allow me I will make a search and see what I can find.

20736. In one or two places in your *précis* you speak about "hideous cruelties" being caused under the Act and "the torture of animals." Were you referring to any particular experiments that you know to have been performed in this country under the Act?—Setting aside the cases where anæsthesia is obtained, there are large numbers of certificates issued for experiments without anæsthetics.

20737. That is not the question. My question was, Do you know of any instances in which hideous torture has been caused under the Act?—Surely where experiments are performed without any anæsthesia there must be hideous torture.

20738. You mean that if they are performed without anæsthesia they must cause hideous torture?—Yes.

20739. Does that sound logical? May they not be performed without anæsthetics because they are not calculated to cause torture? I should have thought that was logical?—The certificates, I believe, are not limited in that way.

20740. That is not my question. My question was do you know of any case in which hideous torture has been caused by any experimenter in this country under the Act?—I answer that by saying that it is impossible under the conditions specified, that hideous torture should not be caused.

20741. (*Mr. Ram.*) But I understand that you are not prepared to give any case. Is that the result?—It is very difficult to give any case of hideous torture, because, after all, the only fair witness as to hideous torture is the animal itself, and you cannot examine the animal.

20742. That is your answer?—You cannot possibly tell except from inference that the animal is suffering hideous torture.

20743. You assert that there is hideous torture, but you say that there is no means of proving it?—That would be so.

20744. (*Sir John McFadyean.*) Then what would be the use of inspection in such a case? I understand that you deprecate inspection; you do not think that inspection would be of any value. You would not rely upon inspection, at any rate?—For what purpose?

20745. For preventing cruelty?—No, because our view is that vivisection should be put an end to altogether, and therefore there would be no necessity for inspectors.

20746. On page 6 of your *précis* you select the ignorance of the medical profession with regard to influenza as evidence, I understand, of the fact that nothing is to be gained by experimentation on animals?—What I say is that in 150 years the medical profession has not discovered either the cause of influenza or how to cure it.

20747. That is a fact which I think is very much open to dispute; but assuming it to be true, I want to know why you set it forth, what influence did you expect it would have upon the minds of the Commissioners?—As an instance of the failure of the medical profession; and they have equally failed in the case of nearly all other diseases.

20748. Failure in what respect; failure to discover something by experimentation, was that it?—Well, they have not made their position any better.

20749. Did you want to prove to this Commission that it is impossible to learn anything by observation either?—No, I think you can learn more by observation; that, in fact, it is the only reliable source of information.

20750. Then would you explain how it is that this fact discredits experimentation but does not discredit observation? The disease has been open to investigation by observation and clinical research, as you say, since 1729. If you are correct in maintaining that nothing has been discovered in that interval, that seems to me to be seriously to the discredit of observation?—The medical profession, as I understand it, have discovered nothing about influenza, as to either its cause or its cure by observation; but since they adopted experimental methods they have not succeeded in discovering anything more.

20751. Might I ask was this put in your *précis* by your medical experts or is it something within your own knowledge?—Partly one and partly the other.

20752. Which is it, is it your part or the medical experts' part which assumes that you can experiment with human influenza on animals? Did you ever hear tell of human influenza, which is the disease you are referring to, affecting any animal other than man?—No, I never did, but I should have thought it could be communicated to animals.

20753. You "should have thought." That is not a thing about which you can ask us to accept your evidence.

20754. (*Sir Mackenzie Chalmers.*) You have no intuitive knowledge with regard to it. Have you inquired into it?—I have no knowledge that influenza has been communicated to animals.

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20755. (*Sir John McFadyean.*) I suggest to you that you have been singularly unfortunate in your selection of an illustration, because you have taken the one disease regarding which it is admitted that there is much ignorance, largely because we cannot experiment upon animals with it. But, if the facts were as you state them, they seem to me to throw no discredit whatever upon experimentation, but to throw great discredit upon what you call clinical research and observation?—These paragraphs are to show how little medical knowledge has advanced. So far as I personally am concerned, I give other instances. The only zymotic disease which I know that is materially less than it was in the days of my childhood, so far as I can see round me, is typhus fever, and typhus fever has been entirely got rid of apparently by sanitation.

20756. (*Mr. Ram.*) What about small-pox? Is that less or more?—If you are asking my personal view, in my opinion small-pox has been stamped out by sanitation also.

20757. Is there less or more than when you were a boy?—A great deal less; it has been stamped out by sanitation.

20758. It is not only typhus, then, that is less in degree since you were a boy?—I say it has almost disappeared.

20759. But not only typhus fever, but small-pox has also disappeared?—That is quite true; I think they have been both stamped out by sanitation.

20760. (*Sir William Collins.*) Was there a greater epidemic of small-pox in living memory than there was in 1871 and 1872?—I am afraid that my memory does not carry me back to that.

20761. (*Sir John McFadyean.*) At any rate, will you admit that, if it is a fact that human influenza is not transmissible to the lower animals, there is no force in your illustration as showing the futility of experimentation with a view to advancing knowledge with regard to human diseases?—I think that, so far as my illustration concerns the barrenness of our present medical knowledge, there is force in the illustration, but if influenza cannot be communicated to the lower animals, and therefore cannot be made the subject of experiments, I admit that under those circumstances one may be wrong in saying or implying in any way that the failure to discover the cause or the cure of influenza is in any way due to the failure of experiments on animals.

20762. What line of research would you suggest as probably being more fruitful?—That is a question that I am not competent to answer.

20763. I can understand that in that answer you hesitate to say observation. You have recommended it before, and clinical research. Would you say that here?—Generally so, but as to the exact methods I am not competent to answer.

20764. I do not mean the exact methods; but does not your argument seem seriously to discredit research observation?—It has shown that research observation, at all events, has never produced any effect as regards a knowledge of the cause and cure of influenza.

20765. Would you turn, please, to page 8 of your *précis*. In the fourth paragraph you say: "Nobody, of course, would suppose that, after the healing of the wound, it necessarily follows that the animal remains in a state of pain or suffering. But he" (that is Dr. Thane) "carefully ignores the truth that in extensive cutting operations, with excision of organs, there must necessarily, upon waking from anæsthesia, be very severe and prolonged pain until, that is, the severed tissues heal." That appears to me to almost amount to a charge of untruthfulness against the witness whose evidence you are quoting, and I want to put it to you that the subject is controversial. There is evidence on both sides, is there not? Do you think that there is evidence on both sides?—I have no doubt there is evidence on both sides.

20766. Why did you accuse the witness who espoused one side of carefully ignoring the truth?—Because he said, as I understand——

20767. He took the opposite view from the one you

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approve of?—Not at all. He said that "when the wound has healed it does not follow that the animal remains in a state of pain or suffering." Everybody knows that when a wound is healed the animal does not remain in a state of pain or suffering; but what he ignores is the condition of the animal between the operation and the period of the healing of the wound.

20768. Is this expert evidence that you are giving?—I am simply reading.

20769. But you are criticising the evidence of this expert?—I am simply reading what he has said. He said: "When, in operations under these certificates, the operation has been performed and the wound has healed, it does not follow that the animal remains in a state of pain or suffering." There is no doubt that it does not when the wound is healed.

20770. You are assuming that it is an obvious falsehood to say that a considerable operation may be performed on an animal under anæsthetics without that animal suffering any pain when it comes out of the anæsthesia?—No. I make the charge that he carefully ignores the truth that in extensive cutting operations, with excision of organs, there must necessarily, upon waking from anæsthesia, be very severe and prolonged pain until the severed tissues heal. What he has ignored is the period, I understand, between the cutting and the healing.

20771. That is the period that I refer to. You want us to believe that a person who contends that in that period the animal may be free from pain is telling an obvious untruth. Will you allow me to read the evidence of Mr. Hobday, of whose opinion you express approval. Just please listen to this question and answer. It is Question 16352. "It has also been represented to us that extensive surgical wounds inflicted in physiological experiments on the lower animals must necessarily be painful when the animal comes out of the anæsthesia. Have you any evidence bearing on that question?—(A.) I have no experience of a physiological laboratory; I never worked in one; but my own experience is that the statement is absolutely untrue. I argue in this way: The wounds are the same, whether the human surgeon makes them with the scalpel or I make them. If they are done antiseptically they are the same in principle. . . . By far the greater majority of my wounds heal by primary union." You would perhaps think that that refers to operations which did not involve the cutting out of organs, but elsewhere in his evidence he told us that it applied to extensive operations in which he opened the abdomen and cut out organs. Does that not lead you to suppose that you have rather severely criticised this witness when you charge him with carefully ignoring the truth?—No, I do not think so, I am sorry to say. You asked me just now whether I believed that that was the case. I simply say that I do not believe that the animal does not feel pain between the time when it comes out of anæsthesia and the healing of the wound.

20772. That is not the question. It is a question of whether, when the opinion is not unanimous on a point like this, you are entitled to charge a person who adopts an opinion opposite to yours of carefully ignoring the truth?—I am absolutely justified in saying that the answer I have quoted above carefully ignores the period between the waking from anæsthesia and the healing of the tissues. As I understand in the evidence that you read to me, Mr. Hobday maintains that under certain circumstances, in certain operations and under certain conditions, there is no pain during that period. So far as that is concerned, I simply say that I could not bring myself to believe it.

20773. It is not a case of asking you to believe it. Can you contest the truth of it? You do not pretend that you have any first hand experience as to the result of healing of large wounds after an operation?—I do not understand what you mean by contesting the truth of it. I do not contest that that is his opinion.

20744. But I ask you, have you any experience of your own to show whether things are as you hold they are?—But there are a certain number of things that one regards as practically self-evident to oneself which one has no experience of.

20775. Would you mind answering the question—have you such experience?—You asked me whether I have operated——

20776. No, I did not?—That is experience.

20777. I asked you whether you had had any opportunity of forming an opinion from observation as to whether this is true or not true?—I really do not understand the purport of the question, but what I have felt always when I have cut myself severely is that I have always had pain between the time of the cut and the healing of the wound. Neither can I conceive that it is possible that it should not be so.

20778. Were these wounds of yours treated aseptically and sutured?—Aseptically, certainly, yes, but not antiseptically.

20779. But the point is that you hold one opinion and the witness whom you criticise holds another opinion, and his opinion is supported by that of a man of great experience, Mr. Hobday. I suggest that when that is so you should not accuse the witness of having carefully ignored the truth?—I really do not know that I am quite responsible for the verbiage.

20780. (*Sir Mackenzie Chalmers.*) I am afraid that I have a few questions that I must ask you, because we naturally attach considerable importance to your evidence, and there are some points that I should like to clear up that I do not quite understand. Your society wishes totally to abolish vivisection?—Yes.

20781. You have given us two definitions of vivisection. When your society is anxious to abolish vivisection, do you mean cutting operations, or do you mean any animal experiments?—When you say animal experiments, that was one of the grave difficulties we had in mind when we tried to draft a Bill. The difficulty as to experiments is that there may be experiments in feeding or other innocuous experiments.

20782. Taking the temperature is an experiment?—Yes, an innocuous experiment. But we wish to abolish all experiments, cutting or inoculation.

20783. Inoculation with any disease?—Yes.

20784. Would that include, for instance, the use of animals which are used for the preparation of vaccine, and the use of animals which are used for preparing antitoxin?—Yes.

20785. You would prohibit those by law?—Yes.

20786. For instance, calf vaccine?—With regard to calf vaccine, I wish to say, as I said before, that I do not consider that vaccination is a subject on which I am prepared to give any evidence, unless the Commission wish to hear my own personal opinion. For my Association I give no evidence.

20787. Your personal opinion would, I think, be of interest and importance. Do you object personally to using calves for the preparation of vaccine?—I object to vaccination altogether.

20788. And also to antitoxin for diphtheria?—Yes, I do so far as my knowledge goes, and so far as my reading goes.

20789. You have seen diphtheria cases; I suppose we all have?—I have seen one or two in my own family, and the patients recovered.

20790. Have you seen any where antitoxin has been used?—No.

20791. I may take it, then, that the main object of your Association, apart from drafting considerations, is to abolish every experiment with an animal, whether by vivisection, inoculation, or otherwise, which causes pain?—Yes.

20792. Would you carry that principle so far as to prohibit mutilations of animals for commercial purposes?—I do not know what mutilations you refer to.

20793. For instance, every horse in England which is not used for stud purposes is gelded, usually without anæsthetics. Would you draw any distinction between an operation for the purpose of increasing knowledge and an operation for commercial purposes?—Yes, I should. Besides, the one is not an experiment, the others are experiments. I should draw a distinction between them.

20794. You would draw a distinction between an experiment for the purpose of increasing knowledge or alleviating suffering and an operation for commercial purposes; would you interfere with the commercial purposes or not—for instance, gelding a horse?—I should not interfere with that. That is not an experiment.

20795. Therefore, it being for a commercial purpose and not for a humanitarian purpose, you would allow it?—I am hardly prepared to admit the word "humanitarian."

20796. An experiment is for a humanitarian object, is it not—for the purpose of alleviating suffering in the future?—It is supposed to be.

20797. And gelding a horse is for purely commercial purposes—to make it a more saleable animal. You would admit that, but not the other?—I should admit that. It is not an experiment. It is a matter of long custom. I should not, for instance, put down circumcision among the Jews.

20798. Or require it to be done under an anæsthetic?—I really do not know that it is a sufficiently serious operation. I am afraid Moses did not use anæsthetics.

20799. I may have misunderstood you, but I thought that you rather complained of Dr. Pembrey doing circumcision without an anæsthetic?—I did not say anything about that; it was merely mentioned casually. I do not complain of circumcision being done without anæsthetics; it is not a sufficiently serious operation, and it is not an experiment.

20800. Take the case of spaying sows. A great many animals are spayed; you would leave that; that is outside?—That would be on the same footing as gelding horses.

20801. Outside altogether?—Besides, it is not an experiment, neither is circumcision, and it is not an experiment on animals either.

20802. Still, I take it that you would not allow purposeless mutilation of an animal, even if it was not experimental?—That, of course, comes under different legislation, and it comes under a different category; it comes under the Prevention of Cruelty to Animals Act; it is quite a different thing, and it is amenable to the ordinary law of the land.

20803. As regards an increase in the number of experiments, I have no doubt that you have looked at the last Return?—I have not seen anything after that 43,000.

20804. The Return ordered by the House of Commons to be printed on the 13th of May, 1907; you have seen it, no doubt?—I thought that was the last; it is for 1906. 43,000 is the last, is it not?

20805. Yes. You are aware that what is called vivisection proper only accounts for about 2,700 of the experiments, and the rest are under Certificate A, which is for inoculation experiments?—I thought there were 21,000 inoculations only, and 6,000 for Government Departments.

20806. No; under Certificate A 43,287 inoculations is the exact number. What I was going to ask you is this. Do you accept Dr. Thane's statement in the report: "In no case has a cutting operation more severe than a superficial venesection been allowed to be performed without anæsthetics"?—I am bound to accept it if Dr. Thane says so.

20807. Does that relieve your mind in any way? Is that any relief to you?—If that is the case, how did it come that Colonel Lawrie saw a dissection of the neck without any anæsthetic?

20808. I am afraid I cannot give evidence; I can only suggest it to you in a question?—Whatever Dr. Thane says, there is a very serious and very painful operation which a witness says is done without any anæsthetic.

20809. I can suggest the answer to you, but only that; we called a witness before us, Dr. Shore, who performed the operation, and if you read his evidence I suggest that you will see the explanation which (whether you accept it or not) was laid before us. You have not seen it, perhaps. He performed the actual experiment that Colonel Lawrie saw. It would take you some time to read it. I can only suggest to you that the Commission at once investigated that statement made by Colonel Lawrie, and we called a witness who gave the explanation. You will judge of that explanation when you read it?—Yes.

20810. As it is late, I will leave out some things that I should have liked to have asked. In your *précis* you speak about certificates given by the Home Secretary. Are you aware that the Home Secretary does not give certificates?—Yes, I know he does not; that is a mistake.

20811. May I put it to you in this way: Those certificates are given by certain eminent authorities?—Yes.

20812. They go through a considerable routine before they are allowed to come into operation, and the Home Secretary has the power of disallowance?—The section says: "The Secretary of State may at any time disallow or suspend any certificate given under this section."

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20813. You agree that a power of disallowance is not the same as the power of making?—A power of disallowing would only be exercised in the event of serious complaint being made against the experimenter.

20814. I must ask you on that, have you made any inquiries as to the process which is gone through with regard to these certificates?—As to whether they are ever disallowed, do you mean?

20815. No, I mean as to what happens before they are allowed to come into operation. Have you made any inquiry as to that?—I thought that had been placed before the Commission in sufficient detail by Mr. Coleridge, so that I did not think it necessary.

20816. Do you know, at any rate, that five pairs of eyes have to examine a certificate before anything is allowed to be done under it?—Yes, I know that a considerable number have, but as I say I did not take up the question of certificates in this evidence because I agreed with the evidence given by Mr. Coleridge on this part of the question, and so I thought that would be sufficient testimony.

20817. Do you know what inquiry he made upon the subject?—I believe he is very familiar with it.

20818. With certain aspects of it perhaps. You referred to the Public Authorities Protection Act, 1893. Why do you say that that affects the case of experiments under the 1876 Act; what is your ground for saying that?—I believe that the persons holding these certificates are for the purposes of the Act decided to be public authorities.

20819. I never heard of it. I should like some authority for it. What is the authority for it?—I do not know; but you will observe that in answer to my question in Parliament the Home Secretary did not canvass that at all.

20820. Did he not?—I referred in my question to the Public Authorities Act.

20821. What was his answer; have you got his answer?—I have got it all down here. I have read it to you.

20822. It is news to me, certainly, that that Act applies?—It does, though.

20823. May I suggest to you that a publican has to have a licence, but he does not come under the Public Authorities Protection Act, although he is called a publican?—It does, though. The Home Secretary added that contraventions of the Act were reported to the Home Secretary as soon as they were discovered, so that the institution of proceedings for performance of illegal experiments was not dependent on the date of the Report. He did not for a moment say that the Public Authorities Act did not apply. I asked him the question, and my question distinctly referred to the Public Authorities Act.

20824. I should like to have the words of that answer because I rather think I am responsible for it?—I can give you the whole question and answer out of Hansard, and then you will see.

20825. Of course, he replied to it at the time, but I do not think he referred to that Act. A street hawker you see has to take out a licence, and a gamekeeper has to take out a licence, but you would not say that they come under the Public Authorities Protection Act, would you?—No, but I understand that the definition of public authorities in the Act covers the holders of licences.

20826. Not so far as I read it, but we need not discuss that?—Is it not so?

20827. No, I think not?—The Home Secretary had his attention drawn in the question to the Public Authorities Act, and he did not in his answer say anything about it, or whoever drafted the answer for him.

20828. What I was going to suggest to you is this: You are a magistrate, are you not?—Yes.

20829. Do you know Section 11 of the Summary Jurisdiction Act?—No, I do not know it.

20830. That is the section which provides that all summary proceedings, in the absence of special statutory direction, must be begun in six months before the

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magistrates. Is not that the enactment which protects the public generally?—I understood that it was the other. I referred to that in the question to Mr. Herbert Gladstone, and he did not seem to question it. It does not much matter which it is if it has the same effect.

20831. Assuming for the moment that the Public Authorities Act does apply, the repeal of it would not help you, would it, having regard to Section 11 of the Summary Jurisdiction Act?—Not if Section 11 of the Summary Jurisdiction Act has the same effect. You do not want to repeal the section in either case; all you want to do is to take the particular offence out of the section.

20832. That is to say, to put licensees under the Act in a worse position than the rest of the population as regards summary offences?—How is that?

20833. You have to prosecute an ordinary person who commits an offence within six months; but in this case you would take away that protection?—You might put it in that way; but you have also to put it in this way, that the method in which the Act is administered makes it impossible for prosecutions to be instituted in consequence of the block put upon them by either the Public Authorities Act or the Summary Jurisdiction Act, whichever it is, as the case may be, and therefore practically when you say in the Act of Parliament, the Cruelty to Animals Act, 1876, that no prosecution can be instituted except with the consent of the Secretary of State, as a matter of fact no prosecution can be instituted at all, whether the Secretary of State consents or not, under the existing law. That is what we argue.

20834. You argue that until the returns are published the Secretary of State knows nothing about what has happened?—Quite so. Our only source of information is in those reports.

20835. Do not you think that the Secretary of State occasionally has information before it is able to be collected and published?—Yes, I know it is a very heavy business.

20836. I am rather puzzled by your evidence generally. I am not quite sure how far your evidence represents what I may call a brief given you by experts. Where does your own evidence end, and where are you speaking to your brief?—I should be puzzled to tell you myself.

20837. (*Mr. Ram.*) Is the *précis* your own creation?—Some of it is.

20838. (*Sir Mackenzie Chalmers.*) I will tell you why I ask the question. I attach great importance to anything that you tell us yourself, but I do not attach much importance to anything that your experts tell you, unless I know who they are and what their means of information is. You cannot, I am afraid, separate for me what is yours and what is, so to speak, your brief?—It is all my brief, but of course the part with which I am specially familiar is the question of plague. I know something about the whole question, but the part I am specially familiar with is the passage of the Act through the House of Lords and the House of Commons, and the strong point which I made as to the hurry with which it was passed through the House of Commons.

20839. As you appear on behalf of the Association, may I ask you this. I understand that there were experts who helped you to prepare this statement?—I believe several, but I do not know them all.

20840. But who were the two you had communication with?—I had communication with Miss Kenealy.

20841. Who has given evidence here herself?—Yes.

20842. And who was the other?—Dr. Bourchier, I think.

20843. Who is Dr. Bourchier?—Miss Bourchier.

20844. Is she a practising medical woman?—Yes; she has been.

20845. Miss Kenealy has retired from practice, I think; she told us she was now a journalist and not a practising doctor?—I believe she is not in active practice.

20846. Miss Hageby, I suppose, you have not consulted with?—I do not know her even.

20847. But it is Dr. Bourchier and Dr. Kenealy?—Yes, and the members of the executive of the Association.

20848. Do you know at all what scientific qualifications they have?—No, I do not know.

20849. I just want to ask you one or two questions about plague. You have taken great interest in plague?—Yes, I have taken some.

20850. Have you read the reports by the Scientific Commission appointed by the Government of India, which have just been published; Dr. Martin's reports?—I have not seen them yet. I do not know whether they are issued as Parliamentary Papers.

20851. They are not issued as Parliamentary Papers, but they have been out for some months, some of them?—You see, my information comes largely from Parliamentary Papers. I get those.

20852. I am afraid those are often rather out of date, are they not?—And anything I get furnished with by others who are interested in the question.

20853. Have you read Dr. Martin's evidence to us. He was Chairman of the Plague Commission?—No, I have not.

20854. It bears a great deal on the statements that you have made to us to-day, but if you have not read it it is hardly possible to discuss it with you, I am afraid?—Is it issued?

20855. It is in our Third Report?—Then I am sure to have it.

20856. Because these statements of yours—I do not know if I am putting it too strongly—are in every direct opposition to some of the statements contained in that report?—As I understand, they are in opposition.

20857. But you know that that Commission was a very important Commission, consisting of very eminent men?—Yes.

20858. Do you think that no weight is to be attached to their findings?—I do not say that no weight is to be attached to their findings, but I do say, as it appears to me, there is an exceptional case of an epidemic of plague, an epidemic which certainly has become endemic. And in what way does this epidemic differ from previous epidemics? It differs in the fact of the use of these sera, that is all; otherwise the conditions are better.

20859. Might you not apply the same line of reasoning to different earthquakes? You have the same conditions, but one earthquake is more severe than another?—I do not admit the analogy.

20860. Then may I put this as an analogy to you: A lifebelt will save you from drowning; but you read in a particular year, say this year, that more people have been drowned than last year. That would be no argument against lifebelts unless more lifebelts were used, would it?—No.

20861. Your argument would rather come to this, would it not: A greater number of deaths from drowning have occurred this year than last, therefore lifebelts are of no use. What you have got to take is the number of people inoculated by Haffkine and the number of deaths among the people so inoculated?—I know that, and I should have thought from what I could see that those statistics were quite valueless. I should have thought they were of very little value, because you see you take a certain number of people and you know the number of people who are inoculated, but you do not know the number of people who are not inoculated, not absolutely.

20862. In the jail case there was, of course, a comparison?—Yes, we know.

20863. How do you account for the very small propagation of plague among Europeans in India who are freely inoculated?—Has that anything to do with it? A good many Europeans are said to be inoculated. Have you considered why it spreads so much more among the native population?—I should be inclined to attribute it to cleanliness, as a non-expert.

20864. Are you aware that the Plague Commission who were mingling with plague night and day, were all inoculated themselves and all escaped?—I do not know that. They escaped a serious attack of plague, but I presume that they had plague in some shape.

20865. But still, if a very mild attack of plague will save you from a fatal disease, is it not worth while?—Yes, I do not deny for a moment that inoculation may give a mild attack of plague and give immunity for a certain time. All that I desire to try to enforce is that these people who are inoculated

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are just as infectious as other people, and may give the plague to others.

20866. But do you say that plague is infectious. There is no danger in going through a plague hospital?—That is one of those points, I believe, which is not yet cleared up.

20867. There is one form of it, the pneumonic form, which is supposed to be infectious; but have you considered how far plague is infectious or contagious, or not?—Of course I have generally never observed cases of plague, but so far as one's reading goes, one is in very great doubt as to what the means of communication of plague are. What between rats and fleas, and all the rest of it, one does not know any more than one does with many other diseases.

20868. There is only one question more that I wish to ask you. How do you account for it (it is a point that presses upon us), that all the heads of the medical profession are unanimous in favour of the necessity of animal experimentation. We have had the President of the Royal College of Physicians, the President of the Royal College of Surgeons, representatives of the Royal Society, and representatives of the great universities, and the whole, what I may call heads of the profession, unanimous. How do you account for that?—The head of the Royal College of Surgeons himself was not, was he?

20869. If you read his evidence you will see that he was?—I was under the impression that he said that he did not consider it necessary.

20870. He only said that it was not necessary for students to perform experiments, but if you look at his evidence you will see that I am right in saying that the Royal College of Physicians, the Royal College of Surgeons, the Royal Society and the great universities are unanimous in favour of the necessity of experimentation. How do you account for that?—Because I think they look at it through coloured spectacles. They look at it through medically coloured spectacles. I do not mean to say that they are not honestly biased by considerations of so-called humanity.

20871. But are they not the people who are in daily personal contact with disease, and therefore have more means of judging than the rest of us?—Decidedly.

20872. (*Mr. Ram.*) Following up what Sir Mackenzie Chalmers has just asked you, can you suggest any reason why the heads of the profession and those who represent the universities and Royal bodies, should wear, as you expressed it, glasses of one colour, and that those who wear glasses of another colour should be few and not distinguished?—I was not drawing a distinction as regards glasses.

20873. I want to draw it rather?—I was not drawing a distinction as regards glasses between those medical men who are in favour of experiments upon animals and those medical men who are not. I was drawing a distinction between the medical profession generally and the general public.

20874. But I am rather drawing a distinction, if I might for the moment, between these two classes of people who have come here and given evidence before us, either directly, as the heads of the profession have done, or indirectly, as some others have done, and are doing now through you. How do you account for the fact that all that are eminent are on one side and those that are not eminent and few in number are on the other side?—They are not undistinguished on the other side. Surely Dr. Bantock is a very distinguished and very successful surgeon.

20875. Dr. Bantock gave evidence in favour of the necessity of vivisection?—No, he gave evidence practically opposed to it.

20876. No. I will read you the passage?—I know that there is a passage or two at the end of his evidence in which he made certain concessions, but the general tenor of his evidence was against it.

20877. Have you read Dr. Bantock's evidence all through?—Yes.

20878. Then you know the passage that I am alluding to?—Yes, you are alluding to a passage towards the end of his evidence. I could not tell you *viva voce* without referring to it.

20879. It justifies me in citing him also as an eminent man in favour of the necessity of experiments on animals?—I should not read it so.

20880. I will refer you to it if you like, but can you give me any answer to the other point that I put to you: Why is it that all the most eminent men advocate the necessity of experiments, if it is a fact, as your Association thinks, that experiments are useless?—I really could not say why they all do. I imagine that they are perfectly honest in their view, but I imagine that the general public, which we represent, and the Parliamentary feeling, which we represent very largely, have a right to our opinion. We look round us and we do not see any great advantage having been gained by these experiments, and we do not see that disease is less or that the population is becoming less degenerate.

20881. But on what ground do you ask the Commission to set aside the evidence of the most eminent persons who have made this subject a study, in favour of the evidence of inexpert persons who have not made this subject a study?—I have not asked the Commission to set aside that evidence. I have asked the Commission to consider the evidence that we give.

20882. We certainly shall do that with the utmost care. You say in your *précis*, on page 6:—"Our medical scientists are by no means driven to use experiments as their only means of research. They have other (and confessedly superior) methods of investigation." What is a confessedly superior method?—The clinical method; that is our view.

20883. By whom is it regarded as confessedly superior?—I presume at all events that they are confessedly superior in the sense that those methods of investigation are open to every medical man (the others, I understand, are not), they are part of the daily practice of every medical man.

20884. Is that the meaning of the words "confessedly superior"?—I cannot tell how those words got there exactly.

20885. Is that the meaning that you put upon them?—At all events, you would imagine that they were equally good, since all such knowledge as we have seems to have come from clinical research, and not from experimental research.

20886. In the next sentence you say:—"The present barren state of medical knowledge compels us to conclude that long years of vivisection have added perhaps not one assured and certain weapon to their armament. To any man of common intelligence this barren state of medical knowledge is obvious." You think that that is a true description of the present state of medical knowledge?—I am inclined to think—if I may say so, with all respect—that it is a true description.

20887. May I point out that, except so far as in comparatively recent years, medical science has been helped by experiment, it was wholly dependent on clinical observation?—Yes.

20888. It does not look, then, as if it had benefited very much by clinical observation if that on which it relied for all those years has left us in the present barren state of medical knowledge that exists to-day?—For many years before vivisection had been in vogue, long before the Cruelty to Animals' Act, 1876, and we at all events (I am speaking for the Association) do not find that any advance has been made beyond the small amount that has been gained by clinical research in all these years during which vivisection has been practised.

20889. Do you think that medical science is no more skilled in combating disease now than it was, say, 50 years ago?—It depends how many years ago you refer to.

20890. I said 50 years ago?—I do not know whether that was in the days when doctors used to hang the rooms in which there were scarlet fever patients with scarlet curtains as a means of keeping away the disease.

20891. Seriously, I should like your opinion as an answer to my question. Do you mean that medical science in your opinion is really in no degree more advanced to-day in combating disease than it was 50 years ago?—I do not think that medical science, so far as my recollection goes, or my observation goes,

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has much advanced since the time when I was a boy. I think sanitary science has improved enormously.

20892. Do you think that Lord Lister has done nothing for us to ameliorate pain and to assist in overcoming disease?—Whatever advantageous changes may have resulted in that way, what I rather meant was that the causes of diseases have not been ascertained so as to get rid of the causes; nor have the cures of diseases been ascertained. If you could only find out what the cure of a disease was you would get rid of the disease.

20893. If the causes of disease are still not ascertained, does it not seem reasonable that experiments should be made to endeavour to find out what the causes are?—Experiments are made with the object of finding out what the causes are, but they never have discovered from the experiments what the causes are so as to be able to prevent them.

20894. That is your opinion, from reading the state of science up to to-day?—That is my view, because the prevalence of disease appears to be very much the same as it always has been.

20895. Do you suggest that operations which to-day are performed, I may say daily, with a very small percentage of deaths—take one that we hear so much about, for instance, the operation for appendicitis—could have been done 50 years ago with any hope of success of saving the life of the patient?—I think that great advances have been made in surgery, but I believe those advances to have been made by operating on the dead body and observation of the dead body, not on animals. One gets rather confused between the medical art and surgery.

20896. (*Sir William Church.*) Might I be allowed to interpose one question? What makes you think that that improvement is due to operations upon the dead body now? How do operations on the dead body differ now from what they were 50 years ago? Are they more frequent, do you mean?—No, I did not mean that. I meant that our knowledge had come from operations on the dead bodies of human creatures rather than upon the living bodies of animals.

20897. That would not tell us anything as to the treatment of the wounds afterwards?—No, I admit that a great advance has been made in the treatment of wounds afterwards.

20898. I did not understand what you meant by operations. I thought you meant operations in teaching students and surgeons how to operate. You did not mean that?—No, because any improvements in dealing with wounds are surely sanitary improvements rather than medical.

20899. May we take it from you that you disbelieve wholly and entirely in the germ theory of disease; that you say is a myth?—I do not say that. I say that it is not proveable.

20900. You would not go so far as to say that it is altogether a myth?—No, I would not.

20901. (*Mr. Ram.*) Then you said that you objected to any kind of experiment on a living animal because it is demoralising to the operator?—Yes.

20902. Do you think it is demoralising to a man to dissect a dead animal?—No.

20903. Take the case of an animal under an anæsthetic from which it can never recover, which is wholly unconscious under that anæsthetic. Why is it more demoralising to a man to examine that animal than to examine an animal which is dead, which you have just told me is not demoralising?—Because the whole process of examining living animals, to a layman, the cutting and wounding and mutilation of living animals is at all events the most sickening process one can possibly imagine. It would be most sickening to me. I feel it would be most demoralising to me. If I could get myself into a condition so as to view without any pang or any feeling of disgust a long series of experiments on a number of animals one after another, day by day, week by week, month by month, tied upon a table and cut to pieces, by the time that I was at the end of the series of experiments I should feel that I was one of the most callous, demoralised brutes in the whole scale of creation. I am putting the instance of what I should feel myself.

20904. That is what I may call an *à priori* idea of what you would feel yourself?—Certainly.

20905. Have you any instance that you can give us of any person who to your knowledge has been demoralised or adversely affected morally after having performed experiments on living animals under anæsthetics?—I do not know exactly what you mean by being adversely affected morally.

20906. I mean in your own word demoralised?—Their sense of humanity with regard to animals and so forth, I should think, would be very strongly affected.

20907. Can you give me a concrete instance of any man in whom you know that that has been the effect?—I do not think that I could give you any specific instance of any man who by indulging in experiments on animals has become a thief or a murderer.

20908. I am not suggesting that?—Or even in other senses immoral.

20908A. I suggest in your own words callous or inhumane?—I answer that, from the analogy of my own feelings. I cannot imagine that any man can go through all those experiments without becoming demoralised.

20909. The question was, can you tell me of any man who, to your knowledge, has been so adversely affected?—Of course I cannot. You know very well that I cannot pick out any man, and more than that, if I did know of a man I should not be able to tell you.

20910. I hope you would. I take it that the answer is that you do not know any concrete case of that kind?—Certainly.

20911. What would you say to this case: Supposing that the Commissioners, after hearing all the evidence—a great deal of it on both sides—and after weighing it all very carefully, came to the conclusion that experiments have been of very great use to humanity, should you still urge them to advise that there should be no experiments at all allowed merely on the ground of possible suffering in a certain number of small cases in those experiments to some animal?—Your premise is that the Commissioners have come to the conclusion that such and such is the case. I should not urge the Commissioners to put in their Report anything which, after due deliberation, they concluded to be wrong.

20912. I am not suggesting that. I say supposing that the Commissioners, after weighing the evidence on both sides find that experiments on animals have been and are productive of great good as a means of the amelioration of pain to human creatures, should you still say on the other grounds that you have urged—grounds of morality and humanity, and so forth—that all experiments ought to be made illegal?—If, as you say, the Commissioners have concluded that such is the case, it is no use my offering any evidence.

20913. You may take it from me that they have not concluded that yet. I want the assistance of your mind in the matter?—But my duty is to place before you this evidence, and for you to weigh and consider and see how far it leads you to modify any views that you hold already.

20914. You said that you objected to all experiments on the ground that they were inhuman and useless—unproductive was your word. I am supposing that the Commission are bound to find as a matter of evidence that they are not unproductive, but are useful and have been of great good to mankind in alleviating suffering. Should you still say that although useful, they ought to be made illegal on the ground of inhumanity or immorality, of which you have been telling us?—I can only place my own view before the Commission. My view is that they are useless, unproductive, and inhuman. But you ask me, supposing that the Commissioners conclude one thing, would I advise them to modify their conclusion? That has nothing to do with me. All I have to do is to place my views before the Commission for them to consider.

20915. I want to go as far as I can with you to get the benefit of your assistance. You put it on the three grounds that it is immoral, inhuman, and useless. Supposing that the Commissioners come to the conclusion that it is not unproductive, but on the contrary is useful, ought they still, in your opinion, to endeavour to make it illegal on the ground that it is immoral or inhuman? It is a very plain question?—I could not say; that would be for the Commissioners to judge. I do not pretend to be the *arbiter pugnæ*.

20916. Reference was made just now by you to Mr. Morris; you quoted him as saying that in his opinion

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students ought not to be allowed to do experiments, and that you were also under the impression that he had said that experiments generally were unnecessary. I will refer you to his answers on page 41 of our Report, on the 18th day, beginning at Question 7658 :—"Experiments on animals in relation to aseptic surgery and the healing of wounds have been of material advantage, in your opinion? (A.) Most material. I quote Lord Lister. We could not have arrived at the knowledge we possess at the present day had it not been for experiments on animals. (Q.) You do not say that that knowledge has arisen from experiments on animals alone? (A.) No. (Q.) But there are other modes of investigation which have been going on in research? (A.) Certainly. (Q.) Chemical investigation, for instance? (A.) Yes, each of the forms of investigation has been essential, but experiments on animals have been equally, if not more, essential or important than the other forms of investigation. (Q.) And it is not easy, in your opinion, to separate the advantages which have been derived from each particular source? (A.) It is impossible. (Q.) It is your decided opinion, as an experienced surgeon, that vivisection has been essential? (A.) I have no hesitation in expressing that opinion." I wanted to bring that to your mind, because I think you are under the impression that Mr. Morris, whose opinion, of course, everybody greatly values, had said that experiments were unnecessary?—As a matter of fact, he himself has never performed an experiment.

20917. He never has. I think there is only one other matter that I need trouble you with, and it is this. You spoke about experiments which would come under Certificate C, that is, experiments for the instruction of students?—Yes.

20918. I want to draw your mind to this because it was not present to your mind, I think, when you were giving evidence before. Certificate C has to be signed by one or more eminent persons such as the Prsident of the Royal Society, the Presidents of the Royal Colleges of Physicians, and so forth, who state: —"We hereby certify that in our opinion the proposed experiments are absolutely necessary for the due instruction of the persons to whom such lectures are to be given with a view to their acquiring physiological knowledge, or knowledge which will be useful to them for saving or prolonging life or alleviating suffering." Then the note to that certificate is this: —"The animal must during the whole of the experiment be under the influence of some anæsthetic of sufficient power to prevent the animal feeling pain; and the animal must, if the pain is likely to continue after the effect of the anæsthetic has ceased, or if any serious injury has been inflicted on the animal, be killed before it recovers from the influence of the anæsthetic which has been administered"; and reference is made to Section 3 of the Act, sub-sections (3) and (4). I want to point out that that can only take place under what are termed the operative sections of the Act, and not under the exceptions. Those words that I have just read are quoted from the Act, and are not included in the savings which subsequently come in the Act, which have been termed relaxations of the Act?—You mean the provisos.

20919. You see this is the operative part of the Act?—Are you speaking of Section 3, sub-section (4)?

20920. I am speaking of Section 3, sub-sections (3) and (4)?—Sub-sections (3) and (4) can be nullified by proviso 3.

20921. But as a matter of fact, under the certificate of which I was speaking they are not only not nullified but the certificate is granted only under sub-sections (3) and (4), and admits of no nullification. Do you follow?—Yes, I see; but there is also sub-section (5).

20922. No, I am speaking of sub-sections (3) and (4), because that is the matter to which Certificate C relates.—Of course, and both those sub-sections (3) and (4) can be nullified by provisos 2 and 3.

20923. But the operation can only be done, if at all, under Certificate C, and Certificate C, as I have just pointed out, is limited to those two sub-sections which I have just quoted?—That certificate combines those two points.

20924. (*Dr. Gaskell.*) Do I rightly understand you that this *précis* of yours was partly drawn up by you and partly by others?—It is a united effort.

20925. You make a good deal of Dr. Pembrey's evidence, I see?—Yes.

20926. Is that your part?—Partly.

20927. But you put it in your *précis*. Are you responsible for it?—I should not like to disclaim the responsibility.

20928. Are you responsible for the correctness of it? —I do not wish to evade my responsibility for all of it. All I say is that I cannot be supposed as a non-expert to have put together the medical part.

20929. I am simply asking you, are you responsible for the accuracy of the statements that you have given us?—I do not wish not to be.

20930. Then you are responsible?—I take the responsibility.

20931. Then, in the first instance, you say, on page 9 of your *précis*, that Dr. Pembrey "expresses the opinion that the infliction of suffering is a meritorious act"?—Yes, he expresses the opinion that it is a meritorious act.

20932. I have read through Dr. Pembrey's evidence, and I have not been able to find that statement. I have not seen that he has expressed that opinion?—If I cannot find it, I imagine that "expresses the opinion" is a bit of loose writing; "he apparently believes," I should have thought would have been better.

20933. Quite so; it is a piece of loose writing in the part for which you have just accepted the responsibility?—Yes.

20934. But which does not necessarily give the actual opinion of Dr. Pembrey?—No.

20935. I am sorry that you accepted the responsibility?—I am quite ready to accept the responsibility.

20936. I will go a little further, please, to the next thing, which is at Question 14149. That is a statement of Dr. Pembrey's of which you have made a good deal. I will read it to you. Dr. Pembrey says: —"I will be perfectly straightforward. I say that you should not inflict pain if you can obtain the knowledge in any other way; but I say that even where there is an operation the pain there is of a protective nature; it may produce syncope, and therefore less sensation of pain. Further, I say that the introduction of an anæsthetic may produce complications, so that more experiments may be necessary than if the experiment were done on the animal in a condition of syncope. . . . . If you give them anæsthetics you are introducing a complication which you could remove, and therefore without anæsthetics you actually save life and actually diminish the infliction of pain." For the accuracy of that you are ready to vouch?—I head this as an except from Dr. Pembrey's evidence. I did not actually write it out myself.

20937. Your comment was that this was an extraordinary doctrine?—Yes.

20938. And one that filled you with sensations of horror, more or less?—It fills me with horror.

20939. Now, may I read what Dr. Pembrey did say? —Yes.

20940. "I will be perfectly straightforward. I say that you should not inflict pain if you can obtain the knowledge in any other way; but I say that even where there is an operation, the pain there is of a protective nature; it may produce syncope, and therefore less sensation of pain. Further, I say that the introduction of an anæsthetic may produce complications, so that more experiments may be necessary than if the experiment were done on the animal in a condition of hypnotism, which I offered to show to the Commissioners. That, I think, is one very important point. These animals pass into a condition, so far as one can see, comparable to hypnotism. If you give them anæsthetics you are introducing a complication which you could remove, and therefore without anæsthetics you actually save life and actually diminish the infliction of pain." Your *précis* has taken out the word "hypnotism" apparently, and put in the word "syncope"?—I thought that syncope was there as well as hypnotism.

20941. No, you say "than if the experiment were

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done on the animal in a condition of syncope"?—Yes. What does he say?

20942. He says:—"On the animal in a condition of hypnotism"; and then where you have put in those dots he explains the condition of hypnotism as I have read it to you?—Yes, I understand that, but that, of course, does not remove——

20943. It removes the whole of Dr. Pembrey's statement there?—I do not think it does.

20944. He is referring there simply to the condition of hypnotism. He is advocating hypnotism instead of an anæsthetic, and for that reason he says "therefore without anæsthetics," that is to say, if you give hypnotism without an anæsthetic "you actually save life, and actually diminish the infliction of pain"?—Surely you are incorrect in that. He says at the beginning:—"I say that even where there is an operation the pain there is of a protective nature; it may produce syncope." I presume that "syncope" is right there?

20945. Yes.—Yes, "and therefore less sensation of pain." The horror to me is in inflicting pain, without any anæsthetic, which shall be so terrible that it produces insensibility. That is what it comes to.

20946. Where in that do you find that Dr. Pembrey says that he was going to inflict pain without anæsthetics? He does not say so?—But he says that the pain in the operation there is of a protective nature. Obviously that must be where there is no anæsthetic given. "It may produce syncope, and therefore less sensation of pain." I presume that when a person is absolutely insensible, in a state of syncope, he does not feel any pain at all, but the horror to me is that pain should be of such a terrible character that it should produce syncope.

20947. Is Dr. Pembrey there giving anything more than an illustration of the protective mechanism?—I should not read it so.

20948. Is he not there giving simply an illustration of the protective mechanism, and is he not, in a subsequent part of the answer, advocating hypnotism instead of an anæsthetic; and have you not put down before us that he is advocating that the animal should be put in a condition of syncope instead of an anæsthetic?—No, I do not agree with that interpretation. As I understand his answer, which begins "Further," immediately after the words "sensation of pain," there is nothing about hypnotism in the early part of the answer. All he says is:—"Further, I say that the introduction of an anæsthetic may produce complications, so that more experiments may be necessary than if the experiment were done on the animal in a condition of hypnotism." But what does he mean by hypnotism? Does he mean the syncope which is produced by a terrible pain?

20949. May I ask whether you have read his evidence, because he explains what he means by hypnotism before. You have not read it?—I have read some of it.

20950. You cannot explain then why the word syncope is used here instead of hypnotism; how that unfortunate mistake got into your *précis*?—No, I do not know how it got there; but what I do say is that if pain produces syncope it is a perfectly horrible thing. If you inflict sufficient pain to produce syncope it must be horrible torture.

20951. Do you mean to say that Dr. Pembrey has ever advocated that? He advocates hypnotism. He is simply illustrating the protective mechanism in the shape of syncope. Is not that true? When you get severe pain you faint?—It may not be so, but I should be inclined to read it as if he advocated it.

20952. It would read so naturally if you substitute syncope for hypnotism, as has been done in your *précis*; but if you put in the words which Dr. Pembrey used, it cannot be read so?—I am very glad to find that he is not quite so bad as I have painted him.

20953. There is one more question that I should like to ask you, and that is, you and your society are desirous of getting at the truth of this matter, are you not, with regard to experiments on animals?—I hope we are all of us desirous of getting at the truth of the matter.

20954. And you have been especially doubtful with respect to the anæsthesia and the condition of the animals when they have been kept alive after an experiment?—Yes.

20955. And you have suggested a want of candour on Dr. Starling's part in other cases with respect to this very question?—Yes.

20956. Dr. Starling gave his evidence here, I think, on the 12th December, 1906?—Yes.

20957. That is a year ago, is it not?—Yes.

20958. In that evidence Dr. Starling requested that those responsible persons belonging to anti-vivisection societies, who were desirous to see what happened in the laboratories themselves should come to him and see what happened. Was that not so?—No, I do not read it so.

20959. He suggested it, I mean?—He said that he would admit any Member of Parliament or layman, but not any doctor.

20960. That is all I meant?—I should be no use if I went there. I am a Member of Parliament, but I should be of no use there.

20961. Has your Association, or have you, during the whole of that year made any attempt whatsoever to accept Dr. Starling's invitation?—No, because we should be of no use. Do you suppose that I should be of any use witnessing an experiment?

20962. But you have just told us that a man of common-sense and intelligence, as you consider yourself, would be able to judge on the subject as to whether a dog was properly anæsthetised or not?—Yes, but in order to do that, what I should like to do would be to be able to see through a hole in the door without it being known that I was there. I do not say that there would be anything underhand or wrong about it, but I do say that if an observer was present special care would be taken.

20963. But even supposing that special care was taken, would it not be better, if you really were desirous of finding out what was going on, to accept an invitation of that kind? What I want to understand is, why no member of your Association has attempted to accept that invitation of Dr. Starling's given in that way?—Because we do not think it would be of any practical use if we did. If we could get admission for a doctor whose views were in accordance with our own to see these experiments, we should be very glad. For instance, there is a Dr. Hadwen, who is very prominent in anti-vivisection societies. If we could get admission for him we should be very glad. If we could get admission for Miss Lind-af-Hageby I daresay it would be very useful to us; but for a mere Member of Parliament or layman as he suggests to go and witness experiments would be of uncommonly little use.

20964. Then the reason why you have not done it is simply that you, as a Member of Parliament or layman, consider that you would get no advantage whatever by so doing?—We think it would be a waste of time.

20965. (*Dr. Wilson.*) There is only one question that I want to put to you. Apart altogether from advantages which may be gained either in the prevention or the cure of disease or for the relief of human suffering, do I correctly understand that your Association takes up the position that on ethical grounds alone you press for the total abolition of vivisection?—I think that is so. At all events I should take that ground myself. If you put it on ethical grounds alone I should press for total abolition.

APPENDIX A.

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I.—DRAFT FORM OF APPLICATION FOR LICENCE SUGGESTED BY SIR V. HORSLEY, F.R.S., F.R.C.S

39 and 40 Vict., c. 77.

APPLICATION FOR LICENCE.

Address____________________

Date____________________

To THE RIGHT HONOURABLE THE SECRETARY OF STATE FOR THE HOME DEPARTMENT.

SIR,

I, ____________________

beg to apply under the above-mentioned Act for a Licence for the performance of experiments on animals, and hereby append particulars of my proposed investigation.

1. General purpose and object of the investigation ____________________

2. Nature of experiments to be performed on anæsthetised animals which are killed before recovery from the anæsthetic ____________________

3. Nature of experiments to be performed with aseptic or antiseptic precautions on anæsthetised animals which are subsequently allowed to recover from the anæsthetic ____________________

4. **Nature** of inoculation experiments to be performed on anæsthetised or non-anæsthetised animals______

5. Class of animals to be employed in the investigation______

6. Place in which the experiments are to be performed______

This application is supported by the recommendations appearing below.

I am,

Sir,

Your obedient Servant,

* Here applicant to sign his name.

*______

We recommend that the above application be granted.

* Here the person recommending is to sign his name.

† Here specify statutory qualification (see Sec. II.).

1. *______

†______

2. *______

II.—DRAFT FORM OF RETURN OF EXPERIMENTS PERFORMED DURING A YEAR BY A LICENSEE

SUGGESTED BY SIR V. HORSLEY, F.R.S., F.R.C.S.

Form No. RETURN OF EXPERIMENTS PERFORMED DURING THE YEAR 190 .

No of Licence________

Total Number of Experiments performed.	Number of Experiments in which the animal was killed before recovery from the anæsthetic.	Number of Experiments in which the animal was allowed to recover from the anæsthetic.	Number of Inoculation Experiments.	Number of Experiments in which pain was definitely observed.	General Remarks.

Reference to journal or periodical in which the results of experimental research have been published during 190 .

Date 190 Signature of Licensee.