

Evidence of Lord Justice Fletcher Moulton before the Royal Commission on Vivisection, Wednesday, 24th July, 1907.

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Research Defence Society



18.

Evidence of

Lord Justice Fletcher Moulton

Before the

Royal Commission on Vivisection

Wednesday, 24th July, 1907

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1908

ONE SHILLING NET

The following letter was published in the newspapers on April 24th, 1908.

RESEARCH DEFENCE SOCIETY.

SIR,

A Society has been formed, with the name of the Research Defence Society, to make known the facts as to experiments on animals in this country; the immense importance to the welfare of mankind of such experiments; and the great saving of human life and health directly attributable to them.

The great advance that has been made during the last quarter of a century in our knowledge of the functions of the body, and of the causes of disease, would have been impossible without a combination of experiment and observation.

The use of antiseptics, and the modern treatment of wounds, is the direct outcome of the experiments of Pasteur and Lister. Pasteur's discovery of the microbial cause of puerperal fever has in itself enormously reduced the deaths of women in child-birth.

The nature of tuberculosis is now known, and its incidence has materially diminished.

We owe the invention of diphtheria anti-toxin entirely to experiments on animals.

The causes of plague, cholera, typhoid, Mediterranean fever, and sleeping sickness, have been discovered solely by the experimental method.

Not only have a large number of drugs been placed at our disposal, but accurate knowledge has replaced the empirical use of many of those previously known.

The evidence before the Royal Commission has shown that these experiments are conducted with proper care; the small amount of pain or discomfort inflicted is insignificant compared with the great gain to knowledge and the direct advantage to humanity.

While acknowledging in general the utility of the experimental method, efforts have been made by a section of the public to throw discredit on all experiments involving the use of animals. The Research Defence Society will therefore endeavour to make it clear that medical and other scientific men who employ these methods are not less humane than the rest of their countrymen, who daily, though perhaps unconsciously, profit by them.

The Society proposes to give information to all enquirers, to publish *précis*, articles, and leaflets, to make arrangements for lectures, to send speakers, if required, to debates, and to assist all who desire to examine the arguments on behalf of experiments on animals. It hopes to establish branches in our chief cities, and thus to be in touch with all parts of the kingdom: and to be at the service of municipal bodies, Hospitals, and other public institutions.



EVIDENCE

OF

LORD JUSTICE FLETCHER MOULTON

BEFORE THE ROYAL COMMISSION ON VIVISECTION

Wednesday, 24th July, 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, LL.D., M.D.

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

The Right Hon. Sir JOHN FLETCHER MOULTON,
called in; and Examined.

12691. (*Chairman*.) You are a Member of the
Privy Council, a Fellow of the Royal Society, and a
Lord Justice of Appeal?—I am.

12692. For many years you have taken an interest
in the progress of curative science?—Yes, I have
taken a very keen interest in it both from the scientific
side and also from what is called the ethical side.
Having been a politician, and realising that the public

take to themselves the right and the duty of controlling everything that goes on in the kingdom, I have realised for many years that the question which this Commission is directed to consider would come on in some form or other in the way of an inquiry; that, in other words, the justification for scientific research in connection with curative science would be examined. As I have in many other things observed, the advance of science takes the workers in science more and more beyond the ken of the ordinary public, and their work grows to be a little understood, and much misunderstood; and I have felt that, as in many other cases, the need would come for interpreters between those who are carrying on scientific research and the public, in order to explain and justify their work. The consequence is that I have considered the subject very much from the very point of view from which this Commission has to consider it. I have come, many years ago, to a very deep conviction upon it, and therefore I am glad to have the opportunity of putting before this Commission the reason for the faith that is in me.

12693. You say you have taken great interest in the subject for a long time, and I believe you have very closely watched what has been going on with reference to it on both sides?—Yes, I have watched it very closely on both sides. Of course, you quite understand that so far as the scientific side is concerned, though I am qualified to judge of the scientific results and of the evidence for them, I am as an outsider immeasurably less qualified to speak with authority on the scientific points themselves than are some of those whom you have had before you; but, on the other hand, I think that with regard to the scientific

methods, and with regard to the bearing and meaning of the results upon the subject of this Commission, I am, as a close observer from outside——

12694. A highly qualified observer, I may say for you?—I am qualified to give some assistance to the Commission.

12695. In your long experience at the Bar, before you went on to the Bench, you were specially interested and employed in matters that dealt with questions of scientific investigations of all kinds?—Yes; I have had, I think, unrivalled experience in being what I have described as an interpreter between those who are doing the pioneer work in science and the people who have to judge of the results of their work; and it is, perhaps, more from that standpoint than from any other that I desired to put my views before the Commission with regard to this question.

12696. In your view, I understand, the real issue raised by the agitation against experiments on animals is, whether or not curative science shall be studied experimentally or whether it shall become a purely observational science?—Yes. I think that in the controversy a great deal has been lost by looking at it in too narrow a light. It is not a question of vivisection—that is to say, the performing of cutting experiments on living animals; that is a mere nickname, a mere catch phrase, which was originally invented, I have not the slightest doubt, for purposes of prejudice. I am not saying that with a desire to find fault. One always likes to get a name which expresses one's own point of view without further explanation; but it is a most imperfect representative of the real issue. The real issue is whether curative

science is to be an observational science or whether it is to be an experimental science, and, as I shall hope to show this Commission, the importance of the decision of that issue is impossible to be exaggerated. But I shall deal with that a little later on, after I have dealt shortly with the grounds of the opposition to its being an experimental science.

12697. You have something to say, I think, on how far the opposition is based on humanitarian grounds?— Yes; the opposition purports to be based on the principle that we are bound to regard the sufferings of animals, and that the avoidance of the suffering of animals is a motive which ought to influence every man with a conscience. It is a consequence, no doubt, of a change which has come over the views of mankind decidedly in the right direction. Of old, cruelty was thought nothing of, whether it was to men or animals. You cannot read the history of the world down to, say, the seventeenth century, without feeling that there was an extraordinary callousness to suffering of every kind. A great change from that time has been passing over the world. First, there came the regard for the sufferings of men, and cruelty towards men gradually passed out of favour; and now the thought of human suffering and the desire to avoid it is one of the most unquestioned and influential motives that guide the action of mankind. Presently there followed the extension of the feeling to the sufferings of animals, and there is no doubt that now, with the best part of humanity, sympathy for the sufferings of animals and the desire to lessen those sufferings are most influential motives of action. Now that appears to me all in the right direction, and so far as my opinion goes it is not only not contested by either party

to this controversy, but both of them would in words accept it as a duty to lessen so far as possible the sufferings of animals as well as of men. I do not wish to be misunderstood when I talk about the sufferings of animals in the same breath, as it were, with the sufferings of men. I must therefore point out a very broad distinction between the two, which must be borne in mind if we are to come to right conclusions in this matter. The sufferings of animals are substantially physical only. The sufferings of men are not solely physical. Almost as important a factor in that suffering is mental suffering, arising partly from a man's relations to society—that is to say, that death or sickness will cause pain and suffering and trouble and misery to those who are intimately connected with the man—and partly in the individual himself. He is capable of feeling anxiety, regret, dread, and many other things which are of the most serious importance in measuring human suffering, but which are practically absent from animal suffering. I do not say this in any way to detract from what I have said about the importance of avoiding and preventing, so far as we can, animal suffering; it is only that we may keep true our ideas of what suffering is, and to do that, one must bear in mind the broad distinction I have just mentioned.

12698. You mean that if a man and an animal have each to be subjected to a serious operation under anæsthetics, the whole of the pain and suffering that the animal experiences after it is properly anæsthetised and dies under the anæsthetic is covered by the anæsthetic?—Yes.

12699. And it has not suffering before or after?—No.

12700. But in the case of a man, he has the

suffering of anticipation, thinking of the future, of those who belong to him, and if he dies he has the suffering of his family. If he survives, he has suffering from that again, which may amount also to disablement and loss of some of his functions?—Yes, that is a very striking example of the difference, and it brings out one other point which you must also bear in mind, and that is, our responsibility with regard to mankind as compared with our responsibility to animals. In the case of an animal, if the future is to be a future of pain we unhesitatingly kill it. No matter how certainly the future must be a future of pain in a man, we must by universal acceptance keep him alive. Therefore, in deciding on our duty to men and animals, we have to realise that no matter what be the consequences we have no right to decide with regard to man as to whether it is better to terminate his sufferings or not. If you take the case of a man with some terrible disease over him which may perhaps be cured by an operation, he has the terror of the uncertainty of the cure. The operation is fortunately under anæsthetics, and he does not feel it, but afterwards there may come an only partial cure, and a painful life ahead of it. Not one of these elements exists in the case of the animal. The animal, if the pain is going to be greater than the value of life, is put out of the way painlessly. So that we must remember that distinction, because it is not permissible in a serious inquiry like this to shut our eyes to any truth. There is no safe side on which one may lean in the way of exaggeration in the examination of a question like this; and although you are going to be loyal to the duty of lessening the pain of animals, you must not mistake the relative position of pain with them and pain with us.

12701. You admit that there is a responsibility on everyone who operates on animals to avoid unnecessary pain?—Oh, yes. In fact, I go further, and I think that one ought to admit as a fundamental principle that it is the duty of all men to work for diminishing pain. The diminution of the totality of pain in animals and in men ought, I think, to be a guiding motive and a prominent motive in the case of people in the position of those who are working in curative science, and also in the position of people in ordinary life; and in my opinion it is just as operative, and, if I might give my private opinion, it is far more operative in those who are connected with the curative sciences than in the people who attack them so bitterly. And it is for this reason: Because those who are dealing with the curative sciences are perpetually brought in face of pain, and it is a point of honour with them to conquer that pain and relieve it, till that aim becomes to them a kind of second nature—a kind of all-pervading motive.

12702. You are speaking rather, are you not, of the practising physician or surgeon than of one who devotes himself entirely to research?—I am; but the reason why I cite that is this, that there is an almost unanimous opinion among those who are engaged in the practical work of the curative sciences in favour of experimental research in connection with them. That support has been discounted by people saying: “Oh, those who are engaged in curative science get callous to pain.” Now, in my opinion, they are more sensitive to pain, because it is perpetually appealing to them, and, if I might say so, it is the lifelong foe which they are engaged in fighting. I mentioned therefore the extent to which the desire to diminish

pain was present to those who are engaged in it, not so much as dealing, as you say, with those who are engaged in research, as with those who, with a full knowledge of the matter, give them their support, in order to point out that that support has the very high sanction of coming from people who all their lives are fighting pain, and fighting it often at very great personal discomfort and self-sacrifice, till it has become to them, as I say, an almost all-dominant motive. The difference between the two schools is this: Those with whom I range myself, who are desirous of extending and supporting experimental research in the curative sciences, consider not only the pain that is inflicted, but they consider the pain that might be prevented, and they hold themselves responsible for permitting pain which they could stop, just as much as for inflicting pain deliberately—they look at the two together. The other school consider only inflicted pain. They think it their duty to prevent pain being inflicted, even though the infliction of pain may lead to the prevention of many times that much pain in the future.

12703. Of course, your argument, if carried out—I am not disputing it, or offering any opinion about it—would lead to this, that if anæsthetics had never been invented, you would still be justified in carrying out experiments on living animals, and inflicting the necessary pain upon them for the sake of the knowledge that would be acquired in the saving of pain in the future?—It would; but if you will allow me I will put my practical conclusions latest. I want first of all to lay down the foundation of the decision. I want to get before the Commission the fundamental principles as I view them, and to establish them, and

then I propose to take the practical conclusions, when I will deal with such a matter as you have referred to.

12704. But, of course, the actual fact that anæsthetics and their use have been discovered now does not render the argument I was putting a very practical one. It is rather a logical consequence than a practical one?—Certainly; I quite understand that. Now I want the Commission to allow me to take what I may call a simile or an illustration. I am going to take a case which in my opinion exactly represents the attitude of the two rival schools; it is not connected with experiments on animals at all, and that is one of the reasons why I have chosen it. I want the Commission to understand that I take it as explaining my views, and I shall trust to bring the question which is before this Commission up to the level of this illustration. I will suppose that a ship which is plague-stricken, and has got rats on board, arrives at a port. A man with a sense of his responsibility, knowing that there is a high probability that rats convey the plague, would unhesitatingly extirpate those rats, even though his only method of doing so was by putting them to a painful death, whether it was by poisoning them with phosphorus or by stifling them, or by even more painful methods. He would not hesitate; he would feel it his duty to extirpate them. Now, supposing that a person were to come and say, "I could not bear to see those poor creatures running for their lives and in danger, trying to save themselves from their relentless pursuers, and so I let as many as I could escape," I have no doubt that the person who did that would think that he acted from humane motives. But what would be the consequence? It might communicate plague not only to a town, but to

a whole nation. It might bring positively measureless misery. The first man would be right, because he would look at the inflicted pain which would be to the bad side of the ledger. No one would have a right to inflict that suffering merely capriciously. But he would see beyond that pain he was inflicting—that in acting thus he was preventing an amount of pain which was beyond all measure greater than that which he was inflicting; and if he was a man to whom pain appealed, who had a heart which felt keenly suffering whether in men or animals, he would do it all the more unhesitatingly. The other man would think only of the inflicted pain, and say, "I am too tender-hearted to inflict it." He would not consider that by not doing it he was causing preventable pain on such an enormous scale. That is typical of the struggle between the two parties. I have chosen that example, not because it is an exaggerated one, or because it is an unfair one. I have chosen it because in that case there is no veil between the act and its consequences. One can see plainly that the letting free of those infected rats might produce those consequences. But now just let me take a hypothesis. I will suppose that, instead of bringing a plague, the killing of those rats would lead to the discovery of the antiseptic treatment. That antiseptic treatment put an end—substantially, of course, I mean—to an amount of human misery that we can scarcely realise. The suppuration of wounds, accidents leading to months of painful sickness and a recovery which was only partial and left the people maimed, the horrors of war doubled, the deaths in war enormously increased, were all consequences of sepsis—it was just as bad as a plague. Now, instead of it being a question of "either you extirpate the rats or you have the plague," it had been "either you extirpate the rats

or the antiseptic method will not be discovered, and the suppuration of wounds and all the horrors which follow from that must remain in the world," you see at once that to the thoughtful man the argument in the two cases is precisely the same—"If I do not inflict this pain I permit an unmeasured amount of pain which I could prevent." In my opinion, when you once see clearly the causal connection between the pain you inflict and the diminution of pain which follows from it, it makes no difference in what way it follows. Your duty is to take that line which produces the minimum total pain, and whether the pain is inflicted pain, or whether it is preventable pain which is not prevented, is in my opinion one and the same thing.

12705. Supposing that you have not got an immediate prospect of preventing pain or of preventing septic conditions, but are pursuing these experiments for the purpose of acquiring knowledge that you do not possess, with the hope and belief that it will lead to some good, how do you deal with that?—You have anticipated the very next step which, in my opinion, we have to take in the examination of the subject. The reason why I gave this illustration was that I wanted to put before you by an example the criterion which I say every humane man ought to follow. It is that he ought to take that line which before his conscience he thinks will lead to the diminution of total pain, and if he is satisfied that the infliction of a certain amount of pain is the right way to diminish the total pain he is bound, from his very feelings of humanity, to take it. And now you say—and, if I might respectfully say so, quite rightly—if that is going to guide you in a matter of this kind, you must be satisfied that a man ought to realise that the practice of the experimental

method is the wisest way to work for the diminution of the totality of pain. If I have brought the question to this point, it is all I want to bring out of the example which I have taken. I admit that those who claim that they should inflict pain are bound to show that it is the wisest thing to do with the view to reduce the totality of pain. But I want to point out one other thing, and that is this—that a man would be bound to take the same course with regard to the rats if there was not a certainty, but a reasonable ground for thinking, that plague would follow. He would have no right to say, “It is not demonstrated; I will risk it.”

12706. There is a case, of course, beyond that, where a man is making experiments upon a living animal with a view to ascertain precisely what the action of certain parts of the body is—what their functions are? —Yes.

12707. Not that he can point to any particular saving of pain or good that can be done by it, but he is doing it to acquire knowledge which he does not possess, with a philosophic belief that it will lead to the good of reducing pain?—It is a real pleasure to me to have such points put to me, for this reason—it convinces me, as indeed I knew before, that I have the honour of speaking before those who have thought very seriously over this matter, and realise the various points which arise. You will find that I shall take that point up in its proper place; because, in my opinion, it is of the greatest importance that we should face specific questions of that kind. You cannot, in this matter, come to a wise conclusion by simply dealing in vague generalities. But if you will allow me, I will take it up later, and you will find, I think, that I have put it in its logical position. Now, I said that I took that

example of the plague-stricken ship, not because it was exaggerated, but because there was an obvious connection between the act and its consequences, and then I pointed out that if you could get an equally obvious connection between pain inflicted and a great discovery it would be the same thing. I said that the parallel I have taken was not unfair to either of the two parties, and I want to digress for a moment to show that I am justified in saying so. I think if you were to ask a man who had given thought to the matter, who, of all living men, had been privileged to do most to reduce pain, the answer would almost, without exception, be "Lord Lister," who introduced the antiseptic method, which has been followed now by the various aseptic and antiseptic methods which obtain all over the world. Yet I saw the other day in a paper that at an anti-vivisection meeting Lord Lister's name was mentioned, and it was greeted with shouts of "Brute." They called a "brute" the man who had done most of all living men to lessen pain. And that was, if I might say so, perfectly consistent. They knew that he had voluntarily inflicted pain in his experiments, and that was all that they looked to; and they did not realise that if their real motive, the thing which was the real spring of their action, was the desire to lessen pain, Lord Lister's name should have been the most highly honoured of all. Now, I am not speaking bitterly at all of these people, though I oppose their action so much; I only want to point out that they are acting according to the effect on their emotions of the contemplation of pain; not according to the pain itself. The prevented pain, which has made Lord Lister's name so esteemed, is not present to their minds. The inflicted pain strikes their imagination, and the consequence is that they, with the best of motives, denounce him

because he is willing to inflict pain, without realising that he, who is the most capable, at any rate one of the most capable, of all men to know the ultimate result of his actions, has been doing it for the purposes of reducing suffering, and has succeeded beyond all hope. So that I am not treating those who oppose me unfairly in pointing out that they do not regard the more distant consequences of their action—they only regard the question of the immediately inflicted pain.

Now, I want to go on to what the Chairman has put to me, viz., that I must establish that experimental research is the wise mode of action if we wish to diminish pain. I start from the following fundamental facts. I say that if you look round the world, the only way in which we can diminish pain is by human action. Animals go on regardless of the pain they cause to one another. In fact, the universe is built on pain. Whole races of animals live simply by killing other races, and killing them without the slightest regard to suffering. So that we must look to human action alone to diminish pain. Now, what is it that prevents our diminishing pain in the world? One feels at once that it is not the absence of desire to do it. I believe that desire is very prominently present. It is because we do not know how to do it; it is our ignorance. Take the case of doctors. Doctors are as desirous as they can possibly be of lessening suffering in the world, and yet how often they stand by the bedside and the patient goes on suffering and they can do nothing. Now, the absence of our power of diminishing pain is due to ignorance, to our not understanding two things. First, not understanding the causes which excite pain; and, secondly, not understanding the action of those causes on the organism—that is to say, how it is that those morbid causes, if I might use the phrase,

produce pain. I call the first one (simply to distinguish it) the pathological factor—that is to say, what are the springs of disease; and the other is the physiological factor—that is to say, how those springs of disease cause pain in the organism; that is just a division for convenience of the knowledge which we must possess in order to be able to lessen and control pain. Let me give an example of the way in which lack of knowledge increases suffering. The discovery that the organism could, by means of anæsthetics, pass into unconsciousness without passing into death, and that all the living operations could go on equally vigorously while consciousness—that is to say, the possibility of pain—was entirely suppressed, led to the use of anæsthetics, and, as a consequence, to the diminution of suffering, both directly during operations, and indirectly in increasing the possibilities of surgery. Without the knowledge obtained from that discovery men were obliged to perform operations in the brutal method of the old surgeons, because there was no other alternative. This is only an example of the way in which our power is limited by our knowledge. And I wish to point out with regard to this point, that here again we have no difference of opinion. Not only the whole medical profession, but the whole public agree that doctors ought to be armed with all the knowledge of the time. There is no difference of view as to this. Every attempt is made to increase the efficiency of the education of the doctor; because we feel that by arming the doctor with the knowledge of the time we arm him as best we can with the means of diminishing pain. That, of course, is true with regard to the knowledge that has been attained during the last, let me take it, forty years—the only years as to which I can speak; and that knowledge has been mainly due to experi-

mental research. Therefore all must feel that this knowledge which has been obtained by experimental research is useful for the purpose of lessening pain, because we try our very best to make all those who are engaged in practical curative work masters of it. I do not think I need point out to this Commission further how all-important knowledge is where you are dealing with organisms, and you want to stop suffering.

There is one other thing I want to say, and that is this. That our inability to stop suffering, owing to absence of knowledge, is not only to be found in the case of obscure diseases. They afford striking instances of it, no doubt, and are by no means rare. I should think there are very few of us who have not seen those near to them suffer from diseases which have baffled the doctors, and whether or not they could be eased or saved was decided by whether or not the doctors could read what was the cause of the trouble. A friend of mine has just died, apparently of a painful disease which the doctors could not touch. A *post-mortem* showed that it was only because they did not realise what the true cause of the disease was; it was one which might almost with certainty have been relieved. Such diseases may have marked symptoms, but the doctors are unable to read from those symptoms what is the cause, and therefore they cannot fight the cause. But, although these cases are striking instances of the principle, they are nothing like the most important; the most important are diseases which are quite common, of which we cannot yet find the cause, and not finding the cause all we can do is to deal with the symptoms.

12708. I think we all agree that there is a very vast field of medical knowledge yet; I think every medical

man would agree to that, as well as ourselves. But really, the question is how far the knowledge can only be found in experiments upon animals?—Precisely. That, I think you will find from the *précis*, is the next point I want to mention. But I wished to put in the forefront that it is want of knowledge which prevents our diminishing suffering, because I shall trust to show to this Commission that the polestar by which we steer is knowledge. It is ignorance that hampers us. I am using the term ignorance in a sense that will not, I trust, be misunderstood. I am aware of the gigantic amount of knowledge that the present and past members of the medical profession, and those who have been engaged in research, have already accumulated, but what I mean by ignorance is the vast field of what yet may be known, but is not at present known. I desire to impress upon the Commission that that is the real cause of our impotence. Now, let me take the question of how we are to get rid of this ignorance—and here we are on firm ground. No man who knows anything of science has any doubt whatever that the right way to advance knowledge is by experiment. 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. Where we are reduced to observation science crawls. Where and in proportion as you can use experiment, the science advances rapidly. I could take any science to show that; it is much easier to show that experiment has led to advance than to find a case in which advance has been made purely by observation; because a scientific man rushes at once to experiment if he can do so. He adds, of course, the testimony of observation—that is extremely valuable for the

purpose of guiding and starting his experiment ; but everybody knows that each observation becomes only the nucleus, or, rather, the basis, from which you start your experiment. I was puzzling to think of some branch of science where there was only observation, and the only thing I could think of was the case of volcanoes. Now, there is not anything that has attracted human attention more, or fascinated it more, or has been on a bigger scale or more open to observation, than volcanoes ; and yet we are almost in the same state of ignorance of the origin and the *modus operandi* of volcanoes as Pliny was.

12709. The difficulty is just as great now as then for anybody to go down a volcano when it is in eruption ?—Precisely. In no way can you bring experiment to bear upon it. As soon as you can bring experiment to bear upon a subject you are free ; but as long as you can merely observe, your progress is very slow. The reason is that experiment is like cross-examination. You can put the question you want, and Nature always answers it. She does not answer the question you meant to put ; she answers the question you actually did put. She swears by the card in the most shocking manner. She does not care in the least what you meant to ask, but she does care what you asked, and she answers it with perfect truthfulness. And the consequence is, that when you adopt experiment, the great experimenter can put a question, the answer to which lets the whole secret out. I am going to give an example that I remember impressed me very deeply. I cannot tell how many years ago, but it was just about the time when it was first discovered that guinea-pigs were susceptible to tuberculosis. I heard from a friend that a doctor of his acquaintance,

a practising physician, I suppose, had been troubled as to whether consumption was a communicable disease or not: I should say that the vast preponderance of opinion at that date was that it was hereditary. I remember, as all of us remember when we were young, the way in which consumption would, as it was called, go through a family, and it was thought that this was because it was in the strain. Then some bold people suggested that it was possible it might be due to its being communicable, although probably to the ideas of that time it looked about as likely to be communicable as spinal disease. What that doctor did was this: he took a dozen guinea-pigs, divided them into two sixes, and he let one six have the run of the wards of a consumption hospital, and the other six have the run of the wards of another hospital, just about in the same conditions of temperature; he left them there for the requisite number of weeks, whatever it was, and then he killed the whole dozen, and he found that the six which had been running in the consumption hospital had tuberculosis, and that the six that had been running in the other hospital had no trace of it.

12710. (*Chairman.*) The other was not a consumption hospital?—No, it was not.

12711. (*Sir William Collins.*) Whose experiments were those?—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 upon tuberculosis, and I believe it to be true. But if it was not actually performed, it would still serve as a typical example of experiment. I have no reason to doubt that this experiment was actually performed, because the man who told me was a most intelligent man, and he told

it to me at the time as having been done quite privately, by a doctor whom he knew, in order that the doctor might settle the doubt in his own mind.

12712. (*Chairman.*) You would say that it is rather an illustration than a typical example, if it is not given us at first hand in some way?—You may take it simply as an illustration, although the very thing has been done, I know, by other people. Klein did it. If you remember, Klein put guinea-pigs into an air shaft which led from the wards of a consumption hospital, and found that they developed tuberculosis. But we will take it merely as an illustration. I am not basing anything on the fact. What I want to point out is this, that such an experiment would settle the question more than would twenty years of observation,—I might say a century of observation—for this reason, that you had placed these similar animals under entirely similar circumstances, with the exception that in the one case, but not in the other, there were present in the immediate neighbourhood persons who were sick of the disease, and you will find as a result that in the one case they become sick, and in the other they do not. There is no question of heredity, and there is no question of the acquired disease being the consequence of different circumstances of living, of different circumstances of nutrition, or anything of that kind. The only thing that is present as a cause is the neighbourhood of the sick people in one case and not in the other. In the result the difference is disease in the one and not in the other.

12713. What was the fact that it was wanted to discover there—whether guinea-pigs could take the complaint?—No, whether the disease was communicable from one person to another.

12714. But there are plenty of examples of people who have not tuberculosis daily frequenting hospitals and not acquiring it, are there not?—Oh, yes; but the point of the experiment was not under what circumstances was the disease communicable, but whether it was communicable at all. Just take the case of persons who are ill of spinal disease.

12715. I follow you.—Their relatives go freely among them, attend to them, and try to make their sickness as easily borne as possible. And so it undoubtedly used to be when members of a family were sick of tuberculosis. Everyone thought it was only kindness that the brothers and sisters should attend to their sick sister, and nobody thought that there was danger. But when you had found that this was a communicable disease, your duty with regard to children and other people in the presence of the disease became very different, and it was only to find out this that the experiment was tried. The experiment showed affirmatively it was communicable. That was only, of course, the first of a necessary series of experiments to show how it was communicable; but from that moment that doctor knew how great was the responsibility of allowing people to attend or be in intimate contact with one who was affected by consumption. Just try and see how you could get an equal degree of certainty from observation. You take a case where it has run through a family. Yes; but it might be that it was hereditary in the family. Then you find that in a particular place it is very largely prevalent. Yes; but that may be because, as compared with other places, the climate or soil of that place produces a predisposition to it. Neither group of cases points necessarily to communicability. Some people will be struck in one way with a series of these cases, others

will be struck in another way, and there will be a perpetual conflict as to which is the true explanation. But when you take such an experiment, there is not a doubt; you cannot explain it, except that the disease is communicable. And the consequence is that you commence your further examination with the consciousness that it has been demonstrated that it can pass from the sick to the well. Now I was very much struck—as illustrating the impossibility of arriving at these things by observation—by a piece of cross-examination by Sir William Collins of one of the witnesses before the Commission; it illustrated it in the most striking way. He was dealing with the result of the serum treatment of diphtheria. An appeal was made to the statistics of diphtheria hospitals in London, and the witness said: “Oh, the serum treatment is being demonstrated as being a great success, because you have here the same hospital in the same place dealing for a consecutive number of years with the same population, and the statistics show a great diminution of the percentage of deaths after the introduction of the serum treatment.” Sir William Collins, if I might say so, quite justifiably cross-examined on that. He will know whether I am fairly representing him, but, as I remember it, he asked are we now getting the cases earlier; is it not true that we have made such advances in the diagnosis that we know better what is diphtheria and what is not? And he went on pointing to differences in the circumstance of the earlier years and the later years as taking away from the conclusiveness of the statistics.

12716. (*Sir William Collins.*) As an element that required consideration.—Exactly, as an element that required consideration. That is perfectly fair. Statistics that are got in that way must be rightly open to

that criticism. And this is the reason: You dare not let the children wait in the later years so as to make the time at which they are taken up for the purpose of treatment comparable with what it was in the earlier years, because you are bound to do for every child the best thing that you can, and the consequence is that you dare not make your experiment scientifically more perfect, because there is the paramount duty towards human life. Now, you could not have had a stronger case, *i.e.*, one in which the observational method was working under greater advantages. You had hospitals devoted to this disease alone, dealing, as I have said, with the same population, in the same place, under, as far as possible, the same circumstances; but you could not, and you dare not, and you ought not to, make the circumstances identical, because of your duty to the individual. Compare that with the case I was putting forward of the guinea-pigs; I could give another and more striking example, which is the classical experiment in connection with anthrax, in which, after Pasteur had worked out the treatment by inoculation against anthrax, he took 50 sheep for his experiment; 25 of them were inoculated and 25 were left uninoculated. Then the whole of the 50 were inoculated with virulent anthrax, and within four or five days every one of the 25 that had not been inoculated died, while of the others 24 were alive, and one was shown to have died from things unconnected with anthrax. You see by these examples how in experimental research you can make the circumstances identical, and one experiment of that kind (properly repeated, so as to eliminate error) gives you a certainty that years of observation could not give. And all those years of observation have paid their toll in suffering. It is no use saying, Well, we shall learn in a few years hence, instead of learning it

now. All that time there has been preventable pain, which has not been prevented because you have not known how.

What, therefore, I want to point out is that we know that the way to acquire knowledge with certainty is by experiment; it follows from its very nature, and from our universal experience in all sciences, that this is the way in which you best acquire knowledge, because you can make each experiment answer some critical question. You can arrange matters so that you have two cases precisely similar in all respects, save that one is without that cause present and the other is with that cause present, and the contrast gives you a light which you could not get from observing instances where you cannot eliminate other causes. 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 am sure there are those here who have made experiments in connection with life, and know from experience the truth of what I say. The number of factors at work is so large, the difficulty of isolating them is so great, that it often takes very much longer to realise how you can put the question to Nature than it does to put it, and to draw your conclusions from it when you get the answer. But the more complex the subject is, I say the more are you driven away from observation, which only gives you the total result of many causes which are varyingly present in all cases, to something which will enable you to isolate several of the causes and determine separately their effects.

12717. (*Chairman.*) That brings you, I think, to the question I was putting to you about pure experimentation, and the justification of experiments on

animals where you have no specific disease in view?—Yes. Many will think me a heretic on this point, but I have a strong view that, when you have once realised that experiment is the right way to acquire the necessary knowledge, it is a bad thing to work too directly and too consciously for practical results.

12718. When you call that a heresy, of whom are you speaking as the orthodox?—Most people say that you should only make such experiments with the direct view of some practical application for the benefit of mankind.

12719. We have had a good many witnesses before us who take what you call the heretical view.—I am delighted to find that I am not alone.

12720. As well as those who take the other view; and others who would say that you do not go far enough.—I am glad to find that I have got my comrades. You must not misunderstand me. I think that the object and the reward and the ultimate justification of research is the benefit it confers. But what I wish to urge is, that it is unwholesome to keep that before your mind when you are working, at all events in the earlier stages. May I give a parallel which I think explains and justifies this view? I take the history of alchemy. For centuries men of the highest intellectual attainments—for there can be no doubt of the ability of the men who worked at these subjects—worked hard at the transmutation of metals, and the result of those centuries has only been snippets of knowledge, if I might use such a phrase. I do not undervalue them; they gave us, I believe, some very valuable reagents, but that is practically all. And the reason was that they did not commence by working so as to get to

know the nature and properties of the things on which they were working; they worked straight for a result.

12721. For an impossible result, was it not?—It may have been an impossible result. But whether it was impossible or possible would have made very little difference if they had commenced by laying the foundation in knowledge, because, although we should not have obtained the transmutation of metals, we should have had modern chemistry some three centuries earlier than we did. But they only worked for a result.

12722. They did not care about chemistry. Their researches were not chemical researches?—Well, transmutation of metals was intended to be a chemical triumph. If I might take a parallel in curative science, this work was like the shots at specific cures for cancer—violet leaves and all that sort of thing—made by people working straight for a nostrum that will cure a disease like that, the nature of which is shrouded in such mystery.

12723. I only meant those people were fighting a particular disease which they had before them, and which was evident under their eyes, and they were seeking remedies blindly?—Yes.

12724. The other people were seeking something in the clouds altogether; they were not pursuing what you would call chemical research, although they used certain chemical experiments?—But if they had only laid a basis; if they had only worked at it as you try to reduce a fortress, by parallels, where each parallel brings you nearer to the fortress. If you attempt to take it by a rush and fail, you are further than you were before; but if you make your parallels you are so much nearer, and that is a permanent advance. Just

in the same way I think that if I were to ask any scientific man of position in the medical profession, "Where do you look for the solution of the great problem of cancer?" he would say, "I look to a gradual acquisition of knowledge about it, and the breaking up of the obstinate silence of the disease which will tell us nothing, neither its preferences nor the things it dislikes, nor its origin, nor anything." Such men do not give much heed to the investigations aimed directly at its cure, because they do not believe they will succeed. They believe that we shall have to know a great deal more about the disease itself before we know in what direction success can come. But what they do think is this, that by gradually acquiring knowledge of it, knowledge acquired quite independently of whether it is a thing we could use or not, the disease will gradually cease to be a mystery, and when it has ceased to be a mystery we shall find some weak point in its armour. Therefore I am satisfied that the wisest thing to do is to realise broadly that our ignorance of the springs of disease and the nature of the organism that suffers are the cause of our powerlessness, and that all research which is intended to lessen our ignorance is in the right direction, and that it is in vain for us beforehand to say which will soonest lead to a beneficial result. I feel, therefore, strongly that this is the principle which, with proper regulations and restrictions, we ought to accept; and that we ought to fix our eyes on knowledge and not on immediate promise—immediate promise is likely to be delusive—and when I come to the instances by which I trust to justify the serviceability of experimental research, I think I shall be able to show that those things have sometimes reaped the richest harvest in which the promise came quite late.

12725. You think, to sum up that part of your evidence, that the experimental method is justifiable even though you have no immediate practical object in view beyond the acquiring knowledge and the hope and belief that it will lead to some ultimate useful purpose? —Yes, you have the hope and belief that it will, and you have the certainty that it is the right way to work for it. Where you are ignorant of the evil and ignorant of how it operates you are like a hunter who is not yet on the trail of the fox. He has to cast about in order to come upon it. Similarly, it takes a very great deal of experiment to know in which direction practical success may be looked for.

I want to say one more thing before I go to the examination of whether, as a matter of fact, the experimental method has shown itself to be of this great practical value, has thus justified its use. So strongly do I hold that what we ought to fix our eyes upon is the acquisition of knowledge and the removal of ignorance, that if I was here without being able to show a single practical result of experimental research in curative science I should give evidence in the same direction, and so far as I personally am concerned with the same confidence, for this reason: Supposing I was to go back, say, 40 years (I am not suggesting for a moment that prior to 40 years ago there was not a great deal done in research, but I take that period as including the great outburst of experimental research, beginning with the early sixties and since then), I should have all my data. I should have the fact that ignorance was vast, vaster even than now, that ignorance paralysed our power of preventing suffering, and that the right way to get rid of that ignorance was by the experimental method; and therefore, even though I could not show that we had obtained any practical

results, I should say to this Commission, if it were sitting in the early sixties, that it was their duty to take care that experimental research was not hindered. The idea that you do not justify experimental research unless you can show that it has already produced practical results would make it never justifiable to commence it, and, in my eyes, it would be just about as ridiculous as to revile winter sowing because in April there were no crops. The moment when the practical results come depends upon the nature of the subject. You cannot foresee it; they may come at a burst. After long investigation which apparently leads to nothing you may suddenly find your reward.

12726. It might not have been unreasonable in Adam, but since his day there has been a very long experience of winter-sown crops?—There has been a long experience of winter crops, but what I mean is, that a man has got no business to say that because the crop has not yet come last winter's sowing has failed.

12727. It would have been an unreasonable thing in Adam?—I agree with you; but, if I may say so, I should use that in my favour; because I should say that if you look at any science you will find that theoretical investigation has always led ultimately to practical power. I could give examples in industries, but they will occur to most of those present. Investigations which have been purely scientific have almost always led to practical results, because the knowledge has been able to be used, and it has produced great results. So that, just as since Adam we have had great experience in winter sowing, so I should say that since the days of Bacon or Newton we have had great experience in this—that you never can

have an increase of knowledge in any subject bearing on practical life without its leading to increased practical power.

I want now to deal with the justification of the use of the experimental method by what it has actually performed, and I am going to ask you to allow me to confine myself to the work of the last 40 years, because that is the period which has fallen under my direct notice. I shall group it in the way I group it in my own mind in order to make out this justification. As I have said, to my mind this direct appeal to practical results is not necessary, though it is immensely helpful, to enable one to make up one's mind as to what is the right course to pursue ; but it is all-important in dealing with the many well-meaning persons who do not go along with me in their views with regard to it. The difference is not, I think, because there is a difference of fundamental principle—they are as loyal, probably, according to their lights, to the doctrine that we ought to do our best to lessen suffering, as I am ; but they have to learn that which scientific training easily teaches those who have had much to do with it, that the right way to do so is to make full use of the experimental method. I wish, therefore, to take the results of experimental work in the last 40 years, and point out how it has removed ignorance—that is to say, how it has given us knowledge, and how that knowledge has become of practical value in reducing suffering.

12728. If you please.—Of course the first example which one must take is that of sepsis and the antiseptic method. If you look at the books of 40 years ago you will find that the idea that sepsis in the case of open wounds was due to organisms derived from outside was rarely held ; I doubt whether it was held by any ; it was certainly held by very few, and it was

not put into practice in any way. And the work of those who demonstrated that sepsis was in all cases due to organisms, and where access was possible, it was almost always due to organisms coming from outside the animal attacked, gave rise to the antiseptic treatment, the value of which it is quite unnecessary to dwell upon. It was purely the result of experimental research, and it is a triumph the practical value of which puts it at the head of all the changes in the last 40 years. But I am not going to deal with that any further than just to refer to it, because I am sure that its value and its origin are present to the mind of everyone here.

I wish chiefly to deal with the case of microbial diseases, which, for reasons that will appear, I shall divide into infectious diseases and diseases communicable otherwise than by what is ordinarily called infection. If you go back 40 years you will find that these diseases were complete mysteries; they were, alas! recognised as facts, because they could not be otherwise; they were ever present with us. They were known as things which ran through a certain course, their symptoms were studied, and the doctors of that time did their best to alleviate them, no doubt, by nursing and treatment. A great number, no doubt, tried to do so also by drugs, which, I think, most authorities to-day would say were practically of little use. But the diseases themselves were, so far as their causation is concerned, perfect mysteries. Now experiment has changed that from top to bottom. The bulk of these diseases are known now to be the result of an invasion of the system by specific organisms which multiply in the system, and the disease is due to the presence and action of these foreign organisms. In fact, I do not think that I should be giving an incorrect idea of the result of the work of these 40 years, if I was to say that it has revealed the fact that

we live not as isolated living organisms, but that we are surrounded by and penetrated by infinitely small living things of very varied descriptions—that we are, as it were, bathed within and without in a sea of microbic life; I am using the word microbic in its broadest sense. Within us we have them permanently and probably normally; in fact, if you include the organisms which live in the blood, certainly normally. We are surrounded by them, and instead of diseases of this kind having to be looked at as due to defects or failure in normal action in the organism itself, we have grown to realise that they are the result of attacks upon the organisms by external foes.

12729. Do you regard all this state of things which you have been describing as wholly due to experiments on animals?—Absolutely. It is absolutely due to them.

12730. We have had a good deal of evidence from other witnesses on the subject, and we need not trouble you to describe the instances.—Quite so. It is difficult for me to say which is the earliest case in which this was established; but I will take one of them, because I wish to show how baseless is the idea that the work which has been done in this respect is work which has only led to doubtful or hypothetical results. I wish to show the actual and certain advance of knowledge. Then I wish to show next the way in which that is due to experimental methods. Then I wish to show the practical methods to which it has led. I will take a single example—any one will do—but I will take the example of anthrax. When Pasteur took up anthrax, it was only known as a terrible disease among cattle, leading to a vast proportion of deaths. He ascertained that it was due to an invasion of a particular microbe, which is known as the microbe of anthrax, which

multiplied with infinite rapidity, and which ultimately led to the death of the animal. He then found out a method of inoculation (I am not going to deal with the details of his method for a moment) against it, and in the classical experiment to which I have referred he exposed to anthrax the 25 sheep which were inoculated and the 25 which were not inoculated. He might have done it by putting them in company with diseased animals. As a matter of fact, he took the severer test of deliberately inoculating them with the disease. The result was that those which were treated with his process did not die, and the others did. Now, that is a scientific result as certain as the action of nitric acid would be on iron; I mean that it was a definite experiment, capable of being repeated, and showing conclusively that there was a way of defeating this disease, a way which might, or might not, be availed of by mankind, according as the importance of warding off the disease was great or was not. There was nothing hypothetical in it; it was a fact which was just as much demonstrated as any fact that has been demonstrated in chemistry or in any other science. I have there taken a case which relates solely to animals. I have not approached the question of the applicability of the process to mankind, because, fortunately, anthrax is very rare among mankind. But if you confine yourself to the consideration of it as a disease of animals, you see the advance that has been made. First of all, there is the knowledge of the cause—that is to say, that it is due to the presence of microbes. Then (as I shall presently point out) the fact that it was due to the presence of microbes led to the discovery of the method of combating it; and that led to the practical application of that method, and, if you choose to avail yourself of that method, the disease is con-

quered. The whole is based upon the discovery of the origin of the disease. It was the knowledge that it was due to microbes which led to the whole of that discovery. Persons might say: "What good does it do to mankind to know that this which they call a disease is really a consequence of a particular type of small life which has got into the organism and multiplies there? It does not alter the nature of the disease; people die of it all the same." And you would think that the pure theoretical knowledge that it was due to this particular cause left the practical problem where it was. May I point out how that piece of knowledge did not leave the problem where it was, but put it in a totally different position, and necessarily led to its solution? You can see it in this way. It instantly put the phenomenon of recovery in a new position. When an animal was healthy, and there were only a few microbes put in it, when you would say it was best calculated to repel invasion, they multiplied furiously. At the moment when it was weakest, when the microbes were strongest, most numerous, and the animal was most exhausted (if it was a case of recovery), the tide turned, and the enfeebled organism was able to fight the strengthened assailants successfully. That necessarily led to the conclusion that there must have been a change in the organism during that period, which entirely altered the relative strength of the two forces; and that was the beginning of the realisation that in these diseases there comes some protective change in the organism that is attacked, which brings recovery at the time when you would least expect it. This led to the inquiry: What is that change? It must be something that survives the disease, because we know that when a man is recovering from an infectious disease, he can give it to other people. Yet although

he is more exposed to infection from himself than any other person could possibly be, he is not infected. Therefore it must be something that survives the disease. Then the question came: If it survives the disease, is it possible to produce it under circumstances so that it may be present in the organism independently of the disease? In fact, men came to realise that recovery in a case of microbial disease means the "turn of the tide" in a process where, on the one hand, the invading microbes are multiplying, and on the other hand these protective forces are multiplying, and recovery or death means whether the turn of the tide comes too late or not. The moment you see that that is so, the whole question becomes—Can you antedate this turn of the tide? Can you contrive that this protective condition shall be produced earlier, before the mischief is done? It is no use putting a fire out when all that is valuable in the house is burnt. From these ideas there sprang almost immediately the two main methods of treatment. The one seeks to produce this protective state during the period of the disease, and it is a treatment of this type which succeeded in rabies; the other seeks to produce the protective state before the disease comes, and is the method that was illustrated in anthrax. Finally, they led to the third line of discovery. Is it possible to get the protective substance from somewhere else, and introduce it during the disease, in order to antedate the time when the system will become strong enough to repel the attacks of these foreign invaders? Now, if you think of all those three lines of discovery which have respectively led up to the treatment of rabies, to the prophylactic methods which are used in the case of anthrax, pneumonia, and other diseases, and to the serum treatment of diphtheria, you will find that they all came from

the knowledge that the cause of the disease was a foreign organism, capable of multiplication, and that Nature defeated it by a change in the organism which made it more capable of resisting. These practical applications were the direct outcome of those two facts, which were pure pieces of scientific knowledge. So long as the disease was considered merely as a disease which ran its course, which was due to an unknown cause, whose consequences were only known by its symptoms, no one of those things could possibly be discovered. Their discovery was solely due to the experimental examination of natural processes. I was much amused by some evidence given by one of the lady witnesses who have given evidence before you. She seemed to be very much disgusted with the consequences of all this knowledge, and, if I might paraphrase what she said, it was: "Nature has made such nice drugs and put them in the plants all around us. Why should we not use these, and not go to such messy things as serum?" Those are not her words, but they express the effect of them. Nature has put all kinds of useful drugs in plants, but she never dreams of using them for curative purposes. Her method of curing is entirely by means of the messy things with which the researches of the last 40 years have led us to work. We are now, thanks to the theoretical knowledge that we have acquired of the cause of these diseases, working to help Nature along the very lines which she has from the foundation of the world herself taken in fighting these diseases; and therefore, in my opinion, if we are to be guided in our action by our respect for what Nature has so kindly done for us, our course would be to pay very little attention to drugs, and to pay very much attention to those lines of treatment which are based on the study of the two closely analogous phenomena of recovery and immunity.

12731. Assisting our natural friends in fighting our natural enemies inside us?—Yes, fighting our natural enemies by those means which, in the cases in which we do beat them, are the weapons which we use.

12732. (*Chairman.*) I think you were going to deal with the question of the influence of experiment on communicable disease?—I have been pointing out that the whole modern conception of the nature of these diseases, and the mode of their cure, arise from discoveries which have been arrived at purely by experimental methods, including necessarily experiments on living animals, namely, the discovery that they were due to foreign organisms that entered and multiplied in the system, and the discovery that the changes in the system which unquestionably produce recovery are closely allied to those which also produce immunity after recovery. So far I have only spoken of our attempts to effect a cure by introducing those protective substances which Nature, when there is need, produces in some strange manner. But it looks at this present moment as if we were going to combat some microbial diseases still more successfully by stimulating other agencies by which she effects her cure. I do not know whether Sir Almroth Wright is to give evidence before this Commission, but if so he will point out how he has devised a method of stimulating the action whereby the white blood corpuscles actually destroy physically the organisms themselves, so that we are gradually utilising both of Nature's methods of cure (and utilising them exactly as Nature does) for the purpose of antedating what I have called the turn of the tide, that is, strengthening the resistance of the organism that is attacked more than Nature would do merely by her methods of reaction. I now want to pass from these to another class of diseases in which

equally we have been studying during the last forty years the cause of the disease and its action on the organism, but where research has led to a totally different method of fighting them—I refer to the communicable diseases which cannot be called infectious. Take the case of malaria, the tsetse in animals, and Malta fever.

12733. And rabies?—Rabies belongs to the first set, because we fight it by generating in the body a protective substance. The way in which we fight it is, we have got a method of giving an attack of rabies which is very quick but very slight, and the consequence is that it produces very swiftly a small amount of protective substances. We then give another one which is also quick, but is stronger, but which can be borne by reason of the presence of the protective substance, and continue doing so while the main body of the attack is moving up. We have thus gradually accumulated a body of protective material, which, when the main attack comes, which would have been inevitably fatal, enables the system to resist it.

12734. I merely thought that the gnat was serving the same purpose and function that the mad dog was; therefore I thought it was really in the same class of disease?—May I apologise? You are quite right. It is not infectious, but is communicable in a different way, and ought to be in the second class. The reason I already mentioned it with the first class was, because we there use the method of cure of which I have spoken—that is to say, we attempt to produce a protective substance. It was I that was wrong. I want now to pass to cases where we cannot imitate the method of cure, but because we know the cause of the disease and its method of communication we are able

to intercept it. Take the case of malaria. I do not know that our methods of curing malaria have advanced substantially, but we now know the cause of malaria, and we are destroying the possibility of communication. The work that has been done at Ismailia is an example of this. They have got rid almost entirely of the mosquito, which communicates the disease by its bite, and have thus almost got rid of the disease.

12735. Would you say that that was discovered by experiments on animals?—Certainly. They had first to discover in what way this disease was communicated. As soon as they had discovered in what way it was communicated, the destruction of the communicating animal presented itself at once as a method of preventing the disease. A similar case is that of the tsetse, which is absolutely fatal to cows in districts where the tsetse fly is found. They find that it inoculates an organism which produces death. I do not think they have made any advances in fighting the disease when the disease is once communicated; but knowing that it is communicated in that way, our attention is at once turned to discovering how one can fight the tsetse fly, or how one can prevent the tsetse fly itself getting the organism into it which it passes into the animal that it bites. In certain places in England, where there used to be ague, ague is now stopped. It is quite possible that that ague was communicated by the bite of an insect that derived the organism which is the cause of ague from biting some affected animal. We have drained the country; the insect probably exists, but in far smaller numbers, and there are fewer people there, if any, who are affected with ague from whom it can get the infection. The consequence is that the

insect is harmless, because it cannot communicate the poison, or rather the poisoning organism, because it cannot get it. And, similarly, we can, when we know the cause of the disease, set to work to prevent the occurrence of those circumstances which enable it to be communicated. This is now being done with yellow fever and many other diseases. It may be that the disease is so bad that the only method we have of fighting its spread is to kill the animal. Take the case of glanders. Now that we thoroughly understand what glanders are, when we thoroughly understand the method of testing whether an animal is suffering from glanders, the animal may be killed at once and that stops the propagation of the disease. Without that knowledge we could not ascertain so soon what animals we ought to kill, and they would be left as centres of contagion. So that it may be equally effective from the point of practical utility whether we find out the means of communication and stop it, or whether we attack the disease when it is communicated. In both cases it is the knowledge of the cause of the disease, and the knowledge of the way that that cause comes to operate on the organism, which enable us to devise these practical methods. And all that knowledge has been derived entirely from research work, conducted for the purpose of acquiring knowledge, and from the knowledge has come the power. I do not suppose the investigation of the cause of these diseases was carried on upon the lines of any direct idea of a protective method, but the object was to find out the real nature and operation of that against which protection was necessary. When this is done, the step to practical utility always comes sooner or later.

I have been dealing thus far with the causes of disease. I will now turn to another matter which I see has

formed the subject of a great deal of evidence, and that is the study of the organism on which these diseases operate. A vast amount of experimental work of the last 40 years has been directed to discover the physiological actions which are going on in man. Mere anatomy, mere structure, will not tell you physiological action; the knowledge of physiological actions must be obtained by experiment from a living creature, because, as a rule, as soon as death comes they cease. The importance of this work is just as great as is the importance of the investigation of the causes of disease. Take, as an example, that which I see has been the subject of evidence before this Commission—the localisation of function in the brain—the discovery from the symptoms of paralysis, or epilepsy, of the place where the interference with the nervous system is taking place. It would be quite impossible to ascertain the laws which govern this excepting by experiments on a working animal—that is to say, a living animal. In many cases it need not be a sentient animal; the experiments may be done when consciousness has ceased; but it is absolutely impossible to ascertain them unless you make experiments on an animal that is still living. But there are similarly important experiments which go to blood pressure, to digestion, to the function of the great organs, such as the liver, or the pancreas, or the kidney, or the suprarenal capsules, in which it may be necessary to trace out consequences which do not occur immediately. In that case you must perform them on animals which are not only living at the time of the experiment but which are allowed to live afterwards. The examples which have been given you, I submit, show how all-important such investigations are for several reasons. One of the reasons is that without the knowledge so acquired you cannot read causes from symptoms. I

should define the great change between curative science, as it exists now, and curative science as it used to exist many years ago, as being that now we try to fight causes, and then they only tried, or mainly tried, to fight symptoms. Fighting symptoms may be an extremely dangerous thing. A man who, not knowing anything about an engine and finding an over-driven boiler blow off, should fight the symptoms by screwing down the safety valve, would come to serious trouble. And in the same way, if in the case of a symptom—say the symptom of the rise of temperature going up—you say, the disease makes the temperature go up; very well, then, we will help the organism by putting it down, you may get into serious trouble supposing it should turn out that the rise of temperature, by lessening the resistance to the circulation, eases the heart and makes its work less onerous; it may be that the fast-beating heart has been able to do its work without injury only because the temperature has risen and its work is lighter. If, now, you put the temperature down without removing the cause of fast-beating, it may be that you overstrain the heart—do mischief just as the man who screws down the safety valve. I am not saying that this is so. That is not a matter on which I can speak. All I want to point out is that it is only by intimate knowledge of the organism that we know how far it is safe to fight symptoms. But, more than that, it is only by such intimate knowledge that we are able to diagnose causes. You get a headache; it is a mere symptom which may come from many different causes. Unless we have perfect knowledge of the organism we may imagine that a thing which has been successful for one headache is useful for another, whereas it may positively aggravate it instead of doing it good. The aim of all this investigation into physiological action, which is so varied

in its character, and relates to every different part of the organism, is to put the medical man of to-day in the position of a man who knows the construction and operation of the engine he is working, who knows what is the meaning of every symptom of irregular action; and the increase of his power of alleviating suffering and staving off death is enormously increased by this increase of his knowledge and power of diagnosing which has arisen from these experimental researches. As I have said, these experiments must be experiments carried on upon living animals, because their object is to ascertain what is the mode of working of each particular organ, and what is the effect of any particular interference with that organ. Sometimes these experiments have resulted in curative methods. You could not have a better example than that which was put before the Commission, namely, the relation of the thyroid gland to myxœdema and cretinism, where, I think, to the surprise of everybody, it was discovered that we have glands within us which are producing protective substances for the normal life, just as Nature will, when there is an attack of a microbic disease, produce *pro hâc vice* a protective substance against the foreign invader. When you take away the thyroid gland, and there is not this protective substance, which is no longer formed, then you find the body gets out of order because that is no longer present which was necessary in order to keep down the inherent forces of dissolution. Then it was discovered that, if you supplied artificially that which this gland produced, you could keep down this form of disease just in the same way as you can stop the effects of diphtheria, if you put in from another animal that which nature would produce in order to repel the microbic attack. Just as in diphtheria you get recovery, so you find in myxœdema that when you supply this substance

which the body has ceased to have the power of producing, you get the remarkable discovery. And this appears to be only the commencement of a series of analogous successes. But, apart from the question of direct curative methods so arrived at, the power of diminishing suffering that is given through increased knowledge of the organism is so great that it is difficult to over-estimate it. I do not think it is necessary to give further instances to this Commission. I only want to put those forward to show that there is abundant justification for saying that knowledge which, from its very nature, must be obtained by experiment has already been of the very widest benefit to mankind. I will go on to the question whether experiment is permissible for the purpose of acquiring such knowledge. Here I must go back for one moment to the example which I gave, as a typical one, of preventing plague getting into a town by killing all the animals which could convey it. In my view, we are just as much bound to prevent suffering due to ignorance, and just as much justified in inflicting such pain as is necessary for the purpose of doing so, as we should be in killing noxious animals. I have pointed out that this necessary knowledge has in the past been and must be obtained by experiment. It must be done by experiment, it can only be done by experiment on men or by experiment on animals. 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. Nor do I think we have any right, voluntarily, to allow serious damage to occur, unless it perhaps may be where it is directly for the purpose of saving life. So I do not think, in view of mankind's power of anticipation, of memory, and the possibility of future suffering, we ought to take the

responsibility of allowing experiments which might produce serious consequences to be made on man. Let me give an example. 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. It is too serious a responsibility to take to allow that to be done, considering that that man will live for years afterwards, and cannot divest himself from social relations.

12736. Supposing he was sacrificing himself in that way, and by his sacrifice he could make probable the saving of a great many lives in future, why should you prevent him on the principle that you have to look forward to ultimate results of torture or sacrifice?— Well, it should be the last resource. But I will tell you what, in my opinion, I think constitutes the real difference. I do not think that you ought to lessen in any way the feeling of sanctity of human life, because the consequences are so very widespread. To let a man, in a moment of enthusiasm, do a thing which might, or perhaps must, have permanent evil effects throughout all his life, approaches, in my mind, very close to an infringement of the great principles which make us refuse to consider whether it would be a benefit that one man's life should be taken. Everyone of us knows men whom it would be a blessing to mankind to remove, whose lives are a curse to those around them, but, however much we feel that this is the case, we do not allow ourselves to act upon it, because if we were once to commence to act in that way towards men, the consequences that would follow would be so far-reaching that we have found it best to lay it down as a rule that it has been, except in special cases

prescribed by law, not permissible under any circumstances for an individual to take human life or to permit a man to die if he can prevent it; and you will find that doctors will not only work as hard for a bad man as they would for a good man, but they will actually bring a man back from the doors of death, although they know that he is going to be hanged for what he has done; because we find it is absolutely necessary to take as an unchallengeable principle that human life is sacred. Therefore I think that, although in small matters you may use experiment on man, and for the very last stage of experiment you probably must do so, you ought to stop there.

12737. (*Sir Mackenzie Chalmers.*) You mean with the consent of the man himself?—Yes, even with the consent of the man himself. Where you have established a well-founded scientific probability that the thing is safe, then I think you are justified, but before you attempt to experiment on man, you must have done everything to prepare for it, and you must know with fair scientific certainty what will happen. And that is the reason why I do not think that experiments on man are to be considered at all when you are thinking of the enormous amount of work that has to be done in scientific research. The necessary work must be done otherwise. Now, I turn to the question of whether we should use animals for the purpose of research. In my opinion, unquestionably, it is not only permitted to us, but it is our duty to try to remove ignorance by, if necessary, experiments on animals; and I want to give the Commission the principal reasons which make me have such a confident opinion on this point. The first reason is the overwhelming preponderance of the prevention of suffering

by this method. The great bulk of the necessary experiments, as you know, can be made without any pain to the animals at all, because the animal never need be conscious, and therefore no pain is caused at all. But taking the others, the amount of experiment, if properly done, which causes pain is incredibly small compared with the results of the knowledge obtained, even if they be measured by what has already been achieved. Just look at the complete change that there is on our knowledge of all those great groups of diseases which I have called infectious or communicable. We are no longer fighting them blindly; we are no longer striking in the dark; we know what they are. This very knowledge enables us to avail ourselves of experiments which are not painful for the purpose of assisting us in the task, and every man who feels his responsibility does all he can without occasioning pain. But what is done is done with a knowledge of the enemy we are fighting, so that the experiments grow more telling. The number of painful experiments, as is shown by looking at the actual experiments which have been made, is quite infinitesimal. I read the description of the experiments done under licence in England, where excellent research work is done, and most carefully done too; I read through the list of those which were thus done in a year, and I very much doubt whether the total amount of suffering caused by those experiments would be much greater than would be caused in a single fairly large shooting party, where there were one or two bad shots. If you consider the amount of suffering that is caused in the world, not only thoughtlessly, but even deliberately done by people who are ordinary normal men, and may be able to show a reasonable defence for what they do, it dwarfs so utterly the amount that is requisite to

produce this useful knowledge, that if the matter was not so serious it would be almost ludicrous to think that there was this organised opposition to the pain caused in scientific research in proper hands. Why, what is attacked here is the only bit of fruitful pain in the world. The greater part of pain had better not be. A man suffers and dies, or suffers and gets well, and all the pain he has suffered has benefited nobody. And in the case of animals there is all this vast mass of pain which is inflicted or permitted, and people tolerate it and say nothing about it, and look upon it as an ordinary thing; but there is one little bit which brings return in lessening the sufferings of the world, and the people are to be found to organise themselves against it, and throw the whole of their strength into denouncing and preventing it. In my opinion, if you look at the medical science of to-day and the medical science of 40 years ago, and realise that the advance has been caused by physiological research, which is largely carried on under the very Acts which restrict, which regulate, but which distinctly permit painful experiments, and then consider on the one hand how much pain that has caused and how completely it has changed our power of dealing with disease and of alleviating suffering, it is really incomprehensible that anybody who sees the totality of misery in the world should think that the small amount of pain that has produced that gigantic result is the first that ought to be stopped. That brings me to the next point, and that is this. Mankind not only claims, but perpetually exercises and properly exercises the right to cause or permit suffering in animals when needful or desirable. It requires considerable thought to realise the extent to which that is admitted by all, and is, indeed, unchallengeable. Take the case which I gave of the

rats on board the ship. I pointed out that it was right to kill those rats in order to prevent the possibility of their spreading the plague, but no such serious reason is necessary to justify killing them. I do not suppose there is anyone who would challenge the right of the captain to do it if they would injure the cargo, if they would produce a small pecuniary loss; under these circumstances it would be considered that it was perfectly right for him to do it. We unhesitatingly kill vermin—we kill things that do pecuniary harm, and no one dreams that it is a thing which a man of high principle will not do. So that nobody ever dreams of questioning the right of causing pain for the purpose of a useful result, when that useful result is merely pecuniary. But when instead of the result being pecuniary, it means diminishing the suffering of the world for all time and for all the world, forsooth, it is challenged. Take the case, for instance, of a herd of cattle attacked by some painful disease, from which, we will say, 50 per cent. of those attacked survive. A man would be considered perfectly justified in allowing them all to go through the disease. The 50 per cent. that would die would die, the rest would have the pain of the disease and get better; and he would say, "Well, but I could not afford to sacrifice the 50 per cent. that would survive—it would be too big a loss to me." For the £10 apiece that they were worth he would have permitted them to be killed on the spot; but he considers that he has a just and perfect right to allow them to suffer all that pain in order to save himself from the loss of the 50 per cent. that survive. And I do not say he is wrong, because the comfort of his family and other things of importance to him may depend upon the question of how great is the loss

inflicted on him pecuniarily by this attack. But if it is allowable to permit the whole of that suffering in order to avoid a pecuniary loss, the question as to whether it is right to increase by a little the suffering by well-designed scientific experiment to secure a return, not in money, but in the power of stopping suffering, seems to me literally to permit of only one answer. No man could consider the matter without realising that such infliction of pain was justified, if he would do what I hold he ought to do: add inflicted and preventable pain together. The moment you take as your guiding principle that you are responsible for pain which you could prevent, and that this and the pain which you inflict must be taken together in order to guide your action, it appears to me that there is no painful treatment of animals which is nearly so completely justified as that which leads to knowledge. Nobody, so far as I know, would hesitate to overdrive a horse in order to save a life or to bring assistance to a life that was in danger, and yet that would be suffering inflicted for the purpose of benefit to a single individual. The people that are engaged in scientific research are working for something which will benefit not one single individual, but will benefit the race for all time.

I feel myself that the justification for the necessary use of even painful experiments, where they are required for the purpose of obtaining knowledge, can only be questioned by shutting the eyes to the further consequences of our action—putting a veil between our action and what it ultimately leads to. And I cannot help thinking that the people who lead the movement on the other side must be aware of this, because they deal so persistently in misrepresentation. It is very easy to misrepresent the work of those who

are a long distance in advance from the ordinary man. I have known this illustrated again and again in manufacture. Processes have been stigmatised as adulteration, as falsification, and everything that was bad, whereas if people had followed out the advance of science they would have known that they were simply better methods for doing the same thing as earlier processes, and doing it with greater certainty and with greater cheapness. But they had not followed it out, and because the new processes were different, a contrast to the old ways, and people thought that therefore they were something to be stigmatised, so it is here. If we could get people to learn what science is and what it has done, they would never attack those who are at work on it. And it is to keep them from learning that there is this frightful misrepresentation. I remember, and I think the Chairman of the Commission probably remembers, how in the seventies the walls of London were placarded with a poster representing a rabbit in the process of being roasted alive.

12738. (*Chairman.*) I do not remember it, but we have been reminded of it; we have heard about the roasted rabbit.—I remember it well, because I wrote about it to the late Sir Michael Foster, and I asked him, What is the lethal temperature? because I recognised at once that the experiment which was thus misrepresented must have been one to find out when temperature alone would suffice to occasion death.

12739. You may take it for granted that we know all about it; we have had it explained by Sir Lauder Brunton.—Very well. It turned out that the temperature was something like 109 or 111 degrees.

12740. (*Sir Mackenzie Chalmers.*) 114 to be exact.—It differs very much, of course, in different animals.

12741. (*Chairman.*) That was the outside temperature, not the blood temperature.—I was giving the temperature of the blood. Anyone would realise, of course, that the object was to do it under such circumstances that no reaction should be set up—that there was simply an increase of temperature, and that therefore there must be no disturbance of the organism, and that the poster was absolutely false. Yet that placard was all over London. It is not surprising that a great number of people join this organisation, because they get their ideas from these very serious misrepresentations. They believe themselves to be humane, but when one realises the evil that ignorance does, and that the only way in which ignorance can be removed is by the experimental method, and the enormous advantages in the way of saving pain that these results have produced, one must feel that the truly humane men are the people who are defending scientific research.

Now I should like to turn to the practical conclusions to which I have come with regard to this subject. You see that the good basis of all these movements is the desire of saving pain in animals and men. Now if that is so, the whole motive ceases with the conscious life of the animal. As soon as it has finally passed out of the conscious life the whole *ratio agendi* ceases. It is as absurd to object to an experiment on an animal that has ceased to be conscious and will never again be conscious as it would be to object to the dissection of a dead animal. The only motive is the desire to save suffering, and suffering then is as impossible as if death had actually occurred. Now what is the consequence of that? Hence there ought to be no interference with experiments on animals which are in anæsthesia, except that they should be permitted only to persons who can and will take care that the anæsthesia is complete, and

that the animal is killed before it comes out. There is no other principle on which any interference could justifiably take place. There must be somebody in charge of or superintending the experiment who, from his character and position, can be trusted with the responsibility of seeing that there is what I may call permanent and effective anæsthesia. Therefore, with regard to such experiments, it is not only not necessary to interfere with their being used for educational purposes, but if the Commission may trust my experience they will indicate that in their opinion it is most important that they should be so used. For there is no comparison between the living effect of an experiment that you have seen and one that you have merely read of. May I give my own experience? I have many and many a time read over carefully-described processes, and have imagined that I understood them; but I knew of old what that meant, and used generally, in cases of any difficulty, to insist on what we call "having a view." I used to go down and insist on being taken, not necessarily by an educated person—I much preferred a workman—right through the whole process, and seeing it done before my eyes. I went back with a knowledge of the case, with a power of handling it, and with a power of drawing conclusions as to the processes, which were quite new-born. So it is with the man whom you are educating for this serious duty of fighting disease. If he has actually seen an experiment, it lives in his memory. But that is not the only thing—it remains in his memory as a living thing, and when he acquires new experience or new knowledge connected with it, he will find that it will make the old experiment mean more than it did to him even at the moment when he saw it; because he sees the whole thing before his mind's eye, and not only reads a description which, after

all, only states certain results. And, as there is no ground, therefore, from the side of humanity to interfere with these experiments which cause no pain, I think they should be largely used educationally. Medical education is of prime importance to the community. The greatest difficulty in the way of giving to the world the full benefit of all the accumulated knowledge of this last 40 years is that you have to educate your practical men up to the level of it. Just as in war it is of no use having medical appliances at the base if you cannot get them up to the front of the army, so it is of no use to have this accumulated knowledge unless the power of using it to prevent suffering is in the hands of those whom you train. Therefore the efficacy of our methods of tuition ought not to be looked upon as a matter of little importance—it is a matter of the very gravest importance; and I should put no restrictions upon the use of painless experiments in connection with it, excepting that the experiments must be under the supervision of someone possessing a certificate such as I have indicated, which should be a certificate granted to persons who could be trusted with regard to anæsthesia. And whether you have reports of those experiments or not I do not think is of the slightest importance, provided it is thoroughly understood that it is the duty of those possessing the certificates to see that the anæsthesia never ceases, and they are held responsible for so doing.

I turn now to the more difficult question, a question which requires more careful examination—namely, that of experiments that are in themselves painful. There, I think, you should divide the certificates into two classes. 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. But I am satisfied that, both for the satisfaction of those who desire to proceed cautiously and for the good of mankind, and also to prevent any possible abuse, they ought to report all that they have done. I do not think that a painful experiment should be allowed to pass into oblivion. It is made, in my opinion, for the benefit of the race and for the increase of knowledge; therefore I think such reports should be made, and I do not think that anybody concerned would really object to that being done.

12742. Do you mean that that report should be made by the inspector or by the operator?—By the operator.

12743. Sent into the Home Office?—Sent into the Home Office. I think he ought to do that. I would put him under no restriction as to the character of his experiments; I would not put him under any restriction as to the place in which they are done; but I would have a full report sent into the Home Office. Of course, a certificate of that kind would be given only in recognition of an acquired position and high worth in research. You can only do harm by interfering with men of that class. There is no danger of such men developing recklessness. I have not examined anything excepting the experiments that have

been done in England, and I am therefore speaking only with regard to them. I have never, in the years that I have followed the matter, found anything which from this point of view I call recklessness of suffering in any experiment made for research.

12744. Have you found levity? We have heard accusations of levity on the part of those who see experiments.—I feel very strongly that such an accusation is unfair even in the case of students. I do not find that the medical student of to-day is a man of the Bob Sawyer type. I think they work extremely hard. Those who see them in their classes can give you better information on this point, but certainly those I have come across are hard workers and are not guilty of any levity.

12745. (*Mr. Ram.*) Would you have any inspection of these experiments?—I would have no inspection of the first-class certificates.

12746. (*Chairman.*) You do not mean the first-class certificates described in the Act?—No; I mean my first class. Just let me give an example. Supposing it was a question of giving a general certificate to Sir Lauder Brunton. The idea of controlling what he should do with it would, in my opinion, be nothing but a blunder. But he would have to report all that he did; and if, by some accident, a mistake was made in any case, if a person developed levity, or even recklessness or cruelty, his reports would show it, and his certificate might be withdrawn by reason of it. He might then be relegated to the second class, which I am going to describe.

12747. (*Sir Mackenzie Chalmers.*) How do you suggest that a man should get into the first class? You

are imposing a new duty upon the Home Office.—I know it.

12748. (*Chairman.*) You begin by supposing that he is a first-class man?—Yes. I am quite prepared to say how I think the Home Office ought to decide. I think it ought to decide by the opinion of the leaders of the medical sciences, because I am satisfied that they possess, to an equal degree with any other body of men in the world, the moral qualities necessary to enable them to advise; and beyond question they possess, better than anybody else, the intellectual qualities and the qualities of knowledge which would enable them to do so efficiently; and a certificate of this kind should only be given where there was a consensus among the advisory body that the person held such a position in medical science that he might have this general certificate. I now pass to those who have not attained to that position—which, as I have said, would be an exceptional position, although I am happy to say that there are many men in England who would merit it. I think that in the case of younger men, or men who had not yet attained to the commanding position of those to whom I would give this general certificate, the lines of the present Act are the right lines. I think it is reasonable to expect them to accept specific limitation of the type of experiment, of the scope of the research, and of the place where the experiment is to be performed, in order that there might be the security of the supervision of those who are over the laboratory, if there be any such persons, and also the security of inspection. Limitation of this kind, which the people would be bound to respect, and I believe would respect, should prevent all danger of abuse, which, though I think it would rarely become a

reality, is more possible to occur than it would be in the case of the exceptional men to whom I have before referred. I see no reason why those restrictions should not be put on; and, as before, full and complete reports should always be made, so that a record of all these experiments and their results should be preserved. The line, then, that I should advise, would be as follows:—Certificates with regard to non-painful experiments—that is to say, experiments where the animal was kept under anæsthetics and not allowed to survive—should be quite general, but should only be issued to persons who would perform or supervise those experiments, and could be trusted to see that the anæsthesia was complete and permanent. For painful experiments there should be a small but select class who should have general certificates. The other class should have special certificates, specifying the nature of the research, the nature of the experiment, and the place where it was to be done; and there should be full and adequate inspection of those places while the experiments were going on.

12749. Strangers you would not have present, I understand?—I would not have strangers present, first of all, because, in my opinion, it would not assist the experiment, and, secondly, I do not think it would be a good thing for unqualified persons to see these things. They are the serious work of research, and persons who are not trained by medical or physiological study would gain no good from witnessing them. But I would encourage students being associated with this research. It is the finest training that they can have. So far as it can be done without interfering with research work, it is a mistake to keep people out who are capable of learning from it; but that the public should be admitted would,

I think, be a still greater mistake. In my opinion, they have no business there, and it is much better to exclude them wholly.

12750. (*Sir Mackenzie Chalmers.*) You mean that the public would go there in the same spirit that the public used to go to an execution?—Exactly. Now I hope that I shall be forgiven if I say one or two words about the two restrictions which have been recently suggested to the Home Office.

12751. (*Chairman.*) Yes, we should like to hear your suggestion about that.—I thoroughly disapprove of them. The first relates to foreign savants who are not naturalised Englishmen. There is no possible reason why they should be put at a disadvantage as compared with British subjects. Of course, for the first class of certificates under the system recommended, they must have a recommendation from English people of the proper position, to show that they deserved the privilege; but I am not talking about that for the moment. I am talking about the existing system. Foreign universities and foreign men of science have been extremely good to our students. They permit them to take part in their courses; they permit them to use their laboratories, and without good reason I would not show any want of reciprocal kindness towards foreigners. No reason is put forward for thus placing them at a disadvantage, if they possess the qualifications which would justify the certificate being granted to an Englishman, excepting that it is suggested that you cannot enforce the performance of the conditions. That is quite a mistake. The only difficulty in the way of enforcing the performance of the conditions is that they are capable of changing their residence; but an

Englishman who wants to break the conditions is equally capable of changing his residence. The true remedy in both those cases is to have a responsible head of the laboratory, who has to see that the conditions are duly performed, step by step as the work goes on, and, in my opinion, there would be no greater difficulty in assuring this being done in the case of a foreigner than there would be in the case of an Englishman.

12752. That would be so in the case of a foreign doctor working in an English laboratory; but supposing that he wanted a laboratory of his own, and hired a laboratory of his own, and started it for the work for a year in England, would you have a supervisor over him then?—I think that you might quite fairly require that there should be some person, resident in England, responsible for the observance of the conditions—that there should be somebody, such as the head of the laboratory, to whom Government can look to see that the conditions are performed, not in a lump, but *pari passu* with the work done. You are justified in recognising a difference in the case of foreigners to that extent, but no further.

12753. Do you not think that if he took a laboratory by the year, we will say, the country has got just as much hold upon him as upon any Englishman who took it?—Yes. There are a great many foreigners in England who should be trusted to the same extent as an English person who is well known in England. But if there is any question about it, you must have somebody in England responsible.

12754. (*Sir Mackenzie Chalmers.*) The difficulty is that they may be well known to scientific men in Eng-

land, but not known to the public?—Yes; but I think the action of the Home Office should be regulated by the knowledge of scientific men. These public reputations are apt to be misleading in both directions. You will find, if you take the public estimate of men, that the best lecturer is considered to be the greatest man in research. My experience is by no means so. There are men with a magnificent power of exposition, who are thereby so well known to the public that they are supposed to be the leaders of science, whereas in truth they are merely bearers of good news from the front, and are in no sense the leading workers. May I pass to the second restriction recently suggested to the Home Office?

12755. (*Chairman.*) If you please.—The second is that a medical diploma should be required. In my opinion, specialisation has gone so far that the distinction between people who actually work practically at the curative sciences, and those who devote their lives to research in those sciences, is becoming marked. A man who is actively employed in practice, as a first-rate man ought to be, has not usually the time for consecutive research; he has to get his time in fragments just when he can. On the other hand there are many men who are so devoted to research that they give up medical practice as a method of earning their livelihood, and devote themselves entirely to research. The wide diffusion of wealth in this country facilitates this. England has always been noted in science for the numbers of its first-class amateurs. Cavendish was an amateur, and I could mention many other great names of men who have had the wherewithal to devote themselves to research, and have made it their life's work without seeking any remuneration for their labour.

What should guide the Home Office entirely is, whether or not the applicants for certificates or licences are suitable for research. Whether or not they have taken out a medical diploma which gives them the nominal right to practise, when they have no intention to do so, but mean to devote their whole time to research, is a matter which should be quite immaterial. I am quite satisfied that there are now, and there will be yet more such in the future, men in the very first rank of research work, who do nothing in the way of the practice of medicine and deliberately abstain from it because it would interfere with their scientific research by taking their time and their attention. Those people ought to be encouraged rather than discouraged. Therefore this proposed restriction would, in my opinion, be a retrograde step. So that I should strongly advise that neither of those two things should be adopted as a principle.

12756. Have you studied the Act with a view to suggesting amendments?—You see, I agree with a great deal of it, excepting that I think there are a certain class of men who should have these unrestricted powers of experiment, and that the practice of the Home Office with regard to non-painful experiments should not have regard to the nature of the experiment, but simply to the reliability of the person on whom is placed the responsibility of the anæsthesia. With these modifications I do not quarrel with the Act, because I do not think that the real worker, if he is not really hampered in his research, chafes against these restrictions, which are, after all, only a recognition of what I might call the fundamental principle of his action. He is working that pain may be lessened. He is a person whom I have always found extremely sensitive

to the cry of suffering, and he has no wish to protest against this Act, which does homage to that principle, provided that it does not hamper him. I have not specially considered the provisions of the Act, because I thought it had been dealt with so much by people who knew its working, and are so much more qualified to deal with it than I am ; but I doubt whether my conclusions clash very much with the Act as it at present stands.

12757. (*Sir William Church.*) I should like to ask one question with regard to those to whom you would grant these first-class certificates. Of course, you consider that any person who is placed in the position of head of a laboratory connected with a University or a public institution approaching in importance to a University would necessarily be a person of sufficiently good repute ?—May I put it another way ? I should hope that a University would never choose a man for the position who did not deserve it.

12758. Then, besides persons in charge of laboratories, you would grant these what you call first-class certificates to such men of repute ; and in their case I understood you to say they should not be necessarily restricted to working in licensed laboratories ?—No ; I should leave them perfectly free, subject to report.

12759. So that they might work either in their own rooms or wherever was most convenient for their work to be done ?—Yes, certainly.

12760. Then, with regard to all others who hold licences, I presume that you look forward to their all working in licensed laboratories ?—Certainly. Of course, I should trust that the best of those who commenced with a licence would pass in time to the higher degree ;

but, as long as they only had a licence, they would work in proper laboratories subject to inspection.

12761. So that, even as you propose, it would not be possible for a research student, whether holding a medical diploma or not, to perform experiments on living animals even in a licensed laboratory, excepting if he held a licence himself?—No; not painful experiments. He could not perform painful experiments.

12762. And, of course, all those who held what you call first-class licences would be enabled to perform the experiments which are now performed under Certificate B without having to get anything further than their licence?—Yes, certainly.

12763. (*Sir Mackenzie Chalmers.*) I should like to take first the two points that you have raised about the Home Office practice. I suppose you agree that, apart from the wording of the Act, the Home Secretary is bound to have regard to public opinion to some extent—that there is a real danger of the powers being lost if suddenly public excitement were got up against them?—He is bound to have regard, I should say, to the Act. I think that he has no need to regard public feeling beyond the extent to which it is expressed in the Act.

12764. You think that his sole duty is to administer the Act, without regard to whether he is imperilling the existence of the Act or not?—I think that the safest way to maintain the Act is to administer it in the way in which it is best for research. The real defence of research will come from the good that it has done; and I think it would be an unwise thing to administer the Act in a niggardly way, in the hope that it would lessen the possibility of a popular storm. I do not

myself fear very much of the popular storm. I know perfectly well the power of a certain small number of determined people to prejudice the minds of the public, and I appreciate that it is considerable ; but the more that the public has experience of the good results of research, the safer it is in the hands of the public.

12765. Of course, as regards the foreigner himself, the man who is temporarily over here, he is to some extent free from the law. He performs certain experiments, and the result of those experiments are not published, probably, until he has returned ; and then the Home Secretary may be blamed for allowing the law to be broken by a man over whom he had not effective control. You recognise that difficulty, do you not ?—He ought to have effective control, and I think that this would be best obtained by requiring that the reports should be sent in from time to time, and that there should be some responsible person to see this is done.

12766. Would you go so far as this, that supposing a foreigner were authorised to experiment, say, at University College, you would make the head of the laboratory there surety for him ?—Yes ; I would make him see that the thing is done. And if the man is not performing his duty, it is the duty of the head of the laboratory to report to the Home Office that the experiments are being carried out without due observation of the conditions.

12767. But what practical power of enforcing it have we ? Would you withdraw the licence of the head of the laboratory if there were any contravention of the conditions by a foreigner working in his laboratory ?—Not unless he connived at it. But, in my opinion, the report of an experiment should be written out

as nearly as possible at the time ; and if he neglects to see that this is done, it appears to me that he is responsible for the breach.

12768. Now, coming to medical diplomas, is there not this difficulty : that when once you get away from the medical diploma, you have no guarantee whatever as to a man ? If he belongs to the medical profession, he has been trained up in a profession whose sole object is, as you say, to diminish pain. When once you get outside that, where are you to get your information from as to the qualifications, moral and scientific, of a person who is to have this licence ?—Would you say that we had no knowledge of the qualifications of Frank Balfour, before he was killed so unfortunately, years ago ? You will remember that he started a biological school at Cambridge which was full of the greatest promise ? Do you think that the addition of a medical diploma would have made Frank Balfour a more reliable man ?

12769. Personally I do not. But then, you must take the case of a good many persons who are not known to the public, and you must remember that public opinion is nervous on the question.—I should require the most exceptional testimony in the case of a man who had not done scientific study at a university and under proper training ; but that it should have taken the form of medical study seems to me unnecessary. You must understand that the scientific degrees, though they are often taken by medical men, and ought to be taken by them, are in themselves independent. They may not qualify for a diploma, but they connote an amount of knowledge which would quite warrant your recognising the bearers as fit men, if they were duly recommended by those who advise

the Home Office. Of course, you will occasionally meet with a man who without any scientific study in a recognised university is a man of pre-eminent value. Michael Faraday had had, I think, no university training at all. But those cases are extraordinarily rare, and would only be dealt with when vouched for in a way which was quite unexceptionable. So that, generally, you would have the security of university recognition that the applicant was properly qualified.

12770. Do you think that the existing certifying authorities, which are mainly medical authorities, under the Act, would be able to deal with the case of the people you refer to without medical qualification?—I think so.

12771. Or ought the advising body to be enlarged?—Those who examine up at the Universities usually possess medical degrees, and a great many of them possess also science degrees. I do not know whether Sir Michael Foster had a medical degree.

12772. (*Dr. Gaskell.*) Yes, he had, and practised.—I did not know that. But you can easily imagine that there may be some who have not.

12773. (*Dr. Wilson.*) Pasteur had not; he was a chemist.—No, Pasteur had not; that is a good example.

12774. And Metchnikoff was a chemist?—Yes.

12775. (*Sir Mackenzie Chalmers.*) Then I should like to ask your opinion upon this. It has been suggested to us by Mr. Coleridge, whose evidence, perhaps, you have not seen?—I have not.

12776. It has been suggested that a certificate of humaneness should be required before a licence was given. What is your opinion on that? He suggested,

I think, that a certificate should be given by one Justice of the Peace and one minister of religion. Perhaps you would rather not express an opinion?—It is not enough to say that it is unnecessary. It would be an absolute insult to the people whom you would be consulting. The suggestion that the heads of the medical profession are not judges of humanity, of humaneness, I ought to say, is a piece of the most intolerable insolence. It is shocking when you consider the way in which, as a rule, medical men disregard their own comfort, and put themselves to any amount of trouble and discomfort for the purpose of helping people who are sick, very often when it does not bring to them the slightest *kudos* or the slightest pecuniary return. To suggest that such people do not know what humaneness is and are not moved by suffering is intolerable.

12777. I think the suggestion rather was, from the evidence that we have had, that the present certifying authorities regard only scientific ends and qualifications and do not consider as within their province the question of humanity.—So far as Englishmen are concerned (of whom I can alone speak from personal experience) the question as to whether they are likely to use power properly from the point of view of regard for suffering is from my knowledge of the people who are likely to take out licences practically settled before the application is made. But the certifying authorities have to advise that licences should be granted to the applicants, and if they think that *par exception* they are people who have not got any care for suffering, and will make a recklessly painful use of their powers, they are bound to say that they are not fit persons to receive licences.

12778. Do you think that ought to be expressed in the Act or the rules in any way?—I should say “fit and proper.”

12779. You think that would cover it?—I think that would cover it.

12780. Now, coming to the ethical side of the question; may I rightly sum up your statement in this way—I want to know if I have understood it—that all pain is an evil, perhaps the supreme evil?—Yes.

12781. And that we are justified in the present infliction of a lesser evil when there is a reasonable prospect in the future of avoiding a very much greater evil?—Yes, quite so.

12782. That would be the test and the touchstone which we should apply to all animal experiments?—Yes, that is the touchstone. And in applying that touchstone we must conscientiously use the whole of the teaching that the history of science and the history of medicine have given us. The particulars I have given were in order to show that where there is serious research there is an overwhelming probability that the answer will be in the affirmative.

12783. Of course, as you know, by other ethical witnesses, we have had other tests suggested. For instance, one of the witnesses suggested to us that the test would be the conscience of a progressive people. I must say it did not convey very much information to my mind.—It conveys none to my mind. I feel satisfied that that is an extremely bad definition. “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." The only thing that can safely guide you is your reason. Your emotions may give you a motive just as I say that the motive which should dominate the whole of this question is the desire for the suppression of pain. When you have got that motive and desire to obey it, it is your reasoning power which ought to tell what you ought to do.

12784. What is pressed upon us continually is this: Admitted that pain is an evil, you have no right to inflict a definite voluntary amount of pain when the future saving of pain is absolutely problematical. What do you say to that argument?—It is simply because the people have not studied the question that they talk about its being absolutely problematical. What you have to do in life is to act on probabilities. It is quite impossible to prophesy exactly what will happen in the future, but when you see from the teaching which the past has given us that there is an overwhelming probability in favour of a thing happening you are bound to act upon that probability. Would a person refuse to take a medicine because it had not always cured people? It would be a doctor's duty to give it if there was a probability that it would do so.

12785. Then passing from what I call first principles to the secondary principles, if I have understood your evidence, you say that at any rate we have no right to prevent experiment for the purpose of increasing knowledge and controlling pain and disease while we allow the infliction of death and pain in sport for the purpose of providing food for mankind, and for a mere commercial purpose, such as spaying sows and

castrating horses?—Yes, and in infinitely more cases than those—cases in which, as I have said, you inflict pain for the purpose of preventing pecuniary loss. If you have a valuable dog you see it through the distemper. If it is not valuable you kill it to begin with. Yet you might be perfectly willing to sell that dog in any case. That is simply permitting suffering for the purpose of avoiding pecuniary loss. Otherwise, I really do not understand why people consider it right to kill beetles. Beetles will only destroy a certain amount of their food, and yet they extirpate them, just as they extirpate mice and they extirpate rats, and consider that they are justified in doing so, because they say that such animals do harm. That harm is really pecuniary harm.

12786. Perhaps the fairest way of putting some of these ethical points that have been put before us is to call your attention to a little publication I have just received from Mr. John Page Hopps, who has given evidence here. May I ask you to look at the second paragraph on page 3, beginning with the word “But”? May I divide it into paragraphs and ask you whether you have any comments to make upon it? May I read the first words that I want to ask you about: “But when all is said on the score of results and the artist in vivisection has done his best to convince us that he is the apostle of mercy, many grave considerations ‘give us pause.’ For instance, what law of God or Nature justifies this treatment of our poor relations? Who gave to this amazing enthusiast the right to say off-hand that he was at liberty to exploit ‘the lower animals’ for experiments?”—May I point out what it is that gives the whole force of that to the people he is talking to? It is the use of the word “experiments.” They do not know what it means: they do

not know its importance. Supposing I was to put, instead of "experiments," "saving life or stopping suffering," the whole appeal would fall to the ground. "Experiment" to an uneducated person does not connote what it does to persons who are acquainted with the nature and results of research. If you read it in that way: "Who gave to this amazing enthusiast the right to say off-hand that he was at liberty to exploit 'the lower animals' for saving human life?" the answer would be "Why not?"

12787. As regards that expression of his, "What law of God or Nature justifies this treatment of our poor relations?" what is your comment?—It is all based on this want of appreciation of the meaning and effect of scientific research. Supposing I was to put it, for instance, "What law of God or Nature justifies our killing things, or permitting them to suffer, in order to save us from pecuniary loss?" Yet we do this, and must do it, every day. A beetle is a poor relation, or a mouse is a poor relation, just in the same way as a guinea-pig is; and yet we treat our poor relations in the shape of mice and beetles with phosphorus, and let them die in pain, because we say we must keep them down.

12788. If his argument was right, as a logical conclusion we should have no right to keep a cat who destroys mice by a cruel and lingering death?—Certainly not. We should first kill all the birds of prey, and then kill the birds they prey on, because they prey on insects; and then we should kill the greater part of those insects because they live, many of them, on still smaller things. You get back to the state of the Hindoo. Nobody ever dreams of doing that. It is only when it is applied to scientific research, the ad-

vantage of which they do not know, that these qualms come in.

12789. They do not believe in scientific research and its results. They honestly do not believe in it?—They do not believe in it; but that is entirely due to their not having studied and mastered the subject; otherwise language such as that would not have any effect.

12790. And Nature herself nearly always provides a cruel and lingering death for every wild animal?—Yes, it is terrible. I think it is Seton Thompson who says: “The end of every wild animal’s life is a tragedy.” And it is perfectly true.

12791. Now may I take you to the next sentence: “For laboratory purposes, his spectacles would not be safe in the streets; and if he put his proposal into effect, and got his babies on the dissecting table, the very costermongers would raid his laboratory, and even medical students might help them. Why? The cutting-up of one baby might save the lives of thousands of other babies; and the torture of one for an hour might save thousands from suffering for years. Why object?”—I object because you ought to get that knowledge in other ways. Everybody who has studied the subject realises that the knowledge can be obtained in other ways. The analogy between living creatures is so marvellously close that you can, without any experiments on mankind (excepting that you ultimately apply to them the methods which you have worked out scientifically), acquire all the knowledge that is necessary.

12792. It has been suggested to us that if we encourage experiments on living animals, it *per se* will tend to encourage experiments on human beings. What do you say to that argument?—It is the alternative of it. I remember a discussion in society on the

subject of these experiments, and a lady at last put this question to the anti-vivisector: "Would you prefer an experiment to be tried on a cat or a baby?" And the person replied, "I would have no experiment tried at all," not seeing that when you have not got knowledge, when you are working blindly, all you do is one long series of experiments on mankind. You are just as really trying experiments; you are not trying experiments under such circumstances that they enlighten you, and the consequence is that the result of what you do is to add very little to our knowledge.

12793. You mean that as long as you are treating disease empirically in mankind, you are experimenting?—Purely experimenting. You are either just sticking to ignorance, or, if you are trying anything new, you are experimenting on mankind.

12794. Pressing Mr. Hopps' argument to its logical conclusion, it would come to this, I suppose: If experiments on animals lead to experiments on babies, the eating of animals for food would lead to eating human beings for food?—You are honouring his argument by attempting to follow it out. That is what it would naturally lead to. But, in the case of experiments, it is because we will not try the experiments on babies that we do them on animals.

12795. Would you kindly look at the last paragraph on page 35, and the first paragraph on page 36? I think you have really dealt with that question. Mr. Page Hopps says, talking of voluntarily incurred experiments: "There is a splendid opportunity here for the suicide. Instead of a coward in retreat, let him be a hero as an offering. 'The world,' he says, 'is full of misery—sordid, diseased and despairing. I will get out of it.' He is full of pity for mankind, and is disgusted at its lot; so he decides to give it no help, but

to go. What a lame and impotent conclusion! What an opportunity for heroism lost!" Does that strike you as somewhat unpractical?—I have dealt with that. It is not only impossible, but to my mind it is immoral in the highest degree, and it shows an absolutely crass ignorance of the nature of scientific research, to think that by a few suicides you could replace the experiments on animals which, in a complicated matter like this, must be numerous, but need rarely be painful, and which, if they are painful, can, in the great majority of cases, be stopped by inflicting death.

12796. I suppose another difficulty would be that you would not get any scientific man to experiment on the would-be suicide?—If he did, he would probably be saved the trouble of suicide himself.

12797. Then Mr. Page Hopps, at page 36, makes another suggestion as regards minor experiments, as he calls them: "Or might it not be possible to connect some minor forms of vivisection with crime? Here is a criminal who has earned his fourteen years of penal servitude. Let him commute it for six months in the hands of a reliable vivisectionist, within limits. And so throughout the whole scale of penalties?"—I think that is loathsome levity; that is all I can say.

12798. Then, leaving that and coming to what you have said yourself, you thought that a man is not justified in experimenting on himself when there is any prospect of real danger?—Yes, I think so, unless he is obliged to do it—and I think he rarely is.

12799. Take a well-known instance. Haffkin invented a prophylactic serum, or injection rather, for plague, as you know?—Yes.

12800. The first person he tried it on was himself. Was he justified or not?—Certainly he was justified if

he had scientifically worked it out till he had a well-grounded right to expect that it would not lead to evil consequences. At that stage a man may justifiably, and to the great benefit of the human race, experiment upon himself.

12801. Either on himself or any other person who thoroughly knows what he is doing and consents?—Yes.

12802. (*Chairman.*) It is very like Columbus taking out his ship to discover America?—Yes, except that Columbus ought to have made a few more observations before he did it.

12803. He had good reason for believing that there was land out there?—No doubt.

12804. (*Sir Mackenzie Chalmers.*) Would you justify the course of experiments of Dr. Leonard Hill and his assistant experimenting on themselves in the way of testing how many atmospheres they could subject themselves to?—Yes. That is a most interesting instance. He had worked at the subject until he felt clear that he had got, if I might use the phrase, the rule of danger, and having worked it out he had the confidence to submit himself to it, and in that way he enormously assisted the acceptance of his methods. I think he was right. But he would have been perfectly wrong to go trying one method after another upon himself before he had worked his method out to that high degree of scientific probability.

12805. I think the only difficulty that I feel is this: where do you draw the distinction between the man who, for a scientific purpose, submits himself to a dangerous experiment, and a doctor who goes into a hospital reeking with typhus at the imminent risk of his life to carry on the ordinary work of his profes-

sion?—I said that I made an exception when it was necessary to save human life. You see, if a doctor knows that if he does not do that, many lives in that hospital will certainly be lost, I think then that he is perfectly right to say, Well, I will risk my life in order to save those lives. But in the other case, I think that he is a bad scientific man if he does not find a way of working it out, without exposing his life, up to a point when he is justified in doing so. That is my feeling. In giving this evidence, I have in my mind all the time what science is. Science is not the bungling, haphazard sort of thing that the world outside believes it to be. It is organised common sense enlightened by appeals to actual fact, which are framed for the express purpose of giving you the information which is necessary for your common sense to act upon. And when you realise this, you feel that the man who is content to follow scientific methods is not driven to these heroic steps; he can, with the minimum of suffering to animals and without endangering human life, work out the problems which are before him. If he is impatient he would like, perhaps, to experiment upon himself or on men, but if he is patient he need not do so. And I want the scientific man to be patient in that way.

12806. There is only one other question that I want to ask you. You urge the importance of painless experiments before students?—Yes.

12807. On the old principle *Segnius irritant animos demissa per aures*?—Yes.

12808. But what we have been told is that the performance of these experiments tends to demoralise students, tends to create a morbid curiosity as to pain in their minds. What is your opinion as to that?—My opinion is that that fear is entirely bred in the

minds of the people who express it. I do not believe there is the slightest justification for it. We compel students to be present at operations on living men and living women because we feel that we must do so, and no one would listen to the suggestion that they ought not to be present there. How in the world can it have a bad effect when they see operations on animals if it does not when they see them performed on man? I do not believe that there is the slightest justification for the fear, and, as a rule, I do not like or attach any importance to suspicions as to impurity in other people without adequate evidence.

12809. (*Mr. Tomkinson.*) All your evidence, I think, is pretty well based upon the assumption that practically perfect anæsthesia can be and is maintained in these physiological experiments?—Yes.

12810. And you have no qualms about it as to the possibility of perfect and continued anæsthesia in operations?—I have no doubt whatever about it. Some one expressed it to you in a way which I think exactly represents the truth, viz., that consciousness is the first thing to go and the last thing to come back.

12811. (*Dr. Gaskell.*) Consciousness of pain?—Yes; consciousness of pain, I mean, is the first thing to go and the last thing to come back. We have the experience of people who have had the most serious and long-continued operations performed upon them, and have come back to consciousness, and their universal testimony is to absolute unconsciousness of pain.

12812. (*Mr. Tomkinson.*) Then that being the case, and anæsthetics being an established fact, there is no reason for me to ask you how far you would press your theory of the justifiableness of inflicting pain, even intense pain, upon individual creatures in order to prevent a much larger amount of suffering spread over a

vast number of individuals; in other words, the infliction of vicarious sacrifices?—You ought only to do that where it is necessary for you to get the knowledge which will enable you to prevent pain in future, just in the same way as in my simile of the plague-stricken ship. I think the man ought to take the most painless method that is applicable to kill those rats, but he must kill them even though it should be painful.

12813. But then that is to avoid and prevent a gigantic danger?—Yes.

12814. That would hardly be on all fours with a very painful experiment—an awfully painful experiment, say—upon an animal for a possibility of discovery?—That is just the point. There have been many painful experiments in discovering the true nature of disease which, as I say, makes us think so differently about all these microbic diseases that we cannot put ourselves back into the ignorance of fifty years ago; but they have been for the purpose of ascertaining the nature of these diseases in order that we might fight them. The number of very painful experiments is very few—extremely few. The greater number of the experiments have been inoculations, and as soon as the disease has developed itself the animal is killed, because what the experimenters have wanted to find out was aye or no, would the disease be taken under such circumstances. The number of cases in which you have to allow really painful experiments is confined to these—that is, the cases where you are examining the distant effects of certain lesions of the nerves, of interference with the digestive organs, in order that when you see symptoms which are so produced you may know where the evil is. They are never, I think, performed casually; the very painful

ones would almost always, I should think, in the history of English research in physiology, be performed for some very definite object, in order to give us this very essential knowledge.

12815. And as a matter of fact, with regard to the destruction of dangerous or destructive animals, the common way of trapping them is about as cruel as can be adopted?—Quite so.

12816. And yet it is recognised?—Quite so.

12817. You probably would agree with me that some more stringent law in the direction of prevention of cruelty to animals would be very desirable in those regards?—Yes, I agree. That is just what I feel. I feel that this infliction of pain for the purpose of knowledge is the last thing you ought to touch. I should be very strongly in favour of legislation for lessening pain, if such a thing were possible; but I am happy to say that the need grows less. I think that there is a growing of increased sensitiveness towards suffering which would help legislation of the kind that you speak of, and which is even rendering such legislation unnecessary. I see it everywhere.

12818. Another instance: that entirely misleading statement of baking rabbits prompts the question of the boiling of lobsters?—Yes. There may be much that one might advantageously inquire into there.

The Society was formed on 27th January of the present year, and already numbers more than 800 members.* It is not an association of men of science or of medical men alone; its membership has been drawn from all departments of public life, and includes representatives of every class of educated Englishmen and Englishwomen, including many who have taken an active part in the prevention of cruelty to animals. This fact is in itself a remarkable protest against the attacks which have been made on the researches that the Society has been formed to defend.

The annual subscription is five shillings to cover working expenses: but larger subscriptions, or donations, will be gladly received. The acting Hon. Treasurer, *pro tem.*, is Mr. J. Luard Pattisson, C.B. (of the Lister Institute)† and an account in the Society's name has been opened with Messrs. Coutts & Co., 440, Strand. The Hon. Secretary is Mr. Stephen Paget, 70, Harley Street, W., to whom all communications should be addressed.

Yours faithfully,

CROMER

(*President*).

* June 1st. The number of members is now more than 1,200, of whom more than 100 are ladies.

† June 1st. Dr. Sandwith has succeeded Mr. Pattisson as Hon. Treasurer.

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