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FINAL REPORT

VIVISECTION

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ROYAL COMMISSION ON VIVISECTION.

FINAL REPORT

OF THE

ROYAL COMMISSION

ON

VIVISECTION

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

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Royal Commissions.

Whitehall, September 17, 1906.

The KING has been pleased to issue a Commission under His Majesty's Royal Sign Manual to the following effect :—

EDWARD, R. & I.

Edward the Seventh, by the Grace of God, of the United Kingdom of Great Britain and Ireland and of the British Dominions beyond the Seas King, Defender of the Faith, to—

Our right trusty and well-beloved Cousin and Councillor William Court, Viscount Selby ;

Our right trusty and well-beloved Councillor Amelius Mark Lockwood, Commander of Our Royal Victorian Order, Honorary Colonel of the 4th Battalion of the Essex Regiment ; and

Our trusty and well-beloved :—

Sir William Selby Church, Baronet, Knight Commander of Our Most Honourable Order of the Bath, Doctor of Medicine ;

Sir William Job Collins, Knight, Doctor of Medicine ;

Sir John McFadyean, Knight ;

Mackenzie Dalzell Chalmers, Esquire, Companion of Our Most Honourable Order of the Bath, Companion of Our Most Exalted Order of the Star of India, one of the Under Secretaries of State to Our Principal Secretary of State for the Home Department ;

Abel John Ram, Esquire, one of Our Counsel learned in the Law ;

Walter Holbrook Gaskell, Esquire, Doctor of Medicine ;

James Tomkinson, Esquire ; and

George Wilson, Esquire, Doctor of Medicine ; Greeting !

Whereas We have deemed it expedient that a Commission should forthwith issue to inquire into and report upon the practice of subjecting live animals to experiments, whether by vivisection or otherwise ; and also to inquire into the law relating to that practice, and its administration ; and to report whether any, and if so what, changes are desirable :

Now know ye, that We, reposing great trust and confidence in your knowledge and ability, have authorized and appointed, and do by these Presents authorize and appoint you, the said William Court, Viscount Selby (Chairman); Amelius Mark Lockwood; Sir William Selby Church; Sir William Job Collins; Sir John McFadyean; Mackenzie Dalzell Chalmers; Abel John Ram; Walter Holbrook Gaskell; James Tomkinson; and George Wilson to be Our Commissioners for the purposes of the said inquiry.

And for the better effecting the purposes of this Our Commission, We do by these Presents give and grant unto you, or any five or more of you, full power to call before you such persons as you shall judge likely to afford you any information upon the subject of this Our Commission; and also to call for, have access to and examine all such books, documents, registers and records as may afford you the fullest information on the subject, and to inquire of and concerning the premises by all other lawful ways and means whatsoever.

And We do by these Presents authorize and empower you, or any five or more of you, to visit and personally inspect such places as you may deem it expedient so to inspect for the more effectual carrying out of the purposes aforesaid.

And We do by these Presents will and ordain that this, Our Commission, shall continue in full force and virtue, and that you, Our said Commissioners, or any five or more of you, may from time to time proceed in the execution thereof, and of every matter and thing therein contained, although the same be not continued from time to time by adjournment.

And We do further ordain that you, or any five or more of you, have liberty to report your proceedings under this Our Commission from time to time, if you shall judge it expedient so to do.

And Our further will and pleasure is that you do, with as little delay as possible, report to Us under your hands and seals, or under the hands and seals of any five or more of you, your opinion upon the matters herein submitted for your consideration.

And for the purpose of aiding you in your inquiries We hereby appoint Our trusty and well-beloved Charles Clive Bigham, Esquire, Companion of our Most Distinguished Order of Saint Michael and Saint George, Captain in Our Army, to be Secretary to this Our Commission.

Given at Our Court at *Saint James's*, the seventeenth day of *September*, one thousand nine hundred and six, in the sixth year of Our Reign.

By His Majesty's Command.

H. J. GLADSTONE.

Note.—His Majesty was subsequently pleased to create Mr. Chalmers a Knight Commander of the Most Honourable Order of the Bath and to cause the late Mr. Tomkinson to be sworn a Member of His Majesty's Most Honourable Privy Council.

Whitehall, December 2, 1909.

The KING has been pleased to issue a Commission under His Majesty's Royal Sign Manual to the following effect :—

EDWARD, R. & I.

Edward the Seventh, by the Grace of God, of the United Kingdom of Great Britain and Ireland and of the British Dominions beyond the Seas King, Defender of the Faith, To Our Trusty and Well-beloved Abel John Ram, Esquire, one of Our Counsel learned in the Law, Greeting!

Whereas by Warrant under Our Royal Sign Manual bearing date the Seventeenth day of September, one thousand nine hundred and six, We were pleased to appoint Commissioners—you the said Abel John Ram being one of the said Commissioners—to inquire into and report upon the practice of subjecting live animals to experiments, whether by vivisection or otherwise; and also to inquire into the law relating to that practice, and its administration; and to report whether any, and if so what, changes are desirable:

And Whereas the Chairman of the Commission is at this present void by the death of Our Right Trusty and Well-beloved Cousin and Councillor William Court, Viscount Selby:

Now know ye that We reposing great trust and confidence in your knowledge and ability have authorized and appointed and do by these Presents authorize and appoint you the said Abel John Ram to be Chairman of the said Commission in the room of the said William Court, Viscount Selby, deceased.

Given at Our Court at Sandringham the Twenty-seventh day of *November*, one thousand nine hundred and nine, in the Ninth Year of our Reign.

By His Majesty's Command,

H. J. GLADSTONE.

Whitehall, May 30, 1910.

The KING has been pleased to issue a Warrant under His Majesty's Royal Sign Manual to the following effect :—

GEORGE, R. & I.

George the Fifth, by the Grace of God, of the United Kingdom of Great Britain and Ireland and of the British Dominions beyond the Seas King, Defender of the Faith, to all to whom these Presents shall come, Greeting!

Whereas it pleased His late Majesty from time to time to issue Royal Commissions of Enquiry for various purposes therein specified :

And Whereas, in the case of certain of these Commissions, namely, those known as :—

The Historical Manuscripts Commission,
The Horse Breeding Commission,
The Sewage Disposal Commission,
The Poor Laws Commission,
The Tuberculosis Commission,
The Canal Communication Commission,
The Mines Commission,
The Welsh Church Commission,
The Coast Erosion and Afforestation Commission,
The Vivisection Commission,
The Land Transfer Acts Commission,
The Ancient Monuments (Wales and Monmouthshire) Commission,
The Ancient Monuments (England) Commission,
The Trade Relations between Canada and the West Indies Commission,
The Selection of Justices of the Peace Commission,
The Divorce and Matrimonial Causes Commission,
The University Education in London Commission, and
The Brussels, Rome, and Turin Exhibitions Commission,

the Commissioners appointed by His late Majesty, or such of them as were then acting as Commissioners, were at the late Demise of the Crown still engaged upon the business entrusted to them :

And Whereas We deem it expedient that the said Commissioners should continue their labours in connection with the said enquiries notwithstanding the late Demise of the Crown :

Now know ye that We, reposing great trust and confidence in the zeal, discretion and ability of the present members of each of the said Commissions, do by these Presents authorize them to continue their labours, and do hereby in every essential particular ratify and confirm the terms of the said several Commissions.

And We do further ordain that the said Commissioners do report to Us under their hands and seals, or under the hands and seals of such of their number as may be specified in the said Commissions respectively, their opinion upon the matters presented for their consideration ; and that any proceedings which they or any of them may have taken under and in pursuance of the said Commissions since the late Demise of the Crown and before the issue of these Presents shall be deemed and adjudged to have been taken under and in virtue of this Our Commission.

Given at Our Court at *Saint James's*, the Twenty-sixth day of *May*, one thousand nine hundred and ten, in the first year of Our Reign.

By His Majesty's Command,
R. B. HALDANE.

ROYAL COMMISSION ON VIVISECTION.

FINAL REPORT.

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ROYAL COMMISSION ON VIVISECTION.

REPORT.

TO THE KING'S MOST EXCELLENT MAJESTY.

MAY IT PLEASE YOUR MAJESTY,

1. We now humbly submit to Your Majesty our final Report on the matters into which we were directed to enquire. The evidence which we have taken has already been presented to His late Majesty, and to Parliament, in our previous Reports.

2. The Terms of Reference to us were "to enquire into and report upon the practice of subjecting live animals to experiments, whether by vivisection or otherwise, and also to enquire into the law relating to that practice, and its administration: and to report whether any, and if so what, changes are desirable."

3. In pursuance of these instructions we have held more than seventy meetings at which a large number of witnesses have been examined. We have also received, from various official, medical, scientific and other sources, numerous papers bearing on the questions before us, some of which have been printed and annexed to our previous Reports.

4. The evidence which we have had before us has, we believe, informed us of every class of opinion interested in the terms of our Reference. It includes that of representatives of the Government Departments which either administer the existing law or carry on researches under its provisions, of the principal scientific, physiological and medical bodies, and of the leading Universities in the United Kingdom, as well as of numerous private practitioners, medical and veterinary, and of representatives of the various anti-vivisection and humanitarian societies which have in recent years occupied themselves with the protection of the interests of domestic and other animals.

5. It has appeared to us convenient to divide the consideration of our Report in the following manner:—

After a brief account of the legislation and enquiries bearing upon the question of experiments on animals previous to the Act of 1876, we set forth with some particularity the provisions of the Act of 1876 and of the Home Office practice under the powers conferred by the Act for the purpose of carrying it out; we then proceed to examine the evidence as to the administration of the Act by the Home Office and deal with the specific charges made against that Department, and generally with the evidence as to alleged breaches of the Act.

But we have been further directed to report generally on the practice of experiments on animals, and we deal with this part of the subject under the following heads:—

(a) Recent history of the progress of medical science in connection with such experiments.

(b) Whether experiments on animals give valuable results in relation to the prevention and cure of disease and generally in physiological knowledge.

(c) How far immunity from pain in experiments is or can be secured.

(d) Whether, and how far, the objection taken by some that such experiments are morally wrong and unjustifiable can be sustained.

Lastly, we discuss some suggestions made to us and submit the recommendations which arise out of our consideration of the above matters.

352. 2,500.—Wt. T.L. 306. 3/12. Wy. & S.

THE EXISTING LAW.

History of Legislation.

6. The general law relating to cruelty to animals was, until this year, contained in Sections 2 and 18 of the Cruelty to Animals Act, 1849 (12 & 13 Vict., c. 92). These enactments authorised justices to fine up to £5, or to imprison for a term not exceeding three months, any person who "shall cruelly beat, ill-treat, over-drive, abuse or torture, or cause or procure to be cruelly beaten, ill-treated, over-driven, abused or tortured, any animal." The Act was originally confined to domestic animals (Section 29), but subsequent legislation enlarged the definition of "animals," and extended the protection of the Act to all animals in a state of captivity (17 & 18 Vict., c. 60, Section 3, and 63 & 64 Vict., c. 33). These enactments are now embodied in and somewhat extended by the "Protection of Animals Act, 1911." The construction of these enactments has sometimes given rise to difficulty, but the uniform tendency of the decisions is to confine their application to the unjustifiable or unnecessary infliction of pain.

7. In the early seventies of the last century a great impetus was given to the study of physiology, and the experimental sciences generally. Physiological and pathological laboratories had recently been founded in England, and animal experimentation was introduced on a more extended scale than previously. The researches of Pasteur led up to the science of bacteriology and opened new fields of investigation which were eagerly pursued. But contemporaneously with this scientific activity public opinion was deeply stirred by descriptions and representations of experiments performed on living animals by foreign and in some cases by English scientists. Fears were entertained that cruel and unnecessary experiments would be performed in increasing numbers. To meet this natural and commendable public apprehension the Government of the day in 1875 appointed a strong Royal Commission "to enquire into the practice of subjecting live animals to experiments for scientific purposes, and to consider and report on the measures, if any, it may be desirable to take in respect of any such practice."

Lord Cardwell was Chairman of the Commission, and the other members were Lord Winmarleigh, The Right Hon. W. E. Forster, Sir J. B. Karslake, Mr. T. H. Huxley, Mr. Erichsen and Mr. R. H. Hutton.

The Commissioners examined the most eminent medical and scientific authorities of the day and carefully considered the question whether experiments on living animals should be prohibited or left to the general law or regulated by a special Act of Parliament. They unanimously came to the conclusion that while they could not recommend the prohibition of such experiments, means should be taken for their regulation and control.

Report of Royal Commission, 1875. C. 1397. p. xvii.

They sum up the results of their enquiry in the following terms :—

> "Our conclusion, therefore, is that it is impossible altogether to prevent the practice of making experiments on living animals for the attainment of knowledge applicable to the mitigation of human suffering or the prolongation of human life :—that the attempt to do so could only be followed by the evasion of the law or the flight of medical and physiological students from the United Kingdom to foreign schools and laboratories, and would, therefore, certainly result in no change favourable to the animals—that absolute prevention, if it were possible, would not be reasonable :—that the greatest mitigations of human suffering have been in part derived from such experiments :—that by the use of anæsthetics in humane and skilful hands the pain which would otherwise be inflicted may, in the great majority of cases, be altogether prevented, and in the remaining cases greatly mitigated :—that the infliction of severe and protracted agony is in any case to be avoided—that the abuse of the practice by inhuman or unskilful persons—in short, the infliction on animals of any unnecessary pain—is justly abhorrent to the moral sense of Your Majesty's subjects generally, not least so of the most distinguished physiologists and the most eminent surgeons and physicians :—and that the support of these eminent persons, as well as of the general public, may be confidently expected for any reasonable measure intended to prevent abuse."

After discussing various proposals for legislation with the object of giving effect to these conclusions, they proceed to recommend :—

p. xx.

> "the enactment of a law by which experiments on living animals, whether for original research or for demonstration, should be placed under the control of the Secretary of State, who should have power to grant licences to persons, and, when satisfied of the propriety of doing so, to withdraw them. No other persons should be permitted to perform experiments. The holders of licences should be bound by conditions and breach of the conditions should entail the liability to forfeiture of the licence; the object of the conditions should be to ensure that suffering should never be inflicted in any case in which it could be avoided, and should be reduced to a minimum where it could not be altogether avoided. This should be the general scope of the conditions, but their detailed application should be left to be modified from time to time by the Minister responsible according to the dictates of experience. In the administration of the system generally, the responsible Minister would, of course, be guided by the opinion of advisers of competent knowledge and experience."

Their final recommendations may be summarized as follows :—

(1) That it is inexpedient to divide the responsibility of the Secretary of State with that of any other person by statutory enactment.

(2) That the Secretary of State's advisers should be from time to time selected and nominated by himself. Their names should be made known to the profession and the public.

(3) That it may be desirable that one of the conditions attached to the licence should be that the experiments should be performed in some particular place, but this is a detail which ought not to be stereotyped by statute.

(4) That the Secretary of State must have the most complete power of efficient inspection and of obtaining full returns and accurate records of all experiments. The appointment of an Inspector or Inspectors will be necessary.

The Inspectors must be persons of such character and position as to command the confidence of the public no less than that of men of science.

(5) Abuse of the power conferred by the licence must of course render the holder liable to its withdrawal; but this will involve great disgrace; and the withdrawal of the licence of an eminent man without real cause might be a serious public mischief. In such cases the licensee ought to have liberty to demand a public enquiry before a Judge of the Supreme Court, with his competent assessors, to be appointed by the Secretary of State.

(6) Magistrates to be empowered, on cause shown, to authorise the police to enter and search the premises of persons suspected of performing experiments without a licence, and the performance of such experiments without a licence should be penal.

(7) To meet cases of urgent necessity, *e.g.*, suspected poisoning when no licensed person is within reach and when a medico-legal investigation by way of experiment on an animal is considered indispensable, the Secretary of State should be empowered to put a veto on prosecution to prevent vexatious proceedings.'

One of the Commissioners—Mr. R. H. Hutton—although he signed the general report, appended a reservation to the effect that dogs and cats should be exempted from experiments on the grounds of their special relation to mankind, their higher sensibility, the degrading and criminal trade which is fostered by their supply to physiologists, and the absence of proof of their indispensability to science.

8. Following on the Report of the Royal Commission, legislation was initiated. Mr. (now Sir William) Byrne, who represented the Home Office, informed us as follows :— Byrne 1.

In 1876 there were two private members' Bills before Parliament which made no progress.

The Government Bill introduced in the same year purported to carry out all the recommendations of the Commission except the appeal against revocation of licence, and also the recommendation of the dissentient Commissioner (exemption of dogs and cats).

The General Medical Council and other medical bodies, by memorial and by deputation to Lord Carnarvon, the Minister in charge of the Bill, represented—not very strenuously or obstinately—that the proposed legislation was uncalled for; but they strongly urged certain practical amendments :—

1. The change of the title of the Bill from "A Bill to Prevent Cruel Experiments on Animals" to its present title.

2. A definition of "animal" to exclude the lower forms of life.

3. That experiments should be allowed for the purpose of gaining abstract knowledge and of saving life or alleviating suffering in animals as well as in man.

4. That experiments should be restricted to registered places only when conducted for instruction.

5. That dogs and cats should not be exempted.

6. That the requirements of the Bill as to the reports to be made by experimenters should be whittled down to a duty to give information when called upon by the Secretary of State.

7. That the scientific authorities having power to sign certificates should be permitted to exempt experimenters at their discretion from the obligation to obtain the special certificates required under the Bill for work without anæsthetics, etc.

All these recommendations, except 4, 6, and 7, were accepted by the Government, and the Bill, with some trifling modifications, passed through both Houses without strenuous opposition, and received the Royal Assent, apparently with general approval.

The Act of 1876.

9. The Act, "An Act to amend the Law relating to Cruelty to Animals," applies to all vertebrate animals and to all experiments on them calculated to give pain. No such experiments may be performed except by a person licensed by the Secretary of State or in certain circumstances by a Judge of the High Court.

Experiments for the purpose of acquiring manual skill, and experiments by the way of public exhibition are absolutely prohibited. Experiments by licensees "must be performed with a view to the advancement by new discovery of physiological knowledge or of knowledge which will be useful for saving or prolonging life or alleviating suffering," or "for the purpose of testing a particular former discovery alleged to have been made for the advancement of such knowledge as last aforesaid, on such certificate being given as is in this Act mentioned that such testing is absolutely necessary for the effectual advancement of such knowledge."

The general or *primâ facie* restrictions on experiments "calculated to give pain" imposed by the Act are as follows :—

(1) The animal must, during the whole of the experiment, be under the influence of some anæsthetic of sufficient power to prevent the animal feeling pain, and

(2) The animal must, if the pain is likely to continue after the effect of the anæsthetic has ceased, or if any serious injury has been inflicted on the animal, be killed before it recovers from the effect of the anæsthetic which has been administered, and

(3) The experiment must not be performed as an illustration of lectures in medical schools, hospitals, colleges, or elsewhere, and

(4) The experiment must not be performed without anæsthetics on a dog or cat, or on a horse, ass, or mule.

All these restrictions may, however, be dispensed with by means of statutory certificates, that is to say :—

(1) A certificate may be given dispensing with anæsthetics on the ground that insensibility would frustrate the object of the experiment. (Certificate A.)

(2) A certificate may be given that killing the animal before it recovers from the anæsthetic would frustrate the object of the experiment, and such a certificate postpones the obligation to kill the animal until the object of the experiment has been obtained. (Certificate B.)

(3) A certificate may be given that the experiments are absolutely necessary for the due instruction of the students, and with such a certificate experiments may be performed before classes of students, but only under anæsthetics. If the animal is injured by the experiment, or would be likely to suffer pain after recovery to consciousness, it must be killed before the effect of the anæsthetic wears off. (Certificate C.)

(4) A certificate may be given which adds the testing of an alleged former discovery to the above-mentioned scientific objects which justify experiments under the Act. (Certificate D.)

(5) A certificate may be given that the object of the experiment would be frustrated unless performed on a dog or cat. When it is proposed to perform the experiment without an anæsthetic Certificate E must be used, and it must be accompanied by Certificate A, which is to the effect that to produce insensibility of the animal by the use of an anæsthetic would frustrate the object of the experiment. When it is proposed to perform the experiment with an anæsthetic Certificate EE must be used, and it must be accompanied by Certificate B, which certifies that the purpose of the experiment would be frustrated if the animal were killed before it recovered from the anæsthetic. These certificates are not required when experiments on dogs or cats are performed under the licence alone. (Certificates E. and EE.).

(6) A certificate may be given that the object of the experiment would be frustrated unless it is performed on a horse, ass or mule, and such a certificate is necessary whether the experiment is performed under anæsthetics or not. (Certificate F.)

Licences.—Applications for licences must be signed by one or other of the 13* authorities specified in Section 11 of the Act, and also by a professor of physiology, medicine, anatomy, or surgery, unless the applicant is himself such a professor. The Secretary of State may grant a licence for such period and on such conditions, not inconsistent with the Act, as he thinks fit, and may, in his discretion, revoke any licences. The bulk of investigations require certificates as well as licences.

A Judge of the High Court also may grant a licence to perform experiments essential for the purposes of criminal justice.

Certificates.—Certificates enabling a licensee to dispense with certain of the statutory restrictions are given by any two, or in the case of a University or College professor, any one of the persons holding the offices named in the Act, and may be given for such time or for such series of experiments as the signatories think expedient. Copies must be sent to the Secretary of State, and the certificates are not to come into operation until a week after

* The President of the Royal Society; the President of the Royal Society of Edinburgh; the President of the Royal Irish Academy; the Presidents of the Royal Colleges of Surgeons in London, Edinburgh, or Dublin; the Presidents of the Royal Colleges of Physicians in London, Edinburgh or Dublin; the President of the General Medical Council; the President of the Faculty of Physicians and Surgeons of Glasgow; the President of the Royal College of Veterinary Surgeons, or the President of the Royal Veterinary College, London, but in the last two instances in the case only of an experiment to be performed under anæsthetics with a view to the advancement by new discovery of veterinary science.

the copies have been forwarded. A judge may give a certificate authorising experiments essential for the purpose of criminal justice, and the Secretary of State may suspend or disallow any certificate except a judge's.

According to these provisions, therefore, a painful experiment under the Act may only be performed on the recommendation of one or more of the scientific authorities above referred to and by a person holding a certificate granted by such authority sanctioning the experiment in question. Such certificates can only be granted to persons licensed by the Secretary of State.

Licences and certificates come up before the Home Office for consideration at the beginning of each year, but subject to any report by the Chief Inspector, licences are renewed, and certificates, if the experiments authorised by them have not been performed, are continued, without requiring the initial proceedings to be recommenced. Byrne 48.

Registration.—All places where experiments are performed for class instruction require approval and registration by the Secretary of State, and he also may require the registration of any other places where experiments are performed under the Act. Thane 337, etc.

Reports.—He may also direct any licensee to report to him the result of his experiment in such form as may be prescribed.

Inspection.—The Secretary of State may appoint inspectors, and he is directed to cause all registered places to be visited by inspectors from time to time, in order to secure compliance with the provisions of the Act. 327.

Prosecutions.—If an experiment calculated to give pain be performed by an unlicensed person, any member of the public cognisant of the facts may institute a prosecution, except in Scotland where the Procurator Fiscal alone can prosecute. If a licensed person contravenes the provisions of the Act he can only be prosecuted by a person who has obtained the assent in writing of the Secretary of State. Any such prosecution however must be instituted within six months of the occurrence of the alleged offence. Russell 590-3.

In Great Britain the Act is administered by the Secretary of State for the Home Department, while in Ireland the Chief Secretary to the Lord-Lieutenant is substituted for the Secretary of State.

Curare is not to be treated as an anæsthetic.

It is to be noted that the Act only applies to experiments for scientific purposes, that is to say investigations made with the view of increasing or testing knowledge. It has been held not to apply to commercial operations, such as the production of vaccines or sera for purposes of sale. Nor does it apply to such operations on animals as are not in the nature of experiments, such as the castration of horses, lambs, etc. Any abuse in connection with such matters can be dealt with only under the general law of Cruelty to Animals.

Colonial and Foreign Law.

10. According to a summary compiled in 1904 and put in by the Home Office the laws as to experiments on animals outside the United Kingdom are as follows :— Vol. V. of Evidence, p. 47. Cd. 4147.

(i.) British India.

There is no legislation restricting vivisection in British India. A Bill drafted in 1892 was allowed to drop as unnecessary. In scientific institutions over which the Government has any control the lines of the English Act 39 & 40 Vict., c. 77, are followed.

(ii.) British Colonies.

Apart from the general law as to cruelty to animals, there is no legislation dealing with vivisection in Canada, Cape of Good Hope, Natal, New Zealand, Jamaica or Barbadoes. In the Australian Colonies the only enactments dealing directly with this question are those of Victoria and Queensland.

In Victoria, Secs. 12 and 13 of the Animals Protection Act, 1890, exempt persons practising vivisection from the penalties of that Act, and provide for the registration of practitioners and for the making of regulations. In Queensland, Sec. 12 of the Animals Protection Act of 1901 is in similar terms.

(iii.) Foreign Countries.

There are no special enactments or regulations restricting the practice of vivisection in Belgium, Bulgaria, France, Greece, Hesse, Hungary, Portugal, Roumania, Russia, Servia, Spain, Sweden and Norway, Turkey or the United States of America.

In two countries there is definite legislation, viz: Denmark and Switzerland (cantons of Geneva and Zurich).

In the following countries the practice is regulated by Government instruction issued to universities, schools and other institutions :—Austria, Baden, Bavaria, Germany, Holland, Italy and Saxony.

In Bavaria, Belgium, Germany, and Russia, there is, however, general legislation providing penalties for those guilty of cruelty to animals, which might presumably be applicable to those who practise vivisection; but in France the general law relating to cruelty to animals only applies to such experiments held in public as are attended with excessive cruelty, not to those conducted in private; in Holland the law as to cruelty to animals does not apply to illtreatment with a scientific object; in Sweden the general law would not seem to affect cases of cruelty to animals occurring in a laboratory, while in Norway an Act coming into force this year (1905) as to cruelty to animals specially provides that the Sovereign or anyone to whom the Royal Authority may be delegated shall not be restricted from granting special permission to special persons for conducting experiments on animals which may entail suffering; in the United States of America, the law as to cruelty to animals specially excepts from the provisions of the Statute scientific experiments conducted under the authority of the faculty of some regularly incorporated college, university or scientific society.

As to other foreign countries, it is not stated whether or not there is legislation as to cruelty to animals which might be applicable to those who perform experiments on living animals.

The information supplied to the Home Office, from which the above summary was drawn up in that Department, will be found in Volume VI. p. 2.

Administration of the Act in Great Britain.

11. The great majority of the experiments under the Act of 1876 are performed in Great Britain, and it therefore becomes material to consider the administration of the Act by the Home Secretary who, as we have seen, is the authority for this country. The Home Office in administering the Statute have, under the powers conferred by the Act, added a number of departmental requirements.

Byrne 6–17.

The practice of the Home Office, we were informed, is as follows :—

Byrne 24, 27–9, 131–3, 175–6; Thane 435–8; Russell 525–7; Stoker 1066–7; Horsley 16100–1.

In Great Britain every application for a licence and every certificate, when signed by the proper scientific authorities, is forwarded to the Home Office. It is then sent by the Home Office to the Association for the Advancement of Medicine by Research for their opinion. If their observations are favourable, the licence or certificate is then submitted, together with the Report of the Association, to the Home Office Chief Inspector for his observations. It is his duty to report in detail on every certificate and every licence, and to advise on the special conditions, dealing for the most part with the protection of animals, which are therein inserted. For the purpose of enabling him to advise the Secretary of State he makes, whenever necessary, such enquiries from the signatories and the investigator as may be in his opinion expedient, and in some instances he takes counsel of other persons having special knowledge of the subject. He then transmits the licence or certificate to the Home Office with his report, and they are both minutely examined departmentally. The examination is directed firstly to see that the requirements of the Act have been complied with, and secondly, to a comparison of the proposed investigation with previous researches, and to the decisions of the Secretary of State, if any, with regard to them. The qualifications of the experimenter are also considered. The papers are then submitted either to the Under Secretary of State or to an Assistant Under Secretary of State, or to both. If the proposed investigation is of a novel or important character, or one which might involve the possibility of considerable pain, it is submitted to the Home Secretary personally. Generally speaking, all applications, coming as they do from and being recommended by competent persons, are granted. An absolute refusal is the very rarest occurrence.

Byrne 155.

218–20.

The Home Secretary, in the exercise of his discretionary powers, inserts, in pursuance of Section 8, conditions which go considerably beyond the requirements of the Act itself. Under the Act a certificate has to be suspended for seven days, but in practice, as we now understand, it is not available until the licensee has been informed that it has not been disallowed by the Secretary of State.

Vol. V. of Evidence. App. III. p. 48. and 10172.

According to Dr. Thane, Certificate A, "which dispenses with the use of anæsthetics has not" during the time as to which he can speak from personal knowledge "been allowed for any operative procedure more severe than the opening of a subcutaneous vein for which no one would think of using an anæsthetic in man; that is, in other words, that vivisection of an animal, in the proper sense of the term, without anæsthetics has not in any case been allowed by the Secretary of State."

H.O. Return for 1910.

Certificate A is chiefly employed for inoculation experiments, of which there were 90,792 returned in the year 1910. Dr. Thane in that Return states : "The Certificate is in fact not required to cover" the operative procedures, "but to allow of the subsequent course of the experiment." A distinction must be drawn between "the operative procedure" and the "experiment," for the latter lasts during the whole time from the initial administration, injection or inoculation until the animal dies or is killed or recovers. The statement which appeared in the Returns for the years 1889 and 1890 to the effect that : "no experiments requiring anything in the nature of a surgical operation or that would cause the infliction of an appreciable amount of pain are allowed to be performed without an anæsthetic" has been amended and since 1901 has appeared in the following form : "In no case has a cutting operation more severe than a superficial venesection been allowed to be performed without anæsthetics. . . . In no instance has a certificate dispensing with the use of anæsthetics been allowed for an experiment involving a serious operation."

Vol. V. of Evidence. App. III.

The "pain condition," with which we deal hereafter in our Report, is inserted in all cases of Certificate A except "in cases where it was obvious that pain would not ensue from the procedures contemplated." It is as follows :—

"That if an animal, after and by reason of any of the said experiments under the said Certificates . . . is found to be in pain, which is either considerable in amount or is likely to endure, and if the main result of the experiment has been attained, the animal shall be immediately killed under anæsthetics."

In the case of Certificate B, when the animal is allowed to recover from an anæsthetic, anæsthesia is required to be kept up until the wound is closed and dressed, and aseptic precautions are required to be used when any cutting operation is resorted to, except when such precautions are inapplicable. The aseptic condition inserted in licences is as follows :— Vol. V. of Evidence. App. I., p. 29.

"That the animals experimented on under Certificates . . . be treated with strict antiseptic precautions, and if these fail and pain results, that the animals be immediately killed under anæsthetics."

The forms of licences and certificates and the special conditions have already been given. Vol. V. of Evidence. App. I. p. 24.

12. The consultation of the Association for the Advancement of Medicine by Research or of any other body is not required or suggested by the Act itself. It was instituted at the suggestion of the Association by the late Sir William Harcourt when Home Secretary in 1882. This Association, which is a purely voluntary organization, consists of certain *ex officio* members such as the Presidents of the Medical and Scientific Corporations, and ordinary elected members who pay 10s. a year; also any licensee is *ipso facto* a member on payment of the subscription. Professor Starling informed us that the aims and objects of the Association are: (1) To advise in the granting of licences. (2) To protect when necessary, the interests of the licensees. (3) To watch proceedings in Parliament affecting the interests of research. (4) To publish and distribute to medical men and others who may desire it, literature on the importance of research and the necessity of experiments on the lower animals. He further stated in answer to a question: "Then the object of the whole membership is favourable to the promotion of vivisection ?" "Certainly." The Association, according to his evidence, has sent back for modification, but has never to his knowledge finally refused licences. In a later paragraph of this Report we shall again refer to this Association when we come to consider the question of an advisory body to aid the Secretary of State. Byrne 31, Thane 397. Starling 3860–3. 3865. 3866.

13. Under the Act the Secretary of State has power to require any place where experiments are performed to be registered. As a general rule the Secretary of State registers only such places as are under the control of a local authority or a medical school or a University; and before registration the Inspector has to satisfy himself that the premises and their equipment are proper for the purpose. In two cases private premises, after inspection, have been registered for the purpose of testing and standardising drugs manufactured on a large commercial scale. There are certain experiments which it would be impossible to carry out on premises previously registered, as, for instance, in some cases of epizootic diseases among cattle or other animals where experiments have to be made on the spot where the disease may, for the time being, happen to appear. In these cases registration of the place is not enforced, but a special report from the licensee is required. Byrne 53. Thane 337–9. 356–8. Byrne 82–8.

14. *Inspectors.*—The Act does not define the duties of Inspectors further than directing (Section 10) that :—

"The Secretary of State shall cause all registered places to be from time to time inspected for the purpose of securing a compliance with the provisions of the Act." Thane 327.

But the requirements of the Home Secretary in this direction were explained to us by Dr. Thane, who added that he believed that no important step is taken in the administration of the Act without his being consulted. There are three Inspectors: one residing in London who inspects all England and Wales except the six northern counties; and one in Scotland who inspects the rest of Great Britain. The London Inspector reports direct to the Home Secretary; the Scottish Inspector reports to him through the London Inspector. The third Inspector inspects Ireland, and is under the control of and reports to the Chief Secretary to the Lord-Lieutenant. In the district of the London Inspector in 1910 there were 324 licensees and 48 registered places. In the northern district there were 218 licensees and 43 registered places. In Ireland there were 23 licensees and 15 registered places. The Inspectors are not required to give the whole of their time to the duties of their office. Byrne 185–7. 33. H.O. Return for 1910.

The Inspectors visit each laboratory in their respective districts about three times a year, to ascertain whether they are properly kept, and whether the animals, especially those which have undergone operations, are properly cared for, and if they find any breach of the Act or requirements they either warn the licensees or report it to the Home Secretary according to the nature of the case. They all agree in stating that they find the animals well cared for and dealt with as required by the Act, and that they have no complaints to make in that regard. Much of the Inspectors' time is also consumed in correspondence and in advising on applications for licences or certificates; and they have to prepare an annual report to the Home Secretary. Thane 441. 452. 443. 442 *et seq.* 480.

Thane 447. In the course of their visits to the various registered places the Inspectors witness a
Russell 564–5. certain number of operative experiments. In the six and a half years from 1899 to 1905, Dr. Thane or his deputy saw 100 such operations. A very large number of animals under experiment though not under operations are seen by the Inspectors in the course of their visits. It may be noted that in 1910–11 only about 2 per cent. of the experiments were performed under Certificate B, while about 95 per cent. were under Certificate A, the remainder being under other certificates, or under licence only.

Thane 450. Dr. Thane found the requirements in regard to anæsthesia strictly carried out
1162. and the experiments which he witnessed conducted as humanely as possible; he
1165. was satisfied with the treatment and care of the animals. Sir J. Russell had never
Russell 534. encountered difficulty in inspection, but stated that licensees had great difficulty
577–8. in understanding the Act and the certificates; anæsthetics were given freely and
543.
547–9. fully, but he had come across a few cases in which there was evident suffering following injection or inoculation. He did not believe that any licensee wilfully violated the Act.

Thane 1085. Objection was taken by one witness to a phrase which occurs in a letter written by
Russell 530. Dr. Poore, the Chief Inspector in London in 1890, to Sir James Russell, the newly-appointed Inspector for Scotland, informing him of his duties. Dr. Poore said that
Thane 1145–7. "he was not expected to act as a detective." Dr. Thane stated that he had seen the expression used in a minute of the Home Office. That minute, which has been furnished to us, referred to certain duties of the Inspectors and used the phrase quoted. In our opinion the use of this phrase is liable to be misunderstood. On the one hand its use in such a connection may seem to minimize the need for circumspection in regard to possible irregularities, and on the other hand the visits of the Inspectors, being generally surprise visits, serve as such as safeguards, but they are made, not in the anticipation of discovering wrong-doing, but in order to secure the due performance of the requirements of the Act. The licensees are persons who have been recommended by the heads of their professions and approved by the Home Office.

15. No legal proceedings have been instituted under the Act of 1876, either by the Home Office or by any individual. In the case of a licensee, by Section 21 of the Act, no prosecution can be instituted except with the written sanction of the Secretary of State.

Sir William Byrne, of the Home Office, told us that since 1876 licences have been
Byrne 177. withdrawn in four cases only. The Home Secretary considered that in each case a licensee had deliberately performed an operation without obtaining the necessary
38. certificate. One of these cases was described as of "gross carelessness" on the part of a licensee who holding no certificates yet performed experiments necessitating the holding of Certificates A and B. Another case described as "deliberate violation of the Act after warning" was one in which a gastric fistula had been made in a cat, which
40 and footnote. was kept alive afterwards, although the licensee had neglected to obtain either Certificates B or EE. Sir William Byrne was under the impression that this licensee had not had his licence re-conferred but on enquiry he subsequently informed us that this gentleman was a few months later granted a fresh licence along with Certificate A. The third case of revocation of licence was one in which two licensees performed injections which induced convulsions without employing anæsthetics and without possessing Certificate A; in this case a renewal of one of the licences was refused, these experimenters having shown also "great carelessness" in making their annual returns. The last case was one in which a licensee had acted on a Certificate B which had never been submitted to the Secretary of State, the certificate being for "removal of mammary tissue and grafting of animals together."

During the same period of thirty years some sixty other cases of minor importance
42. had come under the notice of the Home Secretary. These breaches are referred to by Sir William Byrne as being trivial and technical in their nature, and for the most part caused by inadvertence, and in nearly every case disclosed by the licensees themselves. The Home Office takes no steps to ascertain whether unlicensed persons practise vivisection, as it is not aware of any practical steps that can be taken for that purpose.

16. *Records, Reports and Returns.*—The Secretary of State may direct experimenters to make such reports as he pleases of the results of experiments, and in such form and with such details as he may require. As regards records, reports and Annual Returns Sir William Byrne submitted to us the following memorandum :—

Records and Reports of Experiments.

All licensees are required to keep records and to furnish certain reports to the Inspector of all experiments on living animals performed by them under 39 & 40 Vict., c. 77. The obligation to keep the records and to make the reports is imposed by the conditions attached to the licence granted by the Secretary of State. The terms of the conditions are as follows :—

(a) "The holder must keep a written record of his experiments. Such record shall be open to examination at any time by the Inspector, and a report of all experiments performed shall be furnished to the Inspector on the 31st day of December, and at any other time when required."

(This condition is endorsed on all licences, and applies to all experiments performed) ; and

(b) "That after the completion of the experiments detailed in the certificate, or on the 31st day of December in each year in which the said certificate is in force, and at any time when required by the Inspector, the holder of this licence is to report to the Inspector, on the prescribed form, the number and nature of the experiments performed."

(This condition is endorsed on the licence of all licensees who hold Certificates A or B.)

The "Record" referred to in the first of the conditions quoted above is the form 9 of the Home Office Forms, and is only to be forwarded to the Inspector when required by him, and the "Report" is the form 10. It was decided, however, in 1902, that where a licensee records his experiments in a book at his laboratory, entering at least as much information as is required by the Home Office form of record, he need not use the Home Office form ; and every licensee is informed of this decision when his licence is granted. Vol. V. of Evidence, App. I., pp. 41 and 42.

Publication by Licensees.

Every licence granted by the Secretary of State has a condition attached which requires the holder to forward to the Home Office any description of his experiments which is published in any journal or magazine or in any report of a lecture printed for publication or private circulation.

A large number of such papers are received—*e.g.*, 103 came to hand in the period of eighteen months preceding December 31st 1905.

These are carefully scrutinised by the Inspectors with a view to the satisfaction of the Secretary of State that no procedures causing pain have been adopted without due authority.

Results of Experiments.

Section 9 of the Act enables the Secretary of State to direct any experimenter to "report from time to time the results of his investigations in such form and with such details as may be required."

No such directions are in force, although in a few instances a licensee has been asked as to the progress of his investigations when an application for sanction to further experiments has been under consideration.

The matter has received anxious consideration at the Home Office. It has been felt that the Secretary of State would be placed in a position of grave responsibility and much difficulty if he were to undertake to formulate and act on a Departmental opinion as to whether the scientific results of a research—possibly of an obscure character and conducted by an expert of unique qualifications—were sufficiently established or promising to justify the pursuit of the investigation. And it would be unreasonable to expect that his Inspectors, however learned and accomplished, could be at home in every branch of the most modern research in physiology, pathology, pharmacology, and the rest. On the other hand, the Act invests the Home Secretary with a regulative authority which would apparently include the function of stopping futile and unnecessary experiments. In more than one instance he has, on this ground, imposed his veto on proposed investigations, and has in a large number of cases limited the number of animals to be subjected to experiments in a given research. The learned authorities specified in Section 11 of the Act would appear to be or to include the persons most competent to advise on this matter, and their well-considered recommendation would no doubt meet with general acceptance. It will be observed that the form of application for licence in use contains a line in which the learned signatories expressly "recommend" that the application be granted. The forms of certificate contain no such "recommendation," although if passed by the Secretary of State they operate to allow a licensee to embark on an entirely new investigation. It may be assumed that no president or professor would as a rule attach his signature to a certificate unless satisfied both of the proper qualifications of the licensee and of the expediency of his proposed research. On the other hand, it has been suggested that the Secretary of State would be justified in calling for a positive recommendation from the signatories of a certificate as well as those of an application for a licence. The point is still under consideration.

The Annual Return under the Act.

The first Return was ordered by the House of Commons on February 19th, 1877, on the motion of Mr. Mundella. It was for a "Return of Licences granted under the Act (39 & 40 Vict., c. 77) to amend the Law relating to Cruelty to Animals, specifying :—

(1) The number of persons to whom such licences have been granted since the Act came in force, and the names of all registered places ;

(2) The number of licences in which the (optional) provision (Clause 7) requiring that the place wherein the experiment is performed shall be registered, has been inserted ;

(3) The number of certificates which have been received under Clause 3, permitting experiments as illustrations of lectures to students ;

(4) The number of certificates which have been received under Clause 5, permitting experiments on cats, dogs, horses, mules, or asses ;

(5) The number of certificates (special) which have been received for performing experiments without anæsthetics, and the number of such experiments in which curare has been employed ;

(6) The scientific authorities who have in each case granted such certificates.

This information was supplied as Parliamentary Paper 100 of Session 1877, but it is worth noting that the names of licensees were not given, and that no explanatory report by the Inspector accompanied the Return.

The next Return (193 of Session 1878) is a continuation of the Return issued in 1877, with a slight alteration of the form in which the further information is furnished.

In 1879, Mr. Evelyn Ashley moved for a Return of "Copy of any report from the Inspectors, showing the number of experiments performed on living animals during the year 1878, under licences granted under the Act 39 & 40 Vict., c. 77, distinguishing Painless from Painful Experiments." This Return was granted (127 of Session 1879) and the attempt to indicate the amount of pain caused was made in a letter from Mr. Busk, the Inspector, which was prefixed to the usual tables. The tables differ from those previously presented, in that no information is given as to experiments in which curare was used, and the names of all licensees who did not object were published; and besides this those licensees who had performed no experiments were entered in a table by themselves. The Returns were presented in this form without any material alteration up to and including the Return for the year 1886, there being, however, a tendency to increase the number of foot-notes explanatory of the tables.

In the return for 1887, Mr. Erichsen, who was now Inspector, introduced for the first time a separate table (Table III.) "showing the number and the nature of the experiments performed by each licensee during the year 1887." The experiments are classified according to whether they were performed under "Licence" or Certificates A, B, etc., and also according to their nature, *i.e.*, whether physiological, pathological, or therapeutical, and a "Pain" column was added, giving such remarks as "ten rabbits" "two cows," the inference apparently intended to be drawn being that these animals suffered pain. There is no correspondence with the file to show that this addition to the Return was considered in the Home Office, and from a minute written in 1890 it would seem that Sir G. Lushington considered the "Pain" column had been added without sufficient consideration. The Return for 1888 is in the same form as the Return for 1887.

A re-arrangement of the Tables I. and II., "List of Licensees who Performed Experiments," and "List of Licensees who Performed no Experiments" was effected in the Return for 1889, the columns showing the certificates held by each licensee being placed in the order of the letter borne by the different certificates, *i.e.*, A, B, C, D, etc., and for the first time the special certificates for dogs and cats, and horses, asses, and mules, are given separate columns, E and F. In connection with the preparation of the 1889 Return, it was decided to send a copy of the Return annually to every registered place.

Dr. Poore, the next Inspector, in his first Return (for 1890), which was headed "Return showing the Number of Experiments performed on Living Animals during the year 1890, under Licences granted under the Act 39 & 40 Vict., c. 77, distinguishing Painless from Painful Experiments," wished, with Sir G. Lushington's approval, to omit the column headed "Pain" in Table III., but the Secretary of State (Mr. Matthews) objected so strongly that the column was retained in a modified form. Its heading was altered from "Pain" to "Remarks," and such notes as "Painless," "Hypodermic Injections," and "Inoculations," were entered therein. The Returns for the years 1891, 1892, 1893, 1894, and 1895 are all in this form. In connection with the preparation of the 1894 Return, however, Sir Kenelm Digby decided that the particulars of all contraventions of the Act, except the names of the offenders, were to be published each year in the letter of the Inspector which precedes the tables.

In the 1896 Return, Dr. Poore divided Table III. into two parts, viz. "(A) Experiments other than those of the Nature of Inoculations, Hypodermic Injections, or similar proceedings;" and "(B) Experiments of the Nature of Inoculations, Hypodermic Injections and similar proceedings." Table III. (A) contained all experiments under licence and Certificates C, B, and B + EE, except inoculations under Certificate B, which were entered in Table III. (B), together with experiments under Certificates A, A + E, and A + F. The use of the word "painless" was confined to Table III. (A), and there are very few entries in the "Remarks" column of Table III. (B).

In the 1897 Return, the word "painless" is not used in the "Remarks" column of Table III. The Department questioned this change, but Dr. Poore said he was not prepared to state which experiments were painful and which painless. As a compromise, a statement was added to the heading of Table III. (A), to the effect that all experiments performed under licence alone or under Certificate C were painless. The Return also contains in the "Place" column of Tables I. and II. the names of all registered places.

In the 1898 Return the column of Table I. and II, which gave the numbers of special Cat and Dog Certificates E and EE, was divided into two columns, viz: E (with A) and EE (with B) for the sake of precision.

During the preparation of the 1899 Return, it was decided, again with a view to greater clearness, that all experiments with anæsthetics should be entered in Table III. (A), and all experiments without anæsthetics should be entered in Table III. (B). The column for "B. Inoculations only" disappeared from Table III. (B), and the heading of the two parts of the table became: "(A) Experiments other than those of the Nature of Simple Inoculations, Hypodermic Injections, and similar proceedings, and (B) Experiments of the Nature of *Simple* Inoculations, Hypodermic Injections and similar proceedings performed without Anæsthetics." The question of publishing (in Tables I. and II.) the names as well as the offices of the signatories was raised, but it was decided not to make such an addition to the Return.

No alteration was made in the Return for 1900, but it was decided in connection therewith and because of the value of the Inspector's Report as a guide to licensees that a copy of each year's Return should from that time forward be sent to every holder of a licence at the time when the Return should be issued. The form of the Returns for 1901 and 1902 remained the same as the 1899 and 1900 Returns.

In the 1903 Return a new table giving a "List of the Places on the Register under the Act, etc." was inserted and the two tables of lists of licensees (now II. and III.) were arranged alphabetically according to names of licensees and not according to names of places as hitherto. The Return for 1904 was in the same form.

In the Return for 1905 (the last one issued) several alterations were made. In the first place the description of the Return was changed to "Return showing the Number of Experiments on Living Animals during the year 1905 under Licences granted under the Act 39 & 40 Vict. c. 77 distinguishing the Nature of the Experiments." In reply to a question in Parliament relating to this alteration Mr. Gladstone said "The change was made because it was found impracticable to make a separation between painful and painless experiments. In some cases it is impossible for anyone even the operator or observer, to say whether pain is caused or not. It is, of course, important to ascertain, and to indicate the extent to which pain is caused to animals by experiment, and the Inspector's report, which precedes the Return, gives this information as far as possible with regard to particular classes of experiments." (*Parliamentary Debates*, June 28th, 1906). Secondly, the last column of Tables II. and III. giving the "scientific authorities recommending licences and granting certificates," was headed by a note setting out the requirements of the Act as to signatories, and stating that all applications for licences and all certificates had been examined and found to be duly signed. This addition was made because it was considered that the information previously given in these columns was not a sufficient guarantee to the public that each separate application and certificate was duly signed. Thirdly, the word "performed" in the headings of Tables II., III., and IV. was altered to "returned." These tables then became "Table II., List of Licensees who Returned Experiments under their Licences and Certificates in 1905"; "Table III., List of Licensees who Returned no Experiments under their Licences and Certificates in 1905"; and "Table IV., Number and Nature of the Experiments Returned." The last change was in the "N.B." at the head of Table IV. (A), first inserted in the 1897 Return. Here, for the statement that the animal operated on under licence alone or Certificate C "*suffers no pain because it is kept*" under anæsthetics during the experiment, a fresh statement that such animal "*is required to be kept*" under anæsthetics during the experiment, was substituted.

Some change in the form required appears to have been made in 1902 whereby the experimenter was allowed to enter his record in his own books instead of on the Home Office form, provided that such entry gave at least as much information as the Home Office form prescribed. When asked if it would be possible in reason to enlarge and get more details Sir William Byrne replied that this question had engaged the attention of the Home Office, that "it was proposed by Sir Kenelm Digby, the permanent head of the Home Office, who immediately preceded Mr. Chalmers, that the special matter of calling for a report of experiments might appropriately be the subject of a conference between the Home Office and some of the learned authorities who act under Section Byrne, 240.
11 of the Act. That proposal was never carried out, it probably being thought that a Royal Commission or some other mode of enquiry might presently be instituted." Sir William Byrne was sure that the Home Secretary would be pleased to receive a recommendation from this Commission on that particular point among others.

Administration of the Act in Ireland.

17. *Ireland.*—As has already been stated the administration of the Act in Ireland is under the Chief Secretary to the Lord Lieutenant, who discharges for that purpose all the functions of the Secretary of State. An Inspector under the Act is appointed for Ireland and he has statutory duties similar to those of the English Inspectors and reports to the Chief Secretary in the same manner. In Ireland there is no resort for advice to the Stoker, 924.
Association for the Advancement of Medicine by Research or any similar body. Sir Thornley 761.
Stoker, who appeared before us, and who is the Inspector in Ireland, stated that on 843.
inspection he had never found anything irregular to complain of, but that he thought 910.
that certain experiments were superfluous or useless. In two cases his advice had been 960.
overruled and certificates not disallowed which he had advised against. 914.

Criticisms by Witnesses.

18. The Act of 1876 has now been in operation more than thirty years, and it has been our duty to enquire whether and how far the control of experiments on living animals thereby assigned to the Home Office has been efficiently maintained, or to what extent, if any, the Act has been evaded or disregarded by those whom it was intended to control.

Mr. Coleridge.

Mr. Stephen Coleridge, the Honorary Secretary to the National Anti-Vivisection Society, formulated a series of charges against the administration of the Home Office which we proceed to examine in detail.

(1) He first charged the Home Office "with repudiating the most important Coleridge, 10266–
duty deputed to them by Parliament, that of protecting animals from unjustifiable 75.
suffering." This general statement is, of course, important only so far as it is shown to be founded upon facts.

After most careful consideration of the whole evidence we find nothing to support this charge, which appears largely to be based upon a confusion arising between what the Royal Commission of 1875 recommended and what the Statute of the next year enacted; on the contrary, every one of the ten Secretaries of State who have successively been responsible for the execution of the Act appears to us to have been solicitous to administer the law in accordance with the provisions of the Act.

(2) His second charge was that "the Home Office officials have constituted them- 10275–314.
selves the injudicial defenders of the vivisectors from criticism by his Society in the past, and in their evidence tendered before the Commission." In support of this charge Mr. Coleridge cited the experiments performed under licence by Dr. Crile to which we allude in Paragraph 20, and also to a report of a demonstration by Dr. Grünbaum 10275.
on the effect of inoculating rabbits with snake poison and anti-venom. It was alleged

10330–70. against the latter that he was not in possession of Certificate C. This demonstration was given before a number of chemists and druggists who had been attending a lecture by Professor Sherrington. It may be that Dr. Grünbaum was guilty of a breach of the Act, but the charge which we are here dealing with relates to the conduct of the Home Office. The Home Office carefully investigated the matter and informed Mr. Coleridge that Dr. Grünbaum regarded the report as misleading, but that he had conveyed to the Secretary of State an assurance that such experiments in similar circumstances would
Coleridge, 10355. not be repeated. We do not think that in the cases of either Dr. Grünbaum or Dr. Crile, which we deal with hereafter (Paragraph 20), the allegations made are adequate to support this charge made against the Home Office.

10412–30. (3) He charges "the Home Office officials with having appointed inspectors who have displayed such bias that they have thought it their duty not to make detective efforts to protect animals from illegal treatment." It may be observed that the Inspectors for England and Scotland are appointed not by the Home Office officials, but by the Home
Russell, 529–35. Secretary of the day, who is answerable to Parliament for his selection. The only
Coleridge, 10412–7. evidence in support of this general charge appears to be that relating to the statement made by Sir James Russell, to which we have already referred in Paragraph 14, together with a speech made by Dr. Poore shortly after he ceased to be Inspector, in which he strongly denounced the methods of the opponents of vivisection.

10431–513. (4) He charges "the Home Office officials with having made entirely disingenuous statements in their official utterances and with having constituted themselves the mere spokesmen of the vivisectors." In support of this charge Mr. Coleridge took
10431–40*a*. exception to the statement in the Annual Parliamentary Return for 1904 in which it was affirmed that animals operated upon under licence alone or under Certificate C suffered no pain, maintaining that such statements should have been to the effect that the Home Office was so informed by the vivisectors. He also cited a paper by Dr. Cecil Shaw in connection with experiments upon the eye, claimed as his own but which were subse-
10451–513. quently explained to have been performed by Dr. Lorrain Smith. These experiments, which were made in Ireland, appear to have been the same as those referred to by Sir Thornley Stoker, who regarded them as "totally unnecessary," but in reporting on them
Stoker, 913–4. to the Chief Secretary after long investigations the Law Officers of the Crown advised that there was not evidence to justify legal action in the case. It does not appear to us that the matters cited as instances of disingenuous statements justify a general charge of this character.

Coleridge, 10514, etc. (5) He charges "the Home Office officials with accepting the suggestion made to them by some nameless adviser that to starve animals for days is not cruel."

The facts of the case upon which Mr. Coleridge relies are as follows :—

In 1895–6 Dr. Noel Paton, a licensee, published in the *Journal of Physiology* an account of certain experiments which he had made with reference to the "relationship of the liver to fat," which involved starving a kitten for 56½ hours, two pigeons for 96 hours, and four rabbits for 2 days. Mr. Coleridge called the attention of the Home Secretary in July, 1900, to the case, and the Home Secretary was informed by Dr. Paton that he took out no certificate, and that he did not consider that his treatment of these animals came within the scope of the Act. The Home Secretary wrote to Mr. Coleridge on July 30th, 1900, as follows :—

10530. "The Secretary of State is clearly of opinion that experiments involving starvation to an extent calculated to cause pain would be within the Act, but the enquiries he has made point to the conclusion that the extent and degree of pain which animals suffer from deprivation of food is a matter of considerable doubt. In any case Sir Matthew Ridley does not propose to institute proceedings in respect of experiments which were conducted so long ago as 1895–6, and not brought to his notice at the time." *

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* The instructions to licensees are as follows :—

Home Office, Whitehall.

"It is requested that you will state on the annexed form, first, the Number of Experiments on living Animals performed by you in the year . . . , under your Licence, or any Special Certificate you may have held during the year; second, the general purpose of the Experiments; and third, the number of those Experiments in which there was reason to believe that any appreciable pain was inflicted, specifying the kind of animal that suffered, and stating in general terms the nature of the Experiment that occasioned pain. I shall be obliged if you will also inform me whether any of the experiments were performed on behalf of any Public or Official Body.

"You are also requested to furnish a complete list (on the enclosed form) of any writings, based upon experimental work, which you may have published during the year . . . . , giving the full title of each paper with an exact reference to any periodical in which it may have appeared, and the name of any Society to which it may have been communicated.

"You are reminded that the furnishing of copies of such papers is made a condition of your Licence, and are requested, if you have not already done so, to forward copies to the Secretary of State without delay.

(Signed) "G. D. Thane, *Inspector*."

In answer to a letter pressing for a further expression of opinion, the Home Secretary stated that he declined to take proceedings or to express an opinion as to whether what Dr. Paton did was an experiment upon animals within the meaning of the Act. He subsequently, however, laid down the rule that experimental starvation of animals for more than forty-eight hours would be treated as an experiment within the meaning of the Act. In the case in point a prosecution was impossible after the lapse of five years as there is a statutory limit whereby any prosecution must be instituted within six months. It appears to us that the action of the Home Secretary in this case shows no trace of any desire to evade performance of the duties imposed upon him by the Statute.

(6) Mr. Coleridge further charges the Home Office officials with "suppressing in the annual Parliamentary Returns the names of those who take upon themselves the very grave responsibility of signing the certificates exempting the licensee wholly, or in part, from the obligation to employ anæsthetics in his vivisections," and in support of this charge Mr. Coleridge cites from the Report of the Royal Commission of 1875, a passage in which they say :— Coleridge, 10573, etc.

"We recommend that the Home Secretary's advisers be from time to time elected and nominated by himself. Their names should be made known to the profession and to the public."

Parliament did not follow this recommendation. The Act itself assigns the duty to the holders of certain offices. Such responsibility as may attach to such procedure rests, therefore, with Parliament and not with the Home Office. It appears, however, that a certificate may continue in force after the original signatory of it has left his office ; it is therefore not always easy for members of the public to identify the signatory of any particular certificate.

(7) He charges the Home Office officials with "shielding the names of such licensees as they know to have broken the law, although the Report of the Royal Commission of 1875 contains these words : "Abuse of the power conferred by the licence must, of course, render the holder liable to its withdrawal, but this will involve great disgrace," a phrase that clearly indicates that the framers of that Report contemplated the publication of vivisectors' names." The Report does not, in terms, recommend publication, but this appears not to be material, as it is the Act and not the Report which prescribes the duty of the Home Secretary, and the Act neither suggests nor directs publication of the names, and the Secretary of State has declined to publish the names of experimenters whose licences have been revoked. During the last thirty years four licences have been revoked, and the circumstances under which they were revoked are set out in Paragraph 15. 10603.

(8) Mr. Coleridge next charges the Home Office officials with "preparing for the Home Secretary evasive and insufficient replies in the House of Commons to plain questions on the administration of the Act, and with making evasive and insufficient replies themselves in official correspondence with his Society, and with leaving perfectly proper questions unanswered altogether." In support of this charge Mr. Coleridge cited the following: "On July 24th, 1899, Sir Matthew White Ridley, then Home Secretary, informed Colonel Lockwood that Dr. Poore, the Inspector, did not himself sign certificates under the Act. It appears, however, that for reasons given in Paragraph 18 (6), Dr. Poore, before he was appointed Inspector, had, as a Professor, signed a certificate, and as this certificate continued in force subsequently, he inspected work done as the result of a certificate signed by himself." Dr. Shipman, on May 4th and June 22nd, 1904, asked the Home Secretary how many dogs were vivisected at University College during the year 1903 and was informed that the Secretary of State had no materials from which he could answer such questions. 10614, etc. 10635.

We do not think that these cases afford justification for the general charge which Mr. Coleridge makes.

(9) He then charges the Home Office "with putting forward annually a Parliamentary Return in which it is asserted, on the official authority of the Government Department, that not a single experiment in thousands inflicted on animals in Great Britain with its permission can be specified as entailing any pain at all, when all the while this plausible assertion is based upon no better evidence than the bare assertion of the vivisectors themselves, who are not to be expected to report themselves cruel men." The forms of the Annual Returns presented to Parliament have varied from time to time. Until recently they purported to distinguish between painful and painless experiments. In the year 1878 the Inspector introduced prefatory remarks in regard to the pain inflicted on the animals under experiment, based upon information supplied by the experimenters. In that year out of 481 experiments, of which 451 were under licence or Certificates A or C, forty were stated to have caused pain, and in sixteen, all under Certificate A, the pain was considerable. It was found, however, that the attempts to divide experiments into painful and painless were most unsuccessful and the attempt has now been given 10640, etc. Byrne, 163. 129–30. Thane 1333–4, 1723.

up, the last Returns not even professing to make the distinction. The Returns now distinguish operative experiments from experiments under Certificate A, which dispenses with anæsthetics and under which, according to Dr. Thane, "no operation more severe than a simple venesection or inoculation is allowed." There appears to be no ground whatever for saying that the form at present in use was "put forward by the Home Office as a plausible assertion" that no operation causing pain was ever performed; but we deal elsewhere (Paragraph 86) with the cases in which pain may follow inoculation experiments.

(10) Mr. Coleridge next charges the Home Office officials "with placing a certain
Coleridge, 10645, etc. vivisector (Sir V. Horsley) year after year beyond the reach of the safeguards erected by the Act to protect animals from illegal treatment in private places, and thereby placing him beyond the possibility of legal inspection." The facts were as follows:—

In 1893 Sir Victor Horsley had a patient who was believed to be suffering from *filaria sanguinis hominis*, a worm which is said to discharge its embryos into the blood of the patient at night time only. It is a rare disease in England, and, at Sir Patrick Manson's suggestion, Sir V. Horsley was anxious to see if it could be inoculated into an animal for the purpose of further investigations. He applied to the Home Office for special leave to inoculate two monkeys direct from the patient, and, as laboratories are closed at night, he asked leave to perform the inoculation at the patient's house. Special leave was
Horsley, 15732-56. granted to Sir V. Horsley to inoculate two monkeys at the patient's house, specially reporting the results of his experiments to the Inspector. As a matter of fact these experiments were never performed at all, as the *filaria* did not appear in this particular patient's blood. The special permission remained unused and unrevoked for about seven years, as it was thought possible that another and more favourable opportunity might occur. It was then withdrawn by the Home Office.

(11) Mr. Coleridge charges "the Inspectors certainly, and the Home Office
Coleridge, 10656, etc. officials apparently, with having made no enquiries public or private, into the vital question of the character for humanity of the licensees to whom they have delivered over the animals to be vivisected." The Secretary of State when asked by Mr. Coleridge whether a guarantee of personal humanity was required of each investigator replied that if any instance of inhumanity on the part of a licensee was brought to his notice he would be prepared to act promptly in the matter. It appears to us that while every precaution should be taken to prevent the granting of a licence or certificate to anyone whom there might be reason to suspect of any lack of humanity, to require affirmative evidence of humanity in the case of every applicant for a licence or certificate would be futile and offensive. We deal further with this question in Paragraph 29.

(12) Mr. Coleridge finally charges "the Home Office officials with having placed
10675. themselves in improper private relations with a private society composed of supporters of vivisection entitled to no more consideration than the National [Anti-Vivisection] Society, composed of opponents of vivisection." Mr. Coleridge here refers to the Association for the Advancement of Medicine by Research. The practice of consulting that Association was, as has already been stated, instituted by Sir William Harcourt, when Home Secretary, in 1882. We have already explained in Paragraph 12 the constitution of this Association and how it came to be consulted by the Secretary of State. While we agree in thinking that it would have been well if the complete recommendations of the Royal Commission had been carried out in reference to the advisers to be selected by the Home Secretary, and to the publication of their names, we find no evidence of any improper private relations between the Home Office officials and the above Association. We deal with this question in our recommendations (Paragraph 122).

19. These twelve charges of Mr. Coleridge's are the outcome of a ten years' investigation of the Administration of the Act by the Department, conducted by an acute and
10256. indefatigable critic supported, as he told us, with ample funds. We have indicated the points in which we think that the administration of the Home Office may be open to criticism, but we are of opinion that, on the whole, the working of the Act has been performed with a desire faithfully to carry out the objects which its framers had in view.

20. In addition to the matters above dealt with, as part of his charge No. (2), Mr. Coleridge brought a specific allegation against the Home Office of callousness to animal suffering, and based it upon certain experiments which had been allowed, or not disallowed by the Home Office, and which have been investigated by the Commission. Mr. Coleridge,
10265. in his evidence, says:—

"I desire to bring before the Commissioners sixteen experiments performed by Dr. Crile in Sir Victor Horsley's laboratory, in one of which the foot of a dog was deliberately crushed under 'incomplete anæsthesia.' Apart altogether from the question of pain, this series of experiments, involving the most repulsive operations, in which every conceivable outrage is perpetrated upon bodies of the victims, have filled decent people who have faced their perusal with disgust and horror."

The facts as given in evidence were as follows: In 1895 Dr. Crile, an American lecturer on surgery in the University of Worcester, Ohio, applied to the then Home Secretary, Mr. Asquith, for a licence to perform experiments upon the causation and prevention of shock in surgical operations, and in his application he said: "I am informed, as a matter of fact, that the experiments I am about to perform do not come within the Act, inasmuch as they do not involve any pain to the animal." Accompanying that letter was the formal application for a licence. The experiments were described as investigation of shock in operations performed under ether, "the experiments proposed will be conducted under the licence, *i.e.*, the animals will be anæsthetised with ether and killed before recovery from same." The application for the licence was referred by the Home Secretary to the Association for the Advancement of Medicine by Research. They hoped that the application would be granted, and stated that at University College he would have Professor Victor Horsley's constant help. They reported that the occurrence of shock after surgical operations upon human creatures, in spite of anæsthesia and of all the precautions that can be taken, is a danger that cannot be too carefully studied, and the council therefore expressed the hope that leave may be granted to Dr. Crile to make these observations which would, in their opinion, be wholly free from pain as the animals would be killed while under the anæsthetic. The Home Secretary then granted a licence (without any certificate) to Dr. Crile, who proceeded to perform his sixteen experiments in the laboratory of University College, London. They consisted of the infliction of very grave injuries on dogs such as cutting out both cerebral hemispheres, tearing out the brachial plexus, crushing the foot before the corneal reflex was abolished, pouring boiling water on the intestines, etc. Sir Victor Horsley stated that certain of these grave injuries were purposely designed to reproduce as far as possible the injuries that occur by accident or in surgical operations. Thus he pointed out that crushing operations of the same kind were performed on human beings in the operation of tarsectomy, and that a jet of scalding steam was injected into a woman's womb in the treatment of inflammation or into the human liver to stop violent hæmorrhage. Sir V. Horsley witnessed about the first six or seven experiments, and he stated that in his opinion the animals were completely anæsthetized and not suffering at all. When he was not present, Dr. Goodbody, the Assistant Professor of Pathological Chemistry, attended, and Sir Victor Horsley remembers that in 1902 he stated that in his (Dr. Goodbody's) opinion the animals were quite unconscious of pain. In 1897 Dr. Crile published an account of these experiments and other experiments performed in America, and referring to his English experiments mentioned that in two cases the animals were under "incomplete anæsthesia." The Home Secretary's attention was called to this statement, and he communicated with Dr. Crile, who had returned to America. Dr. Crile replied: "I beg to say that in the entire series of experiments no animal suffered pain." Dr. Crile appears to have used the term "incomplete anæsthesia" in the sense in which certain anæsthetists use the term "light anæsthesia," *i.e.*, sufficient anæsthesia to prevent pain, though not sufficiently deep to abolish all the reflexes. Dr. Buxton thinks that Dr. Crile was using the term "incomplete anæsthesia" somewhat loosely and in a different sense from that in which he himself uses it.

Horsley, 15971. 15974. Coleridge, 10943. Horsley, 15721–8. Buxton, 12578-82.

With regard to these experiments, there was a difference of opinion among the eminent surgeons who appeared before us as to whether they were or were not justifiable. Mr. (now Sir Henry) Morris stated as follows:—

Q. (*Mr. Tomkinson.*) In your opinion were the results, from any point of view, at all commensurate with the severity of such operations on the observation of shock or the effect of shock?—*A.* I hardly feel that I can form a just opinion about that. I did not like many of the experiments that were performed, but I would not like to express a definite opinion. Morris, 8023.

You hardly feel prepared to justify them altogether?—*A.* No; and I would hardly be prepared to say that they were not desirable to have been made once, but I certainly think that such things ought not to be repeated for the sake of repeating them. 8024.

Professor Starling said:—

Q. (*Dr. Wilson.*) I am not going to ask you anything about Dr. Crile's experiments, but I may say as a medical man, admitting everything about anæsthesia that I read them with horror at the time, and even now I cannot see the justification for them; I will ask Professor Victor Horsley about that. You, of course, know them; do you contend that they are justifiable from your own point of view?—*A.* They are not experiments which I should have done myself. Starling, 4237.

Then personally you would not consider them justifiable?—*A.* I would not like to say that, because it is not a subject with which I am connected. I think you must ask the man in whose laboratory they were carried out. 4238.

and Sir Victor Horsley stated:—

Q. (*Dr. Wilson*) Then as some of those experiments were carried out in your laboratory, of course you would be perfectly prepared to carry them out yourself, if necessary?—*A.* Certainly. Horsley, 15853.

And, as you have said, you believed they were all painless?—*A.* Yes, absolutely. 15854.

No matter how ambiguous the language Dr. Crile used?—*A.* Quite so, no matter what his language. 15855.

And you are of opinion that more experiments even of this kind are still required?—*A.* Undoubtedly. 15856.

We express no opinion as to the usefulness of such experiments nor as to the desirabilit of repeating them, but we are of opinion that such experiments as were performe by Dr. Crile could not in any case be justifiable unless the animals throughout the whol experiment were fully and completely insensible to pain.

Mr. Coleridge further complains of the allowance of Dr. Schäfer's experiments o drowning. He says : —

Coleridge, 10264. "I may, perhaps, be permitted to say that the permission recently given to a vivisector to drowr resuscitate, and drown again dogs without any anæsthetics, which have been already alluded to here have been regarded by my Society with particular detestation."

Mr. J. Hughes, of the Canine Defence League, also drew attention to thes experiments.

In 1895 Professor Schäfer, F.R.S., carried out in connection with an enquiry by th Royal Medical and Chirurgical Society of London, a series of experiments for studyin what happened during death by drowning and the methods of resuscitating th apparently drowned. They were strongly recommended by the Association for th Advancement of Medicine by Research. These experiments were not witnessed b the Inspector, Sir J. Russell, but Professor Schäfer gives the following account of hi experiments :—

Schäfer 10104. "In this series of experiments, which were undertaken in order to determine exactly what happen during death by drowning, all except two (and the total number, I think, was thirty-six) were conducte under the influence of complete anæsthesia during the whole time of the experiments; but it was o the highest importance, that one could appraise the value of these experiments, to do a certain numbe of control experiments in order to observe whether, so far as could be determined, the phenomena woul be the same without an anæsthetic as with an anæsthetic. I therefore got permission from th Home Secretary to do ten experiments without anæsthetics, and I did two of them. The result which were obtained with those two showed so conclusively that the anæsthetic did not invalidat the object of the experiment that I left the other eight experiments, and did not perform them at all and these are the two to which I refer. In these two experiments the animals were simply drowne by being held under water and not allowed to recover at all; and the obvious phenomena, such as th pulse and respiration, were observed and a post-mortem examination was held in order to see whethe the post-mortem conditions were the same when they were drowned without any anæsthetic as wit an anæsthetic."

It is not the province of the Commission to give any opinion on the relative value of the various methods which may be employed for resuscitating the drowned but Professor Schäfer's method is alleged to have this advantage that it can b employed by an unskilled person and requires very little physical strength The certificate appears to have authorized Professor Schäfer to submerge and resus citate ten unanæsthetized dogs and this was the *gravamen* of Mr. Coleridge's charge Only two unanæsthetized dogs were, however, used and these were drowned withou resuscitation and, so far as we can judge, suffered no more pain than stray dogs tha are destroyed by drowning.

Miss Lind-af-Hageby.

21. Among the many witnesses we examined on behalf of the Anti-vivisection Societies there were, not unnaturally, few who could speak with any personal experience o experiments on animals. The most important witness was Miss Lind-af-Hageb

Lind-af-Hageby, 7186–94. who attended a series of demonstration lectures in connection with the Universit of London, and who in conjunction with Miss Schartau published an account of he

9630–1. experiences in a book called "The Shambles of Science." The fourth and revised edi tion of this book was published in May, 1904, and Miss Lind-af-Hageby in her evidenc before the Commission stated that she adhered to the views therein expressed At p. 13 she gives an account of an experiment she witnessed on a rabbit, as follows :—

"a rabbit, which after having inhaled some ether, was put in a freezing machine where a bi piece of ice had been put previously. The rabbit was forgotten and left too long in the box, and whe it was taken out after fifty-five minutes it was found to be beyond the stage for observation."

and on p. 14 refers to :—

"the rabbit, which has been crouching on the table since it was taken out of the freezing machin and put there, quite conscious, but frozen stiff like a piece of wood."

We identified this experiment as an experiment performed by Dr. Waller, F.R.S

Waller, 18020. He was called before us and his account of the experiment is as follows :—

Q. (*Sir M. Chalmers.*) Would you tell us what really happened ?—*A.* What happened was tha the rabbit was put in an ice-chest, a refrigerator, to lower its temperature, but the refrigerator wa not cool enough as a matter of fact, and the animal's temperature was not lowered.

18021. *Q.* Was it anæsthetised or unanæsthetised ?—*A.* It was not anæsthetised.* It was simply put int the refrigerator to show that its temperature did not go down as that of a cold-blooded animal would.

18022. *Q.* Nothing whatever happened ?—*A.* No. I have this memorandum "Minimum temperature = 8°." It was not low enough to act.

18023. *Q.* 8° Centigrade ?—*A.* Yes.

* But see correction in Dr. Waller's letter, Vol. vi, p. 20.

Q. What would that be Fahrenheit ?—A. I make out 46°. Waller, 18024

Q. (*Chairman.*) But what do you mean by its temperature ? Do you mean its internal temperature ?—A. That was the intention, no doubt. The temperature of a cold-blooded animal in a cooling box would go down; the temperature of a warm-blooded animal would not go down, or at any rate not so quickly. But there was no vivisection at all. 18025.

Q. I understand that a rabbit's natural temperature is a little higher, if anything, than man's ?—A. About 37° or 38° (about 99° F.). 18026.

Q. About the same. I do not understand your bringing it down to 45° ?—A. That is the temperature of the box, not of the animal. 18027.

Q. I was asking what you brought the rabbit down to ?—A. I cannot tell you. I have no idea. 18028.

Q. (*Mr. Ram.*) But there was no vivisection ?—A. There was no vivisection; there was no freezing of the animal. That is pure imagination. 18029.

Q. (*Sir W. Church.*) When she states: "It was found to be beyond the stage for observation," that leads the reader to think that the animal was dead. I understand from you that the animal was not dead ?—A. The description is: "The animal was taken out of the freezing machine quite unconscious,* but frozen stiff, like a piece of wood. With all signs of terror the animal springs back trying to get away, but half paralysed by the cold and half fascinated." My comment upon that is simply that all this is "impossible." 18030.

Q. (*Sir Mackenzie Chalmers.*) Was the animal never exposed to a lower temperature than 45° ?—A. No; the cooling box did not act properly, as a matter of fact. I remember that much; but the details about the animal I do not remember. I generally remember this description of an animal frozen stiff, and, with all signs of terror, springing back, and think to myself that was an imperfect description. 18031.

Q. (*Chairman.*) There must be some imperfection when they say at the same time, "frozen stiff like a piece of wood" and "the animal springs back" ?—A. I have simply got a joking comment of this as being an obvious impossibility. Then my general comment upon this lecture at the time was, "No vivisection was performed at this lecture (at which I was present)." 18032.

Q. Do you mean that none was performed at that lecture or that none was performed whilst you were present ?—A. I was present at the whole lecture, and no vivisection was performed while I was present. My comment continues, "One or two frogs were killed, and their hearts used for experiment." The lecture was on animal heat, and some animals hot and cold-blooded were cooled in an ice-chest, and their temperatures compared. That experiment was on December 3rd, and that is misdated again in "The Shambles of Science" as December 2nd, but that is simply a mistake of Miss Lind-af-Hageby's. It should have been the 3rd, according to my laboratory book, which I have here. 18033.

A careful examination of this evidence leads us to the conclusion that the experiment was designed to show the effect of a low temperature upon a warm-blooded animal,—that it was not intended to lower the temperature to a point which could cause appreciable pain,—that "the cooling box did not act properly," and that there was therefore in fact no freezing effect produced upon the animal by the experiment. We are compelled, therefore, to believe that Miss Lind-af-Hageby misapprehended what occurred and that her description of the state of the animal was erroneous. 18031.

On p. 27 of the fourth edition of the "Shambles of Science" Miss Lind-af-Hageby says:—

"We now see a marmot, the spinal cord of which had previously been divided by a vivisector. His finger was bleeding a little, and he tied his handkerchief around it, and looked a martyr. 'Are they not nasty animals ? You have to be careful of them,' said another vivisector who was engaged in cutting up mice. And the nasty, unscientific animal was accordingly removed to a safe place."

Dr. Pembrey informed us that no operation was ever performed on that marmot, and that the marmot in question lived for some two years after the alleged operation and then died a natural death. We were also informed that the body had been preserved and it was submitted to us in proof that the spinal cord was intact. The explanation of what Miss Lind-af-Hageby saw was given by Dr. Pembrey in the following words:— Pembrey, 13975–14017 14124–7

Q. (*Chairman.*) We know now that the spinal cord of this marmot has never been divided, you have produced the marmot. What was the condition of the marmot on that day of which Miss Hageby spoke ?—A. It was in a condition of hibernation, and it was waking up. I can quite easily explain why the mistake arose as to the paraplegia. The condition of the animal was that the hind legs were quite paralysed and drawn out behind the body. Instead of the animal walking like that (*describing*), it would be drawn out thus in a condition of paraplegia. This is mentioned in one of my papers, and there is no excuse for the statement that the animal's spinal cord was cut, because I can give you reference to the actual papers, in which I say that no operation had been done, and in which I also give the explanation of the paraplegia. On p. 80 of this paper of mine I actually explain the paraplegia (*handing in the same*). 14009.

Q. (*Sir W. Collins.*) When was the paper published ? A. In 1901, two years before that statement in "The Shambles of Science" was published. 14010.

It is obvious that this statement was founded on a misapprehension on the part of Miss Hageby, who mistook the state of paraplegia incident upon partial recovery from hibernation for the paralysis which would have been caused had the spinal cord been divided.

* By a misprint in the Evidence (Vol. IV. Q. 18030) the word "unconscious" was wrongly used; in the book, p. 14, the word is "conscious."

Mrs. Cook.

Cook, 1780–3. 22. Mrs. K. Cook, who stated that she was an authoress and journalist, and no expert, informed us that she herself had witnessed "some experiments at the Imperial Institute, but only baking and freezing." In her opinion they were cruel experiments,
1947–9. but we failed to elicit any particulars from her. She informed us that she could not tell us who performed the experiments, or the date of the experiments, saying that she could not answer any question with regard to them without looking the matter
2019. up, because she "regarded them as of very little importance." As far as we can ascertain the experiments referred to by Mrs. Cook must have been the experiments already referred to by Miss Lind-af-Hageby, and explained by Dr. Waller. Dr. Waller is at any rate quite clear that no painful experiment was performed.

Waller, 18164. *Q.* (*Sir M. Chalmers.*) Was any painful experiment performed ?—*A.* Oh, no, none was performed at any time, of course.

18165. *Q.* There was no experiment that would expose the animal to such a rise of temperature or such a fall of temperature as to cause real pain ?—*A.* No heating experiments are ever made in the laboratory. The only ones ever made were on reducing the temperature.

18166. *Q.* The baking is imagination ?—*A.* Pure imagination absolutely, and the freezing was never there. I notice the temperature of the ice-chest at the time is noted as 8° Centigrade (about 46° Fahrenheit).

18167. *Q.* And no animal was exposed to a temperature below the freezing of Fahrenheit ?—*A.* No, certainly not.

As we have before stated, we are satisfied that in this case no cruelty took place nor was it shown that any pain was sustained by the animal.

Lieut.-Colonel Lawrie.

Lawrie, 16815–21. 23. We now come to the evidence of Lieut.-Colonel Lawrie, I.M.S., who is himself an experimenter, and whose evidence gave rise to a somewhat serious issue of fact. He came before us first on November 20th, 1907, and described a cross-circulation experiment which he had witnessed in the Cambridge Laboratory in 1894. Two dogs were used for the experiment and the necks of the animals were dissected and the large
16869. blood vessels connected. This experiment, in Colonel Lawrie's opinion, was performed without anæsthetics, the only attempt at an anæsthetic according to him being a ten-drop dose of morphia solution, this dose being equal to one-twelfth of a grain of solid morphia. It is obvious that if Colonel Lawrie is correct, this experiment constitutes a gross violation of the Act. We therefore made careful enquiry as to the conditions under which the experiment was performed. The experiment in question was devised by Dr. Gaskell, F.R.S., a member of this Commission, with a view to test the effect of the action of chloroform on the heart, but the actual experiment was carried out by Dr. Shore. Dr. Shore's evidence is as follows :—

Shore, 19458. "In each of these two experiments we had two dogs. Of the two dogs, one dog received 10 cubic centimetres of a 2 per cent. solution of hydrochloride of morphia, and the other dog 6 cubic centimetres
19459. of the same solution." "A 2 per cent. solution of morphia contains 30 grains in the 100. As the dog
19460. had received 10 cubic centimetres, it therefore had 3 grains of morphia, not one-twelfth of a grain, as stated by Colonel Lawrie, and the other one, which, as I have said, received only 6 cubic centimetres of the solution, had received 1·8 grains."

19463. *Q.* (*Chairman.*) And that dose which was given was a completely full and satisfactory dose ?—*A.* Yes. I see in the statement that Colonel Lawrie implies that no chloroform was given until the test (for the action of chloroform on the heart) was purposely made with the chloroform. That is not the case. In addition to the morphia, chloroform was given to the animals before the skin was cut, and during the cutting operation the A.C.E. mixture, just as it was required.

Lawrie 20967, etc. Colonel Lawrie again came before us as a witness on March 25th, 1908, and amplified his statement as to the operation he had seen performed on July 7th, 1894. He said that when he saw the dogs they had been prepared for the operation by dissecting their necks, and that Dr. Gaskell told him that the dogs had had no anæs-
21001, etc. thetic except morphia, and that Dr. Gaskell led him to understand that they had had it "so as to be able to say that the dogs had had an anæsthetic," so as to hoodwink the Inspector, and that the anæsthetic consisted of ten drops of morphia solution. Dr. Gaskell denies this.

Q. (*Chairman.*) If it was only that memories might be mistaken about whether you said 10 drops or 10 grains, that would be a very different matter; but he goes on to say what is more important. Did you
Gaskell, 21718. say to him that the morphia had been given not to prevent pain, but in order to hoodwink the inspector ? —*A.* I have told you I have not the faintest remembrance of what I did say, but I am absolutely certain that I never said that. I should never have dreamed of saying it ; and it is such a silly thing to say.

Lawrie, 20986–92. The dogs, according to Colonel Lawrie, appeared to be in pain, and showed pain by
21029–32. shivering. Colonel Lawrie stated that he said nothing to Dr. Gaskell at the time
21089, etc.

because he could not interfere with what went on in another man's laboratory, and Lawrie, 16811–21,
that being in the Indian Service he was not aware of the provisions of the English Act. 16870, 21048,
After hearing this evidence we recalled Dr. Shore, who informed us that on July 21056, 21149.
7th he performed the experiment referred to by Colonel Lawrie. In these dogs a
solution of morphia had been injected to the amount of 3 grains in one and 1·8 Gaskell,
grains in the other, but the evidence is not clear as to whether such dose alone would 19458–60, 21379,
cause complete anæsthesia. In this particular experiment A.C.E. mixture was also 21735–7.
administered for a while, and this we were informed supplemented the action of the Shore, 21550–2.
morphia. Inasmuch as the object of the experiment was to test the effect of chloro- 21415.
form it was necessary that the dogs should not be deeply under chloroform 21447.
at the time but that the anæsthesia should be chiefly that of morphia. We 21479–85.
were, however, informed that the animals were tested throughout the whole experiment 21485–9. 21412,
from time to time to see if there was any consciousness, and that they exhibited 21424, etc.
no sign of consciousness. Colonel Lawrie, as before stated, made no protest to Dr. Shore. Hall, 21581, etc.
Dr. Shore's evidence is confirmed by Mr. Hall, the laboratory assistant, and also by
Dr. Gaskell. For Colonel Lawrie's satisfaction the same experiment was performed Gaskell, 21705,
again on two other dogs on July 11th, 1894, and according to Dr. Shore under exactly etc.
similar conditions. On that occasion both Dr. Anderson and Sir Clifford Allbutt were
also present. Sir Clifford Allbutt, owing to illness, was unable to give evidence
before us, but Dr. Anderson, who witnessed the experiments, came before us and stated
that he was perfectly certain that the dogs were anæsthetized, that they were under Anderson,
morphia and that they were absolutely unconscious. 21636, etc

After a full comparison of the evidence in this case, of which the above is a summary, we can only come to the conclusion that Colonel Lawrie was mistaken as to the quantity of morphia injected, and that since his statement as to the suffering of the dogs is contradicted by the other persons present, he may well have been mistaken in this matter also.

Mr. Graham.

24. There was one further experiment as to which evidence was given before us and
to which we desire specifically to refer. It was an experiment by Sir T. Lauder Graham, 5836.
Brunton, and is thus described by Mr. J. W. Graham, who appeared as a witness on
behalf of the Parliamentary Association for the Abolition of Vivisection. He says
that Sir T. Lauder Brunton

> "slowly heated, or shall we say baked, rabbits to death, and along with Dr. Theodore Cash did the 5844.
> same to cats, the operation lasting from three to five hours." . . . "The object of the research was to 5845.
> ascertain the action of digitalis with reference to the pulse. The temperature of the rabbits was raised
> to 111° and 113° F. in one series of cases, and to something a little less in another."

For this, Mr. Graham quotes the *Practitioner*, Vol. 33, p. 273, and asserts that the
animals were confined in a kind of stove. Sir T. Lauder Brunton, F.R.S., in his 5844–8.
evidence before us, gave the following explanation of the experiment, which was 6786–95.
performed in 1884. The animals were narcotised by a large dose either of chloral
or opium injected subcutaneously and were placed on a kind of covered bath filled with
hot water. A model of the bath was produced before us. It is a closed hot water
tin, with a depression at the top for the animal to be laid upon, the animal being in
no way confined or enclosed, but simply laid upon it. Cotton wool was placed between
the metal of the bath and the animal. The external temperature was raised to 106° F.,
which in some cases had the effect in the case of the narcotised animal of raising the body
temperature to 111°, 112° or 113° F. To make respiration easier a cannula was intro- 6787.
duced into the trachea, and in some experiments the inspired air was passed over warm
water so as to warm it and saturate it with the moisture, and lessen the loss of heat
from the lungs. The only other operation done was to put an ordinary sewing needle
between the ribs and attach it by means of a thread to a lever so as to record the 6791.
movements of the heart, in order to see how quickly the heart was beating in accordance
with every rise of temperature in the animal. The object of the experiment was
stated to be to find out what the condition of the heart was in what is known as hyperpyrexia, and to imitate the conditions of a patient suffering from high fever and lying in bed. The object of the experiment was, according to Sir Lauder Brunton, to some extent attained, and he informed us that the animals were absolutely free from pain and in no case were allowed to recover consciousness, and that if any pain had been inflicted it would have tended to vitiate the experiment.

Conclusions.

25. After careful consideration of the above cases we have come to the conclusion that the witnesses have either misapprehended or inaccurately described the facts of the experiments.

Since the passing of the Act no fewer than ten different Home Secretaries have been charged with the responsibility of granting licences, disallowing or not disallowing certificates and controlling the conduct of those to whom such licences were granted, viz., Sir R. A. (now Viscount) Cross, the late Sir W. Harcourt, the late Mr. Childers, Mr. H. Matthews (now Viscount Llandaff), Mr. H. H. Asquith, the late Sir M. W. (afterwards Viscount) Ridley, the late Mr. C. T. (afterwards Lord) Ritchie, Mr. Akers Douglas (now Viscount Chilston), Mr. H. (now Viscount) Gladstone and Mr. Winston Churchill. We deal immediately (Paragraphs 28 and 29) with two cases requiring especial consideration but in our opinion the Act has been administered by each and all of the Home Secretaries and by their subordinates with good faith and with careful consideration of its terms.

26. So far as we can judge we believe that holders of licences and certificates, with rare exceptions, have endeavoured with loyalty and good faith to conform to the provisions of the law.

27. We desire further to state that the harrowing descriptions and illustrations of operations inflicted on animals, which are freely circulated by post, advertisement or otherwise, are in many cases calculated to mislead the public, so far as they suggest that the animals in question were not under an anæsthetic. To represent that animals subjected to experiments in this country are wantonly tortured would, in our opinion, be absolutely false.

Pembrey, 14083.

28. The evidence of Dr. Pembrey calls for special remark. He propounded to the Commission a theory of his own to the effect that pain from the physiological point of view is a protective mechanism, and is in that sense beneficent, and that therefore the modern idea of trying to abolish all pain is absolutely absurd. On the other hand
14067. Dr. Pembrey stated that while he thought it right to inflict pain on animals he thought it not right to inflict unnecessary pain, and he claimed to be the judge of what was painful
14070. or not. He stated that he had performed painful experiments upon animals both in Germany and in this country, because he regarded them as absolutely necessary. He mentioned such experiments as the transfusion of blood, performed in Germany, and the destruction of rats by sulphur dioxide performed in this country. Indeed
14119-20. he deprecated the frequent employment of anæsthetics alike in vivisection of animals, in surgery and in midwifery. He considered that their use was often liable to introduce
14089. complications into an experiment, and that it would be wiser to allow some operations
14071. for which anæsthetics are now required to be performed without anæsthetics. He explained that in his opinion if an animal is bound down on its back it often passes into a condition of hypnotism, and that in that condition anæsthetics could be dispensed with, and from such experiments made by him in Germany he held that animals so treated do not appear to feel pain even without an anæsthetic. We think that Dr. Pembrey's application of a theory of pain as a protective mechanism in the scheme of nature to the case of painful experiments on animals led him into a position which is untenable, and in our opinion absolutely reprehensible, and we dissent entirely from the view that hypnotism should be regarded as a substitute for an anæsthetic in animal experimentation.

Cook, 1919, 2064-7, Graham, 5883, Coleridge, 10656-74, 10852-78,

29. Our attention has also been repeatedly directed to statements made by Dr. Klein before the former Royal Commission in 1875. Reference was made to the evidence formerly given by him and to the fact that he has nevertheless since held a licence and certificates, and holds them still and has done much work for Government departments.
Kekewich, 20450. Dr. Klein did not appear before us, being unfortunately prevented from doing so by ill health. We think that it is not within our province to deal with evidence other than that given directly before us, and we have no means of knowing whether Dr. Klein still adheres to his earlier views; but it appears to us that to grant a licence or certificates to any person holding such views as those formerly expressed by Dr. Klein, and as those entertained by Dr. Pembrey, is calculated to create serious misgiving in the minds of the public.

PROGRESS OF SCIENCE AND RESULTS OF EXPERIMENTS ON ANIMALS.

General Considerations.

30. We next proceed to consider generally :—

(*a*) The progress of medical science in recent years ; and

(*b*) Whether valuable results have been attained by the practice of experiments upon animals.

These two portions of the enquiry can most conveniently be considered together as they are intimately related.

For the purpose of this enquiry the term experiment may be taken in its ordinary acceptation to mean an act or operation designed to discover some unknown truth, principle or effect, or to establish it when discovered by varying at will the combination of circumstances and observing the result.

31. Biology, or the science of living things, and the applied sciences of medicine and surgery which spring from it, are advanced like the other sciences by observation and experiment. By these two processes the discrimination of cause and effect is sought, and new truth discovered. The distinction between these two processes has been thus stated, "observation is finding a fact, experiment is making one." The difference however is one of degree rather than of kind and is not fundamental. The value of the fact depends on what it is rather than upon the mode in which it was obtained. Some sciences, from the nature of the facts with which they deal, are more purely observational, such as astronomy, others like chemistry are largely dependent upon experiment. The more recourse can be and is had to artificial variation of the conditions at will in the obtaining of knowledge the more experimental is that science.

The sciences of physiology and pathology on which the progress of medicine and surgery mainly depends have been built up partly by observation and partly by experiment. Inasmuch as the subject matter of physiology is living things, the question naturally arises as to whether that science is not *a priori* likely to be advanced by experiments on living animals, and it is necessary to enquire whether, in fact, the history of physiology does or does not show that it has been thus advanced.

32. An important consideration at once confronts the advocate of the experimental method in physiology which is absent in the case of an enquirer in the kindred sciences of physics and chemistry, viz., the fact that some experiments on animals are, in the absence of anæsthetics, very painful. Not only is this element of pain liable to introduce a complication into physiological experiment, but its infliction at once introduces moral or ethical considerations which have no counterpart in the scientific pursuits of the chemist or the physicist.

Reserving for the present the ethical or moral aspect of the question, and confining ourselves exclusively to the question of the value to physiology, pathology and the cognate sciences of the practice of experimenting on living animals by vivisection or otherwise, we proceed to examine the voluminous evidence laid before us on this aspect of the question.

33. Among the eminent representatives of learned bodies who have favoured the Commission with their advice, many have laid especial stress on the value of the experimental method to science, regarding medicine simply as one of the sciences. Thus, the President of the Royal Society stated to us that in the opinion of the Council Rayleigh, 5532.
of the Society

> "There could be no doubt that the main cause of the remarkable development of science in modern times has been the adoption of the experimental method of investigating nature and . . . . that in no branches of investigation have the theoretical and practical successes of experimental work been more conspicuous in recent years than in physiology and its practical applications in medicine and surgery,"

or, as Professor Starling observes :—

> "Just as the mechanical sciences, when viewed from a broad standpoint, represent man's struggles for Starling, 3442.
> the control of the energies available in his environment, so the medical sciences have, as their ultimate aim, the acquisition of control over the functions of man's body."

Similar evidence as to the value of the experimental method to medicine, and as to the necessity of regarding medical science in the same light as, and studying it by the same method as the other sciences, was given by Dr. Taylor on behalf of the Taylor, 5585.
Royal College of Physicians, by Professor Schäfer on behalf of the Royal Society of Schäfer, 9991.
Edinburgh, by Sir Lauder Brunton, and by many other witnesses. Brunton, 7082.

It is clear that a knowledge of the processes of the healthy body is to be regarded as essential to the understanding of the processes of disease and for the control of this condition by treatment. It is maintained that research in physiology, *i.e.*, the science of the workings of the healthy body, should be encouraged, not only by reason of immediate practical advantage to mankind to be gained by any given research, but on the ground that any advance in science may acquire at one time or other a practical significance and will ultimately contribute directly to the welfare of mankind. It is therefore contended that the effect of advance in physiology on medicine is twofold. In the first place any increase in the knowledge of the healthy body alters in some respect or other the conception of the diseased processes, and in various ways may affect indirectly the treatment of this condition. In the second place it is maintained that experiments made simply for the advance of physiological knowledge have sometimes had as an immediate result a direct bearing on the treatment of disease. Evidence has been furnished to the Commission with a view to substantiate both these claims.

34. (A) *The Influence of Experiment on the Advance of Physiology as a Pure Science.*—Scientific witnesses have insisted on the necessity for experiment in the advance of physiological knowledge. More than one witness has quoted the evidence given by Mr. Darwin before the Royal Commission of 1875. Mr. Darwin stated that although he had never himself either directly or indirectly practised experiments on living animals, yet he was fully convinced that physiology can progress only by the aid of experiments on living animals. He could not think of any one step which had been made in physiology without their aid. Surmises there might be as to the circulation of the blood formed from the position of the valves, in the veins and so forth, but certainty, he held, could in the case of physiology be arrived at only by means of experiments on living animals. Professor Starling, regarding physiology "as the
Starling, 3442. science of the workings of the living body," considered that "to those who have studied physiology, this statement of Mr. Darwin must be self-evident." He contended that it would be impossible to imagine any rational system of medicine which was not founded on our knowledge of the animal functions as gained in this way. Sir Lauder Brunton maintained that the interpretation of the heart sounds which
Brunton, 6936–8. are so important in the diagnosis of cardiac disease and the whole treatment of heart disease is dependent on the results of experiments on animals. It has also been claimed that the treatment of disorders of digestion is determined by the physiological knowledge gained by the experiments of Bernard, Bidder, Schmidt, Pawlow and others, and that even when medical men have not succeeded in determining the full causation of a disease, as in the case of diabetes, the treatment of this disease has been improved by the application of knowledge gained from the experiments of Bernard, Schiff, Pavy and others.

No witnesses attempted to place before the Commission a full record of the advance in physiological knowledge during the last thirty years, most having confined themselves to dealing with those instances in which it has been claimed that a gain to mankind in the treatment of disease has been the direct or indirect result of experiments upon living animals. Of such instances the Commission have received many examples.

35. (B) *Physiological Discoveries which are stated to have been of Immediate Advantage in the Practice of Medicine and Surgery.*—Under this heading attention may be confined to those discoveries which are said to have resulted—so to speak, accidentally—in the course of an investigation whose object was purely physiological, *i.e.*, directed to the advance of knowledge generally, and not to the increase of knowledge of, or the control of any particular disease. Professor Starling argues that
Starling, 3442. since physiology represents the basis on which medicine, with the aid of observation on man and on cases of disease, must be built up, it is evident that in most cases it would be impossible to quote such and such an improvement in treatment as the result of any given research. In a considerable number of cases, however, he claims that it has happened that a physiological fact has been capable of immediate utilisation in treatment, just as the purely physical researches into the nature of the Cathode rays resulted in the discovery of the Röntgen rays with their manifold applications in practical medicine.

Thus, adrenalin, which is much used by surgeons for the control of hæmorrhage and congested conditions generally, was discovered by Oliver and Schäfer in the course of a research on the physiology of the suprarenal glands. The treatment of poisoning by carbon monoxide, a constituent of coal gas, as well as of the "choke damp" resulting from explosions in coal mines is attributed to the work of Dr.

Haldane on the compounds formed between the colouring matter of the blood and certain gases. The work of this observer and of Mr. Leonard Hill on the influence of variations in the pressure of the air breathed are held to have confirmed the view that the blocking of blood vessels by bubbles of gas absorbed by the blood when under pressure is the chief cause of the serious symptoms of Caisson disease. Rules based on these conclusions are now in operation in the Diving Department of the Royal Navy.

According to Sir Victor Horsley "there is not a single function of the nervous system, the principle of which we know, which is not derived from experiments on animals," and as the result of discoveries made since 1870, "we are now able to cure epilepsy, we are able to cure abscess of the brain, and we are able to cure tumours of the brain." Similar claims were advanced by the same witness in regard to operations on the spinal cord. Horsley, 15680. 15684.

It is admitted that a full understanding of the nature of the morbid condition known as diabetes has not been arrived at by the medical profession; at the same time it has been stated to us that the treatment of the disease and especially of the coma which is apt to complicate its later stages has been due to the results of physiological experiments. Brunton, 7124–6. Starling, 3813–7.

The evidence of Sir Henry Morris, who had not himself practised vivisection, is to the effect that the knowledge gained by physiologists in regard to operations on the viscera, the kidneys and the internal organs of animals, led surgeons to undertake experiments or operations on those organs with direct reference to the alleviation of diseased conditions in man. Similarly, it has been urged by Professor Langley that the suture of divided nerves as a department of surgery practically owes its existence to experimental results obtained by physiologists in their pursuits of knowledge as to the functions of different nerves.

36. Important as these examples are as instances of the claim that physiology is a necessary foundation for medical knowledge, and that advances in medicine during the last thirty years have been largely determined by the advances in the science of physiology, the larger portion of the professional evidence we have examined has been directed to advances in medicine and surgery attributed to the direct result of application of the experimental method of investigating morbid conditions. On this point the Commission have had the advantage of hearing the evidence of the President of the Royal College of Physicians of London, of the Presidents of the Royal Colleges of Surgeons of England and Ireland, of the Regius Professor of Medicine in Oxford, of many distinguished pathologists, as well as of those who have been sceptical of the advance alleged or who have combated the results which have been claimed. In dealing with this evidence it will be convenient to divide it into the headings of Medicine and Surgery, reserving for later consideration the question of the infectious diseases in the knowledge and prevention of which great advances have been made within the last generation.

37. (1) *The Benefits to the Practice of Medicine claimed as the Result of Experiments on Living Animals.*—The successful practice of medicine—the healing of the sick—depends in the first place on the recognition and location of the disorder, in the second place on the knowledge of its causation, and in the third place on the possession of effective means in the shape of drugs, diet, etc., for combating the abnormal conditions and restoring the patient to health.

In many cases when a full control of the disease has not been acquired, either by reason of lack of knowledge or by reason of the nature of the disease rendering restoration impossible, the practitioner may still have it in his power to relieve symptoms and alleviate suffering. The practice of medicine depends on physiology—the science of the normal functions of the body,—on pathology—the science of disease,—and on pharmacology and therapeutics—the sciences of the action of drugs, etc., in health and disease. Witnesses of eminence and experience have testified to the necessity of experimentation on animals in building up these basic sciences. Thus, Mr. (now Sir W. H.) Power, though not a licensed vivisector himself, stated the opinion of the Local Government Board to be, in the words of Mr. (afterwards Sir John) Simon's minute, that :—

"Experimental studies of disease are of the utmost importance in the progress of medicine, that indeed such progress must at present in large proportion depend on them is certain." Power, 4299.

Sir Lauder Brunton stated that the power of dealing with disease has increased incomparably since he was a student, and that he attributes such increase not to clinical observation, but to experiments on animals. Brunton, 6925, 6927–32.

Dr. Taylor, speaking on behalf of the College of Physicians said :—

Taylor, 5585. "So far as experimentation on animals is concerned, we feel very deeply the absolute necessity of such observations being made in order to provide us with the means of dealing with disease in an efficient way. Much, of course, can be learnt by observation; that is what we are struggling with every day in our clinical observations in hospitals and amongst our private patients, but it must be helped by experimentation."

Powell, 5583–4. Sir Douglas Powell, President of the Royal College of Physicians, had not himself practised vivisection, but referred to instances in which, since the last Royal Commission in 1875, bacteriological experimentation had, in his opinion, led to the successful treatment of disease; notably in the case of diphtheria, anthrax, swine fever, and the improvements in prevention of sepsis associated with the names of Pasteur and of the late Lord Lister. Other discoveries attributed by various witnesses to vivisectional experiments are those of the circulation of the blood, of the causation of the cardiac sounds, of the functions of the thyroid gland, of the treatment of cretinism and myxœdema, of the production of fever, of the causation of diabetes and
5684–94. diabetic coma, of the causation of epilepsy, of the localisation of cerebral disease and of the distribution and functions of the nerves. Several witnesses called special attention to the disease known as myxœdema whose causation has been made clear in recent years, while a very successful mode of treatment based on the ascertained cause
Osler, 16625–35. has been adopted. Professor (now Sir William) Osler pointed out that as a result of knowledge attained partly by clinical observation and partly by experiments on animals, it has been found that this disease is connected with absence or disease of the thyroid gland, and that by administration of a preparation from the gland of the sheep cure may be effected. By this disease, as Sir William Osler says :—

16569. "A woman may be reduced to a condition of dementia, to a simple frog or toad-like caricature of her former self, and in a hopeless, helpless condition. Within six months that woman may be perfectly well, and she stays well."

Cushny, 4664–5. The treatment of diseases in many cases consists in the avoidance of the conditions which are the cause of the disease, and is, therefore, determined when the latter has been ascertained. In a large number of cases the treatment consists in the administration of drugs. Very full evidence on the subject of drugs has been laid before us by Professor Cushny, Sir Lauder Brunton, Professor Fraser and Professor Dixon.

During the last forty years many new drugs have been introduced, and knowledge of the action and indications for administration of drugs previously in use has been largely augmented. Thus, it has been pointed out to us that although digitalis has been regarded for many years as a remedy in heart disease the indication for its administration was not properly understood. According to Professor Cushny, as the result of experiments on animals by Sir Lauder Brunton and Professor Schmiedeberg, definite indications for the use or disuse of digitalis in cases of pneumonia
4665. have now been ascertained, and he added :—

"This example of the influence of experimental research in therapeutics might be repeated in regard to dozens of drugs were it necessary."

Sir Douglas Powell and Dr. Taylor, however, stated as follows :—

Powell and Taylor, 5704. (*Sir W. Collins.*) We were also told by a previous witness that by experiments on animals definite rules had been obtained, so I gathered, for the administration of digitalis in cases of pneumonia. Would you kindly give us your opinion as to that ?—(*Sir Douglas Powell.*) I am not aware of those experiments. I should think that the use of digitalis has been largely established by means of experiment, unquestionably; but I am not aware of any experiments which have established the usefulness of digitalis, especially in pneumonia. (*Dr. Taylor.*) Experiments on animals, I believe, are used for standardising digitalis and similar drugs, but I should agree with Sir Douglas Powell. I have no experience of any special experiments on animals directed to the treatment of pneumonia by digitalis.

Of drugs introduced during the last forty years, it was stated that one only, namely pilocarpin, was introduced as a result of clinical observation; all the rest are stated to have been introduced as a result of animal experimentation. Among these new drugs may be mentioned :—

(1) Soporifics such as chloral, sulphonal, veronal.

(2) Local anæsthetics such as cocain, eucain, stovain.

(3) Analgesics and antipyretics, such as antipyrin, antifebrin, phenacetin, exalgin.

(4) Physostigmin (or eserin) derived from the calabar bean, which, by its action on the pupil relieves the painful disease of the eye known as glaucoma.

(5) Amyl nitrite and other nitrites employed for their properties in dilating the blood-vessels in the painful disease of angina pectoris.

(6) Diuretics such as caffein, theobromin and diuretin, which increase the secretion of urine, and urotropin employed to disinfect the urinary tract.

It has also been stated that animal experimentation is necessary for the standardisation of certain drugs with a view to determining the precise strength and physiological effect of any dose of any particular drug.

38. (2) *The Advances in the Practice of Surgery claimed as the Result of Experiments on Living Animals.*—The great advances made in the practice of surgery during the last forty years are universally recognised. The President of the Royal College of Surgeons informed us that:—

"Many operations that used to be looked upon as too dangerous to be undertaken except under stress of necessity or as offering a last chance of saving life, are now performed with safety; many new and important ones, which were formerly never dreamed of, have now become matters of successful and almost daily performance. Finally, whereas the mortality, even after quite simple operations, was at one time so appalling that patients were often deterred from submitting to and the surgeon from performing them, operations of considerable magnitude are now undergone with the confidence which Morris, 7715. comes from knowing that the mortality is, in many instances, but a fraction of a unit per cent. To-day, the abdominal cavity of a man is opened, and every organ within is inspected and palpated for operative purposes. The wound is closed and the patient recovers from the effect of the anæsthesia and takes a complete and uneventful course towards convalescence. This course is very commonly without pain, without mental shock or distress, without discomfort of any kind beyond some anæsthesia sickness, some flatulence and the weariness of lying still. Within ten or fourteen days after such an operation the patient has generally quite recovered."

Sir Victor Horsley referred to the absolute disappearance of pyæmia and blood Horsley, 15669. poisoning from University College Hospital, due, in his opinion, wholly to the introduction and proper execution of antiseptic surgery, owing to Lord Lister's teaching. The great improvement in military surgical practice is, according to Sir Henry Morris, evident from comparison of the results of the practice in the Hiroshima Hospital during the Japanese War, 1904–5, with the results obtained by surgeons in the Franco-Prussian War in 1870. In the former case the ratio of deaths and invalided was a little above 1 per cent., Morris, 7745–9. and almost all the wounds of the soft tissues healed within ten days, whereas "Nélaton, the famous French surgeon, in despair at the high death rate attending almost all operations on the wounded, declared that he who would conquer purulent infections would deserve a golden statue."

39. This complete transformation of the results of surgical practice has been attributed to the following causes: (*a*) The antiseptic or aseptic method, (*b*) the introduction of anæsthesia, (*c*) improved technique with regard to ligatures, sutures, etc., and greater knowledge in reference to the extent to which the various internal organs of the body can withstand surgical interference.

Dealing with the first two factors as being of the greatest importance, and as affording the necessary conditions for the working out and the success of the improved technique, we proceed to enquire how far this enormous improvement in man's power over diseased conditions is to be ascribed to experiments on animals.

40. (*a*) *The Antiseptic and Aseptic Methods.*—Sir Henry Morris, in tendering evidence on 7656. behalf of the Royal College of Surgeons of England, dealt with facts and discoveries which had occurred since 1876. He deemed experiments on living animals an essential 7660–2. mode of investigation, while fully admitting that other modes of research are employed and that it is impossible to separate the advantages derived from each particular source. Sir Henry Morris referred to the evidence of Lord Lister before the Royal Commission of 1875, wherein he stated that he could not have made his way in the sub- 7664. ject of antiseptics without the assistance he had derived from experiments on the lower 7837–40. animals. Sir Henry Morris stated that Lord Lister had been led to regard the infection of wounds as due to putrefaction of the discharges, and following Pasteur he traced putrefaction to the presence of bacteria and sought for means to exclude them from wounds. Sir Henry Morris, in answer to questions, deemed it necessary for further 7778–9 experimental investigation to be made on the lines of the discoveries of Lord Lister and Pasteur, and further stated that Professor Bastian had recently again contested the alleged conclusions of Pasteur as to spontaneous generation. 7843–4.

Lord Lister, in his earliest method, attempted to exclude all such organisms in the following ways. The skin of the patient and the hands of the operator were cleansed by strong antiseptic lotions. The instruments were sterilised by soaking in similar fluids and the access of germs to the wound from the air during the operation was to be prevented by means of a fine spray of carbolic acid solution constantly playing on the wound. In order to exclude the air, the dressings of the wound, which were also soaked with Bantock, 14745. antiseptics, were sealed by some impervious material. In consequence of further re- 14758. searches and experience some modification of the technique first introduced by Lord Lister occurred, and the evolution of the aseptic method resulted. It was recognised that the number of pathogenic micro-organisms contained in ordinary air was insignificant in comparison with the numbers on the skin of the patient or on the hands of the opera-

tor, so that it became possible to omit the carbolic spray since its use was founded on a theory which was physically impossible. The irritating effects of the various antiseptics on the tissues were found to interfere with the processes of healing, and in some cases to give rise to symptoms of poisoning in consequence of their absorption into the system. Their use was, therefore, confined to the cleansing of the patient's skin and of the surgeon's hands. It was found that exposure to boiling water was an infallible means of sterilisation, and this method was, therefore, introduced for the same purpose in the case of instruments and dressings instead of the soaking in antiseptic solutions which had been previously employed. At the same time scrupulous means are now taken for the avoidance of any accidental contamination of the wound. The operation is carried on with as scrupulous care and with the same technique as the bacteriologist employs when he is isolating one organism from another, and the operation being finished the wound is closed, covered with aseptic dressings, and the processes of repair are observed to proceed without check, so that on removing the dressings at the end of ten days, complete recovery is often found to have taken place. Certain witnesses have emphasised the distinction between the antiseptic and the aseptic methods, and have pointed out that the late Mr. Lawson Tait and others successfully used what is now known as the aseptic method at a time when most surgeons were using the antiseptic method, but both methods are clearly traceable to the recognition of the need of cleanliness in surgery, and neither can be dissociated from the work of Lord Lister, as founded on Pasteur's researches on the origin of putrefaction and septic disease. The spray which formerly was regarded as necessary is now an abolished thing. Aseptic surgery has superseded antiseptic surgery. Subsequent experiments by Koch led to the distinction between pathogenic and non-pathogenic bacteria and the recognition of the part played by chemical products of bacteria in the causation of disease. It is also true that Semmelweiss before 1850 had attributed the blood-
Bantock, 14761. poisoning then so common after child-birth to putrid infection, and had urged cleanliness in the lying-in room as the means of preventing it. The importance of his doctrine, though not generally admitted during his lifetime, alike in surgical wards and in maternity departments, had not escaped the attention of his contemporary Haller of Vienna.

Horsley, 15669. The pain of the operation itself under former conditions was only part, and often the smaller part of the total pain suffered by the patient. So long as suppuration regularly attended the healing of wounds, even a simple case like the removal of a breast for cancer, or the amputation of a limb might require painful dressings for several weeks. At the present time only one or two dressings are usually requisite pending the healing of the wound in the course of a fortnight The pain of an aseptic wound, which does not usually last longer than about forty hours, is in the words of Sir Victor Horsley, "a perfectly tolerable pain." After that, in the healing, there is little or no pain. Aseptic surgery has, in this way, saved an amount of pain which is incalculable.

41. (*b*) *Anæsthesia.*—The elaborate precautions against microbial contamination which form such an essential part of modern surgical practice would be of limited application if operations had to be performed on a sentient patient, and, therefore, with the rapidity which was aimed at by the older surgeons. The introduction of anæsthesia not only
Morris, 7891–2. rendered it possible for the surgeon to undertake more extensive operations, but also allows him to proceed leisurely about the task and to devote his whole attention to the observation of all the minute details which make for success. For the production of anæsthesia during an operation the anæsthetics usually employed are chloroform or ether. The discovery of anæsthetics owes nothing to experiments on animals. Thousands
Buxton, 12539. of experiments have, however, been made on animals with a view to elucidate the mode of action and the methods of preventing danger from the use of anæsthetics. No general anæsthetic appears to be entirely free from risk. A number of deaths still occur in each year as a result of the administration of anæsthetics, deaths which it is hoped may be prevented by a more exact knowledge of the action of these substances.
12457. Dr. Dudley Buxton stated that although the actual discovery of anæsthesia was not due to experiments on animals, our knowledge of the precise action of anæsthesia is entirely due to such experiments, and evidence was presented by Sir V. Horsley and by Dr. Waller intended to show that, as a result of such experiments, certain methods had been introduced for the administration of chloroform free from the risks attendant on administration in the way which has been usually employed. These witnesses anticipated that by the use of improved methods there would be a considerable reduction in the number of cases of death from the anæsthetic itself. The risks, and certain other disadvantages attendant on the use of respirable anæsthetics have led to a search being made by animal experimentation for safer methods of abolishing the pain attendant

on such operations. The introduction of such methods of local anæsthesia as the subcutaneous injection of cocain or eucain, and the injection of stovain into the spinal canal, are claimed as the result of such researches, also the employment of subcutaneous injections of morphia mixed with hyoscin for the production of a state of general anæsthesia.

42. (c) *Improvement in Technique.*—The power of controlling the conditions of the operation placed within the hands of the surgeon by the introduction of anæsthesia and of the aseptic method, has rendered it possible for him to interfere frequently with organs and to employ operative procedures which, in the absence of these methods would have been impossible. In the elaboration of such new operations and operative procedures resort has often been had to experiments on animals. Evidence on this point has been given by Sir Henry Morris, by Sir H. Swanzy and by Sir Victor Horsley. Examples are given in the following excerpt from Sir Henry Morris's evidence :—

"I must refer in this connection to the localisation of the motor centres in relation to brain abscess, brain tumours, and brain injuries, and to the operation of trephining for these conditions. Morris, 7726.

"I would not for a moment wish to imply that much had not been learnt as to the localisation of function from clinical observation apart from experiments on animals ; but I do not think it can seriously be contested that the extent and rate of acquisition of our knowledge of this subject, the precision of our knowledge, and the confirmation of what had been otherwise learnt, have been most important results of experiments on the brains of animals.

"When it was pointed out that it was, possibly, not so much the original escape of bile, after rupture of the gall-bladder, etc., which brings about the fatal result, as the continual filtration of bile into the peritoneal sac, the question arose as to how far surgery could afford aid in preventing the further escape of bile in this kind of injury. To this end M. Herlin and M. L'Anglas made experiments on dogs and proved that these animals could live after their duct had been tied or the gall-bladder removed. Laparotomy in man followed by suturing the gall-bladder, tying its duct, or removal of the gall-bladder, has since saved a good many lives.

"Dr. V. Czerny, to investigate the question whether the larynx could be extirpated without death resulting, performed a series of experiments on eight dogs, all of which underwent total extirpation of the larynx. The first four died from the consequences of the operation. The last four survived and remained well. It having been thus shown that the larynx is not indispensable to life, and that the operation is feasible, extirpation of the larynx became a recognised surgical operation. To what perfection it has been brought, may be seen from the results described in the latest contribution on the subject by Dr. Chevalier Jackson, of Pittsburgh (*British Medical Journal*, November 24th, 1906). Of eight consecutive cases of total laryngectomy for laryngeal cancer not one patient died from the operation ; one lived seven years, one three years, one apparent cure was lost from observation, three recurred within one year, and one is too recent to record. One died from alcoholism eight months after operation. It deserves particularly to be mentioned that once the feasibility of removal of the larynx having been shown, Czerny's experiments have never, so far as is known, been repeated.

"The repetition of operations of an experimental kind on animals has, however, often been the prelude to the adoption of most important operations on man.

"Take, for instance, nephrectomy or extirpation of a kidney : Zambeccarius in 1670 and Roonhuyzen in 1672 and several others in the beginning of the nineteenth century, had shown by experiments upon animals that life could be well maintained after removing one of the kidneys. Still, when Gustav Simon of Heidelberg in 1869 wanted to relieve a woman of a distressing condition, and could only do so by removing the kidney, he was reluctant to operate without first making experiments for himself. He knew that pathology afforded him many instances of persons living after one kidney had been rendered gradually useless by slow disease, but he wanted to confirm, if he even knew of them, the experiments by others which showed that the sudden withdrawal of the functions of one organ could be physiologically tolerated. He wanted also to know what were the immediate risks of the operation as distinct from the risks of interfering with the urinary functions—the risks namely of hæmorrhage, of embolism of the renal vein, of pyemia, etc. Hence his careful observation of fifteen hysterectomies on bitches contrasted with the same number of nephrectomies on dogs. ('Hunterian Lectures,' 1898, H. Morris.)

"Again, when the numerous operations on the stomach and intestines for short circuiting, excision, etc., were rendered possible by the great safety with which the abdominal cavity could be opened and dealt with under aseptic precautions, surgeons were anxious to test for themselves the various methods and modifications of methods, for suturing the bowel, though these or some of these had been employed in experiments on animals, as well as in cases of injury in man, by surgeons of some centuries before.

"It shows only a praiseworthy and natural solicitude and anxiety on the part of the surgeon that, before adopting a new operation on a human being, he should, as far as possible, test it beforehand on one of the lower animals under conditions and circumstances as nearly as possible similar to those of his patient."

Sir Henry Morris, however, while thus approving of vivisection for the purpose of 7828. the investigation of technique in the performance of particular operations, did not approve of its use for the purpose of obtaining manual dexterity generally—he thought it unnecessary for this purpose. We deal with this question further in our recommendations.

Additional evidence on the development of the surgery of the alimentary tract was given by Sir Victor Horsley. He said :—

"Operations on the stomach and intestines are now done on a very large scale, with great benefit Horsley, 15686. to man, and this is being extended to the lower animals for obstructions of various kinds, and for cancer and tumours generally. The first direct experiments—this is an instance of direct investigation for a surgical purpose—were made by Barischewsky. That was in 1875, but I would like to point out that Barischewsky himself was already indebted to the experiments of Lembert in 1836, who, by his experiments on dogs showed what had always been the weakness of this department of surgery, the correct manner of applying a suture. I only mention that again to emphasise what I said about the teaching of students, that the actual technique even in such a minutia as the passing of a stitch ought to be done by experiment first. Then our own observer and great surgeon, Travers, in 1821 had made similar experiments, but in 1875 Barischewsky showed a remarkable series of experiments on dogs, in which he had extirpated portions of the bowel, and proved the detailed technique, whereby dogs could be got to survive. Following on his work, in the next year Gussenbauer and Winwarter made direct experiments on joining the stomach and the bowel of the same kind; and then Billroth, on the strength of these experiments, performed the first successful excision of cancer of the stomach, and that inaugurated not only the operations for the extirpation of the disease, but also operations for the anastomosis of different parts of the canal, getting round obstructions and removing dilations, and so forth."

Not only is it claimed that experiments on animals have afforded a basis for a knowledge of the possibility and limitations of surgical interference with various internal organs, but also that they have played a part in the elaboration of some of the details of
Morris, 7893–4. surgical technique as commonly employed in operations. This is especially the case in regard to Lord Lister's researches on the method of employment of ligatures, and on the best materials for this purpose. The arrest of hæmorrhage from a wounded artery by ligature was discovered long ago by Ambroise Paré, or even before his day. Although some surgeons had suggested and practised the cutting short of ligatures, when Lord Lister began his researches the more general practice was to leave the ends of the ligatures hanging from the wound. In such cases suppuration commonly ensued and the ligature separated and came away with the discharges. During this process secondary hæmorrhage not infrequently occurred. Moreover, the ligatures hanging out of the wound possibly constituted a source of infection. Lord Lister argued that if the ligatures hanging out of the wound were thoroughly sterilised, their ends could be cut short, and they could be left in the wound, either to be buried in the surrounding healthy tissues, or to be absorbed, and largely in consequence of his researches a number of different materials for sutures and ligatures such as catgut, etc., were placed at the disposal of the surgeon. A knowledge of the possibility of leaving ligatures in the wound without harm was essential to the modern aseptic treatment of wounds.

Evidence was given by the President of the College of Surgeons of Ireland, Sir H. Swanzy, with regard to the part played by experiments on animals in relation to ophthalmic surgery, to which he devotes himself. He attached great weight to the part played by such experiments in the building up of our present knowledge of
Swanzy, 9793. inflammation, of the functions of the ciliary body, and of the treatment by eserin of the
9801. disease known as glaucoma. He also gave instances where, in his opinion, experi-
9802. ments on animals offered prospects of future successful operative interference with
9838. diseased conditions which have previously been regarded as intractable.

Infectious Diseases.

43. Amongst the diseases which afflict mankind it is perhaps acute infections which are the most impressive in their effects. Throughout the history of civilisation we have records of constantly recurring epidemics of one sort or another which have made their appearance in some populous centre, have spread thence in a manner unknown, and after killing numbers of human beings have disappeared in as unexplained a manner as they first arose. In addition to these epidemic disorders, there are a number of endemic diseases which occur in certain localities, being most numerous in the tropics, and which have been regarded as a necessary risk of residence in the places so affected. The result of such endemic disorders has been either to shut out certain regions of the earth's surface from occupation by Europeans, or has, at least, rendered the stay of Europeans in these parts in the highest degree dangerous. While the causes and mode of spread of these disorders were unknown, it was impossible for civilised man to safeguard himself and to take means to prevent the recurrence of the epidemic.

During the last forty years great progress has been made in regard to the knowledge and control of infectious diseases. The Royal Commission of 1875 in their Report, drew attention to the commencement of researches having for their object the production of communicable disease in the lower animals, with a view to studying their nature and cause, and thus, if possible, arriving at means for preventing their occurrence or spread in man. That Report dealt with experiments under three categories, the first relating to physiological operations, the second to the administration of drugs and poisons, and the last to the subject we are now considering. Sir John Simon was the first to advise the Government Health Department, then the

Privy Council, and later the Local Government Board, to make systematic use of experiments on animals for the elucidation of disease which threatened the public health. This was in 1865, and from 1870 to the present time there has been a special annual grant from the Treasury for the purpose. As Sir John Simon pointed out :— Power, 4406-8.

"In modern endeavours to increase the power of preventing different diseases of man and domestic animals, usually the first aim is to obtain exact scientific knowledge of the causes, and the mode of attack of any disease which is in question."

and this knowledge may be obtained in two ways :—

"On the one hand we have the carefully prearranged and comparatively few experiments which are done by us in pathological laboratories, and for the most part on other animals than man; on the other hand, we have the experiments which *accident* does for us and above all the incalculably large amount of crude experiment which is *popularly done by man on man* under our present ordinary conditions of social life, and which gives us its results for our interpretation."

44. The development and general adoption of the doctrine of *contagium vivum*, traceable largely to the labours of Pasteur, has been amplified and illustrated by his followers, and also by Koch and his pupils. Thus, Dr. C. J. Martin, of the Lister Institute, stated that :—

"Pasteur first showed, in a series of epoch-making researches into the nature and causation of fermentations, that each fermentation (*e.g.*, alcoholic, lactic acid, acetic, etc.), was due to the operation of a specific micro-organism. If the germs of one particular organism were sown in a nutrient fluid the products of one particular fermentation resulted. His experience on this subject led him to the great generalisation that infectious diseases might themselves be interpreted as particular fermentations, and as due to specific micro-organisms. By a series of masterly experiments on animals he established the truth of his hypothesis in the case of anthrax and chicken cholera, and swine erysipelas. These results of Pasteur's may be regarded as the foundation of the whole modern study of contagious diseases both in man and in animals; and their extension by Pasteur and his pupils, and by bacteriologists and pathologists all over the civilised world, has led to the discovery of the causation of most of the infectious diseases to which man is liable." Martin, 11643.

In the case of each infectious disorder, attempts were made to determine :—

(1) The nature and the life history of the micro-organism which caused the disease.

(2) The conditions under which such organism can live and can be destroyed.

(3) The circumstances which determine its virulence.

By an accurate and careful study of the life history of the organism, attempt is made to determine how the disease is transmitted from one individual to another, whether directly from man to man, or indirectly by other living agency, or by dead material. The study of bacteriology has led to measures being taken to hinder the extension of epidemics or to prevent altogether their occurrence. Thus, it is found that some diseases are spread only by contact of man with man. In others the specific micro-organism is stated to be spread by the agency of articles of food, such as water or milk. In another large class of cases there is no direct infection, but the disease is attributed to the action of biting insects carrying the disease from man to animal, and from animal to man. Accurate knowledge has, it is claimed, been acquired with regard to certain diseases, and in many cases the application of such knowledge is held to have resulted in stamping out diseases, such as malaria and yellow fever, from places where, until recently, their ravages were responsible for a heavy annual mortality.

45. Another line of research which has been investigated experimentally is the question as to how it is possible to render a man immune, *i.e.*, to place him in such a condition that the micro-organism is unable to infect his system. It had long been held that in some of the infectious disorders one attack affords some degree of protection from subsequent attacks, and working on this as a basis the attempt has been made to protect a man from infection by giving him a mitigated form of the disease, and evidence has been laid before us to prove that success has attended such practice. In the experimental studies of the changes occurring in the animal as a result of infection, information has been acquired on the question of the nature of "immunity." Thus, it has been stated to us, as the result of these experiments, that in certain diseases all the injurious effects of the disease are due, not so much to the micro-organisms themselves, as to poisonous products derived from the micro-organisms. This is said to be especially the case in *tetanus*, *diphtheria* and *dysentery*. Observers have found, moreover, that the injection of a poisonous substance of animal or vegetable origin, a so-called toxin, into an animal, if it does not kill the latter, evokes the production in the animal's blood of a substance—called antitoxin—to which is attributed the property of neutralising the original toxin. Such antitoxins have been utilised

for the curative or prophylactic treatment of *diphtheria*, *tetanus* and *dysentery*, and an extension of the same principle has led to the employment of antitoxin for the animal poison which is held to be the effective agent in the lethal properties of snake poisoning. Dr. Martin told us that :—

Martin, 11643.

"Knowledge of the nature of the exciting causes, their conditions of existence and the re-action of the animal organism to their introduction has already led in some cases to the finding of a curative agent and has indicated a way in which we may hope to gain curative powers over other infectious disorders. Even in certain cases where the causation of the disease was not directly established by experiments on animals because no susceptible animal was available, *e.g.*, in typhoid fever, the discovery of the cause of the disease in man was indirectly due to such experiments, since the experience gained by Pasteur, Koch, and others on animals with regard to the characters of specific infectious disorders, has enabled us to correlate our observations on man so as to establish the causal nexus between the typhoid bacillus and the disease."

Morris, 7679.

46. The following postulates were laid down by Koch as necessary to be fulfilled before any particular micro-organism could be regarded as the cause of any given infectious disorder :—

(1) The micro-organism must always be found in cases of the disorder.

(2) The micro-organism must be cultivated in media outside the body apart from admixture with any other organism.

(3) It must be possible, by injection of the pure culture of the organism (obtained by cultivation outside the body) into animals, to produce a disease identical with that of the animal from which the micro-organism was originally obtained.

We are informed that researches conducted on these lines have led to the discovery of the specific micro-organisms of the following among the diseases of man, *viz.*, *pyæmia*, *puerperal fever*, *erysipelas*, *surgical gangrene*, *gonorrhea*, *Malta fever*, *tubercle*, *tetanus*, *diphtheria*, *plague*, *dysentery*, and *pneumonia*, and also in the case of animal diseases communicable to man such as *anthrax* and *glanders*.

In typhoid, cholera and dysentery a micro-organism regarded as specific has been found and cultivated outside the body. The evidence in the case of typhoid is said to rest on accidental infection of man himself, since the disorders produced in animals by the injection of these microbes are not identical with those recognised as characteristic of the disorder in man. In the cases of malaria, sleeping sickness and certain allied disorders, micro-organisms belonging, not to the vegetable, but to the animal kingdom, *i.e.*, to the class of protozoa, have been found, and their life history and mode of transference from one subject to another traced. It has not yet proved possible to cultivate these protozoal organisms outside the animal body. In yellow fever no micro-organism has yet been isolated, but the experience gained in the study of other infectious disorders has led investigators to lay down the conditions of existence of the micro-organism involved, and acting on the knowledge thus gained, the disease has, it is said, been banished from places where it formerly raged unchecked.

47. The results claimed as due to these experimental investigations are of so great importance to the welfare of mankind and are attributed so directly to experimental researches on animals that we have thought it worth while to examine more closely the evidence in regard to a number of these disorders. At the time of the last Royal Commission's Report bacteriology was in its infancy, but it was even then claimed by Sir John Simon that valuable information had already been obtained by experiments on animals, in regard to the causation of *cholera*, of *sheep pox*, of *tubercle* and of *pyæmia*. In the case of *sheep pox*, as the result of the researches of Dr. Klein, Sir John Simon, in his evidence, claimed that :—

[R. C. of 1875, 1417.]

"By these experiments on sheep it has been made quite clear that the contagium of sheep pox is something of which the habits can be studied as the habits of a fern or a moss can be studied, with a completeness not yet attained in regard of any other such case."

These results, Sir John Simon said :—

"while they complete, as regards the special disease in question, the broad pathological outline which previous inductions had rendered probable, must also be regarded as tending very importantly to confirm while they illustrate the general doctrine of vitality of contagia."

[R. C. of 1875, 1417, 2782. 1656, 1669–70, 6254–9.] Stockman, 2733–7 Russell, 575–81.

Great store was set upon these results at the time, as may be gathered from the evidence of Sir John Simon and Dr. Burdon Sanderson, and the questions of Professor Huxley and Sir John Karslake. Unfortunately their conclusions have proved to be unfounded ; sheep pox has been shown not to be due to these organisms and its cause still remains unidentified.

In regard to *cholera*, much work has been done since the Report of the Commission in 1876. At that time Sir John Simon claimed that as the result of experiments on mice the infectious quality of the human excreta had been established but it was not until 1883 that Koch announced the discovery of the comma bacillus as the true cause of the disease. This, though disputed by Dr. Klein for a time, has now come to be almost universally accepted. Power, 4429–30.

As indicative of the trend of expert evidence before the last Royal Commission, it may be mentioned, that while in the case of *tuberculosis* the communicable nature of the disease had been advanced by Villemin, no microbe had at that time been identified as its cause, and Dr. Burdon Sanderson, who had worked at its pathology under the Privy Council by experiments on animals, believed that he had shown that there was nothing specific in the disease, and that the morbid process when once it had begun must be regarded as incurable. The volume of work carried on in connection with the causation, prevention and cure of the infectious disease since 1875 is enormous. [R. C. of 1875, 2296–9.]

48. *Tuberculosis.*—Amongst the inhabitants of temperate climates, tuberculosis is responsible for more deaths than any other disease. Although the mortality from tuberculosis has declined greatly of late years, yet during 1903, 58,150 persons, or 11·3 per cent. of the whole mortality, died from this cause in England and Wales. Up to the middle of the nineteenth century phthisis or consumption and the other tubercular diseases were not recognised as connected, and the prevailing view was that phthisis and other forms of tuberculosis were the results of constitutional peculiarities, and that its incidence was mostly determined by heredity. Dr. Martin stated that in 1865, Villemin, by experiments on animals, arrived at the conclusion that tuberculosis was an infective disease due to some *materies morbi* introduced from without; this had been previously suggested by some clinicians. He also demonstrated the essential unity of the various manifestations of tubercle, such as phthisis, scrofula, lupus, some forms of joint disease, and certain common diseases of animals. In 1881 Koch isolated a bacillus, cultivated it outside the body, and claimed that on injection into animals it produced tubercular disease. Moreover as Dr. Frederick Taylor, who represented the College of Physicians, informed us, Koch in 1890 introduced tuberculin—a derivative from tubercle bacilli as a cure for consumption. The proposition was taken up in a most sanguine way by the public as well as by the profession; but it proved then to be a vast failure so far as the treatment was concerned. Dr. Taylor was of opinion that if further experiments had been made by Koch and others on animals "the loss of time, and expense, sometimes perhaps fatal results and certainly disappointment might have been spared to many." Koch, however, appears largely to have employed experiments on animals in arriving at his conclusions, but at the same time if we may judge from his pamphlet on the "Cure of Consumption" (1890), he appears to suggest that "an experiment on an animal gives no certain indication of the result of the same experiment on a human being." Sir Douglas Powell and Dr. Taylor stated that while Koch's tuberculin was a failure as far as the cure of phthisis in man is concerned, it was yet employed with, it is claimed, "very fair success" as a test for the presence of tubercle in animals and also sometimes in man. Sir Douglas Powell stated that he would be very much indisposed to use it himself in man for this purpose, although he believed that it was valuable as treatment in certain cases. Martin, 11644. Taylor, 5594. 4469. 5695. Taylor, 5699. 5700. Powell, 5799. 5819.

Treatment of tuberculous disease of the joints has, it is stated, been improved as a result of the increased knowledge obtained. It is claimed that by the recognition of the bacillus, either by the microscope or by injection into animals, it is possible to arrive at a conclusion as to the character of disease of the lungs or kidneys at an early period of the illness, so that it may be treated with greater chance of success. More recently it has been maintained that it is possible by the injection of minute doses of tuberculin or of dead tubercle bacilli into patients afflicted with tuberculosis to induce a reaction which may beneficially influence the course of the disease. Martin, 12374.

About six years ago Koch, in the course of a lecture delivered in London, denied that there was any relationship between the bovine and human tuberculosis. This assertion has been the subject of enquiry by a Royal Commission and has necessitated a very large number of experiments on animals, by way of re-testing preceding experiments on farms registered for the purpose. A large amount of evidence has, according to Dr. Sims Woodhead, been obtained by the Commission in opposition to Koch's contention and proving that the diseases are transmissible from cattle to man and *vice versa*. Powell, 5811. Woodhead, 15500. Byrne, 88. Thane, 1116–7. Woodhead 15503.

Power, 4455. The Local Government Board awaited the results of the experiments of the Tuberculosis Commission before devising measures in the interest of the public health. There is however now much knowledge at hand to guide any government or community in an attempt to grapple with the disease by administrative measures, including under this term notification, segregation and education. No community has yet attacked the disease on a large scale along these lines, though the work of Koch and his pupils shows that any improvement in the general sanitary conditions of the community which will involve an access of increased air and light, the avoidance of overcrowding and the provision of proper food, will serve to diminish the incidence and the mortality of the disease.

Martin, 12216. "It is (says Koch) the overcrowded dwellings of the poor that we have to regard as the real breeding places of consumption; it is out of them that the disease always crops up anew, and it is to the abolition of these conditions that we must first and foremost direct our attention if we wish to attack the evil at its root and to wage war against it with effective weapons."

In this connection also, Sir Douglas Powell said :—

Martin, 5787. "My belief is that the virulence of the contagion of consumption is due largely to the association of dirt with that contagion. In insanitary surroundings the tubercle bacillus is associated with other micro-organisms. Just as in the case of the tetanus bacillus, which, when mixed with other infections, is far more virulent, so I believe the tubercle bacillus, when mixed with streptococcal and other organisms, is definitely more contagious, and therefore in clean places, like consumption hospitals and sanatoria, consumption is practically not infectious at all."

It is to general improvements in sanitary conditions that must be ascribed the steady decrease which has occurred during the last hundred years.

By recognition of the tubercle bacillus, County Councils and other public bodies in exercising supervision over the food supply of the community, feel justified in rejecting foods such as milk or meat which are derived from infected animals and might affect prejudicially those who partook of them.

Martin, 11654. 49. *Plague.*—In regard to Plague the Commission was informed by Dr. C. J. Martin that notwithstanding the antiquity of this disease it was not till 1894 that the causation of the disease was traced to a bacillus. Dr. Martin observed that :—

"As long as the causation of plague was not comprehended, and the means by which it spread were unknown, suitable preventive measures could not be rationally prescribed. Consequently, even in recent times, wealth and human effort have been squandered in directions which have led to no useful result. The constant association of a mortality of rats and mice with outbreaks of plague amongst human beings has been forced upon the minds of many ancient people, and is found to exist among many primitive races of mankind at the present time. The true significance and importance of this observation could not, however, be realised until the causation of the disease was discovered by Yersin and Kitasato (*Lancet*, 1894) in Hong-Kong in 1894. These observers found a bacillus constantly present in the swollen lymphatic glands (buboes) of patients suffering from the disease, and they succeeded in growing it upon artificial media. Inoculation of the cultures of this organism into animals reproduced the disease with all its characteristic symptoms. Once the cause of the disease was known, it was easy to determine that the disease from which rats and mice simultaneously suffered was also plague. All the conditions favourable and unfavourable to the existence of the infective agent, could be studied, and the means by which the infection leaves the bodies of persons or animals dead of the disease, could be determined. The relationship of rat plague to human plague has been found to be of such a character as (taking into consideration the small extent to which bubonic plague is infectious from man to man) to indicate in the strongest manner that the spread of the disease in man is conditioned almost entirely by the occurrence of plague among rats in his vicinity. The Commission at present investigating the question of the spread of plague in India have, by extensive experiments on animals, succeeded in showing that the infection is carried from rat to rat by the agency of fleas infesting these animals (*Journal of Hygiene*, 1906 and 1907, *special numbers*). By the same agency they have been able to produce epidemics among other animals, guinea pigs and monkeys, whereas all other means failed to give rise to the epidemic spread of the disease. The Indian rat flea also feeds upon man when his natural prey is not available, so that a possible and indeed probable means whereby the infection is carried from rats suffering from the disease to mankind, has been established."

The magnitude of the importance of an accurate knowledge of the causation and prevention of plague may be gathered from the fact that :—

11654. "during 1905, 1,040,429 persons succumbed to plague in India. Over 350,000 deaths occurred in the Punjab alone. In some of the villages of this province 75 per cent. of the population were destroyed in two years. The mortality amongst Asiatics varies between 70 and 80 per cent., and amongst Europeans at the present time it is over 35 per cent. of those attacked."

12302. In 1906-7 the mortality from plague in India was higher still and the disease was more prevalent than ever.

A correct knowledge of the mode in which plague is transmitted explains why the introduction of better housing and sanitation generally has practically abolished the disease from the cities of Europe and serves as a basis on which must be devised administrative methods for stamping out this disorder in India and the East. The institution of such measures must, however, be a task of some difficulty in view of the conditions prevalent in oriental countries.

On the other hand, prophylactic and curative measures by way of inoculation have been attempted in the case of plague. After numerous experiments with the plague bacillus which had been killed by a variety of means, Haffkine finally adopted a vaccine which consisted of a broth culture of the organism sterilised by heating to 65° C. This vaccine is known as Haffkine's prophylactic, and since 1897 has been extensively Martin, 11792. used in India and has been officially adopted by the Government of India.

The attempts to obtain a curative agent by the production of antitoxin serum (Yersin's serum) have not been successful, owing, according to Dr. Martin, to the 12071. impossibility "at present by injecting plague bacilli into the horse to make it respond by the manufacture of a sufficient amount of immunising bodies to be of great practical value."

Investigations were made in this country under the Local Government Board with Power, 4329. a view to obtain something better than Haffkine's fluid. The new material has been derived from "the actual dead tissues of the plague organs of an experimental 4330. animal" and has been employed in the rat, guinea-pig and the monkey, but not as 4437. yet on man. The Board are considering whether they will manufacture any considerable amount of this material. They do not recommend these sera, but place 4445–7. them at the disposal of the Health Authorities. It is not as yet known how long the protection in the experimental animal lasts; the prophylaxis in practice among human beings may prove less than in laboratory results. By statistical comparisons made in Martin, 11792. the case of persons inoculated with Haffkine's prophylactic and uninoculated persons respectively it is claimed that the attack rate from plague has been reduced to one-fifth and the mortality to one-twelfth. The Plague Commission of 1901 concluded that under certain circumstances those vaccinated, even as many as four times, may yet be 12293. attacked by the disease, and that there are varying influences at work determining the attack rate in different places. This opinion was formed by the Commission from Kekewich, the early records of inoculations, and more recent statistics are held to show that the 20485–90. influence of Haffkine's prophylactic is of higher value in checking the spread of the Martin, disease. In Egypt it is alleged that ordinary sanitary precautions apart from prophylactic 12298–12301. inoculations have been not less effective. [Cd. 106, 4435–6.].

Some untoward results attended the use of Haffkine's plague vaccine at Mulkowal in India. This occurrence was reported on by the Government of India and by the Lister Institute. It appears that nineteen of the vaccinated contracted tetanus shortly after inoculation and they all died of the disease. The Institute and the Commission to whom the matter was referred came to the conclusion that:— Martin, 12295–8.

> "In all probability the tetanus was, at the time of the inoculation, in the fluid contained in the bottle, 12298. but that it is impossible to determine at what stage in its history or in what way bottle 53N became contaminated."

The probability favoured the view that the cause of the tetanus was in the vaccine fluid when the bottle was sent out from the laboratory. Carbolic acid is employed Power, 4441. to counteract such contaminations, though this may itself give rise to carbolic acid poisoning, and one of the objects of the Local Government Board in their investigations is to obviate its use.

50. *Cholera and Typhoid.*—Both these diseases are attributed to the presence of specific organisms which have been isolated and cultivated outside the body. The conditions of transmission of these organisms have also been studied, and in most cases infection can be avoided by the observation of the rules as to pure water and food supply which had been previously arrived at by hygienists. In the case of typhoid fever, a method of protective inoculation has been devised by Sir A. Wright, and has been largely used in the Army, especially in India. A Committee was appointed by the Army Medical Department to enquire into anti-typhoid inoculation. Dr. Martin, who was Chairman of this Committee, states that:—

> "The value of anti-typhoid inoculation can no longer be regarded as doubtful, and considerable Martin, 11803. effort is being made by the Commander-in-Chief in India to secure the inoculation of as large a proportion as possible of the British Army in India."

The value of the statistics, as prepared by Sir A. Wright, which purport to show the 11943–6. protective effects of anti-typhoid inoculation, have been contested by Professor Karl Pearson.

51. *Malaria.*—Malaria is one of the most widespread of the diseases which affect man, and its severity in tropical countries has long been regarded as one of the greatest hindrances to the settlement of such countries by Europeans. Until recently it was regarded as originating in emanations, miasmas, arising from marshy places, this being a deduction from the experience that it was prevalent in such places, and that

it tended to disappear with the introduction of effective drainage. That which is generally regarded as the true cause of the disease was first discovered by Laveran, who described a micro-organism in the blood of patients affected with this disorder, and assigned the organism to the class of protozoa. The changes it undergoes in development and the mode in which it infects man were first described by Major Ross. This observer maintains that malaria is a human disease, that it is carried from one man to another
Osler, 16704–6. by means of a certain species of mosquito, that the reason why malaria is prevalent in marshy places is that little pools of water afford the best possible breeding places for the mosquito and that in the absence of an infected case to serve as a source of infection the bite of the mosquito is harmless.

Upon the lines indicated by these researches it is held a community must proceed if it is desired to eradicate the disease from its midst. In the first place the multiplication of the mosquito has to be prevented by the abolition of all pools or other collections of stagnant water, *i.e.*, by effective drainage, and by covering the surfaces of any water which cannot be got rid of with petroleum, so as to prevent the breeding of the mosquito. In the second place the transmission of the disease has to be prevented either by the segregation of any infected patients or by so placing the healthy individuals as to be beyond the flight of the mosquito, or by filling in with wire gauze all the windows and doors of the dwellings inhabited by the healthy individuals so as to prevent the ingress of the infected mosquito. These methods, especially the extermination of the mosquito and the protection of healthy individuals against its attack, are now being actively carried out in many quarters of the globe. As a result, it is stated that Ismailia, which was a hotbed of malaria, is now free from the disease. Sir William Osler pointed out that the failure of the first attempt to build the Panama Canal was determined chiefly by the enormous mortality from malaria and yellow fever among the workmen. The knowledge gained with regard to the causation and method of spread of both these diseases has led the United
16541. States Government to take such action in the peninsula as has led to so great a reduction of both these diseases, that the health of the workmen on the canal is not inferior to that in any part of the States. Sir William Osler holds that " these discoveries will make the tropics habitable for Europeans."

16531, etc. 52. *Yellow Fever.*—Yellow fever has been the great scourge of the regions round the Caribbean Sea and every year it has spread to the Southern States of America and occasionally has reached Philadelphia and even as far north as Boston. All attempts to find out the cause of the disease had failed up to the year 1900, although experiments not only on animals but on man had been freely made; and it was held by
16611. many that the disease was not contagious. In 1896, Sanarelli, as a result of experiments on animals, claimed to have discovered a bacillus as the cause of yellow
16613–24. fever and prepared from it an immunising serum to the use of which was attributed the arrest of an outbreak of the disease at San Carlos Gaol. In 1901 Drs. Durham
16618–9. and Myers reported a bacillus as the probable cause of yellow fever but not that described by Sanarelli. A later Commission to New Orleans reported an extensive
16620. series of investigations which seemed rather to favour the views of Sanarelli.

Sir William Osler, however, informed us that Sanarelli's work has not been substantiated, and reliance cannot be placed upon it, Dr. Goldberger having conducted experiments to show that this bacillus stands in no causative relation to yellow fever.

16620–1. More recent observations have shown that we do not know the nature of the parasite although it is suggested that it is of the nature of a protozoon rather than
16531. a bacterium. In the year 1900 a Commission was sent to Havana by the United States Government specially to investigate the cause of yellow fever. The experiments
16623. were made on men who volunteered for the purpose. The result of these experiments according to Sir William Osler showed that although the disease was not entirely contagious, it could be communicated by the bite of a species of mosquito. It was held that the mosquito to become infective must bite the yellow fever patient within the first
16539. three days of the patient having the disease. After this time for twelve days it is stated that the mosquito is not infective but that after this interval it remains infective all the rest of its life and can infect any other man whom it bites. The organism itself has not been discovered, but as a result of the knowledge attained measures have been devised with a view to check the spread of the disease by destruction of the mosquito and by segregating any individual shown to be infected with yellow
Woodhead, fever, but not by immunising the community by any system of vaccination. As
15555. a result it is claimed, that within two years Havana was cleared of yellow fever for the first time during the three hundred years of its existence, and Sir William Osler states that he looked forward to the total abolition of yellow fever within five years.

53. *Hydrophobia.*—It was largely due to the work of Pasteur that it has come to be commonly held that the disease of hydrophobia, or as it is generally called in animals, rabies, never arises *de novo*, but is always to be referred to infection by inoculation from some previous case. This knowledge has, it is claimed, rendered it possible to abolish the disease altogether from the British Isles, the insular position of which made it practicable to institute a sufficient quarantine for imported dogs, the disease having been previously stamped out by the measures attendant upon the rigorous Hughes, 17127.
enforcement of the muzzling order. The cause of the disease, whether an organism or virus, has not yet been determined, but it would appear that the nervous system is an especial seat of the poison. Hydrophobia is one of the most horrible diseases to which mankind is subject. It is almost invariably fatal. Of the cases bitten by rabid dogs it is variously estimated that from 5 to 50 per cent. may get the disease. In consequence of the relative prevalence of the disease on the Continent, Pasteur was led to search for some means by which the effects of a bite by a rabid animal might be counteracted, and the development of the disease prevented. As a result of a number of experiments he devised a system of protective inoculation which, when applied sufficiently early after the bite had been inflicted, is held to afford a high degree of protection against subsequent development of the disease. A number of statistics of results of the Pasteur treatment were laid before the Commission by Dr. Martin, which purported to show that if the general incidence of hydrophobia among persons bitten by dogs regarded as rabid be taken to be about 17 per cent., in cases treated by Pasteur's method the incidence is less than 1 per cent. These results, though subjected to much criticism, led to the foundation of institutes for the application of Pasteur's treatment to patients bitten by rabid animals in India, in Hungary, in New York, and in many other places. Rabies has, however, disappeared from England and Martin, 12244.
Wales since 1899, and with it the necessity for anti-rabic inoculation.

54. *Sleeping Sickness.*—African sleeping sickness was formerly only found in a few localities on the African Continent. It is in most cases a complaint of several years duration, its main feature being the excessive somnolence to which the patient is subject. It is free from pain. In some cases the disease runs an acute course with high fever, and results in death within a few months. The opening up of trade routes during the last few years has led to a very serious spread of the disorder, and on this account a Commission was appointed by the Royal Society in 1902 in order to study the disease, and it is claimed that the work done by the Commission is excellent and that it has obtained very valuable results. A Portuguese Com- Bruce, 14321.
mission which investigated the disease in West Africa arrived at the conclusion that the cause of sleeping sickness was a streptococcus, a microbe belonging to the same class as that to which the causation of Malta fever was traced by Colonel (now Sir David) Bruce. In 1903, after a year's work, the Royal Society's Commission confirmed that conclusion arrived at by the Portuguese Commission; early in that year, a member of the Commission, Dr. Castellani, drew Sir David Bruce's attention to the fact 14428.
that an organism of quite another kind—trypanosoma—had been found in a few cases, but it was held to have no causal relation to the disease. A careful investigation by Sir David Bruce satisfied him that this animal parasite—trypanosoma—was to be found in the cerebro-spinal fluid of every patient suffering from the disease, and also in the blood of all but one examined, whereas the parasite was absent from the blood and cerebro-spinal fluid of natives living outside the infected area. It was then found that by the inoculation of the cerebro-spinal fluid containing the micro-organism into monkeys, a similar disease could be produced in these animals. On further investigation it was found that this parasite, trypanosoma, could be communi- 14324.
cated by the bite of an insect—a species of tsetse fly. Sir David Bruce described fully 14335.
to the Commission the distribution of sleeping sickness in Uganda and the conditions which seem to determine its distribution. It is not known whether the disease is limited to man, or whether or not it is propagated by the tsetse fly from some animal to man. The parasite has not been grown outside the body, and to this extent therefore the proof of its being the cause of the disease is not complete. The need for caution in accepting a trypanosoma as the cause of sleeping sickness has been pointed out. Not long since another blood parasite—*filaria perstans*—was confidently 14431.
asserted to be the cause of the disease. It has, however, been found to be contained 14433.
in the blood of healthy natives, to the extent, according to Sir David Bruce, of 100 per cent.

On the hypothesis that the disease is essentially human in its incidence measures are being taken on a large scale with a view to preventing its further spread in Uganda.

These measures consist chiefly of segregation of the cases already infected and a temporary clearing out of the inhabitants from the infected zone. It is as yet too early to determine how far these measures will be successful.

55. *Diphtheria.*—Diphtheria is one of the commonest of infectious diseases in this country. It is characterized by the production of a membranous exudation on the inflamed surface. It is a disease more particularly of childhood. Treatment of the disease until recently consisted in little else beyond good nursing and the operation of tracheotomy or intubation when the larynx became obstructed by membrane. In 1875, Klebs found a bacillus in the membranous exudation in cases of diphtheria. As it was generally mixed with a variety of other micro-organisms it was not generally accepted as the cause of diphtheria until Loeffler in 1882 succeeded in isolating it, growing it upon an artificial medium, and producing, by inoculating it into animals, a disease in some respects said to resemble diphtheria of man. This bacillus is not found throughout the body, but chiefly on mucous membranes or on infected raw surfaces. The general symptoms of the disease, the weakness, the paralysis, and death when it occurs, must therefore be due not to the presence of the micro-organism in the body at large, but are attributed to the circulation within the blood of poison produced by the bacilli at the localised seat of the disease and absorbed therefrom into the blood stream. Roux and Yersin were able to separate a poison by filtering off the bodies of the bacteria from the fluid in which they had been grown, and found that by the injection of this sterile fluid they were able to produce poisonous symptoms like those observed in diphtheria, and especially the
Martin, 11696. degeneration of nerves occurring at a late stage of the disease. These experiments led to the isolation and employment by Behring in 1890 of *diphtheria antitoxin*. As has already been mentioned, when an animal or vegetable poison—a toxin—is introduced into an animal, the latter reacts, if the disease is not immediately fatal, by the production in its blood of a substance—an antitoxin—which tends to neutralise the original toxin introduced. The preparation and sale of diphtheria antitoxin is now carried on on a large scale. For this purpose a horse is injected at first with minimal doses and then with gradually increasing amounts of diphtheria toxin until it may receive at one time many thousand times the fatal dose. The horse reacts to this injection by producing an antitoxin in its blood serum and the antitoxin is obtained by bleeding the horse at intervals, allowing the blood corpuscles to settle, and decanting the serum (antitoxin serum) into sterilised bottles. In this form the antitoxin is retailed to the medical profession. Behring claimed that if an animal were injected with a lethal dose of toxin no effect was produced if it received at the same time, or within the next two days, a corresponding dose of antitoxin. After this interval the injection of antitoxin was without effect, for it is held that the poison has by that time obtained a seat in the tissues of the body from which it cannot be dislodged. These experiments and considerations suggested that the antitoxin might be used as a means of curing diphtheria in man provided that it were injected not too long after the onset of the disease. At the Medical Congress at Buda Pest in 1894, Roux communicated the results of the treatment with antitoxin of 300 cases of diphtheria in the Paris Hospital where the mortality of those treated with antitoxin was stated to be 24 per cent. as against 60 per
11711. cent. in the non-treated. The experimental clinical data and statistics brought forward by Roux led to the trial of this method of treatment in all civilised countries, and, it is claimed, with remarkable results. The remedy is now used throughout the civilised world, and there is a general consensus of opinion in the medical profession as to the benefits to be obtained from its employment. The Commission has received large numbers of statistics on this point. Considerable diversity of opinion was exhibited however between the professional gentlemen and other witnesses who were opposed to experiments on animals, as to the proper interpretation to be placed on these statistics. In the first place it was pointed out by Mr. Coleridge that the introduction of diphtheria antitoxin in 1895 had not been followed by any decrease in the total mortality from diphtheria, as shown in the Returns of the Registrar General. This fact does not tell necessarily against the value of antitoxin as a curative agent since, although it has been used occasionally as a prophylactic to prevent the further spread of an outbreak of the disease, the extent to which it is used in this way is practically negligible. Moreover the fatality of the disease may have been reduced while the mortality per population has not fallen. The mortality from diphtheria alone teaches us therefore nothing as to the value or otherwise of antitoxin, unless we have regard to the extent to which it has been used and the total number of cases of diphtheria which occur. If the number of cases of diphtheria were doubled, and the use of the remedy diminished the

mortality by one half, the total number of deaths from the disorder would remain unchanged. Most of the professional witnesses accordingly insisted that in judging of the value of the remedy it is important to have regard to, not the total mortality from the disease, but to the case mortality, *i.e.*, the percentage of cases of diphtheria that die of the disease. Now there is no doubt that, when we regard case mortality, we find that practically in every country of the world since the introduction of the remedy in 1895 into general use there has been considerable diminution in the case mortality. This is illustrated by some of the tables which have been laid before the Commission and are here reproduced. There are, however, other possibilities which might account for this reduction of case mortality. The fall of mortality referred to is a fall in the case mortality or fatality of the disease as measured by the percentage of a given number of cases of diphtheria which terminate fatally. Thus in the year 1891, before the introduction of the antitoxin treatment, of 100 cases admitted into the Taylor, 5590.
hospital of the Metropolitan Asylums Board, 30 terminated fatally, whereas in the year 1896, after the introduction of that treatment, the case mortality was 21 per cent. and in the succeeding nine years it continued to fall, the percentage mortality for those years being 17·4, 15·1, 13·6, 12·5, 11·14, 11·13, 10, 10, 8·3. This fall in case mortality from 30 per cent. to 8 per cent. or 10 per cent. is, of course, not incompatible with a persistence or even an increase of the mortality of the disease reckoned as a rate upon the number of persons living in a great town or country. Thus in the year 1905 the mortality from diphtheria per million of the population in England and Wales as given by the Registrar-General was 160, in 1904 170, in 1903 Powell, 5648.
182, in 1902 236, and in 1901 273, whereas in 1881 it was 121, in 1882 152, in 1883 158, and in 1884 186. The death rate from diphtheria has therefore on the whole been not less in the years 1901 to 1904 than the corresponding years twenty years ago, and in view of the lowered case mortality it follows that the number of cases which occurred in the later series of years must be more numerous than formerly.

The question of the use of antitoxin serum as a prophylactic and its possible dangers were not brought before us in sufficient detail to justify any statement in regard to them.

It was claimed by Sir Douglas Powell and Dr. Taylor that "the recognition of the exact bacterial nature of the specific fevers has resulted in more precise and efficient measures for their prevention," and it is doubtless the case with diphtheria that by means of bacteriological diagnosis (*i.e.*, the examination of cultures made from the throat of persons suspected of being infected) a large number of cases are now diagnosed Power, 4613.
as diphtheria which in earlier years would not have been recognised as such. Such patients had bacterial as distinguished from clinical diphtheria. The inclusion of such cases, as Sir W. H. Power pointed out, "might have the effect of giving a seeming reduction of the case mortality which was not altogether real." "That," in the opinion of Sir W. H. Power, "would have to be taken into consideration in judging the case 4403.
mortality now as compared with the case mortality years ago," "since the case 4404.
mortality might be stationary while the identifications of the disease were becoming more abundant."

Further, it is true that the Metropolitan Asylums Board statistics exhibit a reduc- Powell & Taylor,
tion of case mortality of *scarlet fever* (for which no similar novel treatment has been 5671–2.
introduced) from 10 to 14 per cent. between 1874 and 1890 to 3 per cent. in 1905, but this Sir Douglas Powell and Dr. Taylor attribute to improvement in the general treatment and better hygienic conditions of the patients. It must be borne in mind Powell, 5674–7.
that no bacteriological test for the diagnosis of scarlet fever is available. This is also the case in measles, typhus and small pox, though research by way of experiments on animals has been prosecuted with the object of securing such discovery. Dr. C. J. Martin, 11713.
Martin, while agreeing with Sir Douglas Powell and Dr. Taylor in their general opinion in regard to the value of diphtheria antitoxin and adding additional statistics drawn from various countries, pointed out that "other factors may have to be examined and taken into account as well as serum or no serum" in drawing conclusions from such statistical data. Thus, diphtheria is not uniform in its incidence or in its severity. Epidemics occur and recur at intervals and vary in their fatality apart from any mode of treatment. The case mortality from diphtheria in London in the Asylums Board hospitals prior to the general introduction of antitoxin treatment 12262–9.
in 1895 had fallen from 59·3 in 1888 to 29·3 in 1894. Moreover, Dr. Martin also points out that "another factor which will undoubtedly have modified the case mortality of diphtheria is the adoption of a more accurate method of diagnosing the disease by bacteriological examination." "This," Dr. Martin holds, "will most certainly have led 11713.
to the wider recognition of the disease in mild and atypical cases, and, therefore, to the inclusion in the statistical data of a number of less severe cases of the disease." Unfortunately it is not possible to measure the influence of this factor in diminishing 12270.
the case mortality.

The diagnosis of diphtheria is now largely based upon finding the Klebs-Loeffler bacillus in the throat, and the antitoxin treatment is applied without waiting for further symptoms such as the development of a false membrane on the throat. Attention has been directed to the fact that the Klebs-Loeffler bacillus considered to be diagnostic of diphtheria is often found in the throat of healthy persons—even in 15 per cent. of healthy persons it is said to have been identified.

Woodhead, 15543-4.

The explanation given us by Professor Woodhead of this fact is contained in an answer to a question. He says :—

"I think it (the true Klebs-Lœffler bacillus) is found (in healthy throats) far more frequently than is realised; but I also think that it does not attack the throat until there is some slight inflammation, some slight breaking down of the tissues. We have, fortunately, an epithelial surface which presents a barrier to the invasion of all these organisms, and it is only when there are slight breaches in that barrier that the organism gets in, and is able to make further breaches."

Moreover, in a certain percentage of cases, otherwise regarded as diphtheria, this organism has been sought in vain, sometimes to the extent of 20 per cent. of the cases diagnosed as diphtheria on the strength of clinical symptoms. This does not, according to Professor Woodhead, necessarily imply that the bacillus was absent at the first onset; the explanation, according to him, is as follows :—

15513. "In many of these cases you have really the Klebs-Lœffler bacillus crowded out by other organisms, and you get a condition which commences as slight diphtheria and ends as a septic sore throat. The one organism is practically crowded out by others."

At the present time it is difficult to make a control experiment since the medical profession generally would not feel justified in refraining from the use of antitoxin which has become the accepted treatment, and, therefore, feel morally bound to administer it in almost every case of the disease.

Martin, 11711. Dr. Martin cited, among others, a table gathered from experiences in Russia with early years of antitoxin treatment, viz. 1895 and 1896, as follows :—

—	Number.	Mortality per cent.
Cases treated with antitoxin - - - - - - - -	44,631	14·6
Cases not so treated - - - - - - - - -	6,507	34·1

He added that :—

11713. "In drawing conclusions from statistical data of this kind it is obvious that other factors may have to be examined and taken into account, as well as serum or no serum. The incidence of a disease like diphtheria is not uniform. It occurs in waves. In certain years it is more prevalent than in others, and no doubt the severity of the disease also varies somewhat in different periods. Another factor which will undoubtedly have modified the case mortality of diphtheria is the adoption of a more accurate method of diagnosing the disease by bacteriological examination. This will most certainly have led to the wider recognition of the disease in mild and atypical cases, and therefore to the inclusion in the statistical data of a number of less severe cases of the disease. The influence of this factor in diminishing the case mortality of the disease cannot be directly measured. The universal decline in case mortality coincident with the employment of a remedy based on national lines, and capable of being experimentally demonstrated, is, however, highly significant. Moreover, in the early experience in Paris and in that in the Russian provinces quoted above, the mortality of treated and untreated cases at the same time and under similar conditions can be compared. The result is just the same, viz., a case mortality of less than half amongst the cases treated with serum."

Another argument may be derived from the consideration of the results of the treatment, according to the day of the disorder on which it was administered. It has already been mentioned that Behring found that the remedy was most efficacious if administered simultaneously with the dose of toxin. If administered later a larger amount had to be given, and after two days in animals it was impossible to prevent the poisonous effects of the toxin, however much antitoxin was administered. On this point we cite here four tables brought before the Commission by Dr. Martin :—

11736.

Day of Disease upon which Antitoxin was first injected.	Total Number of Cases.	Total Number of Cases of Paralysis.	Percentage Incidence of Paralysis.	Number of Severe Cases of Paralysis not Fatal.	Number of Fatal Cases of Paralysis.
First - - - - - -	69	4	5·7	0	0
Second - - - - - -	277	28	10·1	1	0
Third - - - - - -	340	53	15·5	5	3
Fourth - - - - - -	323	61	18·8	8	7
Fifth, and later - - -	571	147	25·7	28	8
Total - - - -	1,580	293	18·5	42	18

THE RESULTS OBTAINED BY ANTITOXIN TREATMENT, ACCORDING TO THE PERIOD AFTER THE ONSET OF THE DISEASE AT WHICH THE INJECTION IS GIVEN (*Dieudonné Schutzimpfung u. Serumtherapie*, 1900).

Author.	Total of Cases.	Percentage of Total Mortality.	First Day.	Second Day.	Third Day.	Fourth Day.	Fifth. Day.	Sixth Day.	After Sixth Day.
Welch	1,189	14·2	2·3	8·	13·5	19·0	29·3	34·1	33·7
Hilbert	2,428	18·3	2·2	7·6	17·1	23·8	33·9	34·1	38·2
Collective Investigation of the American Paediatric Society	5,794	12·3	4·9	7·4	8·8	20·7	35·3	—	—
Collective Investigation in Austrian Sanitary Department	1,103	12·6	8·0	6·6	9·8	25·5	28·8	30·7	21·0
Collective Investigation of the Imperial German Health Office	9,581	15·5	6·6	8·3	12·9	17·0	23·2	—	26·9

Martin, 11733.

MORTALITY AT THE BROOK HOSPITAL (METROPOLITAN ASYLUMS BOARD), 1897 TO 1902, ACCORDING TO THE DAY ON WHICH ANTITOXIN TREATMENT BEGAN. (*Report, Metropolitan Asylums Board for* 1902. *Lancet*, 1903.)

Day of the Disease on which Treatment commenced.	Mortality per cent.					
	1897.	1898.	1899.	1900.	1901.	1902.
First	·0	·0	·0	·0	·0	·0
Second	5·4	5·0	3·8	3·6	4·1	4·6
Third	11·5	14·3	12·2	6·7	11·9	10·5
Fourth	19·0	18·1	20·0	14·9	12·4	19·8
Fifth	21·0	22·5	20·4	21·2	16·6	19·4

EIGHT THOUSAND AND THREE CASES OF BACTERIALLY-VERIFIED DIPHTHERIA TREATED WITH ANTITOXIN BY THE CHICAGO HEALTH DEPARTMENT BETWEEN 1895 AND 1905. (*Biennial Report of Department of Health, Chicago*, 1904–5, *p.* 137.)

Day of Disease	1st.	2nd.	3rd.	4th.	Later.
Number of Cases	608	2,063	2,802	1,496	1,034
Cases of Mortality per cent.	·32	1·66	3·64	11·03	21·08

Dr. Martin, referring to these tables, was led to the conclusion that :—

"From the above records it is clear that those cases in which antitoxin was administered on the first day have the lowest mortality, and that the death-rate rapidly rises as the period between the onset of the disease and the administration of the serum increases. This is exactly what happens in animal experiments. If antitoxin is administered to an animal on the same day as it is injected with a fatal dose of diphtheria bacilli, 100 per cent. can be saved. If on the day after, a large percentage of treated animals recover. If on the third day, only a small percentage, and after the third day antitoxin is unable to prevent death, that is, in animals. In other words, antitoxin is a remedy definitely known to be proportionately more effective the earlier it is administered, and the experience quoted above, derived from a wide source, shows that this is the case when serum is administered to children, just as was to be expected from animal experiments. If antitoxin were a remedy of no value, whether it were administered on the first or the fifth day of the disease would be immaterial. 11733.

56. *Tetanus.*—Tetanus, or lockjaw, is a malady common to men and to most animals. "The sufferings caused by the intense muscular spasms which are characteristic of the disease render tetanus one of the most horrible maladies one can witness." "It has long been known that tetanus was particularly prone to follow badly lacerated wounds, especially such as become contaminated with dirt." The infective nature of the disease was enunciated by Karl and Rattoni in 1884, and in the same year Nicolaier claimed that a certain bacillus in earth and dust, when injected into the tissues of animals, gave rise to tetanus. Kitasato in 1889 succeeded in isolating and cultivating a bacillus from cases of tetanus, and claimed to have proved by the injection of a pure culture of the organism into animals that it, and it alone, 11653.

was responsible for the disease. We have been informed that the bacillus of tetanus, like that of diphtheria, remains in the situation at which it finds entrance to the tissues, and that it produces the poisonous effects on the central nervous system by means of a poison which it secretes, and which is absorbed into the blood stream. So, also, witnesses have told us that as in the case of diphtheria toxin, so by the injection of tetanus toxin, it is possible to give rise to the production of an antitoxin for tetanus. Evidence has been put before us to the effect that this antitoxin is effective in preventing the disease when injected at the same time or shortly after the poisonous
Martin, 11740. dose, but it has not been proved of practical value in the treatment of tetanus. Dr. Martin explains this by saying that the disease is usually only discovered after severe lesions of the nervous system have been already produced, upon which, unfortunately, the antitoxin can exert but little influence. The use of this remedy has been, therefore, chiefly as a prophylactic in cases where from the extent of laceration or contamination with dirt or stable soil, the supervention of tetanus is apprehended. Sir Henry Morris mentions that such prophylactic use has been made of value in an outbreak of tetanus at Prague in 1889, and also as an immunising agent in man in relation to the injuries caused by toy pistols during the Independence Day celebrations in America. Sir Henry Morris adds that:—

Morris, 7726. "The modern study of tetanus has done away with the useless operations of nerve stretching and nerve dividing which were based on the fallacious theory that tetanus is ascending inflammation of a nerve." . . . "It has led also to the more frequent, early, and complete excision of the wounded tissues because it is now known that the bacilli at the outset are limited to the tissues in the immediate area of the wound."

Martin, 11755–6. 57. *Dysentery*.—The bacillus of bacterial dysentery was first announced by Shiga in Japan in 1898, and the same bacillus has been found in an outbreak in the South of Germany, as well as in Manilla. We have been informed that the bacillus is pathogenic for the guinea pig and rabbit, and that in these animals the bacillus itself or the poison obtained by growing it in broth produces dysentery, diarrhœa, certain nervous symptoms, paralysis and death within a few hours or days. We have also been told that by an injection into horses it is possible to get an antitoxin serum, and that this anti-dysentery serum possesses preventive and curative properties when employed on animals inoculated either with the living bacillus or its toxin. The data as to the value of anti-dysentery serum as a therapeutic agent in man are not yet extensive or conclusive, but so far as it has been used the results are said to be of a very promising
11758. character. Thus in 298 cases treated by the serum the death rate was 10·8 per cent. while in 2,599 cases treated at the same time by other means the mortality was 35·3 per cent. Similar results have been published by Creuse, Rosenthal and others.

Bruce, 14209. 58. *Malta Fever*.—The disease is found all over the world. In Gibraltar it is known as Rock fever; in Kimberley as Camp fever, and the Americans found it in the
14216. Philippines. It is usually, however, known as Mediterranean fever. A number of cases classed as simple continued fever or remittent fever are, according to Sir David
14395. Bruce, really Malta fever, and he told us that when he first went out to Malta every
14493. severe case was called enteric fever. The disease is characterised by long duration. There is extreme weakness, the patient becomes emaciated and suffers from severe pains in the joints and nerves, so that although not nearly so fatal as enteric
14234. fever or typhoid, it is generally regarded as a serious affection. At one time Malta
14406–13. fever was universally attributed to effluvia; the grand harbour of Malta having been used as a sewer for centuries had, until recently, been in a disgraceful condition,
14493. and was thought to be the source of morbid emanations. Captain Hughes attributed
14408. the fever to excremental pollution of the soil. The mosquito theory was strongly held for some time, but Sir David Bruce could not get any proof of the mosquito being the carrier. According to him up till 1905, in the garrison at Malta, there was
14201. each year an average of 700 men affected with the fever for, on an average, 120 days each, *i.e.*, four months. Many were invalided from the service altogether, and a
14206. certain percentage died. Sir David Bruce claims that the results of experiments (almost painless) have shown that Malta fever is due to the presence of a minute organism, the micrococcus melitensis, and that it is possible to produce the disease in monkeys by injecting them with this micro-organism. It was not found possible to infect the ordinary laboratory animals such as the rat, guinea pig or rabbit. During 1904 a Commission was appointed to investigate this disorder, and especially to determine its mode of transmission. As a result of a number of experiments on animals the conclusion was arrived at that practically all channels of infection other than food might be excluded, and that milk derived from the native goats was the source of the disease. It was also found that in a certain percentage of cases the

blood and milk of those goats contained the specific micro-organism although their health did not appear to suffer in any way. As a result of these and other observations the Commission came to the conclusion that the only way that a man takes Malta fever is by the drinking of goats' milk. Orders were given in July, 1906, that no goats' milk should be supplied to the troops, and from that date, it is stated, the practical disappearance of this disorder from the soldiers and sailors stationed at Malta began. Sir David Bruce put in charts showing the incidence of this disease from 1899 to 1907, together with the following table :—

NUMBER OF OFFICERS, NON-COMMISSIONED OFFICERS AND MEN INVALIDED FROM MALTA TO ENGLAND FOR MALTA FEVER, 1897–1907.

Bruce, 14220.

Year.	Officers.	Non-Commissioned Officers and Men.
1897 - - - - - - -	9	110
1898 - - - - - - -	10	121
1899 - - - - - - -	8	120
1900 - - - - - - -	15	43
1901 - - - - - - -	9	92
1902 - - - - - - -	21	36
1903 - - - - - - -	21	133
1904 - - - - - - -	14	100
1905 - - - - - - -	21	382
1906 - - - - - - -	12	165
To end of September, 1907 - -	—	1

In answer to a question as to how he would have set to work to discover the cause of Malta fever with a positive prohibition against experiments on animals, Sir David Bruce stated that :— 14261.

"In the first year of the Commission a Local Government Board man, who was recommended by Mr. Power, went out to Malta to work at the fever from the epidemiological standpoint, to examine, by the method of statistics, the incidence among those who drank water, beer, milk, and all that kind of thing, in order to see whether any light could be thrown on this fever; but after very hard work for six or seven months, during which time he lost 4 stone in weight, he came to the conclusion that milk, at least, was not the cause; so that the epidemiological way of working at the causation of this fever absolutely failed. We sent out others; we sent out one or two men, and we sent out Colonel Davis, who is a man who worked at it in the same way, and he came to the conclusion that milk was not the cause, showing the difficulty of any other method of discovering the cause." 14256–8.

It is not clear that one attack of the disease causes immunity from subsequent 14264–74.
ones. The epidemiology of the disease has not yet been completely worked out. Sir David Bruce allows that the micrococcus might have been discovered by microscopic examination alone, and that from differential observations of those who drank, and of those who did not drink goats' milk the causal relation of the microbe might have 14374–9.
been discovered, but he regards animal experiments as having led to the complete determination of the cause. Sera and injections with preparations derived from the 14368–9.
dead micrococcus have been tried, but so far without success.

Treatment, according to Sir David Bruce, must be on broad lines, rather than on serum lines, and he claims that the obligatory disuse of goats' milk since July 1st, 14245.
1906, has led to the practical blotting out of Malta fever among the troops in Malta 14283–6.
since that date. He also cited the case of certain Maltese goats infected with the disease which were shipped from Malta to Antwerp and thence to America, in which 14399–14407
the same association between the consumption of goats' milk and the supervention of Malta fever was stated to have been exhibited; but in this case the association does not appear so striking as in the case of the statistics of the troops in Malta. There it is claimed that through stopping the consumption of goats' milk the cases of Vol. IV. of
Malta fever fell from 643 in 1905 to 161 cases in 1906 and to 10 cases from January Evidence.Cd.3955
to September in 1907. In the same three periods, cases of continued fever fell from Table, p. 61.
1,199 to 508 to 283, and of enteric from 64 to 14 to 5. Sir David Bruce considers that 14396–7
the falling off in cases of so-called continued fever and enteric fever may be attributed also to the stoppage of goats' milk. Sir David Bruce informed us that among the civil population of Malta who have continued to consume goats' milk the number of cases of Malta fever had not been reduced.

59. *Snake Poison.*—The question of finding an antidote for snake poisoning by experiments on animals engaged the attention of the former Royal Commission in 1875. It was stated that many thousands of the natives of India and great numbers of domestic animals perished annually from snake bites, and the late Sir James Paget laid stress upon the hope of discovering an antidote, as proof of the absolute necessity of the performance of such experiments, since he held that :—

R.C. of 1875. Q. 305. "We cannot expect that short of fifty years hence a true antidote for snake bites should be found by any other means."

Brunton, 6872–3. At that time, 1876, the deaths from snake poisoning in India were 15,819, or 85 per 1,000,000, and in 1905 the number had risen to 21,797 or 94 per 1,000,000. Sir
6868. Lauder Brunton directed our attention to a method of treating snake poisoning by the application of permanganate of potash, the efficacy of which against the poison of all venomous snakes when properly applied is said to have been conclusively demonstrated.

Rogers, 8034–6. A serum prepared by Calmette has been used as an antidote but is said to be of value only against the venom of the particular class of viper from which it is prepared and
8040–54. is not practically of very great value. In 1901, Major Rogers, at Sir Lauder Brunton's suggestion, made further experiments with the local application of permanganate of potash and he considers that it neutralises the poison of the bite of the cobra in animals
8060. and also in man. In the latter case it has been used together with a ligature applied
8093. above the bite. The natural mortality of persons bitten by venomous snakes is given
8090. Martin, 12272. variously as from 30 to 75 per cent.

Dr. Martin, quoting Wall, gives reason for doubting the practical value of perman-
12276 ganate of potash as advocated by Sir Lauder Brunton and Major Rogers, at any rate in the human subject.

Work of Public Health Authorities.

60. Evidence has been laid before us as to the important part played by micro-organisms in the production of disease, and the value of researches into the conditions of infection to those charged with the health of public communities, and a considerable amount of such work is carried on under their supervision and at their suggestion.

Evidence of this kind has been laid before us in connection with the work of: (1) The Local Government Board; (2) the County and Urban District Councils; (3) the Board of Agriculture.

Sir W. H. Power, of the Local Government Board, stated that experimental investigations were undertaken for the Board of two classes—investigation of problems, the prompt solution or provisional solution of which promises to be of immediate administrative advantage to the Board in the exercise of their public health functions; and studies which from their nature cannot promise rapid results, but require systematic and continuous labour extending over considerable periods of time, and he gave as an instance the researches initiated by the Board in the year 1901–2 under the head-
Power, 4301. ings of: (1) research for administrative purposes; (2) research for prophylactic purposes; (3) research for investigatory purposes, in all of which experiments on animals were deemed necessary. In that year owing to the threat of plague obtaining an entry into England and the obligation of the Foreign Office to notify at once to all other Governments all cases of plague making its appearance in this country, as well as in the interests of public health and commerce and because the tests of suspected plague material in the opinion of the Board involved in almost every instance intraperitoneal injection of guinea pigs, the Board, in the exercise of their function of safeguarding the country against exotic disease, directed the use to an exceptional extent of animal experiment.

Again, in case of an outbreak of a disease such as cerebro-spinal fever, about which comparatively little is known, although at the same time it is a very serious infectious disease, Sir W. H. Power told us the nature of the procedure adopted by the Local
4304. Government Board. They would investigate for themselves the nature of the disease, endeavour to find out its causation and seek by means of experiments on animals to obtain a prophylactic against it. If they were successful and had satisfied themselves of its value they would then notify the local public bodies that they were able to supply a certain amount of such prophylactic.

The Local Government Board have already in previous years notified their willingness to supply, *e.g.*, antitoxin serum for diphtheria, Haffkine's prophylactic and the plague prophylactic. With respect to the first of these, there is no longer any need for the Local Government Board to manufacture it, since it is now a commercial product easily obtainable. As to the plague prophylactic, Sir W. H. Power has great hopes that from investigations now undertaken by the Local Government Board great improvements in the prophylactic may result.

Evidence has also been laid before us by Mr. Lorrain Smith, Professor of Pathology in the University of Manchester, in connection with the work of County and Urban District Councils in the diagnosis and notification of infectious disorders, testing of food supplies, especially milk and water for tubercle typhoid, bacillus coli and other bacilli, also for the presence of micro-organisms of putrefaction, and investigations of cases of food poisoning, such as those attributed to the bacillus enteritidis. In all these cases it is deemed necessary for the Public Health Authorities to have recourse to experiments on animals.

Diseases of Animals.

61. A most important part of the epizootic work of the Board of Agriculture is that in connection with the prevention of the spread of contagious diseases of animals in this country. For this purpose it is imperative that the diagnosis of the suspected disease should be accurately made, and in many cases of contagious diseases it is held to be only possible to obtain this end by inoculation experiments on animals; among such diseases may be Stockman, 2465.
mentioned anthrax, glanders, tuberculosis, swine fever and swine erysipelas. Further, 2466.
there are a number of diseases of a very destructive nature, some of which have been and others might be imported into this country, such as rabies, foot-and-mouth disease, rinderpest, pleuro-pneumonia, sheep-pox, dourine, Texan fever, etc. The responsibility placed on the officials at the ports, who are entrusted with the diagnosis of diseases such as these, is a very serious one, and in such cases again resort is had to inoculation or kindred experiments on animals.

In addition to the important duties relating to the diagnosis of contagious diseases affecting animals, the Board is also largely concerned with the prevention of such diseases, and among the methods advocated by the Board that of preventive inoculations takes a place. With respect to these, evidence has been given us by Mr. Stockman, Chief Veterinary Officer of the Board of Agriculture and Fisheries.

62. *Rinderpest.*—One of the most serious diseases in all stock raising countries is cattle 2480.
plague or rinderpest, the serious nature of which was brought home to the British public when it invaded this country in 1865. The disease raged off and on between 1865 and 1869, and was stamped out successfully by the slaughter of all infected animals and of all animals which had been in contact with such infected animals; it cost the country in compensation for cattle slaughtered and other expenses, £1,119,994.

It came again in 1877 and cost £13,423 in compensation for animals slaughtered to prevent its spread, and since that date has not re-appeared in this country. Attempts 2484.
at vaccination against this disease were made but abandoned, but according to Mr. Stockman the appearance of rinderpest in South Africa in 1897 led to the disease being studied afresh with a view to discovering a more reliable method of preventive inoculation. He states that:—

"The investigations were successful, and the benefits derived from anti-rinderpest serum are 2484.
recognised in every country where the disease has appeared since the method was introduced."

"The discovery of the serum method has," in the opinion of Mr. Stockman, who 2491.
was principal veterinary surgeon to the Transvaal Government between March, 1903, and November, 1904, "been an enormous boon to South Africa." A system of 3331-4.
inoculation recommended by Koch appears to have been discarded. Whether the use of anti-rinderpest serum will obviate entirely the recourse to slaughter remains at present doubtful. The cause of the disease has not been identified, but it is held to be an 2714.
invisible organism. 2684.

63. *Pleuro-pneumonia.*—Another very serious disease is contagious pleuro-pneumonia, a disease also supposed to be due to a microbe which, though so small as to be invisible 2503.
or barely visible under the highest powers of the microscope, can nevertheless, it is stated, be grown in artificial culture—a pure virus being thus obtained. This is not the 2699-2702.
first micro-organism which has been alleged to be the cause of pleuro-pneumonia. Lustig, 3350-5.
Arloing, Sternburg and others, as the result of experiments on animals, claimed in 1889 to have discovered the cause of the disease. But none of these micro-organisms are now 2702
considered to be the true cause.

In Great Britain from 1845 to 1878 there are, according to Mr. Stockman, records of 1,021 cattle having died of this disease, and to prevent further spread there were slaughtered 29,722 diseased and 3,019 healthy cattle in contact. This cost the local authorities £176,137 in compensation. From 1879 to 1883, 389 cattle died of pleuro-pneumonia; 10,322 diseased and 4,142 healthy cattle were slaughtered to prevent
Stockman, 2494. further spread. From 1884 to 1890, 315 cattle died of the disease; 12,166 diseased and 28,451 healthy cattle were slaughtered to prevent further spread. The cost in compensation was £334,302. From September, 1890, to the end of 1898, when the disease was finally stamped out in Great Britain, 1,605 diseased cattle and 21,092 healthy in-contact cattle were slaughtered, the cost in compensation being £357,626. In 1888 a Departmental Committee was appointed under the Chairmanship of Lord Cranbrook to enquire and report, *inter alia*, "upon the nature and extent of pleuro-pneumonia in the United Kingdom, and the effects of inoculation and other preventive measures in that disease." A large portion of the enquiry was directed to a searching examination of inoculation as an alternative policy to slaughter for stamping out the disease, and the conclusion come to was that "inoculation (for the reasons already detailed) cannot be recommended as a means of eradicating pleuro-pneumonia, nor as practicable under existing conditions." Mr. Stockman, on the other hand, drew attention to:—

2494. "what took place in the Transvaal after the war, during the re-stocking operations, which resulted in the dissemination of pleuro-pneumonia. The country could not afford to stamp out the disease by wholesale slaughter, and in addition to the financial difficulty such advice would have been unjustifiable in a country where most of the farm work and local transport has to be carried on by oxen, and where the animal herds had already been reduced to an almost impossible number, owing to a long war coming on the top of rinderpest. The Veterinary Department advised, therefore, that pleuro-pneumonia be dealt with by slaughter of the affected and compulsory inoculation of contact animals by means of a pure virus."

He states that:—

2506. "From May, 1903, to June, 1904, 256 outbreaks of the disease were dealt with in this way; 741 affected animals died or were slaughtered, and 9,000 in-contact animals were inoculated. By the method of slaughtering contacts, the compensation payable would have been about £135,000. In the following year (1904–5), the results of inoculation became apparent, as only thirteen outbreaks occurred, and the number of in-contacts which had to be inoculated fell to 3,109."

2522–3. 64. *Tetanus* affects animals as well as man, as stated above, and it is attributed to a bacillus found in the soil and in horse dung. Wounds of animals, the result of accident or made in the course of stock breeding such as castration, are liable to contamination.
2510–7. The disease is much more common in some districts than in others. Anti-tetanus serum has been employed in animals both as a curative agent and as a prophylactic. For
3276. curative purposes its use has not been very encouraging but for the prevention of tetanus, according to Mr. Stockman, "serum has been successfully adopted all over the world."

65. *Anthrax*.—Another very serious disease of flocks and herds is anthrax. According to Mr. Stockman its spores persist for a long time in a pasture when it has once obtained admittance. He stated that:—

2524 "There is no known method of effectively disinfecting a contaminated pasture. Some regions and some farms are so badly infected that farming operations are utterly impossible unless something is done to protect animals against anthrax infection."

A method of anti-anthrax serum protective inoculation introduced by Pasteur has been somewhat modified and is largely employed, and "in France," according to Mr. Stockman, "over four and a half million animals have been inoculated during the last sixteen years in anthrax infected districts, with the result that the death rate in infected places from anthrax has been reduced from 10 per cent. to ·91 per cent." Professor Muller of Berlin, and others have opposed these preventive inoculations, preferring careful destruction of the carcases and disinfection as a surer method of combating the disease.

Mr. Stockman stated:—

2743. "that it may not be always worth one's while to inoculate, that is to say, on a farm where a few cases occur every year, you would not inoculate."

but he also said that

2541. "in South America there are large tracts of cattle breeding country, in which it is necessary to protect every bovine animal by inoculation, or risk a death-rate from anthrax which means financial ruin to the farmers."

66. *Blackquarter.*—Again, in the case of blackquarter, the spores of the microbe are said to remain virulent in the pastures for an indefinite period. By means of a protective inoculation the death rate from this disease has, it is claimed, been reduced from 14 per cent. to less than 1 per cent. Stockman, 2544.

67. *Other Diseases.*—In tropical countries certain diseases of animals are attributed to protozoa conveyed by insects; these are held to be of a different character to the diseases attributed to bacteria, and the study of any preventive inoculation in relation to them is very much in its infancy. According to Mr. Stockman :—

"It has already been found possible to give mules a high degree of immunity against *South African horse sickness*, a seasonal disease which in some parts of Africa annihilates practically every horse in a district, and makes the settlement of these districts almost impossible. Advances have also been made in the prevention of such diseases as *Redwater*, *Heartwater*, and *Blue-tongue*." Stockman, 2568.

68. *African Coast Fever.*—The exhaustive and costly experiments carried out at the instigation of Koch with a view to protective inoculation against African Coast Fever and horse sickness resulted in failure and disappointment.

He devised methods of protective inoculation with a view to eradicate these diseases. According to Mr. Stockman, however, these methods of protective inoculation have proved to be failures; experiments conducted by the latter and Dr. Theiler gave opposite results to those of Koch, and it is stated that these diseases are not directly inoculable and are accordingly not amenable to serum treatment. Stockman, 3341–8. 3349. Transvaal Report of Agriculture 1903–4, pp. 40–59

69. *Swine Erysipelas.*—Two diseases of swine were also referred to by Mr. Stockman as requiring a diagnosis to be established without dubiety before putting in operation the draconian methods needed for combating such diseases, viz., swine erysipelas and swine fever. Experiments on animals have been resorted to for the investigation and prevention of these diseases. Stockman, 2464–5.

In the case of swine erysipelas a method of protection was devised by Pasteur, which consisted of an inoculation of the virus. This method was not very successful, as many as 5 per cent. of the vaccinated animals succumbing to the inoculation, and it has been superseded by a method which consists of the simultaneous injection of the microbe and an antitoxin serum obtained from the horse. This method is said to have proved of great value. We were told, for instance, that in certain districts of Hungary which were infected with swine erysipelas the death rate on 4,000,000 observations was reduced in inoculated animals to 1·6 per cent, whereas in the non-inoculated it amounted to about 20 per cent. The antitoxin serum is also said to have a curative effect on those already infected. 2755–6.

70. *Swine Fever.*—In the case of swine fever no fewer than fifteen varieties of microbe have been put forward by different observers as the cause of the disease and subsequently disproved. No visible organism can, according to Mr. Stockman, be identified as the true cause, although he believes that the fever is due to a microbe. The nature and prevention of the disease is still under investigation by the Board of Agriculture. 2675–6. 2682. 2836–7. 2677. 2466.

71. *Tuberculosis and Glanders.*—Two other very serious diseases in animals, both of which are communicable to men, are tuberculosis and glanders. It is held that these two diseases may exist in animals in an occult form which ordinary methods of diagnosis completely fail to discover. By means of experiments on animals two substances, tuberculin and mallein, the one a product of the tubercle bacillus and the other of the bacillus of glanders, are now employed for the diagnosis of these two diseases. By the injection of these agents in respective cases, a reaction occurs which is regarded as evidence of the existence of these diseases, *i.e.*, a horse affected with glanders will react to mallein and an animal affected with tuberculosis will react to tuberculin.

72. *Louping-Ill and Braxy.*—We have also had before us further examples of the experimental work of the Board of Agriculture in the evidence of Professor Hamilton, who represented the University of Aberdeen and who was Chairman of a Departmental Committee of the Board of Agriculture appointed in 1901 to investigate the two diseases of sheep known as louping-ill and braxy, which he informed us are "the cause of enormous mortality all over Great Britain, especially in Scotland and the northern counties of England and also in Ireland." The investigations of the Hamilton, 20138. 19996.

Committee resulted in the discovery of a bacillus which they believed to be the cause of "louping-ill" and confirmed Nielson's discovery of a bacillus as the cause of
Hamilton, 20033. "braxy." These bacilli are similar to each other and also to that found in anthrax.
20024. There appears to be a family likeness, according to Professor Hamilton, between
20143–54. louping-ill, braxy, blackquarter, malignant edema and other diseases of sheep, which
20162. renders their differentiation a matter of difficulty. The Committee proceeded to consider the question how to obtain immunity from these diseases. In regard to braxy they noticed that the second-year sheep were practically safe from the disease and that the great mortality took place in the first-year sheep. They noticed also that braxy begins in September and goes on to the middle or end of February, the sheep being able apparently to resist the disease much more during the
20039. summer than during the winter months. Further, in those animals that die of the disease the intestines are found to be full of bacilli, and they concluded that the disease
20057–8. is introduced through the pasture. They accordingly argued that the two-year-old sheep that were practically immune owed their immunity to having contracted the disease in a mild form during the months from March to September. Acting upon
20080. this supposition, they proceeded to vaccinate the animals with an attenuated virus, but this was found to be a dangerous method, a good many sheep being killed by these experiments; they then made pure cultures of the bacillus and administered them by the mouth to all first-year sheep during the month of August, a time of year when sheep are naturally most immune. This method was also applied in the case of "louping-ill," and, indeed, the attempt has been made to administer a protective
20082. drench against all this group of braxy-like diseases. The drenched animals remain ill for a day or two in the case of louping-ill. One proprietor described it "as if they were all intoxicated." He thought they were all going to die, but, he said, "most remarkably they all recovered," and he did not lose a single one of them. An element of uncertainty in interpreting results consists in the fact that at one season
20166–70. of the year the blood of the healthy sheep destroys the bacilli, while at other seasons this bactericidal property is absent; during the latter period great danger may be incurred by preventive treatment.

20163–71. As yet there has hardly been time to estimate the effects of this treatment, but Professor Hamilton himself was well satisfied with the results so far obtained, and the experiments themselves have been watched with great interest and favour by the farmers of Scotland. The Departmental Committee over which Professor Hamilton presided commented on the complexity and intricacy of the question of contagious diseases of sheep, and issued a warning against prematurely formulating conclusions which may prove disappointing. Professor Hamilton considers the question of
20165. the treatment and prevention of those diseases to be still *sub judice*. The Committee state :—

20163. "The problem of the prevention of the contagious diseases of the sheep is a large one, and will most
Report of Departmental Committee on Louping-ill and Braxy, Cd. 2932 of 1906, pp. 27–8. likely require years of patient observation and experiment before it will reach perfection. Those which are the most destructive in this country are closely related, and hence there is good reason for believing that they may be combated upon the same principles as those which have proved so effectual in Louping-ill. It may turn out that the sheep can be immunised to several of them at the same time—our experiments indeed, limited though they may be, point in this direction—but the subject as yet is almost
20163–4. untouched, and will require lengthy and patient enquiry before the truth can be ascertained. In the meantime we would beg those interested to exert a little patience, and not to draw conclusions at present from what may seem to them, in certain instances, a disappointing result. The pathology and prevention of this class of diseases of the sheep is far more intricate than the ordinary layman may suppose, but we feel assured that we are on the path which will ultimately lead to a successful issue."

Public Recognition of the Value of Experiments.

73. Finally, in recent years the value of the experimental method has been very largely recognised by the public at large as well as by various public bodies. This recognition has taken practical shape in various ways, such as :—

(1) The foundation of Schools of Tropical Medicine, subsidised by the Colonial Office and Colonial Governments, and the appointment of research expeditions or commissions to investigate on the spot such diseases as sleeping sickness, plague, malaria, Malta fever, etc.

(2) The foundation of an Imperial Research Fund for the purpose of investigating cancer.

(3) The appointment of a Royal Commission to investigate by experimental methods, and otherwise, that great scourge to the human race, tuberculosis.

Conclusions.

74. It will be apparent from the foregoing analysis of the evidence brought before us that we have made full enquiry into the practice of subjecting live animals to experiments by vivisection and otherwise, as regards :—

(*a*) The recent progress of medical science ; and

(*b*) The practical results that have been obtained.

We have received evidence from persons eminent in physiological, pathological and sanitary science who have testified to their belief that knowledge has been acquired in regard to the vital functions, the causes of diseases and also in regard to means for their prevention and cure which, in their opinion, but for such experiments, could not have been acquired. We have on the other hand heard many witnesses, some of them having medical qualifications, who have disputed that valuable knowledge has been obtained by such experiments, maintaining that this knowledge has been erroneously attributed to such experiments, or who have contended that success has not attended the application of the knowledge to the preventive or curative treatment of disease.

Having regard to the witnesses who have appeared before us and to the evidence which we have received, there can be no doubt that the great preponderance of medical and scientific authority is against the opponents of vivisection. This is more markedly so now than was the case before the Royal Commission of 1875.

75. On these questions, and apart altogether from the moral and ethical questions involved in the employment of experiments on living animals for scientific purposes, we are, after full consideration, led to think :—

(1) That certain results, claimed from time to time to have been proved by experiments upon living animals and alleged to have been beneficial in preventing or curing disease, have, on further investigation and experience, been found to be fallacious or useless.

(2) That, notwithstanding such failures, valuable knowledge has been acquired in regard to physiological processes and the causation of disease, and that useful methods for the prevention, cure and treatment of certain diseases have resulted from experimental investigations upon living animals.

(3) That, as far as we can judge, it is highly improbable that, without experiments made on animals, mankind would at the present time have been in possession of such knowledge.

(4) That, in so far as disease has been successfully prevented or its mortality reduced, suffering has been diminished in man and in lower animals.

(5) That there is ground for believing that similar methods of investigation if pursued in the future will be attended with similar results.

PAIN IN EXPERIMENTS ON ANIMALS.

76. The third question that we proposed to consider at the commencement of our enquiry was :—

(c) How far immunity from pain in experiments on animals is or can be secured.

We will proceed to give the results of our investigation of this most important matter in some detail. We deal with it under the heads of Anæsthetics, Inoculations and Miscellaneous Questions.

Anæsthetics.

R.C. of 1875. Report, pp. x–xi. 77. The Royal Commission of 1875 stated in their Report that :—

"The recommendations we shall humbly submit to Your Majesty will turn in a great measure upon the use of anæsthetics." and that :—

R.C. of 1875. Report, pp. xvii–xviii. "The whole subject (of vivisection) has been, or, at least, ought to have been, relieved of the greater part of its difficulty by the discovery of anæsthetics.

and among the conclusions at which they arrived was this :—

R.C. of 1875. Report, p. xvii. "that by the use of anæsthetics in humane and skilful hands the pain which would otherwise be inflicted may, in the great majority of cases, be altogether prevented, and in the remaining cases greatly mitigated, and that the infliction of severe and protracted agony is, in any case, to be avoided."

While they recorded the opinion that

R.C. of 1875. Report, p. xviii. "it is not to be doubted that inhumanity may be found in persons of very high position as physiologists."

they held that the support of

"the most distinguished physiologists and most eminent surgeons and physicians may be confidently expected for any reasonable measures intended to prevent abuse."

Effect was sought to be given to these views by Section 3 (3) of the Act of 1876, which requires that

"the animal must, during the whole of the experiment, be under the influence of some anæsthetic of sufficient power to prevent the animal feeling pain."

But this is followed by the proviso that

"experiments may be performed without anæsthetics on such certificate being given as in this Act mentioned that insensibility cannot be produced without necessarily frustrating the objects of such experiment."

And similar provision is made for keeping the animal alive after it has recovered from the anæsthetic if its destruction before such recovery would necessarily frustrate the object of the experiment.

78. We proceed to give a summary of the evidence received by us with a view to determine how far it is possible to subject animals used for experimental purposes to complete anæsthesia, the relative efficiency of the various agents employed for that purpose, the degree to which the disuse of anæsthetics has been sanctioned or practised, and the extent to which the requirements of the law in regard to anæsthetics are in fact secured or may need to be amended.

Cushny, 5040–3. Dr. Cushny, who is Professor of Pharmacology at University College, assured us that it is quite easy to administer anæsthetics to animals and thereby to secure complete immunity from pain; he preferred the use of general to local anæsthetics lest
5046. the animal should be frightened, and he mentioned urethane and paraldehyde as
5048. well as ether, chloroform, and A.C.E. mixture (composed of alcohol 1 part, chloroform 2 parts, and ether 3 parts) as suitable anæsthetics for animals, especially if given
Starling, 3605 in large lethal doses. Professor Starling confirmed this view and expressed the belief that all English physiologists intend to secure and do secure that experiments are
3607. so performed throughout as to prevent any infliction of pain. He employs morphia either as an adjunct to chloroform or sometimes alone as an anæsthetic. He said :—

3451. "Though I have been engaged in the experimental pursuit of physiology for the last seventeen years, on no occasion have I ever seen pain inflicted in any experiment on a dog or cat, or, I might add, a rabbit, in a physiological laboratory in this country."

Not only in his opinion is this result secured by "the normal humanity" of the operator but also by his desire to remove all other disturbing factors, of which he regards
Langley, 15105 *et seq.* none as greater than that of pain. Professor Langley, of Cambridge, and most of the other physiologists who have appeared before us, have corroborated the views of Dr. Cushny and Professor Starling.

79. There was some conflict of opinion as to the feasibility and the safety of administering anæsthetics to animals. Mr. Hobday, F.R.C.V.S., informed us that he had administered chloroform to many hundreds of animals, including dogs, cats, horses, cattle, lambs and pigs, with very few fatalities. He is satisfied as to the practicability of inducing and maintaining a condition of complete insensibility to pain in animals by the administration of chloroform. Want of skill in administration was in Mr. Hobday's opinion responsible for the mortality which formerly attended the chloroforming of dogs. This may account for a view which has been repeatedly urged that it is more difficult to maintain anæsthesia by chloroform in a dog than in man and some other animals. Sir W. Thornley Stoker, Inspector under the Act for Ireland, entertained this opinion. Professor Schäfer had met with cases in which dogs took the anæsthetic badly or died while being put under, and he made a series of investigations on the action of chloroform and the way in which it kills. He suggested the use of atropin to counteract the inhibitory action of chloroform on the heart through the vagus nerve. We do not think there is any evidence to show that a condition of complete anæsthesia cannot be obtained and maintained in dogs as in other animals, although liability to death under certain anæsthetics is somewhat greater in dogs than in men.

Hobday, 16301–16312. 16327–30. 16299. Starling 3482–4. Smith 13082. Cushny 5091. Stoker, 812. Schäfer, 10088–10100. Horsley 15909.

80. No doubt great importance attaches to the manner in which the anæsthetic is administered to animals in order to make certain that complete insensibility to pain is established and maintained during the whole course of an experiment, otherwise the advantages which anæsthetics are capable of affording may not be secured. Dr. D. W. Buxton, who has had a very large experience in the administration of anæsthetics to the human subject, described three stages in the induction of anæsthesia, the first from the commencement of inhalation to the loss of conscious voluntary movements, passing into the second, in which, though there may still be movements, they are not controlled by the higher perception centres, and lastly the third stage of complete anæsthesia which is marked by the loss of all such movements. In the first of these stages sensation is very much lessened, in the second it is still more lessened and in the third it is absolutely lost. Some reservation, however, is made in regard to certain movements spoken of as "reflex," which may be evoked by stimulation of certain parts of the body, although such movements are unconscious and involuntary and unaccompanied by any sensation of pain. Thus touching the surface of the eyeball may cause blinking even though anæsthesia may have been established and this "conjunctival reflex" is habitually employed in men as an indication by its disappearance of the subject having passed from the second into the third stage. There is, however, it would appear, a period during both the going under and the coming out of the anæsthetic state, in which this conjunctival reflex can be evoked in a sluggish way even though insensibility to pain may be complete. Such a period in the process of recovery is of fairly narrow limits, is narrower still in the process of induction and can be maintained only with difficulty. These "reflex movements" are held to be compatible with complete anæsthesia.

Buxton, 12390–2. 12398. 12414–7. 12544–56.

Dr. Buxton uses the term "incomplete anæsthesia" as meaning "that degree of narcosis where pain is felt"; on the other hand Sir Victor Horsley assured us that the term "incomplete anæsthesia" was well understood among surgeons and that :—

12544.

> "By the term 'incomplete anæsthesia' we do not mean that the subject of the operation or experiment as the case may be is conscious of pain at all. All that we mean is that its nerve centres are not so poisoned that it is not capable of making various demonstrations, even reflex acts. Another expression for the same thing that is very frequently used is light anæsthesia or very light anæsthesia."

Horsley, 15724.

Professor Langley informed us that the term was not used with very great scientific accuracy. It is unfortunate that this term has been used with such different signification and much misunderstanding has in consequence arisen.

Langley, 15264. Horsley, 15728. Coleridge, 10991–11043.

Other scientific witnesses have spoken of movements other than reflex movements, purposive movements, struggling, and vocal cries, as occurring under anæsthesia and yet not in their opinion indicative of any suffering, since in the case of man these are found on recovery to have been either unconscious or unassociated with any painful recollections. Sensibility to pain, we are assured, disappears during chloroform administration before loss of consciousness, later still voluntary movements cease, though reflex movements persist, while the movement of respiration continues after these have disappeared; the sequence is reversed on cessation of the administration. In the case of animals, besides the presence or absence of reflexes, the blood pressure is said to be also a guide as to the completeness or otherwise of anæsthesia.

Horsley 15717–9. Morris 7937–40. Langley 15119. Morris 7937–52. Schäfer 10222–4. Langley 15274–5. Dixon 18788. Starling 4054.

81. The evidence shows that while by respirable anæsthetics such as chloroform it is possible to establish and maintain, even for long periods of time, a condition of complete insensibility to pain in the lower animals, as in man, yet their administration in order to secure such anæsthesia requires caution and watchfulness as well as the natural solicitude of a humane and skilful operator.

82. The Act does not define the term anæsthetic nor prescribe the kind of anæsthetic which is to be employed. Indeed there is some ambiguity between the wording of Section 2, which forbids the performance "on a living animal" of "any experiment calculated to give pain, except subject to the restrictions of the Act," and Section 3 (3) which (apart from exemptions conferred by certificate) imposes the restriction that "the animal must during the whole of the experiment be under the influence of some anæsthetic of sufficient power to prevent the animal feeling pain." It may of course be urged that an animal under an anæsthetic cannot be the subject of "any experiment calculated to give pain"; just as it has been urged that in the case of a decerebrated cat, whose respiration was artificially maintained, the Act did not apply, as the animal was to be regarded as no longer a living animal. As Professor Starling said :—

Starling, 3462. "We can cut away the higher parts of the brain, so that the animal, as an individual, no longer exists, and then we can study reflexes which, if the animal were conscious, would result from what we should call pain; we can cut away, so to speak, the soul of the animal and keep its machinery."

Similarly, "pithed" frogs are regarded by experimenters as being outside the Act and large numbers of these are subjected to experiments by persons without any licence under the Act. We have dealt with this matter in another portion of our Report (Paragraph 90).

Other means of inducing anæsthesia are practised by experimenters besides the use of chloroform, ether, and A.C.E. mixture. Such are the administration of opium, morphia, 3607. urethane, chloral, or paraldehyde, either by subcutaneous or intravenous injection; and of local anæsthetics, such as cocain, which may be applied locally to mucous surfaces or injected into the sheath of the spinal cord or under the skin.

Coleridge 11576–9. Buxton 12541–2. Starling 3836–8. Several witnesses have urged that opium and morphia should be regarded as narcotics and not as anæsthetics and that by their use for animals undergoing experiments pain is not prevented. These drugs are used in man to relieve pain, but they are not employed, at any rate alone, in cases of operation. Their action appears to be somewhat different on the nervous system of man from what it is in the case of some of the lower animals, and in the frog morphia is said to excite convulsions. It is stated that the dose required Buxton 12569. Dixon 19004. to produce complete anæsthesia in man would be dangerously large and that some relation exists between the dose required to produce anæsthesia and the amount of the brain substance of the animal.

Thane, 1631. Dr. Thane has no doubt that morphia can be administered so as to cause complete anæsthesia in a dog, but it would probably require a fatal dose to do so.

Hobday, 16488. Mr. Hobday considered that morphia affected different dogs differently; he did 16499. not use it for major surgical operations, as he considers chloroform a better anæsthetic; 16483. he has given large doses to dogs—as much as 20 grains—without a fatal result, and Lind-af-Hageby, 9262. considers them to be very insusceptible to a toxic dose of morphia. Morphia is also given by experimenters to animals as an adjunct to chloroform and ether. Dr. Starling explained Starling, 3607. that it facilitates the induction of anæsthesia and prolongs it :—

"So that with our rough methods of anæsthetisation it is much safer." "If an animal has had a previous dose of morphia it will then go on with a small dose of A.C.E. mixture, instead of having to have a large dose constantly administered to it. That is the chief value of morphia."

As an adjunct to chloroform or ether $\frac{1}{6}$ to $\frac{1}{4}$ of a grain of morphia is given, and when given as an anæsthetic from $1\frac{1}{2}$ to 15 grains are given. It is also sometimes combined with curare.

No doubt there is greater certainty of being able to establish and maintain continuously complete insensibility by some drugs rather than by others, and the nature of the anæsthetic employed, the adequacy and mode of its administration, the use of more than one drug in association, and above all the employment of curare under any circumstances are questions requiring resort to the best skilled advice on the part of those granting licences to practice vivisection and vigilant supervision by those whose duty it is to inspect the laboratories in order to ensure that the full benefit of anæsthetics is secured to the subjects of such investigations.

83. By Section 4 of the Act it is enacted that curare shall not be deemed to be an Russell, 643–5.
anæsthetic. This drug appears to vary to some extent in its composition and effects, Thane, 1320,
but it is generally held to paralyse the motor nerves, thus preventing the expression of 1321–3, 1599–1605,
sensations. As to its anæsthetic properties, if any, there is great conflict of opinion. It 1610–7, 1705.
is, however, used in conjunction with anæsthetics, and such conjunction needs great watch- Starling, 3618,
fulness lest return to sensibility should take place while the influence of the curare would 4115.
prevent the exhibition of some of the signs of such return. Cushny, 5003–11. Buxton,

Several experimenters expressed themselves as opposed to its compulsory disuse; 12559–65.
Sir Victor Horsley saw no objection to an inspector being present in all cases in which Gotch, 13874–7,
curare is administered, provided that the convenience of the investigator is safeguarded. 13607–8. Langley, 15179. Horsley, 16058–9.

84. After careful consideration of the whole question of anæsthetics as applied to ex- Dixon, 18826–7,
perimental investigations on living animals, we are led to the conclusion that by the 18910–1,
use of one or other or of a combination of several well-known anæsthetics complete 18905–7.
insensibility to pain can be secured.

Inoculations.

85. We think that we may at this point usefully draw attention to the fact that the term "vivisection" is often applied to all experiments upon living animals. Such use of the term is liable to be misleading as appears from the Returns published for the year 1910, which show that of the total number of experiments, 95,731, to which living animals were subjected, about 95 per cent. were "of the nature of simple inoculations, hypodermic injections, and similar proceedings performed without anæsthetics." The remaining 5 per cent. include the experiments involving cutting operations, and in more than half of these cases the animal was "required to be kept under the influence of an anæsthetic from before the beginning of the experiment until it was killed."

86. Inoculations of animals with morbid products may in some cases be followed by negative results and in others occasion but little pain or even discomfort. On the other hand there are cases in which, according to Dr. Thane, "the injection is followed Thane, 457.
by great pain and suffering"; and he instanced the infection of rats and guinea pigs with tetanus or with plague and also the injection of certain drugs. Sir James Russell also cited cases and called attention to the fact that under Certificate A the experimenter is not required by the Statute to kill his animal at the end of the experiment as he is in the case of experiments under Certificate B as soon as the object thereof has been attained. It has also been stated that inoculation of a dog with the blood of a mule suffering from trypanosomiasis caused rapid emaciation, dropsy and inflammation of the eyes. Our attention was also directed to some experiments performed in 1889 by Dr. Klein for the Local Government Board, an account of which appeared in the Medical Officer's Supplement to the Annual Report of the Board for that year. Dr. Klein inoculated the eyes of cats with diphtheritic membrane. Ulceration and swelling of the corneæ resulted in the course of several days in the case of some of the cats, and it was contended that such experiments must have been very painful. Besides these there are other experiments 467–476.
performed under Certificates B and B+EE (cats and dogs) or B+F (horse, ass or mule) in which the animal is allowed to recover from an initial operation under an anæsthetic and is submitted to a second operation, or a fistula is established or some organ removed, and the subsequent effect of these operations observed. In such cases the primary operation is performed under aseptic precautions and it is alleged that no considerable amount of suffering results therefrom. Dr. Thane hesitated to give a general statement 471–4.
as to the degree of pain consequent on such procedures; the animal may be very ill, suffer from severe shock and a fatal result may sooner or later ensue, yet he thinks there may be no acute pain. It is the general practice, we understand, for the Home Office to attach to Certificate A a condition in the following terms:—

"That if an animal, after, and by reason of, any of the said experiments under the said Certificate is found to be in pain which is either considerable in amount or is likely to endure, and if the main result of the experiment has been attained, the animal shall be immediately killed under anæsthesia."

From this it will be seen that the animal need not be killed though in considerable pain—considerable and likely to endure—*if* the main result has not been attained. Thus official sanction is given in terms to keeping an animal alive for an indefinite time though suffering considerable pain, at the sole discretion of the operator. The question here raised is dealt with in our recommendations (Paragraph 121).

7. As to animals which have been subjected to inoculation, the Commission realise that their case gives rise to peculiar consideration and requires the most careful examination. The facts relating to inoculations and the law and practice at present existing in regard to them are set out in a Return to the House of Commons (H.O. Return for 1910), showing the number of Experiments on Living Animals during 1910, from which we desire to give the following extracts :—

"The experiments included in Table IV. (B), 90,792 in number, are all performed without anæsthetics. They are mostly inoculations, but a few are feeding experiments, or the administration of various substances by the mouth or by inhalation, or the abstraction of blood by puncture or simple venesection. In no instance has a certificate dispensing with the use of anæsthetics been allowed for an experiment involving a serious operation. Inoculations into deep parts, involving a preliminary incision in order to expose the part into which the inoculation is to be made, are required to be performed under anæsthetics, and are therefore placed in Table IV. (A).

"It will be seen that the operative procedures in experiments performed under Certificate A, without anæsthetics, are only such as are attended by no considerable, if appreciable, pain. The certificate is, in fact, not required to cover these proceedings, but to allow of the subsequent course of the experiment. The experiment lasts during the whole time from the administration of the drug, or injection, until the animal recovers from the effects, if any, or dies, or is killed, a period possibly extending over several days, or even weeks. The substance administered may give rise to poisoning, or set up a condition of disease, either of which may lead to a fatal termination. To administer to an animal such a poison as diphtheria toxin for example, or to induce such a disease as tuberculosis, although it may not be accompanied by acute suffering, is held to be a proceeding 'calculated to give pain,' and therefore experiments of the kind referred to come within the scope of the Act 39 & 40 Vict., c. 77. The Act provides that, unless a special certificate be obtained, the animal must be kept under an anæsthetic during the whole of the experiment; and it is to allow the animal to be kept without an anæsthetic during the time required for the development of the results of the administration that Certificate A is given and allowed in these cases.

"It must not be assumed that the animal is in pain during the whole of this time. In cases of prolonged action of an injected substance even when ending fatally, the animal is generally apparently well, and takes its food as usual, until a short time before death. The state of illness may last only a very few hours, and in some cases it is not observed at all.

"In a very large number of the experiments included in Table IV. (B), the results are negative and the animals suffer no inconvenience whatever from the inoculation. These experiments are therefore entirely painless.

"In the event of pain ensuing as the result of an inoculation, a condition attached to the licence requires that the animal shall be killed under anæsthetics as soon as the main result of the experiment has been attained.

"During the year 1910, 49,662 experiments were performed by twenty-seven licensees working at eight institutions, in the course of Cancer investigations. Of these 816 are in Table IV. (A), and 48,846 in Table IV. (B). The latter are almost entirely inoculations into mice."

"A large number of experiments, almost wholly simple inoculations and similar proceedings contained in Table IV. (B), were performed either on behalf of official bodies, with a view to the preservation of the public health, or directly for the diagnosis and treatment of disease. Several County Councils and Municipal Corporations have their own laboratories in which bacteriological investigations are carried on, including the necessary tests on living animals; and many others have arrangements by which similar observations are made on their behalf in the laboratories of Universities, Colleges, and other Institutions. A sewage farm is registered as a place in which experiments on living animals may be performed in order that the character of the effluent may be tested by its effects on the health of fish. The Board of Agriculture and Fisheries has a laboratory which is registered for the performance of experiments having for their object the detection and study of the diseases of man and animals. In other places experiments have been performed on behalf of the Home Office, the Naval Medical Service, the War Office, the Army Medical Advisory Board, the Army Veterinary Service, the General Post Office, the Local Government Board, the Metropolitan Asylums Board, the Royal Commission on Tuberculosis, the Advisory Committee for Plague in India, the Tropical Diseases and Glass-blowers Cataract Committees of the Royal Society. Eighty licensees return nearly 19,000 experiments which were performed for Government Departments, County Councils, Municipal Corporations, or other Public Health Authorities; and seventeen licensees performed over 8,000 experiments for the preparation and testing of antitoxic sera and vaccines, and for the testing and standardising of drugs."

88. From the foregoing conclusions it is clear that although in the large majority of experiments performed under Certificate A (which dispenses with the use of anæsthetics) the animals do not appear to suffer pain, it is also clear that, even if the initial procedure in cases under that certificate may be regarded as trivial, the subsequent results of this procedure must in some cases, at any rate, be productive of great pain and much suffering.

We deal in our recommendations with the additional safeguards which, in our opinion, should be made applicable to inoculated animals as well as to those which have been subjected to operative procedure.

Miscellaneous Questions.

89. It will be convenient here to deal with certain miscellaneous points that have been brought to our notice by the Secretary of State as regards the interpretation of the existing law and its administration. These points concern

(1). Pithing.

(2). Experiments on fish.

(3). Experiments for Demonstration.

90. (1) *Pithing.*—We were informed that a difficult situation had presented itself in administering the Act in regard to so-called "pithed" animals, mostly frogs. It appears that for many years experiments have been allowed on such animals under licence only and without a certificate dispensing with anæsthetics, and indeed have been held not to come within the operation of the Act at all. In the first case it has been claimed that the animal, although still a "living animal," had been rendered permanently and entirely devoid of sensation, that the operation of pithing could, in fact, be regarded as the equivalent of an "anæsthetic of sufficient power to prevent the animal feeling pain." In the second case it has been claimed that the animal after being pithed was no longer "a living animal" and that, therefore, Section 2 of the Act did not apply in such cases. Although a very large number of frogs are used in this way, a similar procedure is sometimes adopted in the case of higher animals. Thus the Inspector reported that he had seen experiments performed on a cat which had actually been decapitated and not merely decerebrated and that from the point of view of pain it was impossible to distinguish between them.

It seems, moreover, that the word "pithing" is now applied to certain operations which are much less complete than the entire destruction of the brain and spinal cord. It appears to be used in at least three different senses :—

(1) The destruction of the brain and the spinal cord: Lind-af-Hageby, 7415.

(2) The destruction of the whole of the brain above the spinal cord: Gotch, 13702.

(3) The simple severance of the brain from the spinal cord:

all of which can be effected through an incision made at the back of the neck. Lind-af-Hageby, 7424.

There is yet another condition of partial destruction of the nervous system which requires consideration along with those to which the term pithing has been applied, viz., the destruction of the cerebral hemispheres (containing what are known as the higher centres) alone, leaving the rest of the nervous system intact. Pembrey, 14128–9.

There can be no doubt, from repeated observation of cases in which, as a result of accidents to human beings, the spinal cord has been completely severed, that painful sensation is entirely abolished from that portion of the body, supplied by nerves arising from the cord, below the site of section. Notwithstanding the anæsthesia, however, movements of the lower extremities take place as the result of irritation applied to the toes or legs. Such movements are unconscious or involuntary and are spoken of as "reflex movements." Reasoning by analogy from man to the higher animals there is good ground for the belief that in their case complete destruction of the brain would cause complete anæsthesia. We of course assume that in such a case the operation of the destruction of the brain has been skilfully and completely carried out, and that such operation has itself been performed under the influence of a satisfactory anæsthetic administered by a competent person.

In dealing with this difficult subject we are well aware of the distinction to be drawn between the consciousness of pain and the facilities whereby such consciousness may be expressed, and we realise how the inability to appreciate such distinction complicates our reasoning in such problems and warns us against the unlimited argument from analogy in drawing conclusions in regard to such questions. Thus it may be urged that even if the evidence suggests that the brainless frog displays signs of purpose and consciousness superior to those which higher vertebrates whose brain had been destroyed would exhibit, yet the susceptibility to painful influences which a fish and a frog, even with the nervous system intact, can manifest appears to be of so low an order as to render it idle to apply to them such rules and precautions against possible sources of suffering as humanity prescribes in the case of the higher animals. We realise the difficulty of attempting to dogmatise in such questions, but in the matter specially referred to us, after careful consideration, we think that no lesser operation than a complete destruction of the brain* or decapitation should be accepted as equivalent to the production of complete anæsthesia. We also think it desirable that the operation of pithing a warm-blooded animal with a view to an experiment should be conducted only by a licensed person, and that the operation itself should be performed under an adequate anæsthetic.

* By destruction of the brain is meant not only destruction of the cerebral hemispheres but also of the basal ganglia.

91. (2) *Experiments on Fish.*—Our attention was called by Dr. Glaister to certain experimental investigations, conducted under the directions of the Royal Commission on Sewage Disposal, in which fish were employed to test the degree of impurity of certain sewage effluents into rivers, and especially with a view to ascertain whether such effluents were likely to be detrimental to fish life. It was held by the Home Office that such investigations came within the provisions of the Cruelty to Animals Act of 1876, and the premises upon which these investigations were conducted were accordingly registered. From the account given in the Sixth Report of the Royal Commission on Sewage Disposal it appears that no cutting experiment was performed upon the fish, but that salmon fry and parr were submitted to the influence of certain effluents from distilleries, both before and after treatment. In some cases the fish showed "obvious distress" after a few minutes' immersion in the effluent and a certain number died in from a few hours to a few days after submission to the influence of unpurified liquor. It was held by the experimenters that their investigation proved that by a certain process of purification effluents from distilleries into rivers, otherwise harmful to fish life, were rendered innocuous thereto. The Commissioners were also satisfied as to the value of the investigations.

Glaister, 12957, 12963.

Cd. 4511 of 1909, pp. 15, 53–56.

We do not think it could be contended that such experiments should be forbidden, and it may well be doubted whether such investigations were present to the minds of the authors of the Act of 1876. That Act by Section 22 was expressly made inapplicable to invertebrate animals, and it is by no means easy to draw a line in such matters between the higher *invertebrata* and the lower *vertebrata* such as fish.

Whether all cold-blooded animals should be excluded from the purview of such special legislation is a question raised by several witnesses who have appeared before us. Fish have not hitherto been largely employed for experimental research; on the other hand, certain *amphibia*, such as frogs, have been very largely used. *Reptilia* again, with the exception of the tortoise and turtle, have not been much utilised in the laboratory. While it would be impossible with any strict logic to define with precision the class or classes of the animal kingdom for which special legislation, in excess of the common law or of general enactments against cruelty to animals, can be justified, we think there is ground for regarding with a different degree of repugnance or acceptance the employment of certain classes of animals for purposes of vivisectional experiments, and to this point we recur elsewhere (Paragraph 97).

92. (3) *Experiments for Demonstration.*—By the terms of the Act of 1876 under no circumstances can any experiments on a living animal be performed before a class in illustration of a lecture except when the animal is during the whole time under an anæsthetic of sufficient power to prevent pain and is killed before it recovers; in other words the Certificate C, under which permission is granted to a licensee to perform experiments in illustration of lectures, is, by the words of the Statute subject to the provisions in regard to the use of anæsthetics.

We understand that applications have been made to the Home Office from time to time with a view to relaxation of such requirements, but we do not think that sufficient evidence has been put forward to make it clear that such requirements occasion any serious impediments to the general progress of science, and we are therefore not prepared to recommend any alteration in the Act in regard to experiments at lectures.

THE MORAL QUESTION.

93. The fourth matter that we have to consider is :—

(*d*) Whether, and how far, the objection taken by some that such experiments are morally wrong and unjustifiable can be sustained.

We propose now to deal with this question, having at the same time some doubt whether we were invited by the order of reference to collect evidence or offer an opinion upon such a matter.

94. Among those who testified to their belief in the immorality of experiments on animals a considerable difference of opinion prevailed. Mrs. Cook, an authoress and journalist, who gave evidence as one of the representatives of the Committee of
the Parliamentary Association for the Abolition of Vivisection, of which she was Cook, 1785.
Chairman, stated that her Committee considered vivisection to be morally unjustifiable, 1984.
whether painful or painless, and that nothing less than the total prohibition of all 1938.
experiments on animals would satisfy them. Nor could she apparently conceive of any 1980.
benefit to mankind great enough to justify by an experiment the sacrifice, painful or 1990–3.
painless, of a single animal.

Miss Arabella Kenealy, L.R.C.P., Ireland (who practised for eight years in and Kenealy,
near London and has since been engaged in literature), expressed much the same 5376–86.
views. She objected to "exploiting" animals for the benefit of man, and considered it an immoral attitude "to look upon sentient, living creatures, which are linked to us by evolution in some way we do not understand, as mere material for experiment."
If the life of a child, or of many children, could be saved by sacrificing one animal, 6276–9.
whether with or without pain, in Miss Kenealy's opinion it would be distinctly
wrong to sacrifice such animal. She added that it would be wrong to try prophylactics 6351–2.
for tetanus in cattle, because "the earth has been sown, if it is sown, with tetanus germs in order to test the healthy or diseased state of these cattle."

Mr. James Graham, M.A., also a witness presented to us by the Parliamentary
Association for the Abolition of Vivisection, and Principal of Dalton Hall, Victoria Graham, 5944–6.
University, Manchester, was opposed to all painful experiments, and did not believe in the efficacy of anæsthetics as applied to animals. He saw no moral objection to experiments on animals, if assured that they were completely anæsthetised and suffered no pain, nor would he object to an animal being allowed to come out of anæsthesia after an operation if such a course did not involve serious pain.
The late Rev. J. Page Hopps, a Unitarian Minister, and Sir G. Kekewich, both Hopps, 8398.
expressed the opinion that all experiments on living animals should be prohibited by Kekewich, 20592
law. Miss Lind-af-Hageby, a Swedish lady who came to England in 1902 to study Lind-af-Hageby,
physiology for the purpose of assisting her action on the question of vivisection, 7192.
expressed the same opinion. 9242.

The Rev. L. S. Lewis, deputed by the Church Anti-Vivisection Society to place Lewis, 8629.
their views before the Commission, stated that he objected to all operations which caused pain, and that while :—

> "I might painlessly sacrifice the lives of many animals to save one valuable human one, I would 8636.
> not have one mouse painfully vivisected to save the greatest of human beings, nor the life dearest to me."

If it was possible to get painless vivisection he would not disapprove of it. 8652.

Mr. G. H. Burford, M.D., a homœopathic doctor, thought that all experiments with Burford, 8995.
drugs for man's benefit should be made on man. This was the course pursued among
homœopaths and better and more certain results were so obtained, and he added that 8954.
volunteers are found to offer themselves. For the investigation and prevention of 9011.
diseases of animals experiments on animals were, in his opinion, justifiable.

Mr. Stephen Coleridge stated his view as follows :— Coleridge, 10709.

> "My objection to vivisection begins and is centred in the question of pain. If an animal be placed under complete anæsthesia and destroyed before it recovers consciousness, personally, I have no objection to that vivisection at all, and anything that might be discovered thereby would be to the benefit of humanity and welcomed by myself."

Mr. John Hughes, Secretary of the National Canine Defence League, which Hughes,
was originally founded as the Anti-Muzzling League, stated that his Society objected to 17129–33.
every kind of experiment on dogs whether with or without anæsthetics. He believed 17434.
that it is impossible to anæsthetise a dog, but did not offer any evidence in support of this belief.

Dr. Charles R. J. A. Swan, M.B., practising as a physician in London, did not Swan, 19216.
object to experiments on animals if dogs were excepted. He would allow operations 19254.
on anæsthetised dogs in certain cases if satisfied that no other animal would serve 19258.
the purpose equally well. 19298.

Cowen, 19384. Mr. R. J. M. Cowen, L.R.C.P. and L.R.C.S., Ireland, practising in London, did not object to vivisection generally, but believed it useless, and therefore unnecessary, in the case of dogs.

Scott, 19488. Mr. A. G. Scott and Sir F. Banbury, Bt., M.P., representing the Society for the Prevention of Cruelty to Animals, stated that they considered that all painful experiments should be performed under anæsthetics continued till death.

Levy, 18477. Mr. J. H. Levy, Honorary Secretary of the Personal Rights Association, combated at length the views expressed on a previous day by Lord Justice Fletcher-Moulton in support of the practice of vivisection, but did not see any moral objection to operating under complete and continued anæsthesia; nor would he object to recovery after the
18484. anæsthesia if such a course did not involve serious pain.

95. On the other side, the Council of the Royal Society by a Resolution presented by their President, Lord Rayleigh, declared, "speaking as a Guardian of the interests of science in this country" that:—

Rayleigh, 5532. "In no branch of investigation have the theoretical and practical successes of experimental work been more conspicuous in recent years than in physiology and its practical application in medicine and surgery. . . . While precautions should undoubtedly be taken against improper use of experiment on living animals, it is not the province of the Society to suggest what safeguards should be adopted. It is, however, the bounden duty of the President and Council to urge that these safeguards should be so framed as not unnecessarily to interfere with that advancement of knowledge to promote which the Society exists. Such restrictions would not only cripple or arrest the growth in this country of an important branch of biological science, but in so doing would reduce the efficacy of both physician and surgeon to cure disease."

Fletcher-Moulton, 12780–804. Lord Justice Fletcher-Moulton, P.C., F.R.S., enlarged on the value and success of the experimental method, and also gave in detail his reasons for believing that ethics not only justify but demand the infliction of death and occasionally of pain on some animals in the interests both of mankind and of animals.

Horsley, 15585–8. Sir Victor Horsley, F.R.S., F.R.C.S., speaking as the representative of the British Medical Association—a body of medical practitioners which includes about half of the British Medical Profession—stated their opinion as follows:—

"The medical profession consider that every effort ought to be made to increase knowledge for the prevention of suffering and disease, affecting not only man, of course, but also the lower animals, and they consider that everyone who opposes such pursuit of knowledge is acting immorally. To them the moral duty is the pursuit of knowledge, and an immoral act is the obstruction of that pursuit of knowledge."

Similar opinions will be found in the evidence of many of the scientific witnesses who appeared before us, and we think it may be safely assumed that they are endorsed by the great majority of the medical profession.

96. We have already stated that after full and deliberate consideration we think it has been shown that useful knowledge has been acquired directly or indirectly from experiments on living animals. We have now to enquire whether such means of obtaining knowledge can be regarded as morally or ethically defensible, and if so, what regulation, if any, should be prescribed by law in regard to such experiments.

The quest for knowledge may, of course, conflict with moral principle. Thus, torture, once largely employed as an instrument for obtaining judicial evidence, is a case in point. The practice, though justified by Bacon, was discarded in England in the seventeenth century, and in Scotland a little later, but was continued on the Continent to the close of the eighteenth century. Here the infliction of human suffering in view of contemporary morals was not only regarded as unobjectionable in the pursuit of knowledge, but was purposely utilised to extract information. The growth of the average moral sense of mankind has condemned and abolished the practice, not because it did not lead to useful knowledge, but because, however useful the result, the means could no longer be justified. It has been stated that in ancient Egypt human vivisection was practised by Herophilus and Erasistratus, and some witnesses have expressed apprehension lest experiments on living animals might lead to a resort to experiments on human beings. On the other hand it is contended that if medicine and surgery are not to remain stationary new operative procedure, new methods of treatment, new drugs and remedies must be tried, and that if animals are not available for trying them they must be tried on human beings. At the present time the average moral sense of Christian communities is not offended by the sacrifice of lower animals for the food, clothing, adornment, and, within limits, the sport of man. The right to sacrifice animal life for such purposes—to exploit animals for the service of man as it is sometimes put—once conceded, would appear to carry with it the right to conduct experiments on living animals, provided that life is terminated without the return of consciousness and that during the whole of the experiment such animals are in a state of anæsthesia. Such experiments may indeed be distasteful to some persons, especially when performed on certain classes of animals, but the right of an owner to terminate the life of an animal at any moment would seem to carry with it the right, during an absolutely anæsthetic condition immediately preceding death, to perform experiment for scientific purposes. To prohibit such action by law in the present state of society and of public opinion would appear inconsistent if not preposterous.

A more difficult question arises in regard to cases of inoculation or injection of disease products, practically painless in their primary performance, but in some cases leading to later results accompanied by more or less suffering, and to cases where the animal is allowed after operation to recover consciousness for the purpose of completing the experiment. Many instances have been cited to us in which it was alleged that useful discoveries had been arrived at by such methods. It would be vain to attempt to proportion the degree of permissible suffering in consideration of the amount of prospective gain or to propound principles of vicarious sacrifice. We are here in the difficult region of relative ethics in which standards of what is or is not justifiable vary with different climes, epochs and temperaments and in the same persons at different periods of their lives. We strongly hold that limits should be placed to animal suffering in the search for physiological or pathological knowledge, though some have contended that such considerations should be wholly subordinated to the claims of scientific research, or the pursuit of some material good for man.

Conclusions.

97. After full consideration we are led to the conclusion that experiments upon animals, adequately safeguarded by law, faithfully administered, are morally justifiable and should not be prohibited by legislation.

As regards the different classes of animals used for experiments and the possibility of making discrimination between them for such purpose, we are again confronted with a delicate question of relative ethics. Here again there can be little doubt that the general moral sense of civilised mankind would be prepared to make such differentiation and would regard with quite a different degree of reprobation the like treatment for such purpose of one of the domesticated animals on the one hand with that of cold-blooded or indeed verminous or destructive animals on the other hand. The *differentia* in such case would probably be found to consist in the degree of association with or of affinity or utility to man.

We feel that recognition should be accorded to the reality and worthiness of such underlying sentiment which would secure a special reservation for animals coming within the aforesaid limits. Thus we think that the higher apes (anthropoid) and the dog and cat present claims for special consideration and with these claims we deal subsequently in our Report.

SUGGESTIONS MADE TO THE COMMISSION.

98. We have now dealt with the history and administration of the law relating to experiments on animals and have considered in detail the four questions which we orginally set before us. These were :

(*a*) Recent history of the progress of medical science in connection with such experiments.

(*b*) Whether experiments on animals give valuable results in relation to the prevention and cure of disease and generally in physiological knowledge.

(*c*) How far immunity from pain in experiments is or can be secured.

(*d*) Whether, and how far, the objection taken by some that such experiments are morally wrong and unjustifiable can be sustained.

On these points we have arrived at certain conclusions which we have already indicated. It remains for us to examine the various practical suggestions which were put before us directed to amending the law which at present governs the practice of subjecting animals to experiments or its administration.

Brunton, 6830. Horsley, 16092. Pembrey, 14090, 14115. Gotch, 13870. Langley, 15253, 15297. Coleridge, 11314–31. Cook, 1938–46. Kekewich, 20384–7. Kenealy, 5411–6. Lind-af-Hageby, 9275–8. Graham, 5944–6.

99. *The Act and its Administration.*—With regard to the working of the present Act, a considerable conflict of opinion occurred. It was regarded as impeding, to a certain extent, scientific investigation, by Sir Lauder Brunton, Sir V. Horsley, Dr. Pembrey and Sir W. Osler. On the other hand it was stated by a number of scientific witnesses including Dr. Gotch, Professor Langley, Sir Henry Morris, Sir Douglas Powell, Dr. Dixon and Dr. Cushny that in their opinion the Act on the whole had worked well and that notwithstanding the inconvenience attending the issue of certificates they regarded it as no restriction to scientific research. Among opponents of vivisection, including Mr. Coleridge, Mrs. Cook and Sir G. Kekewich, there were some who advocated the statutory prohibition of vivisection altogether, while others desired a repeal of the Act of 1876 as affording protection to the vivisectors, whom they wished to be dealt with under the ordinary law as to cruelty to animals. Others again suggested amendments of the Act with a view to obtaining greater protection for animals by additional inspection and otherwise.

100. *Suggested Amendments in the Act or its Administration.*—The recommendations for amendments in the law or its present methods of administration were many and varied and may conveniently be divided into the following heads :

Lind-af-Hageby, 7462. Coleridge, 10755, 10765, 11208, 11250, 11572.

Inspection.—An increase in the number of Inspectors and in the number of visits which they should pay to the various laboratories was strongly urged by Mr. Scott, of the Royal Society for the Prevention of Cruelty to Animals, and by Mr. Coleridge, the latter desiring that an Inspector should be invariably present at every operation. Miss Lind-af-Hageby, however, considered the increase of Inspectors as useless, and the same view was held by Sir George Kekewich. In the opinion of Sir Victor Horsley and Dr. Dixon inspection is superfluous but desirable in the public interest, and an increase in the number of Inspectors was held to be unobjectionable by Dr. Gotch and Professor Langley.

Gotch, 13873. Langley, 15348. Horsley, 16045. Dixon, 19169, 19170.

Brunton, 6845, 6919, 134. Horsley, 16046.

Such increase, however, was held to be undesirable and useless by Sir Lauder Brunton and unnecessary by Sir James Russell. Sir William Byrne, of the Home Office, suggested that there should be more constant observation by Inspectors of inoculated animals, and Sir V. Horsley was of opinion that if further inspection is necessary it could be secured by providing that the present Inspectors should give their whole time to the work of inspection.

Lind af-Hageby, 9610. Dixon, 19085. Fletcher-Moulton, 12749.

It was further urged that certain specified persons other than Inspectors should be allowed to be present during experiments on animals (Miss Lind-af-Hageby). Dr. Dixon was of opinion that there was no objection to such admission of accredited witnesses, but this suggestion was strongly opposed by Lord Justice Fletcher-Moulton.

Langley, 15367. Horsley, 16058.

101. *Use of Curare.*—The suggestion that an Inspector should be present in all cases in which curare was administered was objected to by Professor Langley, but held to be unobjectionable by Sir V. Horsley.

Stoker, 980. Powell, 5612, Morris, 7751, 8011, Starling, 4050–2. Horsley, 15638, Osler, 16587.

102. *Experiments for Demonstration.*—It was stated by Sir Thornley Stoker that experiments on animals under anæsthetics for the instruction of students were in his opinion not necessary, were demoralising, and were an offence against humanity. On the other hand there was a strong consensus of scientific opinion that such demonstration

was necessary (Sir Douglas Powell, Sir Henry Morris, Professor Starling, Sir W. Osler, Sir Victor Horsley, Lord Justice Fletcher-Moulton), and it was urged by Dr. Dixon and by Dr. (now Sir J. R.) Bradford that at any rate for advanced students such demonstration should be permitted.

Fletcher-Moulton, 12806.
Dixon, 19087, 19027, 18742, 18918.
Bradford, 17667, 17701.

103. *Manual Dexterity.*—It was further urged by some witnesses that students should be permitted to experiment under supervision upon animals which had received a lethal dose of some anæsthetic (Dr. Gotch, Professor Langley), and it was the opinion of Sir W. Osler, Sir Douglas Powell, Sir Victor Horsley and Sir H. Swanzy, that such experiments by students upon animals should be permitted in order that they might thereby acquire skill in subsequent operations upon human creatures (Sir W. Osler, Sir V. Horsley, Sir D. Powell), but on this latter point reference must be made to the opinion of Sir Henry Morris that the use of experiments on animals for the purpose of obtaining manual skill was not necessary. In this connection we would also call attention to the quoted opinions of and the letter written by Sir F. Treves which were given in evidence before us. A full description of the method adopted in the Johns Hopkins Medical School in America as to the instruction of students both in curative operations on diseased animals, and in the acquisition of technique in experiments on anæsthetised animals, was given by Sir W. Osler.

Gotch, 13813, 13855.
Langley, 15301.
Swanzy, 9778.
Osler, 16716.
Horsley, 15632. 15794, 15936, 16115.
Powell, 5765.
Morris, 7828.
Horsley, 16131 *and footnote.*
Osler, 16582–3, 16752, etc.

104. *Sera and Vaccines.*—The question as to the production of sera and vaccines for commercial purposes, although possibly not within the scope of our Reference, was incidentally alluded to. Opinions were expressed that, for the public benefit, sera and vaccines should be manufactured in laboratories under public control. The Act, however, concerns itself not with the manufacture but only with the testing and standardising of sera and vaccines, which processes, of course, are at present performed by licensees under the Act.

Thane, 1371–5.
Martin, 11847–9.

105. *Experiments on Dogs and certain other Animals.*—Except in the case of operations under anæsthesia without recovery, it is necessary, for experiments on dogs, to obtain a certificate which states among other things that the object of any such experiment will be necessarily frustrated unless it is performed on an animal similar in constitution and habits to a dog or a cat, and that no other animal is as suitable for any such experiment. We need scarcely say that neither of these provisions nor even the total exemption of dogs from experimentation would satisfy those who desire the total abolition of all experiments on living animals (*e.g.* Mr. Graham), but such exemption was urged as being strongly desirable by the three witnesses put forward by the Canine Defence League. It was, however, the opinion of many of the scientific witnesses called that the total prohibition of all experiments on dogs would seriously hinder the progress of science (Professor Starling, Sir Lauder Brunton, Sir Douglas Powell, Sir Henry Morris, Sir V. Horsley, and others).

Graham, 6157.
17109–17620, 19192–19427.
Starling, 3479, 4035.
Brunton, 7066.
Powell, 5610.
Horsley, 15808.

Considerable evidence was given as to the possibility of using for experimental purposes some of the larger domestic animals other than dogs, and pigs, calves, goats and sheep were suggested. It was, however, urged that a serious objection to the use of these animals lay in the fact that they could not be kept in the laboratories to which licences are attached, and in which the experiments would take place. The use of other animals was also advocated by Dr. Swan and Mr. Cowen, but in the opinion of Sir J. R. Bradford the use of a pig, sheep or goat in substitution for a dog would, at any rate, as regards all the experiments for which a dog is now generally employed, be quite impracticable; and the general view of most of the scientific witnesses was that in construction and organization the dog was the animal best adapted, and in many cases the only one available, for such experiments.

Osler, 16732–7.
Swan, 19233–4, 19337–9.
Cowen, 19372–4.
Russell, 609.
Bradford, 17705.

106. *Pithing.*—This question has been already dealt with in Paragraph 90.

107. *Cold-blooded Animals.*—The omission of cold-blooded animals from the application of the Act, on the ground of their comparative insensibility to pain, was urged by Professor Schäfer, Dr. Gotch, and Sir V. Horsley, but Professor Langley was of opinion that they ought not to be excluded from the Act, but that they should be the subject of a special certificate.

Schäfer, 10187.
Gotch, 13823, 13868.
Horsley, 15952, 16267.
Langley, 15399.

108. *Licences and Certificates.*—Several scientific witnesses advocated an alteration in the present method of granting certificates (Professor Starling, Dr. Cushny, etc.) Dr. Dixon suggested the substitution of a comprehensive licence and the abolition of certificates. Sir V. Horsley held that certificates were not necessary at all. He dealt at

Starling, 3723, 3981.
Cushny, 4945.
Dixon, 19025, 19096, 19116.

Horsley, 15767. length with the manner in which they are at present used and summed up his criticism by stating that he regarded them as giving unnecessary work in the Home Office and unnecessary trouble to investigators who have to obtain them, while Lord Justice Fletcher-Moulton
16090. suggested that general certificates should be granted to certain men of acknowledged capacity in research, entitling them to perform even painful experiments subject only to one condition, namely, that they should fully report every experiment. With regard to all
Fletcher-Moulton, 12741. younger men, or to those who had not attained the commanding position of those to whom he would give the general certificate, he held that the lines of the present Act are the right lines on which to grant them licences and certificates. A further point was raised by him with regard to the granting of certificates, and he urged that they should not be limited to holders of a medical diploma, but granted also to those who,
12755. without holding such a diploma, are devoting their lives to research work, but he added
12769 that with regard to any such non-medical applicants for certificates, he should require the most exceptional testimony as to their qualifications.

109. *An Advisory Body.*—At present, as previously noted in Paragraph 12, the Home Secretary before granting a licence to an applicant, and also in the case of certificates, seeks the advice of the Association for the Advancement of Medicine by Research. Objec-
Coleridge, 11098. 11109. tion was taken to this practice by Mr. Coleridge, and he suggested that a Committee of the National Anti-vivisection Society, of which he is Honorary Secretary, would be a very
Horsley, 16100. proper body for the Home Secretary to consult.

Sir V. Horsley was of opinion that it was well that the Home Secretary should be advised as at present by the above-mentioned Association, and this was approved by Dr. Dixon, who suggested that very great dissatisfaction would be caused among
Dixon, 19153. 19155. physiologists and those engaged in experimental research if the Home Secretary were to decide the question of granting a licence for special work without first referring it to some body of experts for their opinion.

Kekewich, 20409. 110. *Prosecutions under the Act.*—It was represented to us by Sir George Kekewich that prosecutions were hindered or rendered impossible by the provisions of the Public Authorities Protection Act, 1893. This suggestion is, we think, founded upon a misapprehension. That Act has, it is clear, no application to a prosecution instituted under the existing Act of 1876.

111. *Records of Experiments.*—As regards the information supplied to the Secretary of State by licensees with reference to the experiments which they may have performed under licence or certificate, we understand that the present practice is for the licensee to keep a written record (Vol. V., page 41, Form 9) of his experiments, which is to be open to examination at any time by the Inspector, and also to furnish to the Inspector a report (Vol. V., page 42, Form 10) of all experiments performed, showing their number and nature, whenever required or at the end of the year. But when these experiments are recorded in a book at the laboratory with at least as much information as is required by the Home Office Form of Record that form need not be used.

Under Section 9 of the Act "the Secretary of State may direct any person performing experiments under this Act from time to time to make such reports to him of the result of such experiments, in such form and with such details as he may require." We were informed by Sir William Byrne that "no such directions are in force, although in a few instances a licensee has been asked as to the progress of his investigations when an application for sanction to further experiments has been under consideration." Some years ago a proposal by Sir Kenelm Digby was considered by the Home Office with a view to calling for a report of results of experiments from licensees, but the proposal was never carried out. (*See* Paragraph 16.)

We think that, whenever it might appear to the Secretary of State when granting a licence or allowing a certificate that the experiments proposed were of such a nature that an immediate or special record or report of results might be of value, he should attach to the licence or certificate a condition calling for such a record or report.

RECOMMENDATIONS.

112. *The Act and its Administration.*—In respect of the various matters referred to us we have come to the conclusion that the present system, although open to adverse criticism which has been indicated, has nevertheless been so worked as to secure a large degree of protection to animals subject to experiment and at the same time so as not to hamper or impede research. Such a system, to which all the parties affected have by long experience become habituated, should not in our opinion, notwithstanding its imperfections, be lightly thrown away. We believe it to be capable of improvement and we have made several recommendations calculated, as we believe, more effectually to secure the objects aimed at by the Act.

It will be for the Secretary of State and his legal advisers to decide whether these recommendations can be carried out by departmental requirements which it is in his power to add under the existing law. Should he be advised that he has not adequate powers for the purpose they should, in our opinion, be provided by fresh legislation.

113. We therefore humbly submit to Your Majesty the following recommendations :—

Inspection.—A relation must necessarily exist between the number of Inspectors and the number of places registered, and we are of opinion that in order to secure adequate inspection the existing number of Inspectors should be increased. We further think that with a view to concentrate the work and facilitate inspection it is desirable that, as far as possible, the issue of licences should be limited to those places which are in connection with Universities or other Public Authorities or Institutions.

The Inspectors should be sufficiently numerous and should have at their command ample time to afford to the public reasonable assurance that the law is faithfully administered. This might be effected by appointing a sufficient number, either of whole-time or part-time officers. We are inclined to think, having regard to the present number of licensed premises and experiments, that there would be an adequate increase of inspection if, in place of the existing arrangements, the Chief Inspector were a whole-time officer, and if in addition to him there were three whole-time Inspectors for Great Britain.

Assuming that it is not thought practicable or desirable to appoint whole-time officers, but that the services of Inspectors in the active practice of their professions should be retained, we think that arrangements should be made to secure a sufficient number of such Inspectors who could give such time to their duties as would be equivalent to the services of the four whole-time men. It is essential that the Inspectors should be qualified medical men of such position as to secure the confidence both of their own profession and of the public.

As to Ireland, we think that, having regard to the comparatively small number of licensed places and of experiments carried out under the Act in that country, sufficient inspection can be obtained by the services of one or more part time Inspectors.

114. *Use of Curare.*—Some of us are of opinion that the use of curare should be altogether prohibited, but we are all agreed that if its use is to be permitted at all, an Inspector or some person nominated by the Secretary of State should be present from the commencement of the experiment, who should satisfy himself that the animal is throughout the whole experiment and until its death in a state of complete anæsthesia.

115. *Experiments for Demonstration.*—With regard to experiments performed by way of demonstration in medical schools, hospitals, or colleges, we think that the provisions of the present Act are sufficient, subject to the suggestions made in Paragraph 118 of our Report.

116. *Manual Dexterity.*—As already pointed out in Paragraph 103, the evidence as to the necessity or desirability of experiments on living animals for the purpose of attaining manual skill which might hereafter be useful in operations on human creatures was conflicting. We are therefore not prepared to recommend any alteration of the existing law in this respect.

117. *Sera and Vaccines.*—We have considered the question as to whether the production of sera and vaccines for commercial purposes ought to be brought within the provisions of the Act of 1876, and we have come to the conclusion that it ought not. In the interests of the public there may well be grounds for holding that the production of sera ought to be under regulation and inspection, but we do not think that it should be dealt with in an Act which deals with experimental research. Operations for gain, such as the production

of sera and the castration or spaying of animals to improve their market value stand on a different footing from operations for the advancement of knowledge, and require to be dealt with under wholly different conditions. We may note that Section 1 (c) of the Protection of Animals Act, 1911 (1 and 2 Geo. V., c. 27), makes it an offence, punishable with six months imprisonment, " to subject or cause to procure, or being the owner permit to be subjected any animal to any operation which is performed without due care and humanity."

118. *Experiments on Dogs and certain other Animals.*—As to the use of dogs and other specified animals, we have already adduced reasons which lead us to justify a differentiation in the use of certain animals from that of others for the purposes of scientific experiments. Such discrimination, though admittedly difficult, we attributed on ethical grounds to the degree of association with or affinity or utility to man, and in this connection we referred especially to the case of dogs and the higher apes.

We realise that the exclusion by law of any particular class of animals may be objected to as logically untenable, and in view of the continuity of the animal series that it is by no means easy to draw a sharp line of demarcation between an exempted and an unexempted class.

But precedents are not wanting, even in law, for making some such attempts. Thus the general law precludes the employment of dogs for purposes of traction in this country, whereas in several continental countries they are habitually employed for such purposes. Even in the Act of 1876 an attempt is made in Section 5, entitled " Special restrictions on painful experiments on dogs and cats, etc.," to effect some such discrimination. This clause (which led to the granting of Certificates E and F) deals with dogs and cats somewhat differently from the way in which it deals with horses, asses or mules, which are the other animals selected in the Act for special treatment. Thus dogs and cats may, if anæsthetised, be the subjects of experiments under licence only, but may not be used for experiments without anæsthesia unless a certificate is given by one or other of the scientific authorities named in the Act, stating for reasons specified that the experiment will be necessarily frustrated unless performed on a dog or cat. On the other hand no experiment even with anæsthetics may be made on a horse, ass or mule unless a licensee is armed with a certificate in similar terms from one or other of the authorities named.

Thus at the present time the law makes a distinction between some animals and others for the purposes of the Act, and discriminates between dogs and cats on the one hand, and horses, asses and mules on the other. No specific mention is made of any other animal (except that invertebrata are excluded from the purview of the Act), and no animal is altogether exempted from scientific experiment. For purposes of demonstration any animal (including cats and dogs) may, under Section 3 (Certificate C) be employed under anæsthetics, but in the case of the horse, ass or mule a further Certificate F would presumably be necessary for the employment of such animal for demonstration purposes. The representations made to us for the complete exemption of any class of animal from all experiments under the Act have been strongest in the case of dogs. On the other hand, many of the scientific witnesses have represented strongly the case for the employment of dogs for certain experiments and demonstrations.

Osler, 16587.

A very prevalent view was expressed by Sir William Osler when, dealing with the use of dogs for the purpose of acquiring manual dexterity, he said :—

> " I think we have all felt that it would be very much better if we could get animals other than the dog to operate on."

and he quoted with approval the opinion of Dr. Cushing to the effect that :—

> " There is naturally a feeling of regret in the minds of many—of none greater than our own—that animals, particularly dogs, should thus be subjected to operations, even though the object be a most desirable one and accomplished without the infliction of pain, and did expense permit we would gladly have used animals with which there is an association of less acute sentiment on the part of all."

Russell, 619. Thane, 425–9. Starling, 4039–40. Cushny, 4824. Bradford, 17703–8.

Other scientific experimenters sharing this sentiment have urged the need for the employment of dogs, not on the ground of less expense, but because of the greater facility for keeping them in or near laboratories and again on account of the greater suitability of their structure and their tissues for certain classes of experiments.

Gotch, 13697–700. Starling, 3470. 3527–33

There is, however, some variety of practice in different laboratories and schools as to the use of dogs both for experiment and for demonstration. This may be due to the different lines of research carried on or the different problems investigated in the respective laboratories. Thus Dr. Gotch stated that no dog had been used for any vivisection experiment in Oxford during the last five years. At University College, London, on the other hand, in 1902, Professor Starling stated that 155 dogs were employed, though only 4 of these were kept alive after the anæsthesia had passed off. At the Medical School of Guy's Hospital Dr. Pembrey does not experiment on dogs at all nor does he use them for illustration of lectures. He said :— Starling, 4033–4. Pembrey, 14173. 14177. 14175.

"I have not used a dog at Guy's for illustration of lectures at all. I use rabbits, rats, and guinea-pigs."

In view of the variety of practice and the divergence of opinion as to the necessity of employing dogs for experimentation and demonstration, we find some difficulty in deciding upon this important question. Some of us regard the provisions of the existing law as sufficient, some of us would prefer that in the case both of experimentation and demonstration the further special protection given to horses, asses and mules should be extended to dogs, while some of us would exclude the use of dogs altogether. But if any alteration is made in the existing procedure, the majority of us would agree that the special enactments now applicable to horses, asses and mules might be extended to dogs, and also to cats and anthropoid apes.

119. *Pithing.*—As stated in Paragraph 90 we are of opinion that no lesser operation than a complete destruction of the brain or decapitation should be accepted as equivalent to the production of complete anæsthesia. We also think it desirable that the operation of pithing a warm-blooded animal with a view to an experiment should be conducted only by a licensed person, and that the operation itself should be performed under an adequate anæsthetic.

120. *Cold-Blooded Animals.*—We have carefully considered the suggestion that all cold-blooded animals should be exempted from the operation of the Act, but having regard to the limited knowledge which at present obtains as to the capacity of suffering in such animals, we do not make such recommendations.

121. *Licences and Certificates.*—With regard to licences and certificates we have given very careful consideration to the various suggestions made as to alteration in their form or in the method of granting them, and have come to the conclusion that inasmuch as the present system, according to the judgment of the majority of those witnesses who possessed the knowledge and the opportunity for forming a trustworthy opinion on such a point, has on the whole worked efficiently, no change is necessary or desirable, but we desire to add that in our view it is of the highest importance that the responsibility of the Secretary of State should be in all respects maintained, and we are therefore strongly of opinion that no certificate should be available until the applicant has received notice that it has not been disallowed by the Secretary of State. We understand that this is the practice at the present time, and we recommend that a condition to this effect should be annexed to every licence as well as endorsed on every certificate.

As to granting licences and certificates to foreigners temporarily resident in England, we are informed that the present practice of the Secretary of State has been to direct that such licences and certificates shall only be granted to such persons on condition that they should perform experiments only under the supervision of the head of some particular laboratory, who is responsible for the due observance of the provisions of the Act. In our opinion this restriction is wise, and we recommend that the above condition should be attached to all licences and certificates granted to foreigners.

We have noted that it is the practice of the Home Office to supplement the provisions of the Act by attaching various conditions to licences or certificates. Where the animal is allowed to recover from the anæsthetic and there is a wound to which it is practicable to apply antiseptics, the following condition is inserted in the licence :—

"That the animals experimented on under Certificates .... be treated with strict antiseptic precautions, and if these fail and pain results, that the animals be immediately killed under anæsthetics." Vol. V. of Evidence, App.I., p. 44, condition 9.

Where the use of an anæsthetic is dispensed with unless it is obvious that pain could not ensue from the procedure contemplated, and in some cases where the animal is allowed to recover from the anæsthetic but the antiseptic condition is inapplicable, the following condition is inserted :—

"That if an animal, after and by reason of any of the said experiments under the said Certificates . . . . is found to be in pain, which is either considerable in amount or is likely to endure, and if the main result of the experiment has been attained, the animal shall be immediately killed under anæsthetics." Vol. V. of Evidence, App. I., p. 44, condition 6.

We think it is well that the fact that such conditions are attached should be generally known, but we are of opinion that additional safeguards against pain might be provided without interfering with legitimate research, and we therefore recommend :—

(1.) That an Inspector should have power to order the painless destruction of any animal which, having been the subject of any experiment, shows signs of obvious suffering or considerable pain, even though the object of the experiment may not have been attained ; and

(2.) That in all cases in which in the opinion of the experimenter the animal is suffering severe pain which is likely to endure it shall be his duty to cause its painless death, even though the object of the experiment has not been attained.

The above conditions should be attached to certificates.

We regret that we cannot recommend any further extension of the " pain condition." We are anxious, as far as possible, to prevent or to limit animal suffering in every case. We have recommended that there should be increased inspection, that wide powers should be given to inspectors to order the painless destruction of any animal under experiment. and that in future, although the object of the experiment has not been attained, no animal should be allowed to live in severe pain which is likely to endure. But we do not feel justified in recommending that, when the object of the experiment has not been attained, an experimenter should in all cases be required to destroy the animal immediately it exhibits signs even of severe pain, which might in some cases be only momentary.

We are satisfied by the evidence that in the great majority of the experiments under the Act the animals do not exhibit any symptoms suggestive of severe pain, and to require the immediate destruction of an animal as soon as it exhibits such symptoms might, in our opinion, put an insuperable obstacle in the way of investigating many widespread diseases (afflicting both men and domesticated animals) with respect to which further knowledge as to their nature and treatment is in the interest of humanity urgently required.

It must not be forgotten that it is in the case of diseases which are naturally painful when they attack men or animals that experiments are most likely to involve pain to animals which are experimentally infected ; as examples we may instance cancer, cholera, plague, tetanus, rabies and snake bite.

We are compelled to accept the weighty evidence given before us to the effect that the study of animals experimentally infected with some of these diseases has given us knowledge which has been instrumental in saving much mortality and suffering both in man and animals, and we believe that discoveries already made in this way justify the hope that by the same methods knowledge may yet be extended regarding the means of preventing or curing other most painful diseases which are at present scarcely or not at all amenable to treatment. And finally we feel that as long as public opinion sanctions the infliction on animals of pain, which is not only severe but of long duration, in the pursuit of sport, and in carrying out such operations as castration and spaying, or in the destruction of rabbits and of rats and other vermin by traps and painful poisons, it would be inconsistent and unreasonable to go further than we have already gone in limiting experiments which are designed to result and, according to experience, will probably result in preventing or alleviating great human or animal suffering.

122. *An Advisory Body.*—As to an advisory body we endorse the views of the Commission presided over by Lord Cardwell in 1875, that :—

Report of Royal Commission, 1875 pp. xx–xxi.

"In the administration of the system generally, the responsible Minister would, of course, be guided by the opinion of advisers of competent knowledge, and experience," "but we think it is inexpedient to divide the responsibility of the Secretary of State with that of any other persons by statutory enactment, and we recommend that his advisers should be from time to time selected and nominated by himself. Their names should be made known to the profession and to the public."

We think that the practice followed by various Home Secretaries for nearly thirty years of obtaining professional advice as a guide in the exercise of their powers and discharge of their responsibility is a reasonable and proper practice, but in our opinion the recommendations of the Commission of 1875 should be strictly followed.

These advisers should, as regards Great Britain, be selected by the Secretary of State from a list of names submitted to him by the Royal Society and the Royal Colleges of Physicians and Surgeons in London. No person so selected should be the holder of a licence, and the names of all persons so selected, as well as the names of the Scientific Authorities under the Act, should be published. The adoption of this suggestion would involve a discontinuance of the present practice of reference to the Association for the Advancement of Medicine by Research.

With regard to Ireland, where heretofore there has been no Advisory Body, we think that the Chief Secretary should be advised by a body chosen upon analogous lines.

123. *Records of Experiments.*—We have already dealt fully in Paragraph 111 with this subject. We are of opinion that in certain cases immediate or special records or reports of results should be furnished to the Home Office by licensees.

SUMMARY OF RECOMMENDATIONS.

124. It now will be convenient to recapitulate briefly the various recommendations which we have already made.

They are as follows :—

(1.) An increase in the Inspectorate.

(2.) Further limitations as regards the use of curare.

(3.) Stricter provisions as to the definition and practice of pithing.

(4.) Additional restrictions regulating the painless destruction of animals which show signs of suffering after experiment.

(5) A change in the method of selecting and in the constitution of the Advisory Body to the Secretary of State.

(6) Special records by experimenters in certain cases.

125. The Commission has during its sittings sustained a very heavy loss in the death of its Chairman, the late Viscount Selby. He presided over our deliberations with conspicuous wisdom and ability, and we desire to record our deep regret at our untimely deprivation of his most valuable assistance.

By the death of the late Right Hon. James Tomkinson, M.P. we have also suffered the loss of another zealous colleague, whose services were of great assistance in our enquiry.

126. We wish to take the opportunity here of expressing our thanks to our Secretary, Captain the Hon. Clive Bigham, C.M.G., and to record our high appreciation of his diligent and efficient performance of his duties throughout the sittings of the Commission.

ALL WHICH WE HUMBLY SUBMIT FOR YOUR MAJESTY'S GRACIOUS CONSIDERATION.

A. J. RAM, *Chairman.*

A. R. M. LOCKWOOD (*subject to the reservations contained in the following memorandum*).

W. S. CHURCH.

WILLIAM J. COLLINS (*subject to the reservations contained in the following memorandum*).

J. McFADYEAN.

M. D. CHALMERS.

W. H. GASKELL.

GEORGE WILSON (*subject to the reservations contained in the following memoranda*).

CLIVE BIGHAM, *Secretary.*

1st March, 1912.

RESERVATION MEMORANDUM BY COL. THE RT. HON. A. R. M. LOCKWOOD, C.V.O., M.P., SIR WILLIAM J. COLLINS AND DR. G. WILSON.

1. It was not until almost the last sitting of the Commission that it became manifest that there were certain matters upon which we were unfortunately not in agreement with our colleagues. A cleavage of opinion was then disclosed in regard to some points upon which there had been previously reason to believe that, even if unanimity were unattainable, a majority of the Commission might have been found to be in agreement.

2. We have signed the foregoing Report, in the preparation of which we have borne our part—even though we could have wished that some portions of it had been differently expressed—and we have endeavoured to limit as far as possible the reservations which we are now compelled to make.

3. There are, however, certain conclusions and recommendations of a fundamental nature upon which we find it impossible to concur with our colleagues, and in reluctantly withdrawing our support from them we must leave these particular conclusions and recommendations to rest only on the authority of a bare quorum of the Commission. We believe that, for the reasons which we shall proceed to adduce, the conclusions and recommendations which we support are not only warranted by the weight of evidence but are more in harmony with the whole trend and tenour of the Report, which we have signed, than are those which have received the approval of our colleagues.

4. The two main points upon which we dissent from the Report are firstly, as to securing by Statute that undivided responsibility of the Secretary of State which was recommended by the previous Royal Commission, but which the Act of 1876 failed to establish, and secondly as to placing a statutory requirement upon an experimenter painlessly to destroy an animal which has been experimented upon when obvious suffering has supervened. Both points lead up to and in our opinion necessitate amendments of the law.

AMENDMENTS OF THE LAW.

5. By our Reference we were directed to report not only upon the practice of vivisection but also " whether any and if so what changes are desirable " in the law relating to experiments on living animals and its administration. While our Report deals in ample and particular detail with the earlier part of our Reference under the various headings set out in Paragraph 5 of the Report, and also in great fulness with the administration of the law, we think that the evidence we have received calls for a wider review and more critical examination of the provisions of the Act of 1876 as well as for more extensive modifications thereof than are involved in the suggestions and recommendations offered by our colleagues. The opportunity afforded by the appointment and prolonged labours of this Commission for reconsidering, in the light of more than thirty years experience, the principles of legislation in regard to vivisection ought not, in our opinion, to be lightly thrown away.

Report, Para. 5.

Report, Para. 112. 6. We think that the weight of evidence is opposed to the view, that only administrative modifications are required in order to give effect to the changes which experience proves to be desirable, and indeed we doubt whether even such modifications as are suggested in the Report can be adequately carried out without legislation. The Report justly states there was " a considerable conflict of opinion " as to the working of the present Act, but with the exception of two or three witnesses, we cannot recall any of those who appeared before us who expressed unqualified satisfaction with the provisions of the present Act and the procedure thereby entailed. Criticism of the Statute proceeded alike from those who are in favour of and those who are opposed to experiments on living animals for scientific purposes. In Paragraph 74 due weight is given to the change of medical and scientific authority, as exhibited in evidence, in regard to the practice of vivisection ; not less remarkable, however, is the change which has taken place (so far as we can judge from the evidence received)

Report, Para. 99.

in the attitude of such authority towards the desirability and practicability of legislation
for the control and regulation of the practice. Thus, before the Royal Commission of 1875 R. C. of 1875;
the late Mr. (afterwards Lord) Lister considered such legislation to be superfluous and felt Lister, 4345.
that it would be a blot on the profession. The late Dr. (afterwards Sir Michael) Foster saw R. C. of 1875;
no necessity for legislation. The late Dr. (afterwards Sir J.) Burdon Sanderson held a Foster, 2321, 2393–5.
similar view; he deprecated inspection as being liable to lead to a spirit of opposition, or R. C. of 1875;
as tending to concealment; and further he believed that no inspection would give any Burdon Sanderson, 2687.
guarantee whatever as to what happened when the Inspector was not present. Other 2352.
medical and scientific witnesses before the previous Commission similarly objected to legis-
lation as being unnecessary or impracticable.

7. Such fundamental objections to legislation for the restriction and control of vivisection
were, however, not largely held by the majority of those favourable to the practice who
appeared before us. It is true that Dr. (now Sir William) Osler, who had had experience
in the United States of America, where there is no legislative restriction, as well as in this Dixon, 19132–9.
country, felt "that the matter could be left safely in the hands of the men who are in charge Osler, 16606.
of the physiological laboratories and the scientific men of this country," and regarded
legislative restriction as "a standing insult to the humanity of these men." Dr. Pembrey Pembrey, 14061.
held that "the limitations of the Act are against research," and Sir Lauder Brunton Brunton, 6830.
regarded the present law to a certain extent as a stop to science. Criticism of the present
law of a less fundamental character, however, was vouchsafed by numerous witnesses.
Dr. Starling, while not advocating abrogation of the Act, yet complained of the complicated Starling, 4077,
list of different certificates and licences required under the present law. Professor Langley 3723.
criticised the complicated character of the statutory certificates and suggested amendments Langley, 15192–230.
in the Act. Dr. Dixon, while thinking some legislation and a system of licensing desirable Dixon, 19020,
as giving security to those who practise vivisection and to the public, was nevertheless 19116–39,
desirous of abolishing all certificates, thus simplifying the present complicated system, 19159, 19171.
and can hardly be cited as in favour of the Act as it is. Dr. Cushny's satisfaction, too, Cushny, 4945–6,
was tempered by a desire for some alteration of the statutory requirements in regard to 5142–4.
certificates.

8. While Sir Henry Morris, it is true, would be satisfied with small amendments of the Morris, 7805–9.
Act, Sir Victor Horsley, though agreeing with Lord Lister that the Act is superfluous,
nevertheless, in deference to the public demand, approved of a system of licensing by the Horsley, 16090,
Home Secretary and of the registration of places where experiments are carried on. He, 16093, 16097.
however, objects altogether to the present system of statutory certificates, which he regards
as so faulty that it should be abolished. He suggested a simpler form of application for Horsley, 15762,
licence, and made numerous proposals for amending the Act, the drafting of which he con- 15606.
sidered to betray ignorance of physiological conditions. Dr. (now Sir John) Bradford held Bradford, 17783–
that the statutory certificates gave rise to grave misconception in the minds of the public 96.
as well as inconvenience to experimenters and was in favour of simplification. The In- Thane, 1777–9.
spectors under the Act, Dr. Thane, Sir J. Russell, and Sir Thornley Stoker themselves
proposed specific amendments, and Sir J. Russell stated that licensees have great difficulty Stoker, 779–80.
in understanding the law especially in regard to certificates. Russell, 555–61.

9. In view of the "adverse criticism" to which the Report justly admits the present Report,
system to be open, and to the strictures passed upon it by those familiar with its working, Para. 112.
we are unable to agree with our colleagues "that inasmuch as the present system, Report,
according to the judgment of the majority of those witnesses who possessed the knowledge Para. 121.
and the opportunity for forming a trustworthy opinion on such a point, has on the whole
worked efficiently, no change is necessary or desirable."

10. The Report further shows that among opponents of vivisection there was not less Graham, 5944–6.
diversity of opinion in regard to the present law, some advocating statutory prohibition of
vivisection altogether—others a repeal of the existing Act. Others again suggested amend- Kekewich,
ments of the Act of 1876, with a view to obtain greater protection for animals by additional 20384–7.
inspection and otherwise, but none approved of the scope and provisions of the Act of 1876. Kenealy, 5411–6. Cook, 1938–46.

11. The Report sets out the reasons for thinking that valuable knowledge has resulted and Lind-af-Hageby,
may result from experimental investigations on living animals. We have after weighing 9275–8.
the ethical and moral considerations stated our view that such experiments, adequately
safeguarded by law faithfully administered, are justifiable and should not be prohibited by
legislation. We have considered the suggestion of leaving to the operation of the ordinary
law, as now amended and consolidated by the Protection of Animals Act, 1911, cases of
alleged cruelty in connection with medical, physiological and scientific investigations—that
is to say the repeal of the Act of 1876. After much deliberation we think that in the
interests of the animals themselves, even apart from other considerations, some form of
special legislation is on the whole desirable.

UNDIVIDED RESPONSIBILITY OF THE SECRETARY OF STATE.

12. The Royal Commission of 1875 recommended that only persons licensed by the Secretary of State should be permitted to experiment upon living animals, and that such licensees should be bound by conditions whose object should be to secure that suffering should never be inflicted in any case in which it could be avoided, and be reduced to a minimum where it could not be avoided altogether. All such experiments whether for original research or for demonstration were to be under the control of the Secretary of State, and they added "we think it is inexpedient to divide the responsibility of the Secretary of State with that of any other persons by statutory enactment." Agreeing as we do with these recommendations of the previous Commission, we regret that the Act of 1876 failed to give effect to them. That Act did not receive very prolonged parliamentary consideration. The Bill was read a second time in the House of Commons on August 9th and passed through its subsequent stages including the Royal Assent by August 15th. It introduced a dual jurisdiction in regard to the permits to be granted to experimenters upon living animals. After enacting certain provisions with regard to the use of anæsthetics, demonstrations to students, killing the animals before the return of consciousness, repetitions of experiments, and the use of certain domesticated animals, the Act proceeds to provide for the waiving of all these restrictions on the strength of statutory certificates granted, *not* by the Secretary of State, but by the holder or holders of certain specified offices in scientific or medical institutions mentioned in the Act. As Sir William Byrne, of the Home Office, put it "that is the framework of the Act—to lay down a number of very strict conditions and then to allow all except three to be removed by certificate."

Report of Royal Commission, 1875. [C. 1397], p. xx.

Kekewich, 20397.

Report, Para. 9.

Byrne, 264.

13. There was thus introduced into the Statute the intervention of certain outside authorities in derogation of the undivided responsibility of the Secretary of State. We think that the distinction between *Licences—granted by the Secretary of State* and subject to all the restrictions in the Act, chiefly for the protection of animals, and *Certificates—granted by the scientific authorities* which waive the aforesaid restrictions—needs to be accentuated. It is true that the Secretary of State may disallow or suspend any certificate at any time, but unless he intervenes within one week after the certificate signed by the scientific authority or authorities has been forwarded to him, it becomes available. Disallowance of a certificate by the Secretary of State is, we were informed, of the very rarest occurrence. In recent years the practice has been to notify certificated persons that the certificate is not to be deemed available until the Secretary of State has intimated that it has not been disallowed. This power had, according to Professor Schäfer, who gave evidence before us as well as before the Commission of 1875, been illegally assumed by successive Home Secretaries. He contended that the authors of the Act intended that the grantors of the certificates and not the Secretary of State should be responsible to the public.

Report, Para. 11.

Byrne, 167.

Schäfer, 10172, 10245.

14. This derogation from the undivided responsibility of the Secretary of State in the matter of certificates was emphasized by Sir William Byrne, who appeared before us on behalf of the Home Office. He laid stress on "a very important difference" between licences and certificates, the former being *recommended* by the scientific authorities and the latter being *granted* by them. Thus in the case of a certificate dispensing with the use of anæsthetics he said "it is not the Home Office that excludes the use of anæsthetics, it is the learned authority that signs the certificate that excludes them"; and again he said "the Secretary of State has not to decide whether an operation is painful or not, ... his attitude is that he himself has no power to decide what is pain and what is not"; and again "the learned authorities have exclusive authority to grant the certificates."

Byrne, 25.

8.

129.

130.

241.

15. We think that difficulty and confusion would have been obviated if the legislature had followed strictly the recommendation of the Royal Commission of 1875 in this regard. We think the present method of certification not only unnecessarily complicated but an undesirable departure from the principle of undivided responsibility of the Secretary of State which we concur with the previous Royal Commission in desiring to see established.

Report, Para. 18. 16. In Paragraph 18 of our Report attention is drawn to the difference between the recommendations of the Royal Commission of 1875 and the provisions of the Act of 1876 in regard to the undivided responsibility of the Secretary of State in reference to several charges made against the Home Office, [*see* Paragraph 18, Nos. (1) (6) and (12)], and the opinion expressed that it would have been well if the complete recommendations of the Royal Commission had been carried out. In Paragraph 121 our colleagues insist that in their view "it is of the highest importance that the responsibility of the Secretary of State should be in all respects maintained."

17. The question is closely related to that of the appointment of competent persons to advise the Secretary of State in regard to the nature of the experiments to be performed by licensees under the Act and as to the relaxation by certificates of the requirements of the Act for the protection of animals from suffering.

18. In Paragraph 122 our Report again repeats and endorses the findings of the Royal Commission of 1875 to the effect that :— Report, Para. 122.

"In the administration of the system generally, the responsible Minister would, of course, be guided by the opinion of advisers of competent knowledge and experience. . . . but we think it is inexpedient to divide the responsibility of the Secretary of State with that of any other persons by statutory enactment, and we recommend that his advisers should be from time to time selected and nominated by himself. Their names should be made known to the profession and to the public."

19. We of course agree with the previous Commission in recognising the need for the Secretary of State to call to his aid professional advisers of competent knowledge and experience. Again we endorse the recommendations of the Commissioners of 1875 that these "advisers should be from time to time selected and nominated by (the Secretary of State) himself," and that "their names should be made known to the profession and to the public." The Commission is unanimous in stating that "in our opinion the recommendations of the Commission of 1875 should be strictly followed." We are at one in desiring to maintain the responsibility of the Secretary of State, but inasmuch as the present Act divides that responsibility by statutory enactment in regard to certification, we dissent altogether from our colleagues when they state they have "come to the conclusion that . . . . no change is necessary or desirable." We are indeed unable to reconcile this finding with the rest of the Report. Report, Para. 121.

20. The undivided responsibility of the Secretary of State then being established by law we think that the advisers might be selected and nominated by him from a list submitted to him either in the way suggested in the Report (Paragraph 122) or from the holders of the offices named in Section 11 of the Act of 1876. In any case, the selection should be personal and not merely *ex officio*, and the names duly published. We agree with our colleagues in deprecating certain suggested relaxations of the law (Paragraphs 115, 116, 120) and concur with the suggestions made with a view to afford further protection to animals experimented upon and to provide more efficient inspection (Paragraph 113). We nevertheless think that much could be done to simplify the present cumbrous system of certification and at the same time to secure that undivided responsibility of the Secretary of State which the Royal Commission of 1875 recommended, which this Commission endorses but which the Act of 1876 failed to establish.

21. The cumbrous system of certification of which complaint was made is indeed the result of attempting to divide the responsibility of the Secretary of State with other persons indicated in the Statute. Such amendment in the law as we advocate has the two-fold advantage of simplifying procedure and at the same time concentrating responsibility.

22. The Commission is further unanimous in recommending a discontinuance of the present practice of consulting the Association for the Advancement of Medicine by Research; in our opinion that practice instituted needless additional derogation from that undivided responsibility of the Secretary of State which we desire to secure. If the recommendations of the Commission of 1875, as endorsed by us, were strictly followed, the advisers to be appointed would supersede not only the Association for the Advancement of Medicine by Research (which the Secretary of State has never been under any obligation to consult), but also the scientific authorities named in Section 11 of the Act, so far as their assistance—other than as certificating authorities—would be required under the simpler system of licensing which we advise. All licences and certificates (or such conditions attached to licences as would take their place) would then be granted only by the Secretary of State himself, after the applications had been fully reported upon by the competent advisers whose names would be known to the public.

PAINLESS DESTRUCTION OF THE ANIMAL WHEN SUFFERING ENSUES.

23. The undivided responsibility of the Secretary of State being secured, and due provision made for expert advice on the one hand and adequate inspection on the other, the object of such special legislation as we are led to support should be to require that all experimental investigations on living animals should be either painless or unaccompanied by any real or obvious suffering. We think that if in the course of scientific investigation the pursuit

of knowledge leads to the infliction of real or obvious suffering it is right that the pursuit should cease rather than that the infliction of suffering should continue. Within these limitations the evidence shows that a large and legitimate field for research upon living animals is included, and we do not recall any really fruitful results obtained by any experimenter who appeared before us which would have been seriously prejudiced by such limitations. The weight of the evidence in this direction is very remarkable.

24. Dr. Starling when asked, "are there any operations performed under circumstances in which the animal is necessarily and intentionally sensitive to some pain," replied "No, never"; and he also stated "though I have been engaged in the experimental pursuit of physiology for the last seventeen years, on no occasion have I ever seen pain inflicted in any experiment on a dog or cat, or I might add a rabbit, in a physiological laboratory in this country, and my testimony would be borne out by that of anyone engaged in experimental work in this country." As regards physiological experiments he said if the animal were in any pain at all it ought to be killed forthwith.

Starling, 4063. 3451. 4017–21.

25. Dr. Cushny stated that he was quite satisfied that "no animal suffered pain" under his experiments.

Cushny, 4932. 5194–5.

26. Mr. Stockman stated that the preparation of antitoxins was unaccompanied by any material pain. The experiments he performed for the Board of Agriculture rarely involved the use of the knife or necessitated the employment of anæsthetics.

Stockman, 2903–7. 2620. 2579–87.

7. C olonel (now Sir David) Bruce said, "I do not think that any experiments, in the performance of which I have been associated, have been what one would call painful experiments."

Bruce, 14367.

28. Sir W. H. Power spoke of experiments conducted for the Local Government Board in connection with plague, cholera and typhoid, and assured us that experiments carried out by the investigators directly employed by the Board were painless experiments. The avoidance of painful experiments was desired by the Board.

Power, 4353-4.

29. Sir Lauder Brunton stated that there is very little pain inflicted in experiments on living animals, unless by inadvertence; he believed that experiments in laboratories are conducted absolutely painlessly. He further informed us that it was now possible to test antidotes for snake poison upon animals in which anæsthesia could be kept up for thirty hours.

Brunton, 7122. 6846. 6833. 6865.

30. Sir Henry Morris, speaking of experiments in relation to cancer, pointed out that they were practically painless, "neither is pain suffered by the animal in which the transplanted tissue grows even into a large tumour." If, however, in exceptional cases, ulceration ensues the animal is at once killed.

Morris, 7726.

31. Dr. Martin, who had had wide experience both in Melbourne and in this country and had worked at infectious diseases, especially plague and the production of antitoxins, informed us that the latter was unaccompanied by pain. He stated that he had never seen any cruelty exhibited, that under the Act anæsthesia was always complete, and that there was not much suffering.

Martin, 12066–71, 12343–5.

32. Dr. Langley stated that if an animal, which had been experimented upon, afterwards showed signs of severe pain, he would at once put it out of pain; he had, however, never seen such a case and thinks that such a case is rare.

Langley, 15341–2.

33. The evidence appears to us to be conclusive that such experimental investigation upon living animals as is now generally deemed essential or necessary, can be performed either painlessly under anæsthetics or under an imperative requirement that should obvious suffering result the animal shall be forthwith painlessly killed. The present Act does not require this, as is clear from evidence given by Dr. Thane:—

Thane, 1209. *Q.* (*Mr. Ram.*) Are there any cases in which operations are necessarily performed on cats or dogs when the state of the animal is a painful state after the removal of the anæsthesia ?—*A.* Not seriously painful. There are cases when the animal is ill, but we have no cases where the animal is in great pain afterwards. I can say that the cases in which the health of the animal is seriously disturbed are few.

1210. *Q.* And in the event of the health of the animal being disturbed or pain being great, would the death of the animal be caused—the animal would be killed ?—*A.* In case the operation goes wrong and the wound does not heal aseptically, then the animal has to be killed at once, but otherwise the investigator is not called upon to kill the animal until the experiment is completed.

Q. I was rather following up the case of an animal that is inoculated with some disease, or suffers the loss of a portion of its body, the subsequent condition of the animal thereby becoming painful. If it becomes acutely painful, would it be killed, or would it be allowed to continue in a state of pain in order to elucidate more and more the object sought ?—*A.* There is no statutory requirement to kill the animal then, but one would not allow an experiment in which one would expect that the animal would be kept in a state of acute pain after the operation. 1211.

Q. I am not sure that I have made it quite plain. I am rather on a case in which the condition of the animal might not be acutely painful at first, but in consequence of the operation might become so. Is there any duty on the operator to kill the animal before it becomes in a state of acute pain ?—*A.* I say that there is no statutory requirement. 1212.

Q. There is nothing but his humanity that would cause him to do it ?—*A.* Nothing but his humanity. Only you are assuming a case which does not, I think, occur—that the condition of the animal becomes acutely painful. 1213.

Q. I am supposing the case of an animal inoculated with disease—and as the disease runs its course in the animal, it is conceivable, is it not, that the condition of the animal might become painful ?—*A.* I was talking about the other set, after the operation. You are talking of AE. You are quite right; there is no statutory requirement to kill the animal until the experiment is completed. 1214.

Q. If you found an animal on one of your inspections in that state of suffering, should you order it to be killed ?—*A.* I should certainly require it to be killed. 1215.

Dr. Thane here contemplated the exercise of a power which the Statute does not give.

34. We think that the inspectors should be armed with such power by law and that a similar statutory duty should rest upon every licensee.

35. It is chiefly from the evidence given by the inspectors that we have heard of experiments in which pain and suffering have ensued (*See* Paragraph 86 of Report). Prior to 1905 the Home Office Returns purported to distinguish between "painful" and "painless" experiments, but this distinction is now abandoned as being unsuccessful or even misleading. The difficulty of making the distinction is especially encountered in the case of inoculation experiments, the number of which has greatly increased in recent years. Thane, 457–72, 543. Stoker, 761, 840, 914. Thane, 1723–30. Byrne, 161–4. Report, Para. 16.

36. A "condition" has, it is true, been attached to certain certificates in the following terms :—

"That if an animal, after, and by reason of, any of the said experiments under the said Certificates......, is found to be in pain which is either considerable in amount or is likely to endure, *and if the main result of the experiment has been attained*, the animal shall be immediately killed under anæsthetics."

37. As pointed out in our Report, by the insertion of the words which we have italicised "it will be seen that the animal need not be killed though in considerable pain—considerable and likely to endure—*if* the main result has not been attained. Thus official sanction is given in terms to keeping an animal alive for an indefinite time though suffering considerable pain, at the sole discretion of the operator." Report, Para. 86.

38. The Secretary of State particularly invited the Commission to express an opinion in regard to the "Pain Condition" and especially with reference to the propriety of retaining or eliminating the words "if the main result of the experiment has been attained."

39. We agree with our colleagues that such a "condition" in such terms is indefensible, and we are clearly of opinion that the proper remedy to apply is to insert in the Act an imperative requirement upon every licensee forthwith painlessly to destroy any animal in which signs of obvious suffering have resulted. Report, Para. 121.

40. We are unable to appreciate the position taken up by our colleagues on this question. They urge the necessity for "additional safeguards against pain" and they recommend that :—

"An Inspector should have power to order the painless destruction of any animal, which, having been the subject of any experiment, shows signs of obvious suffering or considerable pain, even though the object of the experiment may not have been attained."

41. They accordingly recognise the principle that in the case of any experiment the requirement of immediate destruction upon the appearance of signs of obvious suffering may be enforced. We recommend that this be a statutory requirement upon the experimenter in every such case.

42. Our colleagues, however, introduce a different "condition" in such cases for the experimenter to that which they would place upon the inspector, viz. :—

"That in all cases in which in the opinion of the experimenter the animal is suffering severe pain which is likely to endure it shall be his duty to cause its painless death, even though the object of the experiment has not been attained."

43. Here it will be observed the discretion of the operator and the likelihood or the unlikelihood of the severe pain enduring (for some undefined period of time) are introduced. In our judgment confusion and complexity would arise by thus laying down a different formula applicable to the inspector from that obtaining in the case of the experimenter. Conflict of opinion and jurisdiction would be the not improbable result.

44. In justification of their recommendation our colleagues go on to say:—

Report, Para. 121. "But we do not feel justified in recommending that, when the object of the experiment has not been attained, an experimenter should in all cases be required to destroy the animal immediately it exhibits signs even of severe pain which might in some cases be only momentary."

45. Here they appear to reintroduce the consideration of whether the object of the experiment has been attained or not, which they previously discarded alike for the inspector and the experimenter. We cannot contemplate the sanction of any such procedure in which either an animal is knowingly and intentionally to be kept for an undefined period in severe suffering if the experimenter thinks (perhaps erroneously) that such endurance will not be prolonged or in which he is authorised to allow the severe pain to continue in the problematical expectation that some result may accrue. We are unaware of any "weighty evidence" which justifies such recommendation.

Report, Para. 96. 46. In the words of our Report we hold that:—

"It would be vain to attempt to proportion the degree of permissible suffering in consideration of the amount of prospective gain, or to propound principles of vicarious sacrifice."

47. We agree with our colleagues in strongly holding:—

"that limits should be placed to animal suffering in the search for physiological or pathological knowledge, though some have contended that such consideration should be wholly subordinated to the claims of scientific research, or the pursuits of some material good for man."

Report of Royal Commission of 1875. [C. 1397], p. xx.

48. We endorse the views of the former Commission that:—

"the object of the conditions should be to ensure that suffering should never be inflicted in any case in which it could be avoided, and should be reduced to a minimum where it could not be altogether avoided."

49. We therefore recoil from the suggestion that an experimenter should be authorised to protract the life of an animal in obvious suffering or "which exhibits signs even of severe pain." Official sanction to any such procedure is out of harmony with the whole spirit of our Report and of the other recommendations which it contains; it would, we believe, re-awaken the "commendable public apprehension" referred to in Paragraph 7 of the Report and create "serious misgiving in the minds of the public," similar to that which the licensing of certain experimenters was "calculated to create." We accordingly recommend the insertion in the Act of a requirement upon all experimenters in every case in which obvious suffering has supervened forthwith painlessly to destroy the animal. It would then be the duty of the enlarged inspectorate, which we agree in recommending, to see that this statutory requirement is duly enforced.

Report, Para. 29.

50. We may in this connection call attention to the regulation in operation in Victoria as being somewhat in advance of our own practice.

51. It is not the case as stated in the Home Office Summary quoted in Paragraph 10 of the Report that in Victoria and Queensland the Animals Protection Acts of 1890 and 1901 exempt persons practising vivisection from the penalties of these Acts. On the contrary, as will be seen by reference to Volume VI., Appendix I., p. 4, such exemption shall not take effect in any case of vivisection or other experiment wherein certain conditions are neglected. In the case of Victoria the regulations require that when permanent injury or even "abiding discomfort" is likely to result the animal shall be without delay killed in as painless a manner as possible.

52. In sum then, the fundamental principles upon which any special legislation should be based should, in our opinion, be such as to secure:—

(1) That all investigations upon living animals of an experimental nature by way of operation, inoculation or infection, &c., shall only be conducted under the sanction and undivided responsibility of the Secretary of State, aided by skilled advisers, and exercising control and supervision by an adequate staff of inspectors.

(2) That all such investigations which, in the absence of anæsthesia would be likely to cause pain or suffering, shall be conducted under adequate anæsthetics, skilfully and humanely administered, or if the nature of the investigation render this impracticable, then that on the supervention of real or obvious suffering, the animal shall be forthwith painlessly killed.

OTHER QUESTIONS.

53. There are one or two minor questions also on which we are not in concurrence with the findings in the Report.

(*a*) *Sera and Vaccines.*—Although the Act of 1876 by its preamble purports to extend the law relating to cruelty to animals to " the cases of animals which for medical, physiological or other scientific purposes are subjected when alive to experiments calculated to inflict pain " we were informed by Dr. Thane that the Law Officers had advised that the processes of obtaining sera and vaccines for commercial use do not come under the Act as not being " experiments." No definition of the term experiment however is given in the Act. Thane, 1732.

Dr. Thane and Dr. Martin favoured the view that the preparation of such products should be brought under the control and responsibility of the Government. 1358–64. Martin, 11847–9.

As stated in the Report private premises have been registered under the Act for the purpose of testing and standardising drugs manufactured on a large commercial scale. The procedures leading to the experimental preparation of various sera have also been subject to the provisions of the Act. In our opinion if it be desirable to require registration, inspection, etc., under the Act in the case of the experimental production of sera and vaccines, it is idle to suggest that similar procedures, involving the use of living animals in a similar way, should be exempted from such control if the object happen to be commercial rather than scientific. From the point of view of the animal such precautions are not less necessary, and from the point of view of mankind such supervision may be deemed more necessary. Report, Para. 13.

We do not share the doubt of our colleagues that the production of sera and vaccines for commercial purposes involving the use of living animals falls outside the scope of our Reference. Even if it be outside the purview of the Act of 1876, our Reference which directs us to report what changes are desirable in the law, in our opinion includes the power to recommend that such procedures should be brought within the scope of the Act and we accordingly recommend that the law be made applicable to the case of the production of commercial sera and vaccines so far as living animals are utilized for such purposes. Report, Para. 104. Report, Para. 117.

(*b*) *Qualification of Inspectors.*—We think that persons qualified in veterinary medicine and science ought not to be deemed ineligible for any of the inspectorships as would appear to be implied by Paragraph 113 of the Report.

(*c*) *Reports of Results of Experiments.*—We agree that the Secretary of State should use the power he has under Section 9 of the Act of 1876 and that he should direct licensees to report to him the results of their experiments. We think, however, that such reports should be the rule and not the exception, or in particular cases only, as the Report seems to contemplate. Report, Para. 111.

A. R. M. LOCKWOOD.

WILLIAM J. COLLINS.

GEORGE WILSON.

RESERVATION MEMORANDUM BY DR. G. WILSON.

In deeming it to be my duty to submit this Memorandum, I beg to express my great regret that owing to permanently impaired health, following upon a long and severe illness, and seriously impaired eyesight, I was altogether unable to render any assistance in the preparation of the first draft of the Report of the Commission, nor could I take my due share in the discussions and deliberations which took place at the various meetings summoned for the consideration of the Final Report. Indeed, ill health prevented my attendance at several of these meetings, but when absent, I was kept fully informed of the progress made, and had ample opportunity afforded me of submitting amendments or suggestions. I was, however, able to attend almost all the numerous meetings of the Commission, commencing in October, 1906, and ending in March, 1908, which were held to receive evidence, and I did not hesitate to cross-examine to the best of my ability the Government officials, experts, and representative medical men who appeared before us. By this I do not wish to imply that my questioning was more searching than that of the other Commissioners, or indeed so searching as that of some of them, but I can frankly assert that the questions which I put were for the most part prompted by a sceptical attitude of mind, and more especially in respect to the prevention of pain, or its unavoidable infliction in various kinds of experiments on animals, and the vast benefits to humanity and also to animals themselves, which are claimed as resulting from them.

I now desire to state that while I am in general agreement with the principal findings and recommendations of the Commission, as set forth in the Report in accordance with the terms of reference, and therefore feel justified in signing it, I certainly cannot fully endorse all the opinions and conclusions which it embodies without qualification or supplement. And I feel bound to make this reservation, not because I have allied myself to any of the so-called anti-vivisection societies, but because I have all along endeavoured to study the whole question of animal experimentation from a perfectly untrammelled point of view, and, for the reasons already indicated, feel compelled to attach far greater weight to the evidence of some of the witnesses representing these societies than appears in the Report. I have taken no part in the fierce public controversies which for years back have surrounded the question. What I have said or written on the subject has been addressed to my own profession, and chiefly in relation to inoculation experiments connected with vaccine and serum therapy.

But before proceeding to present my reservations on this section of the Report, I wish to submit some comments on the administration of the Act of 1876, and the various instances of its alleged infringement which were laid before the Commission by several witnesses. So far as the Home Office and its officials are concerned, I am in full agreement with the findings which are set forth in the Report, that the charges brought forward by the Hon. Stephen Coleridge were only partially sustained, though I have no doubt he honestly believed them to be well founded. It is true that in consulting the Association for the Advancement of Medicine by Research as an advisory body, the Home Office, as stated in the Report (Paragraph 18 (12)), laid itself open to criticism, but I also feel convinced that this course was adopted solely with the view to ensure greater efficiency in the administration of the Act, and not in any sense to shield experimenters or encourage vivisection. Still, it cannot be doubted that its tendency was in this direction, because the Association consisted largely of men who were engaged in experimental or research work, or of those who were avowed advocates of vivisection.

With regard to the specific instances of infringement of the Act which were investigated, I must agree that with very few exceptions they have not been proved, and am of opinion that they were mainly based on misunderstandings and misconceptions. At the same time, I cannot question the good faith of the witnesses who brought them forward, and I sympathise in large measure with the motives which prompted them to give evidence in support of their convictions. For I am free to confess that, like a great many others, I was somewhat ignorant of the manifold safeguards provided under the Act for the prevention of needless pain, and as a continuous and close reader of the medical journals in which many of these experiments are published, I was often horrified with the details, and entertained grave doubts as to whether such severe and prolonged operations could be performed under anæsthesia so complete as to abolish all consciousness of pain. Moreover, these doubts were in great measure raised, and in some measure justified, by opinions expressed before the previous Commission that dogs, which are so largely used for physiological experiments, are more or less intolerant of chloroform, and readily succumb under its administration; while further doubts were raised either by a very scanty or perfunctory reference to anæsthesia, or by the not infrequent use of the terms "light" or "incomplete" anæsthesia, which were employed in describing the experiments. Then, too, suspicion was aroused by the variety and multiplicity of the agents used, either singly or conjointly, when these were specified, such as chloroform, ether, A.C.E. mixture, morphia, chloral, and the milder sedatives, urethane and paraldehyde, together with the occasional intrusion of curare. In experiments for which pithing or decerebration is resorted to for the purpose of ensuring complete anæsthesia, there was additional room for question on account of the different meanings attached to the words themselves, and the variety of methods recommended by different experts. This was clearly set forth, and references given, in Miss Lind-af-Hageby's evidence, and is dealt with in the Report (Paragraph 90). Lind-af-Hageby 9272.

But even when an animal is rendered completely insensible to pain by whatever means, there is still room for uncertainty that during a prolonged physiological experiment, or a demonstration of the severer kind before students, it may not recover a certain degree of consciousness, because it is not always easy to determine whether in such cases full anæsthesia has been continuously maintained. For example, tracheotomy is often performed on the dog, cat, or other animal employed in these experiments, and a tube inserted into the trachea, generally no doubt to keep up artificial respiration by mechanical means, while the air which is pumped into the lungs is charged with a regulated amount of the vapour of the anæsthetic used. It is clear that when a tube is inserted in to the trachea, when artificial respiration is not deemed necessary, the animal is rendered powerless to cry out, or emit any vocal indications of pain, even should there be a return of consciousness. Then, too, it is sometimes difficult to tell, especially on the part of an onlooker, whether any movements or quiverings are the results of merely reflex action, or are more or less purposive, and therefore possibly indicative of pain, especially when the experiment is of the severe type. This is well illustrated by the very prolonged and severe operation of cross circulation devised to demonstrate the mode of death by chloroform poisoning referred to in the Report (Paragraph 23), when there was such serious difference of opinion between Lieutenant-Colonel Lawrie, himself an experimenter, and Dr. Shore, who performed the operation, and the other medical men who were present. Indeed, the whole trend of Colonel Lawrie's evidence contested the view that animals, and especially dogs, could be kept fully anæsthetised by such mechanical methods with automatically regulated doses of the anæsthetic.

But though onlookers may on some occasions have entertained doubts, or indeed may have felt positively certain that perfect anæsthesia had not been induced or maintained, as stated in Miss Lind-af-Hageby's evidence, all the expert workers who appeared before the Commission gave the most positive assurance that no matter how prolonged or severe the experiment or demonstration might be, no matter what anæsthetising or pain-destroying agent was used, or how it was used, there was entire abolition of all pain from first to last. They maintained, in short, that if the animal is not allowed to recover, as in demonstration and many other physiological experiments, it should be regarded as practically a dead animal before the operation is commenced. And there is the same positive assurance even when curare is used, though it is agreed that this drug prevents the manifestation of all signs of pain of whatever kind by paralysing the voluntary muscles without obliterating sensation. As examples of these most positive assurances in respect to the complete anæsthetisation of animals during the severest operations, and also when curare is used, I will now quote some replies given by Professor Starling in answer to questions put by the late Mr. Tomkinson, by Mr. Ram the present Chairman of the Commission, and by myself:—

Q. (*Mr. Tomkinson.*) You have spoken with very great certainty, and no doubt perfect assurance, of the completeness of the administration of anæsthesia?—*A.* Yes. Starling 4153.

Q. Are you quite satisfied in your own mind that there is no room for doubt upon that subject?—*A.* Yes, I am quite satisfied. 4154.

Q. Are you quite satisfied that, in the case of a dog, it is possible to keep it for a long time under perfect anæsthesia between sensibility and death?—*A.* Perfectly. The dangerous part in anæsthetising a dog is at the beginning. Starling 4155.

4156. "Q. You are aware that there is some difference of opinion on that subject? We have had one witness here who has taken a contrary view?—A. But anybody who knows anything about it will confirm my evidence. You can get Mr. Hobday, for instance, who is very keen on this subject of the anæsthetisation of animals. Many veterinary surgeons perform operations without anæsthetics. Mr. Hobday is very keen on the subject of the anæsthetisation of animals, and I think he is coming up as a witness, so that he will be able to tell you how many thousands of times he has anæsthetised animals, and with what proportion of deaths. But, as a matter of fact, we physiologists know more about the anæsthetisation of animals than probably any veterinary surgeon does, and I have no hesitation in saying that there is no serious difficulty in anæsthetising any kind of animal.

4157. Q. And keeping it perfectly anæsthetised?—A. And keeping it perfectly anæsthetised. It is easy to keep it so. The dangerous part is during the period of induction, when you are first getting them under. After that it is only gross carelessness if they die under the anæsthetic.

4158. Q. Does the anæsthetic continue to be administered almost automatically?—A. That depends.

4159. Q. Or has the animal to be watched carefully by an attendant?—A. When you are giving the anæsthetic by artificial respiration, blowing air in and out of the animal's lungs, as we sometimes do, then that is purely automatic. The taps are arranged, and a certain amount of A.C.E. mixture is blown in regularly with each breath. It does not want watching, except to see that the whole of the anæsthetic is not used up in the bottle. When the animal is anæsthetised by a mask, there is somebody always there, and when the anæsthetic is used up the animal may show signs of movement, just as a human patient would, and then there is some more anæsthetic put on the mask. I think it would be a good thing if some of the Commissioners who doubt as to the completeness of anæsthesia would come and see an actual experiment, and then they could convince themselves of the reality of the measures which are taken to prevent sensation, and of the general nature of the experiments which are performed.

With regard to curare, Mr. Ram put the following question :—

4054. Q. (*Mr. Ram.*) Are there any means, other than the cries or struggles of the animal, by which you can tell whether the anæsthetic is passing off?—A. Yes, you can tell it by the blood pressure. Struggles have also what we may call their visceral side. This activity of the muscles of the body is associated with activity of the centres which govern the blood vessels, and when one is working without curare one notices that the pressure goes up, and then, if one does not attend to it, after that comes a little movement, and you give more anæsthetic.

4055. Q. So that the presence of curare does not prevent your knowing whether the anæsthesia is complete or not?—A. No, it would make it more difficult, but you have that clue. What one does, of course, is to ensure the complete anæsthesia, and continue that anæsthesia during the curare—continue the same amount.

4056. Q. Is curare ever given under Certificate B in cases of animals that are to recover from the anæsthesia?—A. Never.

4057. Q. Therefore, every animal that has curare is, so to speak, bound to die under the anæsthetic?—A. Yes.

Further, in reply to a question by myself, Professor Starling made this statement :—

4242. "The tendency, of course, of anæsthesia is not to become less. If you continue the administration of a certain dosage of chloroform, the anæsthesia gets deeper and deeper; it does not get less and less, but deeper and deeper. In those cases where you are going to give curare, you have this volatile anæsthesia automatically being delivered by pumping, and it continues; you give curare, and that continues until the animal is dead. If anything happens, it will be a continual deepening of the anæsthesia, not a recovery from the anæsthesia. It cannot stop."

As all the other expert witnesses testified to the same effect when the numerous questions bearing on this part of the subject were put to them, one can only conclude that Colonel Lawrie was altogether mistaken in his general contention. He admitted, however, that dogs could be readily and fully anæsthetised by chloroform in the ordinary way, by mask or otherwise, or even through a tracheotomy tube, provided there is no pumping into the lungs. Further, the evidence of Mr. Hobday, referred to by Professor Starling, when he appeared before the Commission, dispelled all doubt that in the ordinary surgical operations which are carried out on dogs for ailments or accidents, as in the case of human beings, perfect chloroform anæsthesia can be secured and maintained throughout without any difficulty, but with the *proviso* that the anæsthetic must be carefully administered by an experienced assistant, and Miss Lind-af-Hageby confirmed this. But it is when morphia is used, either by itself, or along with chloroform or ether, that some degree of uncertainty creeps in, because dogs can take enormous non-lethal doses of this drug, amounting according to Professor Starling to as much as 15 grains, and, according to others, even much larger doses. It cannot, however, be regarded as an anæsthetic in the ordinary sense of the word,—it is an analgæsic or pain-killer, as pointed out by Professor Langley, and when administered with due care as to dosage, there can be no question that it can effectually abolish all pain, no matter how severe and prolonged the experiment may be, and this also applies, according to Professor Cushny's evidence, to the analgæsic agents urethane and paraldehyde when injected into the veins in very large or lethal doses.

While, therefore, I feel bound to accept the assurances of all the expert witnesses who appeared before us as assurances of their honest conviction that vivisectional or cutting experiments can be, and are, carried out without the infliction of any pain from the moment the first wound is made by scissors or scalpel until either the animal dies or is killed, or until the last stitch is inserted into the wounds, if the animal is allowed to survive under certificate, I can only accept them as opinions to which the greatest weight should be attached, and not as statements of absolute fact so far as specific instances are concerned. For it appears to me that even the most careful and conscientious physiologist or research-worker may at times be mistaken as to whether in some of the severer operations the animal is entirely unconscious of pain throughout. If, for example, he relies too much on his laboratory boy or assistant, there may be a return of consciousness, the signs of which might easily escape his notice, especially when he becomes engrossed in his experiment, or too intent on the lecture which an experiment is intended to demonstrate. There is, therefore, a very grave responsibility resting on the licensee, alike on humane and legal grounds. In my opinion, strict compliance with the provisions and intentions of the Act cannot be ensured, no matter how extended or inquisitorial inspection may become; it must always mainly rest on the care, ability, and honest endeavour of the licensee. When curare is used, for instance, an inspector could not tell on entering the laboratory whether the curarised animal was completely anæsthetised or not, unless he had the opportunity of assuring himself of this before, as well as during, the experiment. And this would apply, though in less degree, to other severe experiments in which such analgæsic agents as morphia, urethane or paraldehyde, are mainly relied on, and in all experiments in which artificial respiration is resorted to. It is for these reasons that I think every licensee, when sending in his report to the Home Office, should be required to forward an accompanying certificate to the effect that the research work or experiments reported have been conducted in strict compliance with the provisions Thane of the Act to the best of his endeavour and belief. Such a requirement need not be 1771–6. regarded as casting any reflection on the good faith or even the humanity of those engaged in animal experimentation, —it should only be looked upon as a perfectly legitimate, and not at all uncommon, form of attestation, and would at all events serve to direct close and continuous attention to the obligations devolving upon everyone holding a licence.

In this connection I may also point out that there may be others engaged in research work who, like Dr. Pembrey or Dr. Klein, as referred to in the Report (Paragraphs 28-29), may regard the infliction of pain as quite a secondary consideration. Indeed, the numerous instances of the interference of complete surgical anæsthesia with successful results, which Lind-af- were quoted by the various witnesses opposed to vivisection, and so fully illustrated by Hageby Miss Lind-af-Hageby in her evidence, warrant the conclusion that however confident the 7300. operator may be that he has abolished all pain, vivisectional anæsthesia, with all its variety of agents and methods of induction, can never be divested of an element of uncertainty, whether incidental or not. Thus, in the book called "The Shambles of Science," written by Miss Lind-af-Hageby and Miss Schartau, which was so prominently brought before the Commission in the examination of other witnesses even before Miss Lind-af-Hageby gave her evidence, there are various demonstration experiments described in which the authors were convinced that full anæsthesia was either not induced, or not maintained throughout, and though I agree that in face of the evidence given by the expert witnesses in respect of these experiments the authors may have been mistaken, I must also admit that throughout her long and severe examination Miss Lind-af-Hageby maintained her contentions with unwavering forcefulness, and an abundance of apposite illustrations and quotations from recognised authorities in support of them. I believe she was wrong in her impressions concerning the marmot and rabbit referred to in the Report (Paragraph 21), but in respect to other experiments, if she failed to prove that pain was inflicted in specific instances, that does not prove that in all those instances the experiments were conducted painlessly throughout.

In quoting the following excerpt from her evidence, I do so partly to give an example of the kind of severe demonstration experiment which is sometimes carried out before advanced students, and also to illustrate more fully the difference between surgical and vivisectional anæsthesia. Admitting that "the whining and struggling" were only manifestations of reflex action, and that the dog referred to was properly anæsthetised, the operation itself was of such length and severity that one can quite understand why an onlooker should have regarded cries or movements of any kind as signs of returning consciousness. In her evidence on the subject of anæsthesia the operation is referred to as under :—

Lind-af-Hageby 7350. *Q.* (*Sir John MacFadyean.*) Have you yourself attended at some veterinary operations ?—*A.* I have attended at some veterinary operations, and I have also attended some vivisectional operations, and surgical operations too, and therefore I am anxious to draw a distinction. Veterinary operations are not disturbed, but aided, by deep surgical anæsthesia. Vivisectional operations, dealing as they do with the delicate measuring of functional activities, are often disturbed by deep anæsthesia. Suffering after veterinary operations can be mitigated in every way. Suffering after vivisectional operations often forms part of the experiments, its causes and development being carefully recorded. Through the courtesy of Professor Hobday I was some time ago allowed to be present at an abdominal operation on a dog which was in a state of perfect surgical anæsthesia, in which every sign of consciousness or pain was entirely absent, and the dog lay in an absolutely limp and motionless condition through the action of chloroform administered by a special anæsthetist who devoted his whole time and attention to this, and who kept the anæsthesia uniform by constant and careful doses of the anæsthetic. The difference between this anæsthesia and conditions which I have seen in the physiological laboratories is very great. The whining and struggling dog which I saw on February 26th, 1903, at the Physiological Laboratory, University of London, in which the abdomen had been opened, the intestines pulled over to the right side, and the kidney placed in an oncometer, the neck opened, and which was subjected to the usual stimulation of nerves, compression of veins, and injection of foreign substances, is an instance of a victim of the light anæsthesia which the vivisector prefers.

7351. *Q.* (*Colonel Lockwood.*) That you saw yourself ?—*A.* Yes. It is described in "The Shambles of Science" under the title "A Troublesome Dog"; it was troublesome because it whined.

7352. *Q.* (*Sir John MacFadyean.*) When you say it was a victim, you intend to suggest that it was conscious of pain ?—*A.* I intend to suggest that so far there were signs of consciousness.

7353. *Q.* What do you wish us to believe, because there is no point in it if the animal was quite unconscious ?—*A.* The difficulty of proving this is that nobody, neither the Commission nor I, nor any vivisector, can say whether that particular dog felt pain. I simply say that the dog whined and cried and seemed to be conscious, and we never can go by any other signs. How can we know whether an animal feels pain ? We can only go by signs.

Apart from demonstration experiments, however, there are numerous others of even greater length and severity, but classed as research experiments, which also entail the destruction of the animal before it recovers from the anæsthetic. To that class belong the notorious experiments carried out by Dr. Crile, referred to in the Report (Paragraph 20), and I may also instance the following quoted by Mrs. Cook as two out of many examples which she submitted in her *précis* of evidence :—

Cook 1785. "In the second volume of the *Journal of Physiology*, p. 267, will be found an account by J. N. Langley, M.A., from the Physiological Laboratory at Cambridge, of an experimental operation on a dog. The dog was narcotised with morphia. I believe I shall have expert support if I say that morphia is not an anæsthetic. The proceedings by which this dog was most cruelly tortured lasted between three and four hours. The side of the neck was cut open, a tube placed in one of the ducts of a saliva gland, one of the nerves in the neck dissected out, tied and cut, and the cut end irritated. The tube was placed in the wound an hour before the irritation of the nerve was begun; during part of this time the nerve was being dissected. The stimulation was continued for nearly three hours, about each third minute."

1919. "In the *Journal of Physiology* for May 28th, 1902, Drs. Brodie and Halliburton report experiments on dogs which were carried out in the following manner: The abdomen was opened, the spleen exposed, and the nerves of the spleen laid bare and a length of nerve dissected out; then the nerve cut and the spleen enclosed in a box. When the nerve is excited by an electric current the spleen contracts. The spleen is of course all this time still connected with the living animal. The vivisectors go on to say that simultaneously with the experiment on the spleen they also measured the blood pressure in the carotid artery, because, as they explain, 'Most of our experiments lasted many hours, and the condition of the arterial pressure furnished us with a convenient means of gauging the general condition of the animal.' At the beginning of the experiment we are told that the animal was anæsthetised with morphia and A.C.E. mixture, but it is, to say the least of it, *most* unlikely that the animal was kept in a state of complete unconsciousness of pain during the "many hours" that it lay cut open with the spleen in a box, while its nerves were being irritated by electric currents."

I quote these as examples of severe laboratory experiments, but do not wish to imply that the experimenters themselves were either careless, or did not believe that the operations were conducted painlessly throughout.

But in addition to the large class of experiments which are designated "painless," in which the animals must be killed before they recover consciousness, there are numerous other experiments carried out under certificate entailing more or less suffering, which can only be approximately guessed at or estimated. There are, for example, starvation experiments, feeding experiments, air experiments, inoculation experiments, and experiments under various certificates permitting the survival of the animal after operation for purposes of observation. In respect to all these experiments, it is next to impossible to distinguish between discomfort and severe or acute pain; it mainly depends upon the personal estimate of the experimenter or observer. Anyone who has watched a pet dog or cat when seriously ill, or even a favourite horse, knows that the animal frequently does not show any very obvious signs of pain, as the human patient generally does, even though one can have no doubt that the pain so silently borne may be very severe. It is also assumed that because human patients frequently do not suffer very much from wounds resulting from operations of varying severity, animals should enjoy a like immunity when operated on under similar aseptic precautions. But there is this great difference between the human being and the animal under operative conditions—the former is operated on to cure a disease or remedy an injury, entailing more or less suffering which the operation relieves, while the animal is in the enjoyment of perfect health until it is operated on, and remains a more or less maimed animal while it continues under observation. I cannot, therefore, accept Professor Starling's contention without considerable reservation, that dogs with fistulous openings made in the digestive tract, which deprive them of secretions necessary for healthy life, only suffer discomfort, or that the numerous researches of a similar kind carried out by Professor Pawlow, of St. Petersburg, to which he refers, are, or can be, carried out without the infliction of a varying amount of pain. As Miss Lind-af-Hageby stated in her evidence, the physiological experiment speedily becomes pathological, and "disease and suffering are by no means absent." Starling 4119. Lind-af-Hageby 7276.

But without dilating further on specific research experiments, and the degree of pain which they are likely to inflict while the animal is kept under observation, I would refer to the replies of Dr. Thane, the Inspector under the Act, to questions put by Sir William Collins, which also include the effects on animals after the administration of drugs, and the results of inoculation experiments. While it must be admitted that test inoculations which are negative are painless, and these constitute a very large proportion of the inoculations reported under the Act, "the pin-pricks of a needle" may nevertheless be followed by very painful results, as after the injection of a toxin such as diphtheria toxin, tetanus toxin, or plague serum, and, in respect to drug experiments, strychnine, which according to Dr. Thane is also sometimes used for demonstration purposes. With regard to the varying amount of pain following experimental operations of the severe kind, Dr. Thane, in reply to Q. 472, made the following general statement:— Thane 451–76.

"There are more severe cases. Excision of both suprarenals causes severe shock, and is probably speedily followed by the death of the animal; removal of a large portion of the second kidney or excision of the thyroid gland makes the animal very ill, and probably leads to a fatal termination, although it does not cause acute pain. Operations on the stomach, intestine and pancreas vary greatly in intensity; some, such as the formation of an ordinary gastric or intestinal fistula, are, from the surgical point of view, comparatively simple operations, and can be performed without causing great danger to life, or subsequent inconvenience to the animal. I have seen a dog with pancreatic fistula which was apparently in good health, though I believe that he suffered eventually from loss of pancreatic juice, and had to be killed. Other operations on these organs may be of a more severe character, and followed by more serious results, but I can hardly make a general statement with regard to these. The same is the case with regard to operations on vessels, ligature of arteries and veins for example. Some of these are comparatively slight; others are followed by severe illness, such as the production of ascites, from which the animal may however, recover perfectly." 472.

And further:—

"I should like to add one other case which is a painful case. Painful irritation of cutaneous and mucous surfaces are cases in which there is pain after the operation has gone off, although they may not be very severe operations." 473.

Though it may be conceded that the capacity for suffering, or sensibility to pain, is lower in animals than in man, and that it becomes less *pari passu* the lower the scale of animal life, it would be illogical to assume that in the absence of any distinct signs of suffering, an animal which has been operated on, or inoculated with a toxin or virus is absolutely free from pain, or suffers only what is understood as discomfort. To judge from human patients, even the return to consciousness after recovery from the anæsthetic must cause more or less pain, and the removal of an organ or part of an organ, as in the much-discussed kidney operations, carried out by Sir J. R. Bradford, must cause proportionately more suffering or discomfort in the healthy animal than in the human being, because natural processes have had no time to develop compensatory adaptations as they do in the diseased patient before an operation is deemed necessary.

Mrs. Cook, who represented the Parliamentary Association for the Abolition of Vivisection, made special reference to the experiments carried out by Sir J. R. Bradford in her evidence as follows:—

"For these operations he used forty-nine female fox terriers. Chloroform and morphia were used for the actual operation, and the animals were then placed in a glass case with a glazed floor for observation. Cook 1798.

Pieces were cut out of the kidneys, and they were mutilated in different ways. In the case of one dog the operator cut a piece out of the kidney and then tried to graft the piece and make it grow on another part of the inside of the animal. The animal died in four days. Out of the forty-nine, one died in six days from loss of blood, two died from blood poisoning as the result of the wounds. One animal lingered for thirty-six days after operation, the cause of its death being unknown. Five died from causes immediately connected with the operation after lingering for various periods. Two animals had the kidneys mutilated three separate times at separate intervals."

After explaining the research objects or purposes of these experiments, Sir J. R. Bradford, when examined, gave the following replies to questions :—

Bradford 17693. *Q.* (*Sir William Church.*) Would you tell the Commission, if you remember at this length of time, exactly what did become of these dogs. You see, in that paragraph, Mrs. Cook accounts for nine out of the forty-nine; she does not tell us what became of the other forty ?—I should be very pleased to put the facts before you. You have had a copy of the published paper, giving the facts as regards each one, of course. To summarise the thing shortly, I would say that a great number of these dogs lived, of course, for very prolonged periods, in perfect health.

17694. *Q.* Without apparently suffering ?—Yes.

17695. *Q.* They were in the condition of a man who has had an operation on his kidney ?—Yes, they were perfectly well. Some lived, for example, for two years, and they were killed, not because they were ill, but because they served no useful purpose to keep any longer. Others wasted; they passed large quantities of urine, and got thin, but they suffered no more than that. They did not suffer at all, of course, except that they were thin. Others, where the quantity removed was greater, got so thin that they became extremely weak, and no doubt ill; they vomited occasionally; and those animals were killed immediately —there was no object in keeping them, of course. Then some of them, as stated by the witness, died within a few days of the operation. I remember this one alluded to here as having lingered thirty-six days, because it was a very remarkable one. That animal lived for thirty-six days, and died suddenly. I do not know from what cause, but it is quite easy to give you the full facts as regards each individual one if they are wanted. They are fully published.

17696. *Q.* So that only two died from blood poisoning as the result of wounds, and one in four days. That is what she says ?—Yes, I have no doubt that is true, but I should have to look it up to verify it.

17697. *Q.* And the others were much in the condition of a man who has had a successful operation performed upon his kidney after he recovers ?—Yes.

17698. *Q.* That is to say, it is not likely that they were suffering tortures or pain ?—There is no truth in the statement that they were suffering tortures or pain. And the only ones where there was evidence of the wound going wrong in the first few days after the operation were killed of course.

His further examination by Mr. Ram was as follows :—

17752. *Q.* (*Mr. Ram.*) With regard to these experiments on dogs, when you extracted part of a dog's kidneys, as I understand, it was, of course, necessary that the dogs should survive after the anæsthesia passed away in order that you might see the effect ?—Yes.

17753. *Q.* In the majority of cases I understand that the dogs throve and showed no signs of illness ?—In a large number of cases.

17754. *Q.* In other cases the dogs, I suppose, were ill in consequence of the effect of the abstraction of more or less of the kidneys ?—Yes, they wasted. Wasting was the prominent symptom of illness.

17755. *Q.* In respect of some of those dogs, I suppose, there was pain, or at all events discomfort, in consequence of the illness ?—I should say that there was discomfort. I should not say that there was pain; but there was in those animals in which there was the smallest amount of kidney left a very considerable physical weakness and inability to take food, and therefore I have no doubt that there was discomfort. There was never any evidence of local pain, and there was no reason for supposing that there should be.

17756. *Q.* Were the dogs in a condition in which they were lying about in discomfort moaning ?—No.

17757. *Q.* It did not amount to that ?—No, I never heard one of these dogs moan.

17758. *Q.* Or give any actual signs of acute suffering ?—No.

17759. *Q.* There was an expression used by Mrs. Cook, I think, which was put to you just now—I do not know whether you adopted it—that one dog lingered for thirty-six days, and died suddenly ?—Yes, my impression is that it died suddenly, but I will look that point up if you wish it.

17760. *Q.* Only to this extent, the phrase she used, I do not know whether it was adopted from you or invented, that it lingered for thirty-six days, gives an idea of great discomfort ?—I do not adhere to the word lingering. I do not think I used it. I should prefer to look it up, and send you an account of it.

17761. *Q.* It was put to us as a case in which it was proved that the dog suffered pain for a considerable time—thirty-six days. Anything that you could give us as to that particular case I should be glad of ?—I have no hesitation in answering at once that the dog did not suffer pain. I do not know in what particular form of words I described its life for thirty-six days.

It will be observed from the above replies that Sir J. R. Bradford contends that while some of the animals operated on did suffer discomfort, none of them, not even the severest cases, suffered actual pain. That is a contention with which I cannot agree, even admitting that there was no infringement of the Act which permitted Sir J. R. Bradford to keep the animals alive until the object of his research was obtained. Further, as I have already stated, so very much depends upon the personal factor in differentiating between what is regarded as only discomfort and what might be regarded as pain.

Amongst other research experiments of a terribly severe kind which were quoted by Mrs. Cook from various medical journals were the following :—

"In the *Journal of Pathology and Bacteriology* for May, 1892, is a record of experiments performed by Mr. Dean, assistant surgeon to the London Hospital, in which twelve bonnet monkeys and six dogs were used. The bone over a part of the skull was laid bare, and a disc of bone removed from the skull. Then a glass disc was introduced into the wound for the purpose of pressing upon the brain. The brain was first observed through the disc, and by a subsequent operation, the disc was removed at varying intervals of time after its insertion, and pieces of brain were taken out from time to time. The operator states that the animals were anaesthetised with ether, but that would, of course, only be for the initial operation; it would plainly be impossible to keep these mutilated, paralysed monkeys and dogs in a state of unconsciousness of pain for the number of days that the experiment lasted. One animal was kept alive for three days, one for twelve, and so on." Cook 1796.

Q. (*Dr. Gaskell.*) Where is that published ?—In the *Journal of Physiology*, February 27th, 1899. Mr. Walter Edmunds carried out experiments on eighteen dogs at the Brown Institution. These animals had the thyroid gland cut piecemeal. One died the night after the operation; another survived the first operation, and had a second piece of the gland cut out, and then died. One lingered two days, another four, one seven, and one as much as twenty-eight days. Successive operations were performed upon the animals which survived. The symptoms produced by these operations were: tremors, a stumbling walk, paralysis, emaciation, and weakness; in some cases sinking of the eyeballs, and in some protrusion. The same operations were performed on ten monkeys. Five of them died in consequence of the operation, in 13, 32, 36, 68, and 262 days respectively. One was killed accidentally, but at the time was seriously ill from the effects of the operation. 1799.

The following are instances of inoculation experiments submitted to the Commission in Mrs. Cook's *précis*, and I lay special emphasis on the two series of diphtheria experiments carried out by Dr. Klein, because they were conducted under the authority of the Local Government Board :—

"In the *Journal of Pathology and Bacteriology* for May, 1894, Dr. Klein gives an account of the inoculation of ten cows with diphtheria. It is a history of tumours—some as large as a child's head—of sores and eruptions. The cows coughed incessantly, were unable to stand, refused their food, etc. Some lingered fourteen, seventeen, and twenty-four days; some of them were killed after days of suffering. This is a description of one of these unfortunate animals, which was in perfect health when Dr. Klein injected his bacilli: 'On April 27 the tumour of cow No. 3 had become much enlarged, and was very painful to the touch. The animal had now conspicuously fallen away on the flanks; she moaned, fed but little, did not ruminate, and her milk secretion had almost ceased. . . . This animal was found dead on the morning of May 5th, the seventeenth day of the experiment.' At the post-mortem nearly all the internal glands were found to be diseased." 1796.

"Take for instance some of the experiments of Dr. Klein recorded in the Supplement to the Annual Report of the Local Government Board for 1889. Dr. Klein is notorious for having admitted in his evidence to the last Commission on Vivisection that he is indifferent to the sufferings of the animals. He reports in this supplement the results of inoculations which are of a most cruel character, several experiments on cats having been inoculations into the eyes with diphtheria bacillus from a human source. The plates with which his report are illustrated show the condition of the eyes sixteen, seventeen, and eighteen days after inoculation with the whole centre of the eye transformed into an ulcer. Can anything more painful be imagined than for the actual centre of the eye itself to have been transformed into an ulcer ?" (*Exhibiting the plates.*) 1919.

Another excerpt from Mrs. Cook's *précis* is as follows :—

"In the *Journal of Pathology and Bacteriology*, dated May, 1903, appears a paper describing experiments on tuberculosis made by the Bacteriologist and Assistant Bacteriologist of the Jenner Institute of Preventive Medicine. Pigs, cats, rabbits, and mice were experimented upon by inoculation with the sputum from advanced cases of human phthisis. In the experiment described on p. 474 the pig was inoculated on August 20th, 1901, and it died on January 4th 1902, 137 days after the inoculation. At the site of the inoculation there had resulted from it a swelling which became hard, and then, before death, underwent softening and spontaneous opening, giving rise to the formation of a small sinus. There was arrest of development in the animal and great emaciation. In the post-mortem examination there was found at the site of inoculation a swelling the size of a small orange from which the above-mentioned sinus opened. Another pig was inoculated with sputum from the same patient on August 23rd, 1901. Swelling took place at the site of inoculation, which within a month gave place to a nodule the size of a large pea. The animal wasted and died on October 9th, 1901, forty-seven days after inoculation. The body was much emaciated. The history of three other pigs is similar to these two except that in one the knee became thickened, and on post-mortem examination a small abcess was found in the thickened capsule of the joint. This animal was regarded as so healthy as not to require the tuberculin test applied to the others, and it died, like them, in a miserable state of disease, the result of the inoculations. The post-mortem showed all the organs, in all of them, to be in a most diseased condition." 1919.

As further evidence of the very severe suffering induced by inoculation experiments on animals in the course of research work, I would refer to the enormous number of such experiments which have been carried out by the recent Tuberculosis Commission, and have been published in the several reports issued by them during the ten years occupied by their enquiry.

With regard to starvation experiments, Professor Starling contended, and supported his contention by quoting fasting experiments, that starvation is not painful. It certainly may not cause acute pain, but as a medical officer of convict prisons during the early part of my career, I have had numerous opportunities of assuring myself that a sentence of three days bread and water, which certainly did not impose starvation by any means, was felt to be a somewhat severe form of punishment, while among the very poor and starving "the pangs of hunger" is not an altogether meaningless phrase.

As to the results of feeding and other experiments, I give the following further excerpts from Mrs. Cook's *précis* :—

Cook 1800. "Mr. Edmunds, who is the surgeon at the out-patients' Evelina Hospital for sick children, has also experimented at the Brown Institution with eight monkeys, by feeding them with preparations of thyroid glands. . . . The symptoms produced by this treatment were: 'protrusion of the eyes, dilatation of the pupils, the eyes were more widely opened; the hair on the head stood up and fell out in patches; there was paralysis of one or more limbs; wasting away, and muscular weakness; and finally death from general exhaustion. The average life of the monkeys after the commencement of the treatment, seventy-six days. . . . Shortly before death the animals show an objection to light, and also to being looked at; they hang their heads down, and put their hands to the back of the neck, which seemed, in one instance, to be tender to the touch.' In the *Journal of Pathology and Bacteriology*, December, 1900, p. 71, in which this is related, the first illustration is a melancholy picture of a lingering and painful death.
1921. There are severe cruelties practised incidentally in the experiments in causing disease. Some of these are referred to in a paper in the *Broadway or Westminster Hospital Gazette*, for January, 1900, on 'Our Natural Protective Agencies against Specific Infection,' by G. Sims Woodhead, M.D., Professor of Pathology in the University of Cambridge, being the Sturges Lecture before the Guthrie Society (delivered on November 23rd, 1899). The following sentence occurs in it:—'Good food, regular exercise in the fresh air, regular and sufficient sleep, no exertion too prolonged, are powerful agents in protecting a patient against the attacks of infective disease to a degree that many scarcely appreciate; and just as pigeons that have been starved, hens that have been deprived of water, rats that have been exhausted by continuous exercise in a revolving cage or fed on vegetable food only, frogs that have had their temperature raised by artificial means, or hens that have had their temperature lowered by having their legs kept in cold water, are readily infected by anthrax, so human patients subjected to similar conditions may be said to acquire a general susceptibility to disease.' It seems strange to one who is not a scientist, that such cruelties should be resorted to in order to prove what everyone is aware of, that a person in an exhausted condition takes a disease more readily than one who is in a good state of health, well fed, and not overworked."

While I fully agree with Professor Sims Woodhead in his views as to the natural protecting agents against infection, and also with Mrs. Cook in her comment, as quoted in the last part of the excerpt, I have given these several examples of research work as showing how very rarely any of these numerous experiments have been carried out without inflicting degrees of suffering, varying according to the object of research. They throw a great responsibility on the licensee, because, I am frankly of opinion that it should be made obligatory on him to painlessly destroy the animal in all cases in which it shows signs of obvious suffering or considerable pain, even though the object of his experiment has not been attained. A very great responsibility is also thrown on the Home Office in granting certificates for all experiments coming under this category which are likely to entail suffering, because they are not only carried out in considerable numbers, in various laboratories in this country, but in far larger numbers in Continental laboratories, and also in the richly endowed animal laboratories of America, and many of them may be merely repeat experiments, or experiments which, for other reasons, are practically useless. It is in respect to experiments of this kind, in which the animal is kept under observation for varying periods that the increased inspection recommended in the Report (Paragraph 113) will be of chief service, especially if the inspectors are empowered, as also recommended, "to order the painless destruction of any animal which shows signs of obvious suffering or considerable pain, even though the object of the experiment may not have been attained." In their more frequent visits to laboratories, therefore, they will frequently have to face that difficult problem previously referred to of differentiating between admitted discomfort and "obvious suffering," or "considerable pain."

PROGRESS OF SCIENCE AND RESULTS OF EXPERIMENTS ON ANIMALS.

In submitting my reservations with regard to this section of the Report (Paragraph 30 *et seq*), I fully recognise that the great weight of the evidence which was laid before the Commission by the distinguished expert and professional witnesses who appeared before us is overwhelmingly in favour of the conclusion that valuable results have been obtained from experiments on animals, not only in respect to the increase of useful physiological knowledge, but also in respect to the relief of suffering and the prevention and cure of disease in animals as well as in man. While I agree in this view, I do so in only a very restricted sense; indeed, I feel bound to state that I have been far more impressed with the fallacies and failures which have attended this method of research than with the successful results which are claimed. In dealing with this part of the subject, I will endeavour now to adhere as closely as may appear necessary to the sequence of the various headings which are given in this section of the Report.

Physiological Results.

The public at large have been so impressed with the wonderful discoveries which have been made in chemical and physical laboratories during the last half-century, that hopes of similar brilliant achievements have been entertained, and diligently fostered, in respect to the research work carried on in animal laboratories. But apart altogether from the element of pain, which is more or less inseparable from experiments necessitating the employment of living animals, it need hardly be pointed out that, when compared with chemical or physical research work, the liability to error is enormously increased, not only in gauging or interpreting results, but is still further accentuated by the loose logic with which such results can be made applicable from animal to man. Granting that useful physiological knowledge has been gained from experimental physiology, there was surely good ground for hoping that after all these years, and with an ever-increasing army of physiologists distributed all over the civilized world, the stage of finality was nearing, if it was not yet reached. But this, according to Professor Starling, is an altogether illusive hope. The will-o'-the-wisp quest for the "control of function" must still go on, though the thoughtful physiologist cannot fail to realise, while he may be loth to admit, that the innermost self-regulated functions of the living animal organism are as insolvable as the "Riddle of the Universe." Even in the crude experiments of the severer kind necessitating the destruction of the animal at the close of the operation, some of which are demonstration experiments before students and others coming under the category of research experiments, it is admitted by the distinguished physiologist Pawlow, previously referred to, that "many sources of error lie concealed." As quoted by Miss Lind-af-Hageby in her evidence, he goes on to say:— Lind-af-Hageby 7592.

"The crude damage done to the integrity of the organism sets up a number of inhibitory influences which react upon the functions of its different parts. The body as a whole, in which an enormous number of different organs are linked together in the most delicate fashion for the performance of a common and purposive work, cannot in the nature of things remain indifferent to forces calculated to destroy it."

But it is strongly contended by Professor Starling, Sir Victor Horsley, Sir J. R. Bradford, Sir Lauder Brunton, and others, that these "crude experiments" in which so many "sources of error lie concealed," are deemed highly essential for the thorough education of the medical student. There is, however, such variety in the number and character of these demonstrations in different medical schools, as well as in the classes of animals used, as stated in the Report (Paragraph 118), that I venture to think they could be dispensed with altogether, and demonstrations be limited to the common classical experiments on the properly anæsthetised or pithed frog or other cold-blooded animal. The costly animal laboratory has of late years become an outstanding factor in the keen rivalry which exists between medical schools, and I would suggest in passing that the regulation of the teaching of experimental physiology as a part of the medical curriculum is a subject which might fitly be considered by the General Medical Council, inasmuch as that Council is made responsible to the State for the efficient education and training of the medical student. I make this suggestion for two reasons:—Firstly, because it will be seen from the following statement made by the late Sir Henry Acland, who was then President of the Medical Council, in his evidence before the Royal Commission of 1875, that this was a subject which was about to be considered by the Council, when the outcry throughout the medical profession, as well as on the part of the public, against vivisectional cruelties, led to the appointment of the Commission. This is Sir Henry Acland's reply to Q. 951 in the evidence given :—

R.C. of 1875 Acland 951.

Q. Has this subject been brought before the General Medical Council ?—No, it has not; and I am glad that you have asked me that question, because it enables me to say that it would have been brought before the Medical Council this year, at the session which has lately closed, had it not been that the Government had appointed your Lordship's Commission. We were going to discuss the question of the extent to which what is called practical physiology, which means in great measure this subject, was requisite for students; but my colleagues who were going to bring the matter forward felt that it would be more seemly if we delayed until after your Lordship's Commission had reported, and so the subject was for the time dropped; otherwise it would have been brought forward.

Lind-af-Hageby 7213. 7214.

My second reason is this—and it is a point which was strongly emphasized by Miss Lind-af-Hageby in her evidence—that the Act of 1876 explicitly commands that only experiments which are deemed absolutely necessary for the due instruction of the medical student shall be performed in illustration of lectures. It is evident, therefore, that the provisions and intentions of the Act in respect to the teaching of experimental physiology are not strictly complied with, because the decision as to what is "absolutely necessary" is left to the individual choice, or research proclivities, of each lecturer. While I admit that a lecturer should not be unduly hampered in his discretion as to how he can most effectively teach his subject, I still contend that the spirit of the Act imposes a limitation on his discretionary power in respect to demonstration experiments, and Professor Langley himself complained of the unfair responsibility which this ill-defined limitation imposes on the teacher. I am also of opinion that, reading between the lines of this clause of the Act, there is the legitimate inference that demonstrations of the "crude" or severe kind, if sanctioned, would exercise some kind of detrimental effect on students witnessing them. Whether these would be demoralising or not, as Sir Thornley Stoker and other witnesses maintained, or whether they would render students more or less callous, I am not in a position to say, but I feel bound to state that in my opinion they must be repellent to a considerable number of them, and tend to dull their sensibilities.

Langley 15205.

Even Sir Henry Morris, when giving his evidence as President of the Royal College of Surgeons, in reference to physiological experiments before advanced students stated :—

Morris 8014.

"I think, as I have said already, that many experiments ought to be permitted to those who are going to teach and practice physiology, which ought not to be permitted, or not to be shown, to ordinary general students."

I would, however, go a step further, and suggest that experimental physiology necessitating other than cold-blooded animals for demonstration should only be taught as part of post-graduate courses to fully qualified medical men who intend to pursue physiological research work, or who desire further tuition in this direction.

But apart altogether from the moral aspect of the question, I venture to think that the prominence given to experiments on animals in the education of the medical student during recent years, and the awards attaching to research scholarships, have the effect of unduly discounting the value of clinical work at the bedside, pathological anatomy and investigation, and all that makes up for the thorough equipment of the average medical practitioner. It has been contended by some witnesses, and I agree with them, that physiology could be as efficiently taught by other experimental methods, such as the employment of the Röntgen rays, and the use of the sphygmograph, spygmanometer, and other modern appliances.

Coming now to the results obtained from physiological research, and admitting that some of them have been of value, it is indisputable that vast numbers of them have been failures, or at all events have been of the most contradictory kind. Numerous instances of these conflicting results were given by Dr. Arabella Kenealy and other witnesses, and as showing the difficulties and liability to error with which the physiologist has always to contend in his endless quest, I may quote part of Professor Starling's reply to a question by Colonel Lockwood :—

Starling 3692.

"We never get the absolute truth, of course, at any time; we get on a little way, and, when we make any new discovery we probably attach to it different interpretations, some of which are true, and some of which are false, and can only be altered by subsequent investigations. Therefore you always have this movement in a spiral. You get, for instance, the humoral theory of pathology, which has been reinstated by the work of Ehrlich on to a much higher level than when it was overthrown apparently for the time being, by the work of Virchow."

For the acquisition of mere physiological knowledge, or the gratification of that valuable asset "scientific curiosity," on which Professor Starling laid so much stress, the animal world, and notably dogs, on account of their constructional nearness to man, still afford a seemingly boundless field for research. Professor Langley, for example, stated that experiments on anæsthetised and curarised dogs are still necessary to investigate the sympathetic nerve fibres which run to the lips and the gums, and that in no other animal can these investigations be so satisfactorily carried out. Yet with regard to many of these nerve experiments, such as stimulation of nerves, excision of portions of nerves, investigations into sympathetic nerves, and the reparation of nerve fibre, as regards all of which Professor Langley is a widely recognised authority in the physiological world, he admitted in reply to a question by Sir William Collins that there are two rival schools, the "Centralists" and the "Peripheralists," and though he stated that he himself was so far a centralist he "did not suppose that he could convince the other people."

Langley 15180.

15287–91.

In respect to other researches on the nervous system, I would refer to the enormous number of experiments which have been carried on to elucidate or localise brain function. These have been very fully illustrated by instances in Dr. Arabella Kenealy's evidence which show how conflicting and contradictory many of them are, and how confusing and unreliable are the conclusions drawn from them. But the experiments still go on, and dogs, monkeys, and other animals must still be used. With regard to brain surgery, for example, and diseases or injuries of the nervous system, Sir Victor Horsley stated :—

Kenealy 5288.

"Further experiments are very much required, and also for more extended study of the nervous system."

Horsley 15846.

Admitting that all these bewildering mazes of research work are of scientific interest, and afford ample scope for contention between various schools of workers, it is difficult to see of what practical benefit the results can be either to man or animal. It is not surprising, therefore, that no witnesses, as stated in the Report (Paragraph 34), attempted to place before the Commission a full record of the advance of physiological knowledge during the last thirty years, and this I contend is because the field, as regards any yield of useful results, has become so bare and barren. It is true that much stress has been laid on the discovery of adrenalin by Oliver and Schäfer, as a sort of accidental outcome in the course of their researches on the suprarenal glands. But no sooner was it discovered than numbers of animals had to be employed to determine its physiological action, and Professor Starling makes this clear in his reply to the following question by Sir William Collins :—

Q. Could not the influence of the extract of the adrenal glands have been obtained by preparing the extract, and applying it by such methods without resort to vivisection ?—*A.* No ; I only mentioned an almost insignificant action of adrenalin in saying that it raised the blood pressure. Adrenalin acts on every organ of the body, which receives a nerve supply from the sympathetic system ; it dilates the pupil, it causes contraction of the blood vessels, it dilates the stomach, it dilates the intestines, and constricts the ileocolic sphincter ; in every way it acts as stimulation of the sympathetic would act. The study of that action is of extreme importance for arriving at the nature of nerve stimulation generally and of nerve excitation.

Starling 3822.

Though adrenalin has been used by surgeons to arrest hæmorrhage, it cannot be regarded as an indispensable, or even very valuable, remedy, and certainly has not proved of any value in the treatment of the disease of the suprarenal glands, known as Addison's disease. Apart, however, from its restricted value either as a remedy or so-called cure, adrenalin has been largely used on the Continent in experiments to produce the disease known as arterio-sclerosis, one of the commonest diseases which assail people about middle life or later. According to the evidence of Professor Lorrain Smith, these experiments have been carried out by Josué, but he admitted in reply to a question by Sir William Collins as to whether this investigation had given us a means of preventing the disease, that it "had not done so yet—it is a new investigation." As regards the production of this disease and others, such as fatty degeneration, rheumatoid arthritis and osteo-arthritis, from a form of which animals themselves suffer, he made the following admission in reply to a question by myself :—

Lorrain Smith 18236.

18294.

18267–8.

Q. Would you, as a pathologist, say that you can ever induce in an animal the exact disease that you find even in a diseased animal ?—*A.* If you mean absolutely identical in every respect, no, I suppose not.

18437.

And so the interminable record of experiments mounts up on a basis, I venture to think, of little science and less logic.

Smith, S.F. 13325–35. Another outstanding result claimed as based on animal experiments is the cause of myxœdema and cretinism, and the cure referred to in the Report (Paragraph 37) by thyroid extract. But I am frankly of the opinion of Mr. F. S. Smith, M.R.C.S., that both the causation and pathology of those diseases, as well as the thyroid extract treatment, could have been evolved without experimenting on the innumerable animals which have been terribly maimed and often with very conflicting results. It was known previously that complete removal of the thyroid gland for disease in man was followed by myxœdematous symptoms, and organo-therapy even then was so far advanced that treatment by thyroid extract might readily have suggested itself as a remedy for the diseased conditions which were observed to be associated with goitre or cretinism. Sir Victor Horsley, who himself performed a large number of these experiments, in the course of his evidence gave some replies to questions put by myself as to the infliction of pain and contradictory results, which I venture to think merit quotation, but as regards which I need not make any further comment, because they speak for themselves.

Horsley 15823. *Q.* Now a few questions with regard to the functions of the thyroid gland and the diseased conditions connected with it. Of course, you yourself have performed many operations on monkeys and dogs ?—*A.* Yes.

15824. *Q.* Am I right in stating that an exceedingly large number of experiments have also been performed by others engaged in this research ?—*A.* Yes.

15825. *Q.* Am I also right in assuming that though the initial operation, the removal of the gland or part of the gland, may be carried out painlessly, the conditions induced may be often accompanied with very considerable pain ?—*A.* Oh, dear, no.

15826. *Q.* When the animal wastes ?—*A.* No.

15827. *Q.* There is no pain at all ?—*A.* No; but the animals run through the same category of symptoms as the myxœdema and cretinism patients; they become more and more stupid and idiotic; they are not sensitive to their surroundings at all.

15828. *Q.* Can it be said that after all these experiments on the thyroid and parathyroid glands the functions of these glands are perfectly understood now ?—*A.* Of course not; they will not be understood in our lifetime. It is impossible.

15829. *Q.* Has it not been pretty fully established by the experiments of Professors Swale-Vincent and Jolly that the functions of these glands appear to differ very widely in different classes of animals ?—*A.* I showed that myself a good many years ago. When you say that they differ very widely there must be no misunderstanding on this point. They differ in degree but not in kind. It is only a question of degree.

15830. *Q.* And myxœdema is believed to be due to a diseased condition of these glands, is it not ?—*A.* Yes, certainly.

15831. *Q.* Did not Professors Vincent and Jolly find that none of the monkeys from which they removed the glands showed the slightest symptoms of myxœdema ?—*A.* In Vincent and Jolly's experiments particularly, many of their animals did not show anything at all.

15832. *Q.* No signs of myxœdema ?—*A.* No. I am not sure, when I say no signs that they analysed the connective tissues. I do not think they did; but, at any rate, the animals did not present the very obvious signs.

15833. *Q.* May I ask whether that has been your experience, too ?—*A.* Yes. I have had monkeys that did not show anything at all.

15834. *Q.* Has there not all through these experiments been a great discrepancy in the results obtained by different observers ?—*A.* No.

Experiments on Animals in Relation to Medicine.

The real advance in modern medicine has, I contend, depended almost entirely on clinical diagnosis, therapeutics, and pathology, guided by a careful study of natural causes, but not upon experiments on animals, which are inherently misleading in their application to man, and therefore always more or less unreliable. Practical medicine, if it be designated a science, is a science of the most empirical kind, and whatever successful results may be claimed from such experiments, the final experiment, as Professor Starling stated, must always be on man. No one realises more fully than the thoughtful physician that he can never cure disease. By a judicious use of remedies, he can materially assist nature in rectifying impaired or perverted function, but it is nature alone that can arrest or cure disease.

For these reasons I submit that though large numbers of new drugs, as stated in the Report (Paragraph 37) have been added to the list of remedies as the outcome of experiments on animals, and others, with the same hall-mark, are continuously being foisted on the market, they almost all of them could be very well dispensed with, indeed, a good many of them have already fallen into disuse. If they have largely contributed to the increasing prevalence of the drug-habit, I venture to think that the medical profession is in some measure indirectly responsible, because it seems to me in the rush after new remedies, especially on the part of the younger medical men, there is seemingly a waning trust in the well-tried and older remedies, whose therapeutic value has been established, not by experiments on animals, but by long-continued trial and usage on man himself.

It was also stated by Professor Cushny and other witnesses, that experiments on animals were not only necessary for testing new drugs, and standardizing them, but that 4838.
they were also necessary for investigating the more precise physiological action of the older drugs, and he laid special stress on the numerous experiments which have been carried out, and are still being carried out, in respect to the action of digitalis. Yet he admitted that digitalis, as well as numerous other drugs, " act upon a number of different functions of the body," so that it is difficult to see how the composite action of all such drugs can be made usefully applicable to diseased man by merely gauging results on healthy animals. Then, again, it is very seldom that the experienced physician, in writing a prescription, prescribes a single drug; there is generally a combination of several, which would tend to obscure still further precise physiological results. I venture to think that far more useful knowledge of drugs, whether new or old, would be gained by Burford
testing their physiological action on healthy men, or students volunteering for the purpose, 8911.
as explained by Dr. Burford in his evidence, and practised by Professor Schulz in Germany, Cushny
and Professor Wiley in America. But, according to Professor Cushny, so long as drugs 5241.
continue to be manufactured, or new drugs put upon the market, so long must these experiments on animals continue, and yet to quote his own words in reply to a question :— 5252
5230.

"There are very few drugs found in any way that actually cure disease. If one excludes mercury and quinine and perhaps salicylic acid in rheumatism, I think there are very few others that one can say are actual cures for disease or specifics."

And even these were discovered without recourse to animal experimentation.

But without commenting further on this part of the subject, I may fitly quote the following replies of Sir Lauder Brunton to questions put to him by Sir Mackenzie Chalmers and the late Lord Selby as Chairman, on " The Case Against Vivisection," by Surgeon-General Sir James Thornton :—

Brunton
Q. (*Sir Mackenzie Chalmers.*) Then the next heading is Poison Tests, and Sir James says: " Poisons 7162.
act in such different ways on men and animals that deductions from experiments on the latter are fallacious and misleading. Rabbits can eat belladonna with impunity; dogs, aloes; apes, strychnin; and goats, hemlock; while birds in general are little affected by morphia. Even in human beings the effects of certain poisons are greatly modified by race. All this tends to show how vain a thing it is to expect to find out remedies for our own diseases by experiments upon animals, which are not constituted as we are, and which frequently find their food in things which would be fatal to mankind." How far do you agree with that general statement ?—*A.* A number of the instances are quite correct. The general conclusion is quite erroneous. For example, rabbits eat belladonna with impunity, just in the same way (as I mentioned before) as, if you cut the vagi in a rabbit, you find no change in the pulse rate. If you cut the vagi in a dog you see its pulse rate going up at once. If you give a dog belladonna, at once you will get the pulse rate go up too; the reason being that in a rabbit the vagi act ordinarily hardly at all, whereas in the dog they are constantly acting. When you paralyse the vagi in a rabbit with belladonna there is no difference. If you paralyse the vagi in a dog you get a great difference at once, just as you get a difference at once in a dog's pulse rate when you cut its vagi. When he says that birds in general are little affected by morphia, they are very much affected by morphia, but in a different way. For example, pigeons are not much affected apparently by morphia in the way of its causing drowsiness, but it acts upon them as an antipyretic—it lowers their temperature as much as if you put them in a very cold place—so that they are affected, though in a somewhat different way.

Q. But the point of Sir James Thornton's criticism is that these substances act so differently on animals 7163.
and man that you can draw no true inference from one to the other. What is your comment upon that ? —*A.* The same may be said of the action of the drugs upon different men. A dose of aloes, which he says may be swallowed by dogs with impunity, may be swallowed by certain men with perfect impunity; and yet we know that in the great majority of human beings aloes will act as a purgative, notwithstanding the failure to do so in those individuals. The same with opium. Sir Robert Christison used to tell us of a particular family who were not affected by opium. Sir William Hamilton, the Scottish metaphysician, would swallow an ounce of laudanum with perfect impunity—it had no action at all upon him; but it was not that he was an opium-eater or accustomed to it.

Q. It was a pure idiosyncrasy ?—*A.* Yes; and it was inherited by his eldest son. But still the majority 7164.
of people are affected by laudanum; so that what holds good in all he says in regard to animals applies in the same degree to man: that we can only tell from observation on man the effect of drugs upon the majority; that we will find exceptions just as we find them in animals.

Q. And by experiments is it possible to make the necessary allowance ?—*A.* Yes. 7165.

Brunton 7166. *Q.* (*Chairman.*) I do not quite see how that is in the instance you gave. If pigeons generally are made cold by taking morphia, and it has not that effect on humanity, does not that show that you cannot experiment on pigeons with regard to the effect of morphia as an anæsthetic or soporific ?—*A.* No, because you see I have dealt with that very question in this paper on the effect of heat and cold, upon the action of aconite on pigeons.

7167. *Q.* I am assuming that what you say about it is quite correct as a fact. But after knowing what the effect of it was on pigeons, you would not experiment on pigeons with morphia with a view to obtaining information for the treatment of man, would you ?—*A.* Yes, certainly; and for this reason, that there are differences in men. There are certain people who are called gouty who tend to pass out their nitrogenous waste in the form of urea or uric acid. Other people tend to pass out their nitrogenous waste in the form of urea, and what Dr. Cash and I were very anxious to find out was whether we could trace any relationship between the action of morphia on pigeons, which, like other birds, passed their nitrogenous waste out in the form of urea or uric acid, and gouty people; because there are certain classes of people on whom morphia or opium does not act as a satisfactory soporific; instead of causing sleep it causes excitement. We thought that very likely by experimenting on animals we should find out whether this bears any relationship to the mode of excretion of nitrogenous waste. We find, for example, that morphia will not act upon pigeons and other animals that excrete uric acid, and we thought perhaps that we might find by going to work at the bedside, that men who excrete uric acid would not be satisfactory subjects for morphin. We were not able to continue the experiments, but that was the line of them; and if they are continued on those lines I think we shall get very valuable information indeed in regard to the action of drugs on account of the very variations in their action upon different animals, which will enable us to isolate the different factors which determine the kind of action of a drug upon man, by finding out one factor or another on the different animals upon which we experiment.

These replies of Sir Lauder Brunton, I think, not only show how difficult it is to reason from animal to man in respect to the varied action of drugs on animals and man, but also show how much more difficult it must be to reason from experimenting on an animal which is placed in an altogether abnormal environment, such as the artificial lowering or raising of its temperature, or by submitting it to fasting, feeding, or other allied experiments. And this liability to error must be still further increased when the animal is submitted to the shock of a severe experiment, such as those terribly painful experiments on dogs carried out by Sir Lauder Brunton himself in his researches on the action of digitalis just before the passing of the 1876 Act, referred to by Miss Lind-af-Hageby and other witnesses, and the equally severe experiments carried out by Professor Cushny and Dr. Dixon in testing ergot, which were brought forward during Professor Cushny's examination.

Lind-af-Hageby 7379–89. Cushny 5196–210.

As an instance of the contradictory results which are the outcome of almost all these experiments on the testing of drugs, I may also refer to the experiments carried out on a large number of dogs by the Hyderabad Commission, of which Sir Lauder Brunton and Colonel Lawrie were members, to investigate the uncomplicated action of chloroform. The Chloroform Commission of 1864, according to Colonel Lawrie's evidence, concluded that death by chloroform was caused by failure of the heart; the Hyderabad Commission concluded that death resulted from arrest of the respiratory functions; while the cross-circulation experiments, already referred to, and numerous other experiments contradicted the results of the Hyderabad Commission, and confirmed those of the 1864 Commission. But the experiments in chloroform administration still go on, and the deaths from chloroform in surgical operations on the human patient still unfortunately occur, while the solution of the problem has recently become further complicated by the supposed influence of that vague factor called the "*status lymphaticus,*" which, I venture to think, must be sometimes intimately correlated with mere shock of having to face and submit to the severity of some of the operations of the "daring surgery" of modern times. It seems a strange irony of the evolution of experimental research work that this liquid substance, whose beneficent action in relieving human suffering was discovered not by experiments on animals, as stated in the Report (Paragraph 41), but by experiments which the discoverer made on himself and friends, should have led to the sacrifice of so many thousands of dogs and other animals. To mention one more instance, alcohol has been freely used by man in one form or another from the earliest ages, and though its injurious effects are tolerably well known by clinical, to say nothing of everyday, observation, its dietetic and therapeutic value is still a matter of dispute, though Sir Victor Horsley admitted that a great many experiments on animals have been carried out to solve problems connected with it which still await solution.

Lawrie 16834. 16983. Horsley 15859. 15866–7.

Experiments on Animals in Relation to Surgery.

While I feel bound to agree that experiments on animals have contributed indirectly to surgical advance, I am of opinion that the value of the results which are claimed have been greatly over-estimated. And against the weight of evidence on the experimental side, which is fully dealt with in the Report, I wish to lay special emphasis on the opinions of surgeons just as distinguished, owing to their large experience and leading positions in their own fields of work, and as competent to express authoritative views as any of the well-known witnesses who appeared before us. Among instances of dissentient opinions, entertained by other surgeons, the following may be noted :—

(*a*) It was very prominently brought forward in evidence that the late Mr. Lawson Tait, who was admittedly one of the greatest pioneers in abdominal surgery, avowed in the most emphatic manner that surgery was not in any way indebted to experiments on animals, and that, as Dr. Burford in his evidence expressed it :—

"He publicly foreswore any alliance or allegiance that he had to knowledge vivisectionally obtained." Burford 9097.

(*b*) Dr. Granville Bantock, another well-known pioneer in abdominal surgery, in reply to a question by Sir William Collins, made the following statement :—

"I think Mr. Henry Morris (now Sir Henry Morris) maintained that abdominal surgery was very much indebted to experiments on animals. I entirely deny that; abdominal surgery is the result of ovariotomy, and to that alone is due the success of abdominal surgery generally." Bantock 14774.

(*c*) A statement by Sir Frederick Treves, another very distinguished surgeon, was also referred to by several witnesses, and it is as follows :— Horsley 16131.

"Many years ago I carried out on the Continent sundry operations upon the intestines of dogs, but such are the differences between the human and canine bowel, that when I came to operate on man, I found I was so much hampered by my new experience that I had every thing to unlearn, and that my experiments had done little but unfit me to deal with the human intestine."

Although it is true that Sir Frederick Treves subsequently qualified this statement as referring more especially to one particular kind of operation, and therefore too much stress need not be laid upon it, I am of opinion that it at all events supports the view that surgical operations on the animal, instead of being an aid to the surgeon, may sometimes mislead him, and there is therefore an underlying element of risk to the patient when he begins to apply his experimentally acquired knowledge on man.

As regards surgical skill it is evident that this can be acquired by operating on the dead subject without experimentation on animals, inasmuch as Sir Henry Morris, then President of the Royal College of Surgeons, admitted in his evidence, as stated in the Report (Paragraph 42), that he himself had never performed such experiments, though he attached very great value to them, but not in respect to technique. The great advance in that direction he attributed in largest measure to the labours and researches of Lord Lister aided by those of Pasteur, in respect to the bacteriology of putrescible matter. Moreover, he and almost all the other eminent professional witnesses, with the exception of Dr. Granville Bantock, contended that what is now known as aseptic surgery is the logical outcome of antiseptic surgery which was introduced by Lord Lister before Pasteur discovered his *streptococcus*, and was practised by him and the leading surgeons all over the world for many years, and in honour of its founder was styled Listerism. While I agree that humanity owes a debt of endless gratitude to Lord Lister for his eminently successful struggle against pyæmia, surgical fever, and erysipelas, which were the terrors of surgeons before his time, I am unable to accept the contention that aseptic surgery was in any degree due to experiments on animals, without very considerable qualification.

The basis of aseptic surgery, which in essence is clean surgery, was laid, as stated in the Report (Paragraph 40), and in reply to a question by Sir William Collins, by Semmelweiss before 1850, who attributed the blood-poisoning which devastated his lying-in wards in a Viennese hospital to putrid infection, and strongly urged cleanliness as a means of preventing it. But the doctrine of cleanliness, either in lying-in or surgical wards, did not take root till Lord Lister led the way by his antiseptic technique. Previous to his time there was no special attention paid to cleanliness of hands or instruments; nurses were neither very neat nor tidy; and the surgeon generally operated in an old black coat, not always free from blood stains, which was kept generally in readiness for operations. To protect the wounds resulting from operations Lord Lister in the early days of his career, in addition to other parts of his technique, adopted solutions of carbolic acid, which at that time had only been recently introduced as a sanitary disinfectant, and these he used for hands, dressings, and instruments. As a further safeguard he introduced the carbolic acid spray, because he believed he could thus destroy micro-organisms floating in the air, which he regarded as the direct cause of wound infection. Whatever influence

Pasteur's researches had upon his mind later on, he all along directed his attention to protecting hands, instruments, dressings, and wounds from what he believed to be these aerial micro-organisms, and in developing this technique, which many of his pupils and followers tried to elevate to a cult, he unquestionably paved the way to surgical cleanliness.

But his theory that the *causa causans* of septicism in wounds rested on micro-organisms in the air was an altogether mistaken theory. The real source of all the mischief was the unclean or putrefying matter which might be conveyed by hands, dressings, or other means, to freshly made wounds, and without this matter, which serves as a medium for their growth, there would be no micro-organisms of putrefaction present. It was also a mistake to assume that a disinfecting spray could destroy the bacteria contained in the air without injuring the tissues more or less. This injury to the tissues by the use of the spray was gradually forced on the attention of Mr. Lawson Tait, Dr. Granville Bantock, and some others whose operations entailed large abdominal wounds. They, therefore, discarded the use of the spray and antiseptics, and eventually used only clean water and clean dressings, or, as Dr. Granville Bantock put it in his evidence, ordinary London tap water, not boiled or sterilised, and dressings in the same condition as they were purchased. Listerism, or antiseptic surgery, was therefore assailed, and more particularly in respect to the use of the spray, as giving rise to injurious effects on the wounded tissues, and more or less systemic disturbance in the patient. Thereupon a bitter controversy arose between the supporters of Listerism, who represented the consensus of the surgical opinion of the day, and Mr. Lawson Tait, Dr. Granville Bantock, and others, who led the revolt. Aseptic surgery, as it is now understood, was in my opinion the result of that revolt, and was not the direct outcome of any bacteriological research or experimentation on animals, as has been so persistently maintained. Antiseptic surgery was introduced by Lord Lister and was adopted by the leading surgeons of the day some time before Pasteur made his researches into the bacteriology of putrefaction in wounds, and Lord Lister's experiments on animals were chiefly confined to researches into the safest means and methods for ligaturing blood vessels, including the kinds of ligatures which should be used, and how they should be used.

With the greatest magnanimity and the rarest candour, Lord Lister, as quoted in Dr. Granville Bantock's *précis* of his evidence, made this clear in his address to the Medical Congress at Berlin in 1891.

The acknowledgment of his error is as follows :—

Bantock, 14545-6. "Dr. Bantock, whose remarkable series of successful ovariotomies may seem to justify his practice does not, I believe, prepare his ligatures antiseptically. The success achieved by Bantock and Tait without, it is said, the use of antiseptic means, proves a stumbling block to some minds." . . . . . "I can see that while the measures" (comprehended under the term cleanliness) "to which I have referred are, so far as they go, highly valuable, it must be in itself a very desirable thing to avoid the direct application to the peritoneum of strong and irritating antiseptic solutions." . . . . . "As regards the spray, I feel ashamed that I should ever have recommended it for the purpose of destroying the microbes in the air. If we watch the formation of the spray and observe how its narrow initial cone expands as it advances with fresh portions of air continually drawn into its vortex, we see that many of the microbes in it, having only just come under its influence, cannot possibly have been deprived of their vitality. Yet there was a time when I assumed that such was the case, and trusting the spray implicitly, as an atmosphere free from living organisms, omitted various precautions which I had before supposed to be essential."

Further on in the same address, as quoted in Dr. Granville Bantock's *précis*, Lord Lister stated :—

"the floating particles of the air may be disregarded in our surgical work, and if so, we may dispense with antiseptic washing and irritation, provided always that we can trust ourselves and our assistants to avoid the introduction into the wound of septic defilement from other than atmospheric sources."

The salient facts in connection with the evolution of aseptic or clean surgery and its simple technique are summarised, as they appeared to me, and do so still, in my brief examination of Dr. Granville Bantock when giving his evidence :—

15048. *Q.* (*Dr. Wilson.*) Returning to the effects of bacteriology on surgery, I do not think your contention has been made quite clear. Your contention is that bacteriological views rather led surgeons astray in the first instance?—*A.* Undoubtedly. If the bacteriologists claim that the study of bacteriology has had any influence upon surgery, then I say that influence has been a gigantic mistake in the form of Listerism.

15049. *Q.* Was the term Listerism applied to what is known as antiseptic surgery ?—*A.* The name was given to the method almost immediately after its introduction.

15050. *Q.* And am I right in defining antiseptic surgery in this way, that antiseptics, carbolic acid say, were used as a spray to prevent the wounds becoming infected by the microbes contained in the air, and that antiseptics were also used for instruments, dressings, and ligatures? Was that antiseptic surgery?—*A.* That was antiseptic surgery as we understood it when Lord Lister invented it.

Q. And it was discarded eventually, just because the use of the antiseptics which were supposed to kill Bantock the bacteria injured the wounded tissues of the body?—*A.* Undoubtedly that was the sole reason why it 15051. was discontinued.

Q. And nowadays, of course, the spray is never used, nor are antiseptics?—*A.* No. 15052.

Q. And you, Sir William Savory, Mr. Lawson Tait, and others, from your own painful experience 15053. by trying this antiseptic method, revolted against what is called Listerism?—*A.* Quite so, and I might instance the late Dr. Thomas Keith, who never improved his statistics by one iota after he had adopted the Listerian method; and the result upon himself was that after a long operation under the spray he invariably got an attack of hœmaturia, and he ultimately died from kidney disease.

Q. But Listerism, or antiseptic surgery as I should call it, was advocated most strongly at the time by 15054. leading surgeons of the day?—*A.* All over the world.

Q. I may quote this passage from your pamphlet giving an extract from an address by the late Sir 15055. William MacCormac—he was then Mr. MacCormac: "The lecturer said, 'other methods I would term inexact. The Listerian method is, in contradistinction, an exact method'"?—*A.* That is so.

Q. That is in using the spray, and so on?—*A.* Yes. 15056.

Q. "'Founded on a special theory, and carried out in all its details, in almost precisely the same 15057. manner in each case: that the access of ordinary air is very generally the cause of inflammatory or putrefactive changes in wounds,' and so forth"?—*A.* Yes.

Q. Those were the words used by Sir William MacCormac?—*A.* Yes. 15058.

Q. So that the consensus of opinion, as it is called, in those days was that the spray must be used for 15059. successful surgical operations?—*A.* Quite so.

That this view of the evolution of aseptic surgery is received with a considerable amount of what I may venture to designate as somewhat scornful impatience, is shown by the following replies to questions put by Sir Mackenzie Chalmers to Sir William Osler of Oxford University, with the *proviso* that the witness referred to is Dr. Granville Bantock:—

Q. (*Sir Mackenzie Chalmers.*) We were told by a witness the other day who has had considerable Osler, surgical experience, that Listerism is now broken down and discredited?—*A.* Where did you produce 16681. that gentleman from—Hanwell?

Q. I will not mention his hospital, but he said that the antiseptic treatment had been absolutely 16682. discarded, and that the aseptic treatment was a reversion to the old pre-Listerian days?—*A.* It is the difference between tweedledum and tweedledee. The antiseptic surgeons practised aseptic surgery, and it is aseptic surgery.

Q. They are both applications of the same principle?—*A.* Yes. 16683.

Q. There being a slight difference in the one, due to increased knowledge?—*A.* Yes, it does not make 16684. any real difference.

It may be interpolated here that Sir William Osler, formerly Professor at the Johns Hopkins University, Baltimore, expressed himself strongly in favour of the acquisition of manipulative skill by permitting students to operate on living animals, such as dogs, and treating the wounds aseptically afterwards in much the same way as human patients are now treated.

It will be inferred from my examination of Dr. Bantock that, while I desire to do the fullest honour to Lord Lister in his persistent and enormously successful efforts in protecting operation wounds, and his experiments in respect to ligatures, I am of opinion that others shared perhaps even more conspicuously than he did in the introduction of the simple technique of cleanliness, now known as aseptic surgery.

Not only has clean surgery made severe operations possible, which could never have been attempted before its general adoption without the greatest risk, but it has happily rendered the healing process as a rule rapid and complete, without the prolonged and painful suppuration which in former days generally followed severe operations even under the most hopeful conditions. Indeed, the lessened liability to suffer from wounds when animals have been operated on aseptically and are allowed to recover, has been so strongly emphasized by many expert witnesses, that it might almost appear as if the aseptic method was regarded as a kind of local analgæsic or anæsthetising agent.

In connection with the question of cleanliness in surgery, I may be permitted to allude in passing to the contributory beneficial effects resulting from clean and trained nursing, clean and better ventilated wards, and the thorough remodelling of the drainage and sanitary appliances of hospitals throughout the world which have marked the advance of sanitary science during recent years.

Infectious Diseases.

Lind-af-Hageby 9675–86

In submitting my reservations under this important heading, I feel I must crave permission to introduce here and there a note of a somewhat personal kind, based partly on close and continuous study of the whole subject, and partly on long and extended official experience, and I do so chiefly for two reasons. Firstly, because among other opinions of mine which were alluded to during the enquiry, I was referred to by one witness, and quite correctly, as one of the few medical men in this country who "uphold the uselessness of anti-toxin in the treatment of diphtheria"; and secondly, because I ought to show some warrant for maintaining views which I am well aware are in conflict with the so-called consensus of medical opinion, on which so much stress was laid all through the enquiry. That, however, is a very fallible and fickle court of appeal. It has just been shown, for example, that the consensus of surgical opinion all over the world was for some time so uncompromisingly in support of the whole ritual of antiseptic surgery, including the carbolic acid spray, that the few who dared to question were regarded as renegades to surgical advance on strictly scientific lines. Further, in the early part of last century, blood-letting or bleeding was practised as a remedy for almost every disease, and in the springtime it was freely resorted to as a preventive or prophylactic. Although the leaders of the profession in those days could not perhaps be classed as scientists in the modern acceptation of the term, I venture to think they were as fully endowed with the logic of common sense as are medical men of the present day; but just because, as in the present day, they had to face the abiding difficulty in medical practice of being able to differentiate between the *post hoc* and the *propter hoc* with any degree of certainty, whether in relation to prevention or cure, they honestly believed that this lowering system of treatment was right until evidence began to accumulate which ultimately convinced them that as a system it was altogether wrong. And yet it was during this pre-scientific period that Jenner's great discovery battled slowly, but surely, to the front, and stemmed the ravages of smallpox. The heroic system of treatment of those days, which consisted in excessive drugging, as well as bleeding, gradually gave way to a more eclectic and less drastic system, and perhaps no one contributed so much to bring about the change as Hahnemann, the founder of homœopathy, who was frankly denounced as a quack all over Europe. His system of treatment by minimal doses, however, had the effect of demonstrating that patients could recover from serious illnesses even if treated with little or no medicine, and however erroneous his famous doctrine of *similia similibus curantur* might be as applied to medical treatment generally, it has become the basic principle underlying vaccine and serum therapy. Though I am not a homœopathist, I am a strong believer in rational medicine, and am very sceptical of all theories or panaceas which emanate solely from the animal laboratory.

But even in respect to infectious diseases, there is already a serious cleavage observable in this consensus of medical opinion, for though experts generally are still agreed that the *causa causans* of every one of these diseases is a specific micro-organism, visible or invisible under the microscope, there is a steadily increasing diversity of opinion among them as to how this microbe, if it can be isolated and cultivated, should be best utilised for prevention or cure, whether in the production of a vaccine or of a serum. Up till recently the seropathists, or serum therapists, held the field; now the supporters

Martin. 11960.

of vaccine therapy are making headway, and under the leadership of Sir Almroth Wright have been casting grave doubts on the value of all serum treatment. And here I desire to point out that the term vaccine, inasmuch as it is generally employed to indicate any bacterial product, whether consisting of living or dead bacteria, or their toxins, or of both, used for inoculation, is a misnomer, because, etymologically, it is only applicable to the virus of cowpox as exemplified in the lymph which was formerly used in vaccination from arm to arm, or, nowadays, calf lymph. But vaccines are being used for the treatment of ailments other than the ordinary infectious diseases which are characterised by various bacteria, such as boils, acne, and other skin troubles, and such diseases as pneumonia and bronchitis, and even osteo-arthritis. Further, it is made a feature of this new departure in therapeutics that if the patient breeds his own bacteria, which can be isolated and cultivated, and after cultivation can be injected into his body, or, in special cases, swallowed in so many hundred millions per dose, the better chance he has of a speedy recovery, at any rate so far as assurance that his system has not been impregnated with microbes of dubious origin can inspire him with hope.

But dealing solely with the micro-organisms which are stated to be the causal agents of communicable or infectious diseases, I desire to confine my observations more particularly to those which are of vegetable origin, that is, belong to the vegetable kingdom, because it is only organisms of this class that can be cultivated or grown in suitable media outside the living body, and are employed for the production of vaccines or sera. I leave out of consideration those other micro-organisms which are protozoal, or of animal origin, because they may be classed as animal parasites, such as those of malaria, sleeping sickness, and other allied diseases. Neither preventive vaccines nor curative sera can be manufactured from these.

Reverting then to these infinitely minute organisms of vegetable origin, and their presumed *rôle* as causal agents in the propagation of infectious diseases, I may state very frankly at the outset that I feel bound to associate myself in almost complete agreement with the views of Dr. Granville Bantock on "the germ theory of disease," as set forth in his *précis* of evidence. And I can do so all the more readily because both Bantock,
he and I, as well as others, arrived at similar conclusions from a very close study of the 14545.
subject, quite independently of each other,—he, in the first instance, from the surgical side, and I all along from the public health point of view. I may, therefore, very fitly quote the following excerpt from his *précis* :—

"Bacteriologists have long since discovered that in order to convert filth or dead organic matter of any 14546.
kind into harmless constituents, Nature employs micro-organisms or microbes as her indispensable agents. Thus, in the modern septic tank, which is now so largely used in the treatment of sewage, it is the action of the micro-organisms, whether ærobic or anærobic, which dissolves the sewage, and it is the continuous action of these microbes which converts all manurial matter into the saline constituents which are essential for the nutrition of plant life. In the natural purification of filth-polluted streams, or in the conversion of dead animal or vegetable matter into the flora of the vegetable world, it is admitted that the micro-organisms play a beneficent part, and so I am prepared to contend that however these innumerable and infinitely minute vegetable organisms may be designated, they always play a more or less beneficent part when they are found to be associated with disease, and that, however characteristic any micro-organism may be of any particular form of infectious disease, it cannot be classed as pathogenic, in the sense that it is the actual agent causing the disease. The microbe in its relation to disease can only be regarded as a resultant or concomitant, and in that respect is of more or less value to the physician in assisting him to diagnose the form or kind of disease with which it is associated. But even in this respect there are abundant sources of error. For example, the bacteriologists themselves admit that there is frequently great difficulty in differentiating the tubercle bacillus from other bacilli belonging to allied groups or species, and the diphtheria or Loeffler's bacillus from the Hoffman or pseudo-bacillus. Again, the true Loeffler's bacillus is often found in healthy throats, and I may say also as a gynœcologist that it is sometimes found in the generative passages of healthy women. On the other hand, in what may be termed to be undoubted cases of diphtheria (clinically) it is very often not found after continued and careful examination, and similiar difficulties and uncertainties present themselves with regard to other so-called specific micro-organisms. Thus, the staphylococcus pyogenes and streptococcus pyogenes, which are supposed to cause suppuration, have been found by various observers in the vaginal discharges of healthy women, as also the bacillus coli communis and the bacillus typhosus. Dr. Stoker's well-known treatment of ulcerative conditions by oxygen gas incontestably proves that the staphylococcus and streptococcus pyogenes were not only necessary for healthy granulation (or healing), but that any interference with them by means of germicides retarded the healing process. In several severe operations which have been performed by myself, and in which the healing process progressed satisfactorily, without any disquieting symptoms, I have had specimens of discharge examined by competent bacteriologists, and with one exception so-called pathogenic organisms were found to be present in all of them. Is it not, therefore, reasonable to conclude that these micro-organisms, while they may be necessary for processes of healing, are certainly not causative of disease? While it cannot be denied that there must be a virus or *materies morbi* to account for the spread of every communicable disease, I contend that another strong argument against their microbial origin is the fact that in respect to several of the most infectious diseases no specific micro-organism has yet been found, in spite of the most minute and persistent research. Thus, no specific or so-called pathogenic organism has been discovered in small-pox, vaccinia, measles, scarlet fever, whooping cough, or hydrophobia, and it seems very singular that the tetanus bacillus, which is believed to be the sole cause of tetanus or lockjaw, should be found so abundantly in manured or garden soil, or on the surface of roads fouled here and there with horse-droppings. It is also worthy of note that in the disease for which microbial origin is claimed to have been most fully established, the pathogenic organism is usually associated with necrosis or death of tissue. Thus it is now generally admitted that the tubercle bacillus is not found in the sputum in the early stage of phthisis, but only when necrosis of the lung tissue sets in. The diphtheria bacillus, again, is associated with the septic or membranous throat, the typhoid bacillus with ulcerated intestine, the cholera vibrio with the inflamed intestinal canal, and the plague bacillus with the inflamed bubo or pneumonic lung. In all these instances is it not logical to conclude that these micro-organisms, instead of being pathogenic, are playing the part of nature's scavengers? On this point Dr. Vicentini says: 'Considering the assimilating activity of bacteria on the one hand, and on the other their reducing activity at the expense of histological elements in decay, I have often asked myself whether this ubiquity of the bacterial elements is not perhaps destined to perform some important service, as for instance, the expurgation of the fluids to act as scavengers in relation to decayed particles or cells of the body. The simple fact that bacteria are present in the

products or morbid seats does not imply anything specific, so long as their totally extraneous origin and their quite independent entity be satisfactorily proved. Losing sight of this truth, as simple as it is essential, one risks falling into strange exaggerations, and is likely to include the most varied complaint (even the ingrowing nail according to Regnault) in the list of bacteriological infections.' "

In further illustration of Vicentini's views quoted above by Dr. Bantock, I desire to point out how essential these micro-organisms are to the maintenance of health as scavengers of the human body, and how erroneous are some of the theories and groundless the microbial scares which are based on experimental results, when some of these organisms are found to be associated with diseased conditions. The healthy human skin, for example, teems with myriads of them, which are continuously getting rid of the decaying layers of epidermis together with the excretions of the skin glands, and Sir Almroth Wright holds their utility, and rightly so, to be of such enormous value, that he has warned people not to bathe or towel themselves too freely lest they should thwart Nature's cleansing processes, and wash or rub too many of them off. And yet there are numberless *staphylococci* and other organisms amongst them which are classed as pathogenic because they are found to be associated with boils and other inflammatory skin troubles. Further, the mouth, nostrils, throat, and air passages, extending to the minutest bronchi in the lungs, swarm with other micro-organisms, including *streptococci* and *pneumococci*, and so many pathogenic specimens have been detected in the healthiest throats that kissing even healthy children has been denounced as a " sanitary sin." Though in healthy subjects they do not thrive in the stomach or small intestine, owing to the inhibitory influence of the gastric juice and other secretions, these organisms are abundantly active in the lower intestine, and play a beneficent *rôle* in the final stage of the digestive and excretory processes in the alimentary canal. No doubt they also produce toxins, which in health give rise to no ill effects, but which under undue fæcal retention may become injurious, and it was to counteract these dangers, and thereby conduce to longevity, that Metchnikoff suggested the removal of the lower bowel altogether, or failing that, the sour milk treatment, which seems now to be rapidly going out of fashion. This prescription he based on the altogether erroneous assumption that the lactic acid bacilli, or better still, the Bulgarian bacilli of proprietary brands, would destroy, or in some way render harmless, the organisms naturally operative in that region. I have ventured to designate Metchnikoff's assumption erroneous, because it appears to me that whether the milk is sterilised or not before it is impregnated with the lactic acid bacilli, so soon as it is exposed to the action of the organisms of putrefaction, as it must be when any undigested portions reach the lower intestine, it is the lactic acid bacilli which would succumb in the so-called bacterial fight, and not the intestinal bacilli.

But apart from theories, however questionable they may appear to be, and coming to facts, this *bacillus coli communis*, which is always present in the healthy lower intestine, is found to be associated with a great many inflammatory conditions connected with the alimentary tract, such as in suppuration of the peritoneum; in the peritonitis which follows strangulation of the bowel; in appendicitis; and in suppuration in and around the bile ducts. It may also be present in lesions in other parts of the body, such as endocarditis and pleurisy, inflammation of the urinary tract, cystitis, abscess of the kidney, and in all of these it is assumed to be the causal agent, just because when cultures of it are injected into guinea-pigs they prove to be more or less virulent, even though they may not produce any one of these various specific lesions. If injected into the peritoneum of a guinea-pig, the cultures generally kill, and if subcutaneously, they may produce local abscesses, and prove fatal by inducing wasting. The bacillus is a constant inhabitant of sewage, and its presence is regarded as one of the tests for sewage-polluted water, and is regarded as affording conclusive proof of the origin of those typhoid outbreaks which sometimes follow the eating of oysters and other shell-fish, though the typhoid bacillus cannot be found. It forms one of a large group of organisms, which resemble each other so very closely that it is admittedly very difficult to distinguish them microscopically or morphologically, without having recourse to all the minute technique of culture and staining. Of this group the bacillus of typhoid fever (*bacillus typhosus*) is held to be the most specific, but strange to say, it does not produce any symptoms or lesions bearing any resemblance to typhoid in man, as stated in the Report (Paragraph 46), whether animals are fed on cultures, or are injected subcutaneously. It is assumed to be the cause of typhoid fever because it is found in the diseased parts of the intestine in fatal cases, and because its presence may be detected by certain delicate tests in the blood, and by the clumping of stock bacilli in the serum of the blood of patients suffering from the disease.

But in spite of the lack of any evidence of specificity in experiments on animals, it need hardly be said that both a vaccine and a serum have been put upon the market with the usual very doubtful results. Unfortunately, too, for the germ or microbe theory of causation, there have been discovered during the last few years persons who are called "typhoid carriers." Though they themselves are perfectly healthy, they are supposed to be a source of constant danger to those around them, because they pass bacilli in their evacuations. Most of them are known to have suffered from typhoid, and all of them are believed to have so suffered at some time, no matter how many years back. Vaccine treatment, however, does not help them to get rid of their bacilli, and it need hardly be said that serum treatment would be of as little value. It has been proved that the typhoid bacillus can thrive in filth-polluted soil, and though it has been rarely found in sewage-polluted water, there has been no fact more clearly established in sanitary investigations, than the potent agency of polluted water in the propagation and spread of the disease. There is no doubt, also, that the disease may be, and often has been, propagated by filth-polluted air, though according to bacteriologists the specific micro-organism is not found in the air of sewers, which they contend is often purer than the outside air of over-crowded streets. My experience of typhoid fever, as it occurs in rural districts, has long since convinced me that sporadic, or scattered cases, of the disease are mostly generated *de novo*, but that when so generated, they may readily become sources of infection to others if proper precautions are neglected. Typhoid fever is essentially a filth disease.

But reserving further consideration of typhoid fever till later on, it may be said generally, that many of the organisms of inflammation have a wide distribution in nature, and are always to be found on, or in, the healthy human body, or in the soil as in the case of malignant œdema. How comes it then, it may be asked, that they should be regarded as the sole causal agents of these local manifestations of disease, and many others of a similar character with which they are found to be associated? Surely there must be other factors of far greater influence at work to explain the conversion of these micro-organisms from harmlessness, or non-virulence, to virulence. Some of these factors are vaguely, but yet very appropriately, included under the classical phrase "soil and circumstance," or in other words, the loss of resisting power in consequence of exposure to debilitating influences thereby increasing the risks of infection, while on the other hand, with the maintenance of sound health under proper sanitary conditions these ubiquitous bacteria or bacilli become powerless for mischief. It is the old parable of the seed and the soil.

No doubt many of them become very virulent on injection into animals, because the defences of the animal body are broken through by the injection, and the blood and tissues of the animal thus become suitable culture media, but many, like the typhoid bacillus, are non-virulent. Yet some bacteriologists are so impressed with their essentially specific causal character, that they feel justified in preparing and using vaccines as the most rational and appropriate remedies for the numerous diseased conditions with which they are found to be associated. On what experimental basis can the bacteriologist brew a vaccine, or concoct a serum as a prophylactic or remedy for typhoid fever, seeing that he has utterly failed to produce any semblance of the disease in animals?

But it is in respect to the acute infectious diseases, that the germ, or microbial theory of disease, as well as the vaccine and serum treatment, based on that theory, appear to me to be so illogical, notwithstanding all the arguments which have been advanced as the outcome of experiments on animals. It is true that the witnesses who gave evidence in support of this contention were comparatively few, but that is of very little moment inasmuch as the whole subject, when divested of technicalities, is one which can be weighed by the common-sense reasoning powers of the layman as well as by that of the professional expert. And first, I would point out that long before Pasteur, the founder of bacteriology, discovered his chicken cholera bacillus, or the streptococcus of puerperal or surgical fever, it was believed that in all these acute infectious diseases there was present a virus, or *contagium vivum* of some kind, which constituted the infecting agent in each, by which the disease was propagated. The virus, or *materies morbi* as it was sometimes called, was believed to be something of the nature of a ferment, because it took varying periods of time, called incubation periods, to develop within the system before the characteristic symptoms of the disease began to manifest themselves. Hence they were termed zymotic diseases, and they are still classified as such in the returns of the Registrar-General. What are called the seven principal zymotic diseases in these returns are the following:—Small-pox; measles; whooping-cough; scarlet fever; diphtheria, including membranous croup; fever, including typhus, typhoid, and relapsing fever; and diarrhœa. But it is admitted, as has already been pointed out in the excerpt from Dr. Bantock's *précis*, that with the exception of diphtheria and typhoid fever, bacteriologists have failed to discover by the minutest research any organisms which they can designate causative or etiological in the other five diseases. It may be stated here that Dr. Klein, in certain researches which he conducted on behalf of the Local Government Board in connection with the well-known outbreak of scarlet fever at Hendon in 1885, believed, from extended experiments which he made on cows and other animals, that he had discovered a micro-organism in cows which proved that "the Hendon disease is a form occurring in the cow of the very disease that we call scarlatina or scarlet fever when it occurs in the human subject." Sir W. H. Power, who gave evidence on behalf of the Local Government Board, was asked some questions concerning this outbreak, but the fact remains that no specific micro-organism has yet been discovered as the causative agent of scarlet fever. With respect to whooping-cough, it may be conceded that Bordet has recently discovered a micro-organism which he believes to be the causal agent, but this lacks confirmation. It is true, however, that though no micro-organism has been discovered as the causal agent of typhus fever, a micro-organism, called a *spirillum*, or *spirochaete*, was long ago discovered in connection with relapsing fever, but both that and typhus fever have become very rare diseases in this country, and no successful vaccine or serum treatment has been devised for either. The organism of typhoid fever has already been referred to, while as regards diarrhœa, and especially infantile, or summer, diarrhœa, it is admitted to be a mixed infection, in which the various specimens of the *bacillus coli communis* group of putrefactive organisms are found to be present in the evacuations, but none of which can be designated specifically causative of the disease.

It is thus seen, therefore, that so far as what are called the seven principal zymotic diseases are concerned, in only two has a so-called causative micro-organism been discovered which can be isolated and cultivated in suitable media, such as broths, and can be made productive of prophylactic or curative vaccines or sera. It is true that bacteriologists assume, as contended by Doctor Martin and other experts, that there exists what is called an invisible or ultra-microscopic organism in small-pox, and the other infectious diseases, in which none have been discovered, but that is merely begging the question. It is just as logical to contend that in respect to the infectious diseases which are characterised by so-called specific organisms, it is an invisible or ultra-microscopic organism, and not the organism, which can be isolated and juggled with outside the living body in all sorts of media, which is the causal or infecting agent. Though it must be admitted that some of these micro-organisms, when cultivated or grown on suitable media, are found to be toxic in their substances, and produce toxins during growth, just as the organisms of putrefaction produce ptomaines or poisons occasionally in meat pies, I venture to contend that they are not the sole causal agents of any infectious disease, though they may, in their evolution under appropriate conditions of soil and circumstance, become contributory factors. When cultivated outside the living body, the toxins which they produce must be modified in varying degrees by the nature or composition of the media in which or on which they are grown, and must therefore differ from the toxins which are produced, when they form part of the actual *materies morbi* or infective material of the disease as it naturally occurs in the living body. The bacteriologist cannot manufacture or originate *de novo* an infectious disease. Pasteur believed he could eradicate the pest of rabbits in Australia by originating a microbial infectious disease among them, if he had been able to travel so far. Danysz, a well-known and distinguished French bacteriologist, was commissioned a few years ago to visit Australia, and had ample opportunity to make experiments, but failed. It is true, the bacteriologist can manufacture a rat poison, and there are several of these bacterial poisons on the market, but however efficacious they may be in killing the rats by poisoning what they are tempted to eat as food, they have all failed to produce a genuine infectious or epizootic disease which will ensure the annihilation of these rodents.

But though bacteriologists must, as I venture to contend, always fail in their attempts to originate an infectious disease among animals, which will spread by the ordinary channels of infection, they strenuously maintain that they have devised means and methods of prevention and cure which both by Pasteur and Koch, the founders of bacteriological treatment, were admittedly based on Jenner's great discovery of vaccination as a protection against small-pox. And it is here that I desire to intrude more particularly the personal note to which I previously referred. It is because I am a firm believer in vaccination that I feel bound to question the results of these new developments in medicine, namely, vaccine and serum therapy. I may also be permitted to state that my opinion is entirely unbiassed in any narrow professional sense, because though I have vaccinated hundreds of patients during the early part of my career, I have never received a single fee, and further, it is supported by a long and intimate experience in various outbreaks of small-pox. When a student in Edinburgh, close on fifty years ago, I was successfully re-vaccinated on volunteering to become a clinical clerk in the small-pox wards of the old Infirmary at a time when the disease was very prevalent in the city, and though I have been re-vaccinated on more than one occasion since, the operation was never again successful, because I had been made practically immune. Further, as a Medical Officer of Health of a large combined district, embracing several urban and rural sanitary districts, ever since 1873 till my resignation on account of failing health, two years ago, I have had most intimate experience of the disease whenever it was prevalent in the country. But whether examining patients or advising as to isolation and other precautionary measures, I never had the slightest uneasiness, because like other medical men or nurses who had been re-vaccinated, I believed myself to be immune. My experience also satisfied me in every outbreak that prompt vaccination and re-vaccination of contacts and others, together with other appropriate measures, were far more important aids in stamping out the disease than removal of sufferers to hospital; indeed, I firmly believe that if vaccination and re-vaccination were as strictly enforced as they are in Germany, small-pox hospitals, which are now plentifully supplied all over the country, and for years remain vacant, might be made permanently available as sanatoria for the treatment of cases of tuberculosis, because if an outbreak of small-pox happened to occur, consumptive patients could be promptly discharged without risk. But as vaccination without successful re-vaccination only confers immunity of limited duration, the difficulties of stamping out an outbreak are always greatly increased by the occurrence of slight or modified cases, which crop up during every outbreak and infect others before they are detected. And these slight cases are almost invariably cases which show marks of vaccination in early childhood, so that, in a measure, vaccination without re-vaccination always tends to contribute to the spread of the disease whenever an outbreak occurs.

All this, I admit, is a digression which, nevertheless, I hope will be excused. But I also desire to explain that as a writer on public health questions ever since 1872, when the Public Health Act of that year was passed, I have made it a prominent part of my life work not only to endeavour to study very closely the literature of new developments in this connection, but, as further evidence of warrant for the views which I hold, I may add that I have summarised from time to time all the most important reports on sanitary investigations conducted by the Medical Department of the Local Government Board, as well as the numerous experimental and bacteriological researches carried out by Dr. Klein, and other outside workers under the authority of the Board. The sanitary reports of the Medical Inspectors of the Board, as well as the classical reports of the late Sir John Simon himself, under whose guidance as Medical Officer the staff was first instituted and worked, have been of enormous national value, but the results of experiments on animals and of bacteriological investigations carried out by Dr. Klein and his co-workers in assisting preventive medicine or sanitary advance, have, in my opinion, been somewhat disappointing from a public health point of view, although Sir John Simon, and many others, like myself, expected so much from them. Yet these researches, in spite of errors and failures, have all along inspired the Medical Department with the largest hope, so much so that Sir W. H. Power, when giving his evidence as Medical Officer of the Board, stated in respect to infectious diseases generally :—

"I should like to get some prophylactic material, of course, for all these diseases." Power, 4309.

and, further on :—

"We have recommended the diphtheria anti-toxin, and provided the diphtheria anti-toxin, we have made 4311.
some cholera preventives, Haffkine's method ; and again we have done the same with regard to plague."

In reply to another question, he stated :—

"Artificial immunisation is, as it were, in its infancy. What one looks to in the future is to find some 4383.
means of protecting the human body by the inoculation of materials such as serum or what not, and the essential thing will be, of course, to secure something which is a lasting protection in that way. At the present time a good many of these preventives are perhaps a little transient as regards immunity conferred by them. The hope of the future is that we shall get something which will not only protect for the present, but will protect almost indefinitely."

Power 4585. One of the few questions which I myself put to Sir W. H. Power was whether the Local Government Board would depute Dr. Klein, as their chief bacteriologist ever since the Board was created, to appear as a witness before the Commission, and on receiving a confirmatory reply, I did not deem it necessary to put any very special questions bearing on Dr. Klein's researches. Unfortunately, in consequence of illness, Dr. Klein was unable to appear before the Commission, and there was therefore no special opportunity afforded of being able to enquire into the results of the many painful experiments on animals following inoculation previously quoted in respect to diphtheria, or the Hendon outbreak, already referred to, and others which in my opinion cannot be said to have been attended with definitely successful results. During all these years of continuous investigation and research work carried out under grants to the Local Government Board, there have been no epoch-making bacteriological discoveries made by the experts employed, though there has evidently been no lack of official
4623. opportunity and encouragement, from Sir John Simon's time down to the present day.

If in these observations I appear to be commenting on the work of Dr. Klein and his coadjutors somewhat too severely, it is because I feel strongly that under the ægis of the Local Government Board, practical preventive medicine on rational lines is being driven into dubious paths by the investigations carried out in the animal laboratory, which are essentially liable to error and misinterpretation.

I admit frankly that much useful experimental work has been accomplished in many directions, but I cannot help concluding that any new bacteriological discoveries or methods of prophylaxis or cure based on bacteriological research which receive the hall-mark of the Board's approval, are in some degree stamped with a measure of infallibility which Medical Officers of Health, and the profession generally, feel bound to accept, or hardly dare to question. In order, however, to prove that there is ample justification for these comments, and also to show how this departmental research work was first instituted and carried on as "piece work," as well as to afford some estimate of the measure of success or failure which has attended it, I will now submit extracts from the evidence given by Sir W. H. Power in reply to questions put to him by Sir William Collins, and I hope the importance and trend of the examination will be regarded as full warranty for the length of the excerpt, which after all does not include all the questions put by Sir William Collins to the witness :—

4405 *Q.* (*Sir W. Collins.*) I think you referred to Sir John Simon as being the first to advise a Government Health Department to utilise vivisection experiments for purposes of scientific investigation ?—*A.* Yes; it commenced under his reign.

4406. *Q.* Could you tell us in what year ?—*A.* I think it was about 1864 when he first mentioned it. I have a note here which perhaps might be of assistance in this matter. I see that Sir John Simon, in his "English Sanitary Institutions," says : "Soon afterwards, *i.e.*, after 1869, the departmental organisation was strengthened in an important outwork, the first beginnings of which, five years previously, *i.e.*, 1865, had been noticed in my eighth annual report."

4407. *Q.* That would have been when he was Medical Officer to the Privy Council ?—*A.* Yes.

4408. *Q.* And then from 1865 until the present time the Privy Council or the Local Government Board has had resort to these vivisection experiments for the purpose of scientific investigation ?—*A.* Yes, systematically, since 1870, when there was a special grant from the Treasury for the purpose of scientific investigation.

4409. *Q.* Could you give me an idea as to the amount of those grants ?—*A.* Yes. Formerly, when it was first granted, I think under Mr. W. E. Forster's dispensation, in 1870 or 1871, the sum was £2,000; and it remained at £2,000 for a good many years, until, I think, soon after Sir John Simon retired, when it became £1,900, and £1,900 it has remained ever since.

4410. *Q.* Then for the last thirty-six years we may take it that it has averaged £2,000 or £1,900 a year ?—*A.* Yes.

4411. *Q.* Did I correctly understand you to say that at one time the Local Government Board had laboratories of their own ?—*A.* They hired a laboratory, and paid directly for the use of the laboratory; it was the St. Bartholomew's Hospital Laboratory. They paid some couple of hundred pounds a year. I think they paid for the laboratory, and attendants, and reagents, and things of that kind; but in more recent years that has been discontinued, and now each worker makes his own arrangements as to the use of a laboratory—of course it has to be a licensed place—and his remuneration takes account of the expenses that he is put to in that way.

4412. *Q.* Do the Board ever employ a whole time bacteriologist ?—*A.* No, they have not done so.

4413. *Q.* Would you give us the names of the scientific authorities who have been employed from as far back as you can remember until the present time ?—*A.* Dr. Andrewes, Dr. Blaxall, Dr. Theodore Cash, Dr. Edmund Cautley (I am giving them alphabetically), Dr. Cory, Dr. Creighton, Mr. Dowdeswell, Dr. Dupré, Dr. Heneage Gibbs, Dr. Mervyn Gordon, Dr. W. S. Greenfield, Dr. Grünbaum, Dr. Haldane, Dr. Hamer, Dr.

V. D. Harris, Mr. (now Sir) Victor Horsley, Dr. A. C. Houston, Dr. A. A. Kanthack, Dr. E. Klein, Mr. Parry Laws, Mr. Alfred Lingard, Dr. MacFadyen, Dr. Sidney Martin, Dr. Burdon Sanderson, Dr. W. R. Smith, Dr. Thudichum, Dr. John Wade, and Dr. Wooldridge.

Q. And would you be so good as to give me the names of those who hold letters of engagement now ?—*A.* Dr. Klein, Dr. Gordon, Dr. Grünbaum, Dr. Sidney Martin, Dr. Horder, Dr. Savage, and Dr. F. W. Andrewes. I think that is all. Power, 4414.

Q. Do you say that Dr. Klein is still employed ?—*A.* He is still employed. 4415.

Q. You told us that you issued a special note to each experimenter that no painful experiments were to be performed without anæsthetics ?—*A.* Yes. 4416.

Q. And that they were to report such to you ?—*A.* Quite so. 4417.

Q. Has your attention been called to the evidence given before the former Royal Commission by Dr. Klein ?—*A.* Of course I, in a sense, know it well. I remember hearing of it. I was a junior at the time. 4418.

Q. On page 183 I see the question was put to him : " Then for your own purposes you disregard entirely the question of the suffering of the animal in performing a painful experiment ? " And his answer was : " I do." Did that evidence have any effect upon you as regards this note that you issue ?—*A.* I think very possibly it had a good deal to do with the origination of Mr. Forster's wishing that each one of the investigators should be specially cautioned against engaging in painful experiments if they could possibly be avoided. I have already quoted a memorandum made upon the subject by Mr. W. E. Forster to Sir John Simon. 4419.

Q. What was the date of Mr. Forster's memorandum ?—*A.* February 17th, 1874. 4420.

Q. The evidence of Dr. Klein to which I have called your attention was given on October 28th, 1875, according to this Blue Book ?—*A.* The date of the minute by Mr. Forster is February 17th, 1874, and I suppose that thereafter Sir John Simon began to give the special caution that is now given. When exactly he began it I could not say. 4421.

Q. The evidence of Dr. Klein could hardly have been the occasion of the minute by Mr. Forster ?—*A.* No, it looks as if it were not so ; I agree. 4422.

Q. In his evidence before the former Commission Sir John Simon stated that it was his aim to obtain exact scientific knowledge of the causes and mode of acquiring of any disease which is in question. I suppose that is still the aim ?—*A.* That is still the aim. 4423.

Q. And in your own *précis* I see that you state that your object is to secure immediate administrative advantage ?—*A.* Quite so ; one of our objects. 4424.

Q. What would you regard as the most typical or most successful example of these researches culminating in exact scientific knowledge or in securing immediate administrative advantage ?—*A.* It is hard to point to anything that has given immediate advantage. Some instances of recent work in scientific research, say within the last ten or twelve years, leading to important results in regard to disease prevention or public health administration, are with regard to intestinal micro-organisms, the lead-poisoning abilities of water supply, bacteriology of scarlet fever and diphtheria, and the preparation of Haffkine's and other prophylactics. For instance, in regard to presence and abundance of particular intestinal organisms, say in drinking water, a guide has been afforded to administrative authorities and their expert officers with regard to the uses and limitations of bacteriology in these matters. A series of reports has been made available when a question arises as to the bacteriological methods to be adopted or the interpretation of bacteriological results. There can be no doubt that these reports have been and are being used and studied, to the advantage of good public health administration. 4425.

Q. Is that what you would cite as an example of immediate administrative advantage ?—*A.* I am speaking in a general way. An immediate administrative advantage, of course, is the detection of imported infectious epidemic disease which may spread over the country, as, for instance, plague, or cholera, or cerebro-spinal fever, and matters of that kind. 4426.

Q. Are you satisfied that you have the means of certainly identifying the presence of cholera by micro-organisms ?—*A.* Yes, I think that the Koch's comma is generally accepted—at any rate, that is the gauge by which it is universally tested throughout this country and throughout Europe. I should like to say that if it is wished that I should give a definite statement of how one thing has led to another for administrative benefit, I could say a good deal as to the question of disinfection on board ship. It would take up rather the time of the Commission, but we have gone on there from one point to another, attaining in the end definitely useful results. 4427.

Q. (*Sir William ollins.*) I think you told me that the Local Government Board were satisfied that the diagnosis of cholera could be made upon the finding of the Koch's comma bacillus ?—*A.* Yes. 4428.

Q. Did Dr. Klein dispute that position as to the diagnostic value of the Koch's comma bacillus ?—*A.* He entertained doubts at one time, more especially after his visit to India, I think, as to whether the Koch's comma bacillus was *per se* the one agent ; but afterwards, on further investigation, he fully accepted, I think, Koch's dictum that it was the essential cause of cholera. 4429.

Q. Did he not state that he had partaken of it with impunity ?—*A.* Yes, but I do not know that that proves very much one way or the other. 4430.

Q. That was during his sceptical period I suppose. In your *précis* you allude to the serum used against plague and enteric fever, and you say that neither has proved altogether satisfactory ?—*A.* That is so. 4431.

Q. Would you kindly amplify that ? In what way have they proved unsatisfactory ?—*A.* Allegations have been made that the sera of both enteric fever and plague are not so completely protective as is to be 4432.

desired; that notwithstanding that people are injected with the prophylactic certain of them nevertheless take enteric fever or plague, as it may be, and die, and that it is therefore desirable, if something which is more fully protective or more lastingly protective could be discovered, that it should be put at the disposal of persons exposed to the one or the other disease.

Power, 4433. *Q*. Does the Local Government Board recommend Wright's anti-typhoid serum?—*A*. No, we have never used it. You will remember that its value was disputed among the military authorities in South Africa, and they appointed a committee I think of the Army Medical Board now sitting with a view to elaborate a more satisfactory serum; and since they have undertaken that we have not of course touched the matter.

4434. *Q*. Does the Local Government Board discountenance the use of Wright's anti-typhoid serum?—*A*. No. We do not supply it, and we have not given the public authorities any special facilities about it. They are quite at liberty to use it, but there would be a question of course as to payment for the use of it. That is what they would have to come to the Local Government Board for.

4435. *Q*. Then besides these sera not proving protective against attack or death have they been unsatisfactory in regard to untoward results?—*A*. Not in this country, certainly. I will not be very sure as to other countries. There was some tetanus, of course, in relation to the plague prophylactic in India, and I think there was something similar in the United States. I am not sure that it was not in relation to the diphtheria anti-toxin. But the results as a rule have not been unsatisfactory. There has been no serious damage or death, so far as I know, resulting from a very free use indeed of the prophylactics. But I am speaking rather of our own experience in this country. None of these sera except the diphtheria anti-toxin have been very largely used here, and the diphtheria anti-toxin has not been so largely used as perhaps it might have been.

4436. *Q*. Were the tetanus cases in India to which you refer in connection with the plague serum fatal?—*A*. I think everyone of them—some eighteen or nineteen. It was a disputed point as to when it was that the tetanus bacillus got into the particular phial of the prophylactic; that I think has been the subject of inquiry by the Indian Government.

4437. *Q*. Do I correctly understand that the Local Government Board recommend the use of sera in the case of cholera, plague, typhoid and diphtheria?—*A*. I do not think they go so far as to recommend, but they are willing, as I was saying, to place some of these sera at the disposal of the authorities to a limited extent, to deal that is with the first beginnings of epidemics; and they facilitate the purchase and use of them by allowing the expenditure. We have had very little to do with the anti-typhoid prophylactic, and I will not be quite sure that we have ever had an application to the Board to sanction expenditure for that purpose; we certainly have not pressed it officially.

4438. *Q*. Are these sera prepared under the authority of the Board?—*A*. The sera that we issue are.

4439. *Q*. Do you sell them?—*A*. No, we give them away; we put them at the disposal of local authorities therefore it is only to a limited extent that we issue them. Chiefly it has been of course as regards diphtheria and plague to these authorities. The diphtheria anti-toxin the Board put more largely at the disposal of the country generally; but there was no very large demand for it, and the Board no longer make the diphtheria anti-toxin with the view of putting it at the disposal of the authorities.

4440. *Q*. Does the Board guarantee the sera that it authorises as being free from possible harmful results?—*A*. They give no absolute guarantee, but they take every precaution so far as they can that the material shall be quite innocent, except for the purpose for which it is intended.

4441. *Q*. You spoke of there being considerable misgiving with regard to Haffkine's serum?—*A*. There was. It contained an addition of 0·5 per cent. of carbolic acid to which some people objected, I believe simply because carbolic acid poisoning has been occasioned by it; and one of our objects when we began to make the plague prophylactic was to do if we could without carbolic acid.

4442. *Q*. And the new remedy which the Board are thinking of putting on the market against plague I understand was to be prepared from the dead tissue of a plague patient?—*A*. Not to put it on the market, nor from the plague patient; the dead tissues of the plague-rodent are in question, and this material will be made available for local authorities in the same way as the others for those who desire to use it; they would not be charged anything for it.

4443. *Q*. Has it not been used on human beings?—*A*. No, not in this country.

4444. *Q*. Only on the rat?—*A*. The rat and the monkey.

4445. *Q*. Are you able to say for how long the prophylactic influence lasted on the rat or the monkey?—*A*. That has to be a matter of observation, of course. In the case of the rat I believe it lasted a number of weeks. In the monkey it has not yet been tested for so long.

4446. *Q*. Are you prepared now to recommend it for the human being?—*A*. I am not very sure about it, I should have to consider it, and I myself should like some further experiments made first.

4447. *Q*. Have you found at all that what may prove to be a success as regards prophylactic in the laboratory has failed when it has been made a matter of application in the ordinary use?—*A*. I suspect that the degree of success in protection that is obtained in the laboratory is not to be confidently anticipated when the prophylactic is applied to the human being. I am afraid it might be a good deal less.

4448. *Q*. Would you state on what grounds you make that differentiation between the state of affairs in the laboratory and that in general use?—*A*. Because in the laboratory you can contrive your conditions and make them tolerably exact and repeat and vary them from time to time, but unfortunately when you come to nature and the human being the conditions are very different, and you do not always have the conditions which suit the material that you are using.

Q. Does the Local Government Board recommend the use of the diphtheria anti-toxin as a prophylactic ?—*A.* Yes, and we made it on a considerable scale a few years ago and put it at the disposal of the authorities. Power, 4449.

Q. Is it used ?—*A.* No, the demand for it entirely fell off, and naturally the Treasury did not see why we should go on making it for no particular use, and it was discontinued. We had a special grant from the Treasury for part of the cost for making it. 4450.

Q. Have you considered the question of advising the Board to seek power to compel its use ?—*A.* No, we have not gone as far as that. 4451.

Q. I think you stated that there are certain diseases which can be identified without recourse to experiment on living animals. Which are those diseases which come within your purview ?—I said rather that I was hoping that we should be able, by differentiation according to culture behaviour, to identify diseases without resort to the animal body. I was thinking of diphtheria and of cerebro-spinal fever—the meningo-coccus. 4452.

Q. You mean by finding a definite specific organism ?—*A.* By finding a definite organism which will react always in a certain way to a certain series of culture tests. It is morphologically tested and as to the temperature at which it grows and that at which it dies; and in that way, if one's hopes were fulfilled, one would be able to do away with a great many animal experiments. As matters stand, a great many authorities cannot resort to the Department, or a good deal of delay is liable to arise; it would require a great many more licences if the practice were generally adopted to test every case that arose in animal bodies. We have no authority to formulate a standard test, apart from the animal test, if we discovered one; but if we found what seemed to be a suitable and trustworthy cultural test, we should make it known and indirectly recommend it for adoption by local authorities, no doubt. 4453.

Q. Let us see in which of these diseases we have been able, in your opinion, to establish such a test ?—*A.* I cannot say that we have got to that stage with any one of them. I am hopeful that we are rather near a satisfactory cultural series of tests with the meningo-coccus. There are morphological and cultural tests for the diphtheria bacillus, but there are so many kinds of diphtheroid bacilli that cultural tests of them, though all very well, perhaps, as regards positive results, do not, I think, as regards negative results, lead to definite conclusion without a test on the animal body. 4454.

Q. What attitude has your Board taken up with regard to tuberculosis, both as regards diagnosis and as regards prophylactic, or the treatment by serum ?—*A.* The Board is waiting for the Report of the Royal Commission on Tuberculosis, I think. Whether in view of the Report which has been recently issued, and of further Report to follow, the Board will see their way to any legislation or not I do not know. 4455.

Q. Have you had an opportunity of seeing the Report which has just been issued ?—*A.* Yes, I have glanced through the Report that has been recently issued; but the Board, of course, as a Board, have not considered it—the officials of the Board have not considered it. 4456.

Q. Do you accept the position that Koch has taken up, as regards the bacillus, as the cause of the tubercle ?—*A.* Yes; I believe that the bacillus of tubercle is the cause of tuberculosis. 4457.

Q. Were there not investigations under your Board, or under your predecessor, by Sir John Burdon Sanderson, indicating that tubercle was not a specific disease, but could be built up from a common septic ferment ?—*A.* I do not remember it of tubercle. There are some of the earlier lectures of Sir John Burdon Sanderson which are capable of being read in more than one way. I do not remember it in that connection. He gave a lot of evidence on the building up of increased infectivity of septic matter in passing from animal to animal, but I do not remember that he mentioned tubercle, as it were, in that way, or that he professed to do so. 4458.

Q. Do you remember his reporting that in rodent animals the tubercular process may originate not only by inoculation of tubercle but by any irritation of requisite intensity applied to the subcutaneous tissue ?—*A.* I think, in the days of Villemin and Wilson Fox, it was affirmed that it might be done by any irritant as, for instance, indiarubber; but in those days they had not got the check since supplied by the discovery in the tissues of the tuberculosis bacillus, and therefore to that extent those experiments would not go for much now. 4459.

Q. But was it not stated at the time—1868—that the truth of this inference has been completely established by the experiments of Dr. Wilson Fox, Dr Kole, and others ?—*A.* As to the transmission of tubercular matter setting up of tuberculosis, *i.e.* by inoculating caseous tubercle, that was established by the experiments; but that anything else than tuberculous matter would do it, I do not think Dr. Wilson Fox said that. I thought he was rather the other way. 4460.

Q. The statement I read to you was in reference to the allegation that tuberculous inoculation could no longer be regarded as dependent upon inoculated material having been taken from a tubercular individual ? *A.* —Is that from Sir John Simon ? 4461.

Q. These are Sir John Burdon Sanderson's and Dr. Wilson Fox's experiments ?—*A.* I expect Sir John Burdon Sanderson modified his view very much before his death. I do not think he would have said that in his later days. 4462.

Q. Probably you do not remember, then, that Sir John Simon, in his preliminary report, called attention to the value of those experiments ?—*A.* No, I do not; that is rather before my day; it is thirty years ago or more. 4463.

Q. But an established fact is an established fact, I suppose, whether it is thirty years ago or later ?—*A.* What I mean is that my acquaintance with this is necessarily less perfect than with recent affairs. 4464.

Q. Has your Department, the Local Government Board, come to any decision as to whether tuberculin is a cure for consumption ?—*A.* No, they have not entered into the question. 4465.

Power, 4466. *Q.* Have they not investigated the question?—*A.* Yes, they have investigated the question in reference to the effect of tuberculin on animals, but they have not considered the question of its application as a cure for tuberculosis in a human being.

4467. *Q.* Do you mean that among the many scientific investigations instituted by the Local Government Board they have not thought it worth while to consider the question of the cure for consumption as advocated by Koch?—*A.* I believe they have had some experiments made on the lower animals, but I do not know that the result of those has encouraged them to recommend its use in the human being.

4468. *Q.* Have you come across any evidence which would confirm the statement of Koch in his book called "The Cure of Consumption," where he states that "phthisis in its early stages has been cured with certainty by this remedy"?—*A.* The use of the tuberculin cure for phthisis throughout Europe fell, after first advocacy of it, into desuetude for a good many years, but I believe there are some physicians now in this country who are rather disposed to recommend the use of tuberculin, or some matter rather closely allied to it, as not without advantage in early cases of phthisis; but I know nothing beyond what I have seen written on the subject.

4469. *Q.* The words that I put to you from Koch are: "Phthisis in its earlier stages has been cured with certainty by this remedy"?—*A.* I think it was anticipated at first that not only the early disease, but almost any stage of it might be curable by tuberculin. I remember that at the time when the cure was first promulgated people flocked to Berlin in enormous numbers to undergo the tuberculin cure, and with very unhappy results in a good many cases; they were a good deal worse for the treatment.

4470. *Q.* My point is whether that statement is to be held to be true now that "phthisis in its early stages has been cured with certainty by this remedy"?—*A.* I do not know. We have not to do with the cure of it so much as the prevention of it, and I cannot say that I have considered the question of cure with the view of advising the Board.

4471. *Q.* Does the Local Government Board recommend it with a view to prevention?—*A.* The Board has not done so.

4472. *Q.* Has it been found by the Royal Commission which has just reported that the tuberculin test for consumption in monkeys is unreliable?—*A.* I had not noticed that, but I believe that they consider it is of extreme use with the bovine animal.

4473. *Q.* Has the Local Government Board investigated many diseases of the bovine animal?—*A.* Not in recent years. In Sir John Simon's day they were doing a good deal, and in Sir George Buchanan's day, but I do not remember anything being done with the bovine animal in later years since there has been a Board of Agriculture established.

4474. *Q.* Was it suggested by the researches carried out under your Board that a cow could suffer from diphtheria and scarlet fever?—*A.* Yes, it was.

4475. *Q.* In the shape of a specific form of eruption?—*A.* In the shape that the particular malady from which the animal suffered, which was supposed to be analogous to diphtheria was accompanied by udder eruption in many cases.

4476. *Q.* There was a good deal of investigation of udder eruptions at one time, was there not?—*A.* Yes, a great deal.

The further questions put by Sir William Collins then proceed to the elucidation of the practice of vaccination and the kind of lymph used from Jenner's time up to the present day. If there is considerable doubt entertained by the Medical Department of the Local Government Board, as appears from the above excerpt, in respect to the preventive value of Haffkine's prophylactic, diphtheria anti-toxin, and Wright's anti-typhoid preparation, all of which require the so-called specific bacteria of the respective diseases as the essential factors in their production, there never has been the slightest doubt, either on the part of the Board or the medical profession as a body, concerning the value of vaccination as a protection against small-pox in which no specific micro-organism has been discovered. But in order to make my contention as clear as possible that there is no real analogy between these newer methods of prophylaxis or cure and Jenner's discovery, on which, as already stated, they are admittedly based, it becomes necessary to refer very briefly to the doctrine of immunity as illustrated by small-pox and vaccination.

It has all along been well known that there are certain infectious diseases one attack of which, as a rule, protects against any future attack, and this can be more emphatically said of small-pox than of any other disease;—in other words, immunity, more or less complete, follows a first attack; and as regards small-pox, it is practically complete during the remaining period of life. Though numerous theories have been advanced to explain how this immunity is conferred, and a whole vocabulary of terms has been introduced by bacteriologists to explain them, they none of them afford a satisfactory solution. Pasteur, for example, from his restricted outlook as a chemist, and not as a trained medical man, assumed that the specific micro-organisms of the disease, after the manner of organisms in his famous fermentation experiments, removed some substance from the blood and tissues during a first attack which is necessary for their growth, thereby rendering further attack impossible. That was called "the exhaustion of pabulum" theory. Another theory assumed that during a first attack the specific micro-organisms developed some substance in the blood and tissues which is inhibitory or protective. That was called the "antidote theory." A third, which was named the "acclimatisation theory," assumes that, in some way or other, the blood and tissues of the body are so modified by a first attack as to be able to resist future attacks; and so on, to Ehrlich's cabalistic side-chain theory in respect to the action of anti-toxin sera. But none of these theories are very illuminating, and especially in respect to small-pox. All that need be stated is that prior to Jenner's time, inoculation, which had been introduced from the East by Lady Mary Montague, was practised to some considerable extent, and was successful in conferring immunity on the person inoculated. The virus was taken from the pustules of patients suffering from mild attacks of the disease, because it was believed that inoculation would also be followed by a mild attack. But though this was the usual sequence, inoculation, even from a mild case, produced all the characteristic symptoms and features of the disease, including infectivity, and unless the patient was isolated, inoculation, assisted rather than otherwise, the actual spread of small-pox. Previous to the introduction of vaccination, the disease spared neither age, sex, nor rank in life; every fifth person attacked died, and many of those who survived were hideously disfigured or "pockmarked" for the rest of their lives, and it was not till some time after Jenner's discovery began to be widely practised that the gradual diminution in the rate of mortality became apparent.

In studying the subject, the line of reasoning pursued by Jenner, according to generally accepted historical accounts, was briefly this:—He knew that it was common talk in the neighbourhood in which he practised, and confirmed it by close inquiry, that milkers who suffered from a slight attack of illness accompanied by sores on their hands, contracted from the vesicles or small ulcers on the teats of cows infected with cow-pox, were somehow protected against small-pox, and did not "catch" the disease. He therefore concluded that if he inoculated such persons with small-pox virus they would prove to be immune. He tried the experiment over and over again, and after assuring himself that he was correct in his inference, he further concluded that he could confer immunity against small-pox by inoculating healthy persons with the matter contained in the vesicles of cow-pox, and in this too he succeeded in case after case. His next inference was that by using the matter from the vesicle produced by vaccination with cow-pox matter he could also confer immunity against small-pox. This also he proved to his satisfaction by repeated experiments, and then the final stage was reached by vaccinating from person to person, that is, by arm to arm vaccination, in continuous series, with the matter which had been originally obtained from the vesicles of cow-pox. He thus made it clear that vaccinia, as it came to be called, or cow-pox in man, was a modified form of small-pox, because it conferred immunity against that disease. Jenner believed that, like inoculation with small-pox virus, it would confer a lasting immunity, but in this he was mistaken. It only confers a modified degree of immunity, because vaccinia is only a modified form of small-pox by transmission through the cow, or calf.

In respect to this specific connection between cow-pox and small-pox, I admit that there are some who contend with very great force that cow-pox, which has become an almost extinct disease, bears no relation to small-pox; that, in short, vaccinia is not a modified form of small-pox. But against this view there is the important fact that calves can be successfully vaccinated with ordinary cow-pox, or arm-to-arm, lymph, and further, that ordinary calf-lymph can be evolved from small-pox virus. For it has been proved experimentally by Copeman, Simpson, and others that by passing small-pox virus through a series of calves, lymph can be ultimately obtained which is indistinguishable in its effects from the old cow-pox strain, as in arm-to-arm vaccination, and can be successfully used in vaccinating human beings. Indeed, Sir W. H. Power admitted in his evidence that the strain of lymph used by the Local Government Board, which has been Power,
renewed again and again from the Continent, is probably a good deal of it variolous in its 4478.
origin, but it is always most carefully passed through a series of calves, and tested in every way before it is allowed to pass into circulation.

But whether cow-pox or vaccinia is of more or less varioloid origin or not, vaccine lymph must be taken from the vesicles at a certain stage of development. As it contains no "visible" specific microbe, it can only be evolved or cultivated in the living body. When vaccination is successful, it is followed by a definite disease of a mild type, known as vaccinia, with a well-defined incubation period, and well-defined and characteristic symptoms, a varying amount of slight fever, swelling and tenderness at the point of vaccination—the vesicular stage, and then the pustular stage. In successful vaccination the lymph, or specific matter, always breeds true. Vaccine or serum therapy bears no real analogy to this :—In respect to both, as already pointed out, there must be a so-called specific organism, which can be isolated and cultivated on suitable media outside the living body, if prophylaxis or prevention is aimed at, and it is, as has been admitted in the evidence of Sir W. H. Power, of a very doubtful kind, and of slight duration. Further, in respect to vaccines, though it is assumed that there is an invisible micro-organism in vaccine lymph, as also in small-pox virus, the bacteriologist dare not inject his cultivated living micro-organisms into the human body ; they must first be killed by heat whether used as a simple vaccine or in a broth culture like Haffkine's prophylactic, or Wright's mis-named anti-typhoid serum. Vaccine lymph, with its supposed invisible organism, requires no sterilisation by heat ; it would be rendered inert.

If comparison be made with sera such as diphtheria anti-toxin, or tetanus anti-toxin, there is again no analogy. It is well known and universally admitted by competent authorities that a successfully vaccinated calf is made immune by vaccination ; that is, it cannot be vaccinated successfully a second time. But the serum of a calf thus made immune will not confer any tangible degree of immunity upon an unvaccinated calf. It is altogether illogical, therefore, to expect that the serum of a horse, which has been immunised by repeated injections of diphtheria or tetanus toxins should confer any degree of prophylaxis against these two diseases respectively, no matter what the results of guinea-pig testing may be. These anti-sera therefore do not confer any tangible degree of mmunity on man, nor can it be conclusively proved that animals when injected with anti-toxic serum in veterinary practice are made effectively immune. Just because an animal, such as a horse, does not suffer from an attack of tetanus after being injected with tetanus anti-toxin after an operation, or when he has fallen and injured his knees, it does not follow that he would have suffered from tetanus had he not been injected. This, however, is a side issue.

Reference has been made in the Report (Paragraph 46) to Koch's famous postulates which formulated the conditions which were necessary to be fulfilled before any particular micro-organism could be rightly regarded as the *causa causans* of any given infectious disease. These postulates, if they are not shelved as entirely obsolete by present-day bacteriologists, are certainly very limited in respect to animal infectious diseases, and can, of course, never be made applicable to human diseases.

I attach far greater importance to that other statement of Koch quoted in the Report (Paragraph 48), namely, " an experiment on an animal gives no certain indication of the result of the same experiment on a human being," and I contend that it is as applicable to all the assumed preventive or curative uses of sera and vaccines, with the single exception of vaccine lymph, as Koch made it specially applicable to his tuberculin, which proved such a disastrous failure as a cure for phthisis. It is all very well for Dr. Taylor to state, as also quoted in the Report (Paragraph 48), that " if further experiments had been made on animals the loss of time and expense, sometimes perhaps fatal results, and certainly disappointments might have been spared to many." That is only begging the question. I prefer to adhere to Koch's own conclusion, and I may be permitted to state in passing that when the leaders of the profession were flocking to Berlin from all parts of the world to see the results of the new treatment, and procure for themselves, or rather, for their patients, samples of the wonderful remedy, I embraced the opportunity, as President of a local medical society at the time, of pronouncing it to be an injurious septic decoction, because it was manufactured from bacilli grown in some kind of broth.

Before proceeding to comment on the several diseases mentioned in the Report, it appears to me to be advisable to refer more pointedly to vaccine therapy in relation to experiments on animals. The broth cultures, whether on gelatin or serum, of tubercle bacilli, or the animal tissue broth cultures of the plague bacillus, or the bouillon cultures for Wright's anti-typhoid vaccine, can all be prepared, and generally are prepared, without the need of experiments on animals, but all three entailed an enormous number of inoculation experiments to establish and demonstrate their uses. The simple vaccines, however, consisting of the killed bacilli which may be found in local manifestations of disease, as in acne, boils and certain skin affections, or in diseased gums, or in the sputum of bronchitic or asthmatic patients or in the diseased conditions with which, as previously pointed out, the *bacillus coli communis* is associated, can all be prepared and are used without any further need for experimentation on animals. But the technique, as devised by Sir Almroth Wright and his school, is so delicate and elaborate, the method of counting the killed bacilli and accurately gauging the number of millions required for a dose, as well as the control of progress by means of the mystic opsonic index, which is so largely dependent upon the personal factor for interpretation, all require such special training that none but highly skilled bacteriologists can pursue this new kind of practice. The distinguished physician, therefore, with his unrivalled clinical experience, and his intimate knowledge of disease, is gradually being ousted from the treatment of cases for which vaccine therapy is deemed essential, by the bacteriologist who has little or no experience of disease except what he can induce in animals. When bacteriological laboratories were first started, the physician evidently placed such little reliance on his own clinical acumen, and ability to cure, when he had to deal with ailments believed to be bacterial that he called in the bacteriologist to help him to clear up his doubts, and was gratified with being able to share in the *kudos* attaching to scientific research, taking it for granted all the while that his employé would stick to his laboratory and remain at his beck and call. But the Nemesis attending a vague leaning towards animal laboratory research as a *sine qua non* to medical advance, is changing all that. The bacteriologist is now so convinced of the physician's ignorance of all the complex and delicate technique necessary for the scientific treatment of these ailments, that he deems it to be his duty to take sole charge of all such cases himself, and this is not at all surprising.

Though no doubt the bacteria found to be associated with many of these local infections are toxic in their substances, when killed and injected, and produce various reactions or rises in temperature, it is exceedingly difficult to understand why the injection of millions of the specific dead bacilli should exercise a curative influence by aiding the destruction of the living bacilli characteristic of the disease. But Sir Almroth Wright explains all that by phagocytic or leucocytic action, and the progress towards cure or otherwise is correctly indicated, so he maintains, by the rise or fall in the opsonic index as obtained on examination of the serum of the patient's blood. Even if it could be proved that treatment of this most difficult and intricate kind were successful beyond all other methods for these so-called local infections, neither the ordinary physician, nor, above all, the average general practitioner could show any claim to be able to carry it out; and I still contend that this method of vaccine treatment, however it may be justified by experiments on animals, is altogether illogical.

Tuberculosis.

Coming now to tuberculosis, which is the first disease considered in the Report (Paragraph 48), I would submit at the outset, that so far as practical preventive medicine is concerned, it is a matter for regret that Koch's discovery of the bacillus which is associated with this disease, and is stated to be the actual causal agent, rendered more or less obsolete the old classification of the various forms of the disease, such as tubercular meningitis; tabes mesenterica, or tubercular disease of the intestines and peritoneum; scrofula or tubercular disease of the glands of the neck; and phthisis, or tubercular disease of the lungs. Bacteriologists contend that when they inoculate pure cultures of the bacillus into the guinea-pig, or other animal, they can reproduce all the features of the disease just because the bacillus appears to thrive in its novel culture medium, and of course, can be regained and isolated from the affected glands or tissues. It is true that they can produce tubercular nodules or glandular swellings, and what they call a generalised tuberculosis, which becomes more or less a mixed infection, but I contend they cannot produce to order the distinctive forms of the disease as above enumerated, with their respective characteristic features, separately and distinctly, as they occur naturally.

The *tubercle bacillus* is only one of a large unmber of organisms belonging to the same group which are found in nature, such as the *Timothy-grass bacillus*, found in grasses, hay, and in cow's dung; the butter bacillus; and the *smegma bacillus*, found on the hands of milkers, and on the flanks of cows, as a skin bacillus. It is therefore extremely difficult to Stockman, 3085.
distinguish these micro-organisms either morphologically or by staining, unless recourse is made to guinea-pig inoculation, and even then there is a possibility of error, because according to Muir and Ritchie, some of them do produce nodules which may closely resemble tubercles. Indeed, Mr. Stockman, principal Veterinary Officer of the Board of Agriculture and Fisheries, frankly admitted this possibility of error, and also admitted that many 3090.
certificates of milk examination for tubercle bacilli had been given on mere morphological 3094.
and staining testing alone. Similar admissions as to possibility of error were made in his evidence by Dr. Lorrain Smith, Professor of Pathology of the University of Manchester, who

Lorrain Smith, 18348. 18352. 18452. stated that he had frequently tried microscopical examination of milk, but without satisfactory results, and that the inoculation test was a very delicate one. In reply to another question, he further stated that the usual time required for arriving at a decision after inoculating a guinea-pig with milk, or the deposit in milk, as to whether tubercle bacilli were present or not, is about a fortnight or three weeks, and certainly within six weeks. But inasmuch as some other allied bacilli do produce nodules on inoculation, resembling tubercular nodules, as is contended, the liability to error must still exist, and the personal factor must still unconsciously influence the bacteriologist's certificate. Indeed, the examination of milk for tubercle bacilli appears to me to be so inseparable from the disturbing influence of the personal factor, on account of this liability to error, that I venture to think that public authorities such as the London County Council, Borough Councils, and Administrative County Councils, should never rely upon the examination of milk by any single bacteriologist, but that there should be always three samples taken from the same milk can, after the milk has been well shaken, and sent to three competent bacteriologists, each of them kept in ignorance of the source of the sample, as well as of each others' examination, and if they do not all three agree as to the presence of tubercle bacilli, the milk should not be certified as containing them. In dirty cow's milk there are always some particles of cow dung, and apart altogether from the presumed agency of milk as a cause of tuberculosis in children, the most stringent measures should be legalised to protect milk from all dirt or tainting of any kind, and from becoming infected with purulent matter from inflamed udders or teats, which I venture to think is the cause of those occasional milk outbreaks which are indiscriminately labelled scarlatinal or diphtheritic, and are in reality septic throat outbreaks. It should be made a penal offence if cows with chapped teats, or inflamed or diseased udders, are found on the surprise visit of a competent inspector to be stalled amongst the cows which are milked daily, and this should also apply to cows suffering from any febrile or other obvious disease, such as tuberculosis.

In this connection I may be permitted to make passing reference to the recently published report of the Royal Commission on Tuberculosis. I ventured to hint, in a
Stockman, 3101–7. question which I put to Mr. Stockman in examination, that some of the experts on that Commission could not help being influenced by what I called the "unconscious mental bias," because they took part in the investigations of the previous Tuberculosis Commission, which arrived at the conclusion that tuberculosis was communicated to children through the agency of the milk of cows suffering from the disease. Indeed, he admitted that all the members of the recent Commission were strong supporters of that theory. When, therefore, this second Commission was appointed to investigate in reality the truth or otherwise of Koch's declaration, which fell as a bombshell on the bacteriologists of this country, that extended experiments on animals warranted the conclusion that bovine tuberculosis was not communicable to human beings through the agency of milk or otherwise, they were placed in a somewhat difficult position from the logical point of view, inasmuch as among other terms of reference they were expected to inquire into and adjudicate on conclusions which some of them had already proved to their own satisfaction, and all of them honestly entertained. No doubt, all the investigations were most painstaking, and were contrived and conducted with an earnest desire to arrive at the truth. But it appears to me to be rather surprising that it took ten years of research work, involving an enormous number of most painful inoculation and feeding experiments, on various kinds of animals, including cows, calves, pigs, dogs, rabbits, monkeys, and guinea-pigs, to prove what they believed had already been proved, and what Koch, the greatest bacteriologist then living, had so forcibly contested on conclusions also based on a large number of similar painful experiments. From the commonsense view of a practical sanitarian, who had studied the subject very closely, I was unable to accept the conclusions of the first Tuberculosis Commission, and therefore welcomed Koch's declaration, apart altogether from its experimental basis. I still believe that Koch was right, and my reasons are briefly these:—It is admitted that abdominal tuberculosis, or *tabes mesenterica*, from which children suffer, is exceedingly rare among children of the middle and upper classes who are plentifully supplied with raw cow's milk, but is almost exclusively confined to the children of the poor, who receive very little or no milk. This distribution of cow's milk in respect to different classes of the community is illustrated in the following well-authenticated

table from Dr. Bantock's *précis*, which was published long after I myself had devoted considerable attention to the subject :— Bantock, 14547.

Statistics of Milk Consumption taken from the Books of Milk Retailers in various Districts.

No.	Description of District.	Approximate Number of Families Served.	Average Consumption per day, per Family.	Average Consumption per head. Per Day.	Average Consumption per head. Per Annum.
			Pints.	Pints.	Gallons.
1.	West-End of London	4,000	3·750	0·750	34·218
2.	North of London	2,000	3·250	0·650	29·656
3.	Manchester	—	2·666	0·533	24·318
4.	„ (Middle-class District)	146	1·500	0·300	13·687
5.	„ (Working-class District)	200	1·000	0·200	9·125
6.	Small Country Town	90	1·624	0·325	14·823
7.	Putney and Wandsworth	700	2·286	0·457	20·838
8.	Small Town (Health Resort)	500	2·000	0·600	27·375
9.	East End of London) one gallon of Milk divided into 37 portions)	—	0·432	0·086	3·923

Dr. Bantock added by way of explanation :— 14548.

"With reference to No. 9 (the East End of London) it is necessary to explain that the figures are based on the fact that a gallon of milk is divided on an average into thirty-seven portions, each portion being, I believe, fetched by the consumer. I have assumed that each family consumes one of these portions every morning and evening. It will be seen that even on this assumption, the consumption per head is infinitesimal, and it reveals, incidentally, a significant fact in connection with the dietary of the poor."

He further stated—and in this I fully agree—

"Again tabes mesenterica is essentially a disease of mal-nutrition, and is often cured by nutritious diet, of which milk should form an essential part with cod-liver oil and some preparation of iron. And why does this form of the disease attack the digestive tract of children rather than the pulmonary? Because, says the lecturer (Sir Richard Thorne-Thorne) the tubercle bacillus gains entrance through the stomach. I venture to affirm that not a single case has ever been put on record in which the disease has been thus communicated. In this connection I may quote Dr. F. M. Sandwith, who has said that in Egypt women suckle their children for even two years, and all other milk was boiled, and among the cattle tubercle was rare, yet tuberculosis was common." 14550.

Although it may appear presumptuous to question the methods of investigation pursued by the Tuberculosis Commission, I venture to submit that in many respects they were illogical because the manifestations of disease which were artificially produced in animals, whether by inoculation or by feeding, did not present the features of natural tuberculosis. The conditions under which the investigations and experiments were carried out were for the most part contrived, or forced, conditions. Tuberculosis is not propagated among animals naturally by surface lesions, represented by inoculation, whether with portions of diseased tissues or organs, artificial cultures, or emulsions of cow-dung and milk; nor are animals fed on cow-dung emulsions, or tuberculous sputum, under ordinary conditions. Even admitting that as the bovine tubercle bacillus could only be differentiated from the human bacillus by its greater virulence on inoculation into animals, I contend that for all practical purposes the only feeding experiments which were required to demonstrate the possibility or risk of communicating tuberculosis by the ingestion of cow's milk should have been limited to the feeding of the animals used, such as calves, pigs, monkeys, cats, guinea-pigs, etc., on ordinary mixed milk as sold, or on milk mixed with the milk of cows suffering naturally from tuberculosis, or on milk solely obtained from such cows; but feeding them on milk and cow-dung emulsions, or on milk from artificially infected udders, imported strained, forced, or unnatural conditions into the inquiry. But whatever the conditions imposed, let it be granted that animals, and especially all the chimpanzees, fed on cow's milk, do become infected with bovine tuberculosis, the most important feature of the inquiry rested on the examination of tuberculous lesions in the human being, such as the examination of tuberculous glands after removal, and of lupus lesions, as well as the post-mortem examination of diseased lungs or other organs. Let it be granted, too, that there was no difficulty in being able to distinguish between the bovine or human bacilli found in these lesions, in spite of the liability to error previously referred to, the practical outcome was this :—(*a*) That with few exceptions the lesions of fatal pulmonary tuberculosis are referable to human bacilli;

(*b*) that in the abdominal tuberculosis of young children nearly one-half the cases are referable to bovine bacilli; (*c*) that in young children and in adolescents suffering from cervical gland tuberculosis a large proportion of the cases could be referred to bovine bacilli; and (*d*) that in a certain proportion of cases of lupus, bovine tubercle bacilli, though of not a pronounced type, are also found. Taken in conjunction with the results of the feeding experiments, and as children are more largely fed on milk than adults, the Commission concluded :—

"That the evidence which we have accumulated goes to demonstrate that a considerable amount of the tuberculosis in children is to be ascribed to infection with bacilli of the bovine type transmitted to children in meals consisting largely of the milk of the cow."

But it has already been shown, and I venture to think that few will dispute the statement, that "the meals consisting largely of milk" are only partaken of by the children of the middle and upper classes, among whom abdominal tuberculosis is rare, while the disease is almost exclusively confined to the children of the poor, whose "milk meals" are of the scantiest. Apart, however, from this consideration, it appears to me to be somewhat singular that though the bovine bacillus is much more virulent than the human, scrofula is rarely fatal, and seldom leads to any further dissemination of the disease, while abdominal tuberculosis among fairly well fed children is often chronic and curable. Moreover, lupus cannot be regarded as either a fatal disease or as one in which there is dissemination beyond the local lesion, and it is generally curable. In respect to strictly logical inferences from all these experiments, there is this further significant admission in the final Report of the Commission (p. 36) :—

"We are inclined to regard transmutation of bacillary type as exceedingly difficult, if not impracticable of accomplishment by laboratory procedure, though in view of certain instances in which we obtained from one and the same human body both types of bacillus, we are not prepared to deny that the transmutation of one type into another may occur in nature."

I venture to suggest that such transmutation is not only possible, but highly probable, because the tissues of children and adolescents are less resistant to their growth, and therefore when injected into animals they would on account of their more vigorous growth exhibit the greater virulence of the bovine type.

But whether it be generally accepted or not that the conclusions of the Commission are finally established, or whether, as is quite possible, they may in the near future be once more called in question by a series of fresh experiments, or by a German Commission, I agree most emphatically in the recommendation that there should be no relaxation of existing milk regulations, and that further measures should be legalised effectually to exclude from milk supplies, as stated in the Report—

"The milk of the recognisably tuberculous cow, irrespective of the site of the disease, whether in the udder or internal organs."

The conditions embraced as "recognisable," however, should not be made dependent on the tuberculin test, but only on the general clinical symptoms of the disease, because a badly diseased cow will not react, and any animal will cease to react on sufficient repetition of the injection of tuberculin.

Though the Commission did not consider it necessary to include inhalation of tubercle bacilli experiments, in addition to the enormous number of inoculation and feeding experiments which they contrived and carried out, it is universally admitted amongst all who insist on the specificity and infectivity of tuberculosis, that the bacilli which induce the disease are almost exclusively air-borne, and that, as they can remain active for considerable periods in dried sputum, they infect the air through the indiscriminately scattered sputum of phthisical patients. So ubiquitous was this bacillus believed to be that bacteriologists found it swarming in the dust of railway carriages, the floor dirt of public houses, the dust even in schools, and, needless to say, plentifully distributed in the dust of workshops, and in hospital wards or rooms occupied by phthisical patients. Hence was instituted the raid on spitting, not because it was a dirty habit, and therefore ought as far as possible to be suppressed by police regulations, but the whole country was placarded with instructions issued by Medical Officers of Health warning people that it was the sputum of consumptive patients which originated and spread tuberculosis by impregnating the dust of inhabited places, and public thoroughfares, and every patient found, or believed to be phthisical was instructed to use pocket spittoons. All these precautions were fully justified as aids to cleanliness, but I have always contended they were not justified by the reasons given, and only served to propagate baseless microbial scares. For, in the first place, there is every reason to believe, that owing to the test difficulties, previously referred to, the bacilli which were found so plentifully in dust, were only varieties of the same large group of bacilli to which the tubercle bacillus belongs, and in the second place, even if they had been genuine specimens, they were only acting as nature's scavengers, and if they possessed the causal properties which it was so universally stated attached to them, active infection would have been widespread and general. I also agree with the statement which appears in the excerpt from Dr. Bantock's *précis* previously quoted, that the tubercle bacillus is not found in the sputum of early cases of phthisis, but can only be detected when necrosis of the lung tissue leading to cavities and mixed infections, sets in, when it also plays the natural *rôle* of a scavenger. Then, again, phthisis is the only form of the disease which is believed to be infectious, and then only I contend when pneumonic patches with mixed infections can be diagnosed in the later stages. Neither lupus, scrofula, nor tuberculous meningitis, is regarded as an infectious form of the disease, and I do not think that though tubercle bacilli are said to be found in the evacuations of children suffering from intestinal tuberculosis, such children are assumed to be as dangerous agents in disseminating infection as "typhoid carriers."

It has all along been admitted that previous to the discovery of the tubercle bacillus by Koch in 1881, as stated in the Report (Paragraph 48), phthisis was only regarded as infectious in a very limited degree, and then only when the disease was in a more or less advanced stage. Sir Douglas Powell, President of the Royal College of Physicians, in giving evidence stated that, until Koch's discovery, it was not generally recognized as infectious, and that in the Brompton Hospital for Consumption there was no special incidence of the disease among the nurses or staff generally, even before any particular precautions were deemed necessary. This he explained by stating his belief :—

"That the virulence of contagion of consumption is due largely to the association of dirt with that Powell, contagion. In insanitary surroundings the tubercle bacillus is associated with other micro-organisms. Just 5787. as in the case of the tetanus bacillus, when mixed with other infections it is far more virulent, so I believe the tubercle bacillus when mixed with streptococcal, and other organisms, is definitely more contagious, and, therefore in clean places like consumption hospitals and sanatoria, consumption is practically not infectious at all."

In all this I most fully agree, with the exception of the strong insistence which is placed on the infectivity of the tubercle bacillus, which in its association with other micro-organisms in dirt, is simply like them a saprophytic, or soil, organism, changing decayed, or decaying, animal or vegetable matter into harmless saline constituents. From the infectivity point of view it is altogether unlike the tetanus bacillus; because no surface wound is required for its pathogenic activity, nor can tetanus, as it ordinarily occurs, be designated an infectious disease. Its so-called *causa causans*, the *tetanus bacillus*, can always be found in dirt, without the assumption of transmission from animal to animal, and why not place tuberculosis in the same plane? Mere infection with the tubercle bacillus does not explain the incidence of phthisis in respect to age, nor explain why the bacillus is the sole *causa causans* of the various forms of the disease previously alluded to, or why there are such destinctive characteristic types of the bacillus, according as it is found associated with human, bovine, or avian, tuberculosis. I venture to suggest that in all three it is an evolution type of the same saprophytic organism which is abundantly found in natural conditions. For all practical purposes the specificity, or infectivity of this micro-organism is, I contend, a negligible factor. Indeed, this lack of any appreciable degree of infectivity, was fully admitted by Sir Douglas Powell. Thus, in his reply to a question as to whether it was not a fact that a 5788. wife frequently did not contract the disease from her husband, even though it proves fatal in his case, and *vice versa*, he stated :—

"I know of a very great many instances, and I believe it has been shown that the infection with regard to husbands and wives and wives and husbands is not more than that of the general population."

Further, this constant insistence of the assumption that the tubercle bacillus is the sole *causa causans* of the disease has, I submit, tended to obscure other factors which are of infinitely greater importance. So far as practical preventive medicine is concerned, I venture to contend that the bacillus is only of interest as a slight aid to diagnosis, and of very dubious value as a basis for the various brands of tuberculin which are now in use, or Marmorek's much vaunted serum, which has fallen into disrepute. There can be no doubt that these tuberculin preparations are toxic in their effects, and, on inoculation, produce re-actions varying according to condition of patient and dosage, but I feel convinced that if these so-called tuberculin dispensaries, which have been so strongly advocated by some, are permitted to gain wider recognition as useful institutions for the diagnosis and treatment of the disease, the ultimate results will not only be disappointing, but may in some cases prove serious. In minimal doses it is used by some as a diagnostic test, and if there is a slight re-action, it is concluded that the patient is suffering from incipient phthisis, forgetting that even a non-tuberculous cow with a slight cold, or which is in an unstable condition, will re-act. Then follows the treatment, commencing with minimal doses, and gradually increasing them. The increasing dosage should, according to the new doctrine, be regulated by the opsonic index, but unfortunately in genuine cases of the disease, this has not been found to work very satisfactorily, though in cases diagnosed by the tuberculin test the disease may certainly appear to become arrested, because there is no absolute proof that it ever existed. If it be desired to fairly test tuberculin treatment, let it be tried on cows suffering from the natural disease, and not on animals inoculated with tubercle bacilli.

I venture to predict that the raid against tuberculosis will never be effectually prosecuted on these lines without leading eventually to some such serious results as followed Koch's tuberculin treatment. Nor does the prospect appear to be very hopeful, so long as so much reliance is placed on sanatorium treatment; while as regards the proposed Government grant in aid of research, there are already laboratories, municipal, or attached to universities, sufficient and to spare, scattered all over the country. Sanatorium treatment, though it undoubtedly assists in arresting the disease in its early stages, is of comparative little value if poor patients when discharged are obliged to return to their former environment, and conditions of life and labour. After arrest of the disease, patients of the labouring, artisan, or workshop class will, in the majority of cases be always more or less liable to relapse. The disease had been reduced 50 per cent. even before Koch's discovery of the bacillus, as a direct result of generally improved sanitary conditions. The mere drying of the subsoil, for example, by the drainage of towns more than half a century ago, effected an enormous reduction in the death-rate from the disease, as was shown in the well-known Report of the late Sir George Buchanan, when Medical Inspector to the Privy Council. In Salisbury, for instance, he found that the death-rate had fallen 49 per cent.; in Ely, 47 per cent.; in Rugby, 43 per cent.; in Banbury, 41 per cent., and in thirteen other towns, while the reduction was not so marked, it was still noteworthy. Further, in the celebrated Report of the Army Commission, published in 1858, it was proved beyond all doubt that the excessive mortality from phthisis which then prevailed in the Army, and in particular regiments, was due to over-crowding and insufficient ventilation, and was greatly reduced when improved ventilation and a larger cubic space per bed were subsequently provided in barracks. Overcrowding, bad ventilation, damp dwellings, *cul-de-sac* courts, and foul smelling slums, lack of cleanliness, poor feeding—these are the exciting causes of tuberculosis, and it is against these root causes that the raid should be incessantly carried on; indeed, it is safe to predict that so long as these root causes are not vigorously and systematically assailed, so long will the "white plague" continue to claim a heavy death-roll, in spite of all sanatorium or dispensary treatment.

But in addition to these root causes, there are also predisposing causes in the individual which are of great force, and none more potent than hereditary influence. Bacteriologists have insisted so strongly as part of their creed, that the disease is not inherited, that even hereditary predisposition has largely been ignored as a contributory factor. Though the disease itself is not inherited, nor was it ever believed to be inherited, no one can question hereditary predisposition, and so far as any influence on public opinion can be exercised in appreciating the risks of persons with this hereditary tendency marrying into families with a like heritage, such influence should never be lost sight of. Heredity plays as important a part in the causation of the disease, I contend, as it does in insanity or gout, and an infinitely more important *rôle* than the ubiquitous bacillus, which is powerless for mischief to healthy life, in healthy homes, and amidst healthy surroundings. Bacteriologists have only confused issues, and in focussing all their research work on the bacillus, have misdirected attention from the essential causal factors. They cannot even be credited with the inception and extension of the open-air treatment; they are not always sure of the bacillus when they find it; their most potent educative preventive weapon is the intelligent disposal of the sputum; and their sole, much lauded, if not infallible, cure, tuberculin. Instead of focussing all their attention on the bacillus, they should cultivate a broader outlook.

Plague.

This is the next disease treated in the Report (Paragraph 49). According to the excerpt given from Doctoı Martin's evidence, the *plague bacillus* is stated to be the causative agent, and the flea the intermediary host in conveying the disease from rat to man. This assumes that an outbreak of plague must be preceded by an outbreak of the disease among rats, and so far as bubonic plague is concerned, this appears to be the usual sequence. The rat flea sucks the blood of the infected rat, or feeds on a dead rat, and in doing so swallows the bacilli which it ejects in its excreta on to the bites which it inflicts whether on rat or man. Both rat and man, it is maintained, become infected in this way, but the flea, though these virulent bacilli are found in its stomach, is not affected. This of itself appears to me to be a strong argument against the flea theory, because it tends to imply that the bacillus behaves like a protozoal organism when inside the flea, which it is not, and that the flea simply plays the part of a host. One can quite understand that in conveying putrid matter in inflicting bites, or infective matter from dead or infected rats, or even from bubonic sores in man, the flea could act as a carrier of the infection, but not by swallowing and then evacuating the bacilli of the disease. It may be stated in passing, that enormous numbers of experiments on animals have been carried out, not only to prove the specificity of the bacillus, but to establish this mode of infection. But bubonic plague is not the only type of the disease; there is the far more fatal variety which recently devastated Manchuria, and yet the same bacillus is regarded as the causal agent of both. Surely there must be other more pronounced factors than the bacillus in operation to account for types so entirely different. In Manchuria it was not found that the outbreak was preceded by a rat plague, but there were vague suggestions among the experts appointed to investigate the epidemic, that the marmot might have been to blame. Then there are wide-spread outbreaks of rat-plague, as the outbreak during the autumn of 1910 in the Fen district and neighbouring counties of England, which are not associated with any serious outbreak of the disease among the population residing in the rat-plague area. It is true that in this particular instance there were a few fatal cases of pneumonia, which, on bacteriological examination, were pronounced to be cases of plague, but which, I venture to suggest, from similar fatal cases of pneumonia which have at times come under my own observation, were in reality cases of "filth" or "septicæmic pneumonia," in which the infection spread from case to case. In support of this suggestion there is this further consideration—that these cases in which the disease was conveyed from patient to patient should have been cases of bubonic, and not pneumonic, plague, if they had in reality been cases of genuine plague. This is an instance in which test specimens, whether obtained from the patient or *post-mortem*, should not have been submitted from bacteriologist to bacteriologist with full knowledge of the source of the material to be tested, but a Medical Inspector of the Local Government Board should have been despatched to send test specimens to at least three well-known bacteriologists to furnish independent certificates without any knowledge of the source. I do not question good faith, I only desire to reiterate what has all along impressed me most forcibly in the evidence of bacteriologists who appeared before the Commission; namely, the great liability to error, and the influence of the personal factor, which is inseparable from the difficulties attending bacteriological diagnosis, especially when the ultimate court of appeal is the guinea-pig.

Then these plague bacilli, like the tubercle bacilli, are saprophytic, that is, can live in dust or dirt, though they are not nearly so resistant. It is universally admitted that plague is intimately associated with aggravated insanitary conditions. It was common enough in this country before the Great Fire of London in 1666, which more or less effectually purified the city, and led to some sanitary advance. Though that was slow, tentative, and tedious, it was nevertheless attended with such a measure of success that there have been no serious outbreaks of this filth disease since that period.

As regards infectivity, it is noteworthy that while bubonic plague is admittedly only slightly infectious, pneumonic plague, on the other hand, is highly infectious, and is spread largely by inhalation of re-breathed or tainted air.

Based on the bacillary theory of the disease, various prophylactics, it is needless to say, have been manufactured to confer immunity. Leaving out of account Lustig's and Yersin's serum, which, as stated in the Report (Paragraph 49), are of very doubtful value, the prophylactic which has been officially adopted by the Indian Government, and has been distributed in enormous quantities, and used for inoculation, is that known as Haffkine's serum. As previously shown, this is not a serum in the strict sense of the word, such as

Lustig's, but it is a bouillon decoction, in which the bacilli are grown, and when the growth is complete, the whole is heated to a temperature sufficiently high to kill all the bacilli. The prophylactic, therefore, contains a certain amount of the toxin developed by the growth of the bacilli as well as the toxic substances contained in the bodies of the dead bacilli themselves. But as a prophylactic, the immunity which it confers is admittedly transient; indeed, as stated in the Report (Paragraph 49) persons who have been inoculated three or four times, may yet succumb to the disease. For these reasons, repeated inoculations are advised, more especially in the case of those who are more or less continuously exposed to risks of infection. Statistics have been published which appear to give some colour to the belief so strongly entertained by many, that the use of this prophylactic has been followed by successful results, but, in my opinion, they certainly are not convincing. Even the Plague Commission, of which Dr. Martin was a member, seem to have placed very little reliance on its prophylactic value, for he admitted in evidence that while experimenting, Martin, 12111. they protected their legs by wearing long-top boots, and when engaged in combing out fleas from live rats, they took good care that, when the rats were chloroformed, they also chloroformed the fleas, or, to put it in Dr. Martin's own words :—

12112. "The animal was chloroformed, and the fleas were chloroformed; otherwise it would have been impossible to have dealt with them; the Commission would have been exterminated."

While there is no reason to believe that though Haffkine's prophylactic has been largely used, it has in any degree assisted in the spread of the disease, as some have stated, there can, I venture to think, be no more condemnatory pronouncement of its utter futility in the aggregate, than the fact that millions of natives have fallen victims to the disease, and strange to say, its use has been almost contemporaneous all through from the first serious onset of the various epidemics.

Under the grinding poverty, often verging on starvation, and the aggravated insanitary conditions which still prevail throughout India, especially in respect to the free application of cow dung to the interiors of the native huts, a raid against rats would in my opinion prove as ineffectual as a raid against the fleas which infect them, because rats thrive on filth as well as agricultural produce. The only effectual protection against plague rests on improved sanitation, systematic scavenging, clean huts, and better feeding, and this is generally admitted. One cannot resist the conclusion that the Government of India, in officially adopting Haffkine's prophylactic, were adopting an easier or cheaper way out under the ægis of impracticable expert advice, based solely on animal laboratory research, in order to placate public opinion. As affording ample confirmation of this view, I may instance the methods so successfully pursued in stamping out the outbreak of plague in Alexandria in 1899, which was prominently brought before the Commission in the evidence submitted by Sir George Kekewich, and I do so because I attach the greatest importance to it. I need add no further comment beyond admitting that sanitation in Alexandria, though no doubt very imperfect, had already made much greater progress than in many parts of India. The evidence is as follows :—

Kekewich, 20485. *Q.* (*Sir William Collins.*) Will you proceed to state the grounds on which you think that the practice of the use of sera has diverted attention from other means which you regard as more satisfactory?—*A.* As the counter proof that this treatment of the plague by serum inoculations has aggravated instead of "stamping out" the epidemic of plague, I will give the history of the outbreak in Alexandria in 1899. Before that time a Commission was sent by the Egyptian Government to inquire into a treatment of plague in Bombay. The *British Medical Journal* for August 14th, 1897, gives the report of this Commission formed by Dr. J. G. Rogers, Director General of the Sanitary Department, Egypt, Dr. Bitter, and Dr. Ibrahim Pasha Hassan. "The Commission agreed that improved sanitation is the only method of staying the inroads of the disease. The members declare that prophylactic inoculation is a method, the efficacy of which is not yet established, and contend that to render a whole nation immune on the outbreak of an epidemic is a practical impossibility." In May, 1899, the Commissioners had an opportunity of proving the truth of their report, when plague broke out in Alexandria. It was "dealt with on lines directly opposed to Mr. Haffkine's views," as Dr. J. G. Rogers reports in the following letter to *The Times* of August 4th, 1899: "Sir,—M. Haffkine's address on the subject of preventive inoculations against plague delivered before the Royal Society, and the leading article in *The Times* on the same subject, will excite considerable interest amongst practical sanitarians all over the world, and nowhere possibly more so than in Egypt, where plague is now being dealt with on lines directly opposed to M. Haffkine's views. In considering those views it must be remembered that M. Haffkine is not a medical man, and that therefore in all probability he looks on disease from the laboratory point of view. It is otherwise difficult to explain his persistent attitude of opposition which, in the opinion of many, has had a dangerous, if not a pernicious, influence in India. It must never be forgotten that, at a moment when, though somewhat late, practical proposals were put forward by a committee of practical medical men of long Indian experience for checking the spread of plague in Bombay, M. Haffkine alone dissented, and by throwing the full weight of his reputation into the scales against any really useful sanitary measures being applied, was mainly instrumental in these measures being rejected. Plague has been known to exist in Egypt since May 4th, and no doubt the infection was introduced before that date. By applying the sanitary measures so despised by M. Haffkine and denounced by him before the Royal Society as useless in the prevention of the saprophytic forms of disease such as plague, results not altogether unsatisfactory have been attained, and the disease has at least been kept well under control. The total number of cases has never arisen above thirteen in the week. In the weeks ending June 28th, July 5th, 12th, and 19th, they were respectively twelve, eleven, nine, and six cases of the disease. In 100 houses in which plague cases or suspicious cases occurred, and which were efficiently disinfected, in not one instance has a second case been noted. Had practical measures been abandoned and protective inoculations substituted, would these results have been attained? I venture to think not.—I am, Sir, yours truly, J. G. Rogers, Director-General, Sanitary Department, Egypt."

Q. Did Dr. Haffkine reply to that letter?—*A.* I have no reply. I do not know. Kekewich, 20486.

Q. Did he not on August 12th in *The Times* decline to discuss details with Sir J. G. Rogers?—*A.* I do not know; but the consequence was that the plague was stamped out in Alexandria within seven months. 20487.

Q. Has Dr. Haffkine not recently returned to India to resume work under the Government?—*A.* I believe he has. At all events this epidemic at Alexandria in consequence of the application of sanitary measures without plague serum lasted only seven months. 20488.

Cholera and Typhoid.

These two diseases, which follow next in the Report (Paragraph 50) receive only brief notice. Both are essentially filth diseases, and both may be propagated, and generally are propagated, by filth polluted air, filth polluted water, or filth polluted food. For all practical purposes these are the sole channels of infection. In respect to each a specific micro-organism has been discovered which is maintained to be the causal agent, namely Koch's vibrio, or *cholera spirillum*, which, as stated previously, was at first disputed by Klein, and Eberth's bacillus, the *bacillus typhosus*. The latter has been already referred to as possessing the unique characteristics of inability to induce any symptoms of typhoid when inoculated into animals, or when animals are fed with cultures of it in their food, nevertheless it is strenuously maintained by bacteriologists, that it is specifically pathogenic to man because it is found in lesions characteristic of the disease, and in healthy "typhoid carriers." Unfortunately, too, for the specificity of Koch's cholera spirillum, concerning which there has been much controversy, the cultures obtained from different localities have shown considerable variation in their morphological characters, and bacteriologists admit further that other spirilla, which bear a close resemblance to Koch's spirillum, have been cultivated from sources other than cases of true cholera. Experiments have, as a matter of course, been performed on an enormous number of animals, by many bacteriologists; but they have failed to produce the clinical symptoms of true cholera, and there is this further disconcerting fact, that experimental ingestion or swallowing of cholera spirilla by the human subject has given negative results.

Much stress is of course laid on specific reactions on testing in respect to both organisms, but the real outcome for each disease is the production of sera or vaccines to confer immunity. Several sera for cholera, the best known of which, with the exception of Lustig's serum, are Russian, have been tried, but results have not been successful; while in India Haffkine's anti-cholera inoculation, with killed virulent bacilli which have been passed through guinea-pigs to increase their virulence, has been used on a large scale. with the usual statistical results, which are always open to serious question.

As regards Wright's anti-typhoid inoculation, I agree fully with Professor Karl Pearson, and others, that statistical results have failed to establish its properties as a prophylactic, and I have no hesitation in stating it as my opinion that the indirect pressure made by the military authorities in India, and by officers commanding regiments about to proceed to India, to induce soldiers to submit to inoculation, is certainly not calculated to make service in the Army popular. If the results had been followed by any measure of success at all commensurate with those resulting from vaccination and re-vaccination against small-pox, there might be some justification for exploiting this method of prevention. As regards the new method of vaccination against typhoid, which shows that Sir Almroth Wright himself must have entertained some misgivings concerning his old prophylactic in spite of the statistical parade with which it was supported, I need only say that it will at all events possess the merit of entailing less pain when used, even if it should not ultimately prove to be more successful.

Malaria.

In briefly referring to this disease which is still so rampant in many parts of the world, I would desire to point out that formerly it was very prevalent in the Fen district of England, and that gradually extended, but effective, drainage finally stamped it out. If the disease is propagated by the *anopheles* mosquito, as is contended, and I am not prepared to dispute the theory, how has it come about that in spite of the large surfaces of water represented by the Broads, the anopheles mosquito has either entirely disappeared or has ceased to convey the *plasmodium*, or protozoal organism, from man to man as in former days? There were no mosquito nets in use, nor was covering the surfaces of water with paraffin resorted to; ague was simply eradicated as a result of drainage, and that after all is the bed-rock principle on which preventive measures are, or can be, effectually carried out. Paraffin sprinkling, which was so much paraded as an effective method for destroying the mosquito, is not now so largely depended on, and no doubt mosquito nets are very serviceable as a protection against mosquitos even in localities where there is no malaria. Granted that the mosquito is the intermediary host, a wider outlook has at last induced experts to become practical sanitarians, and in addition to drainage and the drying up of ponds and pools, they are even devoting attention to scavenging and other commendable sanitary measures.

The micro-organism, as it is protozoal, is not capable of cultivation, and therefore no serum or vaccine can be exploited, so that treatment of the disease fortunately still rests on the use of the old-fashioned remedy, cinchona, or quinine.

Yellow Fever.

Passing over the very painful and revolting experiments which were made on man as well as on animals by Sanarelli to isolate an organism characteristic of this disease and to manufacture a prophylactic or cure, it is a remarkable fact that though, as stated in the Report (Paragraph 52), no organism, whether protozoal or bacterial, has been definitely labelled as the *causa causans* of the disease, bacteriologists have satisfied themselves, by carefully devised experiments, I admit, that a particular mosquito is the carrier of the infection, whatever its nature may be, and the American research workers deserve all the *kudos* which Sir William Osler claimed for them. But as the mosquito is assumed to be the active intermediary, its destruction on the same lines as in the prevention of malaria is indicated, and whether the peccant insect can be exterminated or not, which is extremely doubtful, the largest measure of success in eradicating the disease has attended the prosecution of sanitary measures, and this has been achieved without a "visible organism."

But I venture to think that all the problems connected with both malaria and yellow fever have not yet been satisfactorily solved, and certainly in respect to yellow fever, against which quarantine regulations were for years maintained with such stringency even in this country. The uncertainty concerning the existence of a specific organism, and the certainty as to how the disease is actually propagated and may be prevented, lack confirmation. This, I think, may be inferred from the following replies to questions put by Sir William Collins to Sir William Osler when giving evidence :—

Osler, 16611 *Q.* (*Sir William Collins.*) There were a good many experiments upon animals in regard to yellow fever prior to these experiments that you have told us of, were there not?—*A.* Very many; there were, of course, the experiments by Sternberg and Sanarelli.

16612. *Q.* Did Sanarelli claim to have discovered the cause of yellow fever?—*A.* Yes, a special organism.

16613. *Q.* I see in Mr. Paget's book he states: "In 1896, at Flores, Sanarelli discovered the baccillus icteroides; and by October, 1897, he had prepared an immunising serum, which was able to give a considerable amount of protection to animals." "Furthermore, Sanarelli was able to show the preventive value of the serum" in a case, I think, at San Carlos, and Paget says: "Every prisoner, except one who had already had the fever, was therefore given the preventive treatment. At once the outbreak stopped; no more cases occurred, though only a weak serum was used, though the state of the prison and its occupants was unhealthy, though the fever, two months later, was still raging round the prison in the town"?—*A.* I do not think anybody places much reliance on that. I think Sanarelli's work has not been substantiated.

16614 *Q.* I see in the paper by Joseph Goldberger that you have just handed in to the Commission, on page 7 there is a heading: "Experiments to show that the Bacillus icteroides Sanarelli stands in no causative relation to yellow fever"?—*A.* That was the first work that the American Commission did.

16615. *Q.* Then if the bacillus of Sanarelli stood in no relation to the cause of yellow fever, I apprehend that this argument of a protective serum falls to the ground?—*A.* Entirely. I think it is not used at all.

16616. *Q.* Did not F—— also describe an organism as the cause?—*A.* No, Sternberg.

16617. *Q.* And did not Durham and Myers also describe another bacillus as the cause?—*A.* I do not know that they described it as actually the cause. Sternberg's organism was a mere suggestion. I do not think it was definite.

16618. *Q.* I see on page 236 of Mr. Paget's book he refers to the work of Myers and Durham, and states that: "The Report gives evidence that the disease is due to a bacillus, which is not the bacillus icteroides"?—*A.* Yes.

Q. Is this bacillus of Myers and Durham to be dismissed as also non causative ?—*A*. I think it is probably a spirochoete like the protozoal parasite—not an ordinary bacterium. Osler, 16619.

Q. Then I see "A later Commission," according to Mr. Paget, "to New Orleans, September, 1901, to January, 1902, reported an extensive series of investigations, which seem rather to support the belief that the bacillus icteroides is the cause of the disease." Were those investigations on animals ?—*A*. I do not know, but I think these more recent observations have shown that we do not know the nature of the parasite. 16620.

Q. Then all theory and practice based upon a bacillus as the cause of yellow fever we may now dismiss as erroneous ?—*A*. It is in all probability, a spirochoete, not an ordinary bacterium. That that seems most likely is shown by the similarity to the cycle of development of the mosquito in malaria taking a specific period of time. It takes twelve days in the body of the mosquito before the poison becomes active. It is very unlikely when you judge from analogy of the filaria of the mosquito parasite, that that is anything but a living organism that passes through a definite cycle, just as the malaria parasite does. 16621.

Q. But morphologically are we able to find the cause of yellow fever ?—*A*. No, but that is not surprising, when you remember that only quite recently the spirochoete of syphilis has been discovered. Probably it is an exceedingly minute organism. 16622.

Q. Is the method whereby you think that yellow fever may now be abolished that of immunising the community or destroying the mosquito ?—*A*. Destroying the mosquito and isolating all early cases. 16623.

Q. By isolation and by the destruction of the mosquito ?—*A*. Yes. 16324.

Hydrophobia.

As stated in the Report (Paragraph 53), a number of statistics were laid before the Commission by Dr. Martin, of the Lister Institute, which purported to show that the death-rate from hydrophobia among persons bitten by mad dogs and other rabid animals had been reduced by Pasteur's anti-rabic inoculation from 15 or 17, to about 1, per cent. But these statistics, I venture to contend, do not bear the close scrutiny of impartial analysis. As the percentage of about fifteen deaths among persons believed to be bitten by rabid dogs not treated has been largely based, according to Dr. Martin, on the statistics published by Hogyes, of the Buda Pesth Pasteur Institute, the subjoined excerpt from the report of the Kasauli Institute for 1910, which I have just received, is of great interest as showing how unreliable such statistics are. Major Harvey, M.B., the Director of the Institute, makes the following frank statement :— Martin, 11592–5.

"We may examine Hogyes' estimates for the mortality amongst treated and untreated a little more closely. They relate to 5,899 cases. Of these 4,914 were treated and 985 untreated. Of the treated 106 died, and of the untreated 100. But because 47 of the deaths amongst the treated developed hydrophobia within 15 days of completion of treatment, they are regarded as cases which the treatment could not possibly have saved, and are transferred bodily to the untreated, a most extraordinary procedure. We thus have 59 deaths, in spite of inoculation, and 147 amongst the uninoculated. It is on the basis of this rearrangement of the figures that the mortality of 15 per cent. is arrived at. This alone is a quite unjustifiable method of dealing with the data. Other criticisms might be passed upon the figures and their interpretation, but this will suffice to show the great necessity which still exists for trustworthy statistics of the mortality amongst the untreated."

Among several other witnesses who contested statistical results, I may refer more particularly to the evidence submitted by Dr. Granville Bantock and Mr. S. F. Smith, M.R.C.S., both of whom had devoted much attention to the subject. I may also be permitted to say that, as I myself had all along taken the deepest interest in Pasteur's work, and had scrutinised the statistics of the Pasteur Institute, I share most fully in their views that these statistics utterly fail to establish the success of the treatment. Dr. Bantock in his *précis* quoted communications which he had received from Professor Luteau and Dr. Bourchier, both leading medical men in Paris, which contested the reliability of all such statistics in most convincing fashion, and I feel bound to attach the greatest weight to their statements. Leaving out of consideration the statistics applying to the Pasteur Institute in Paris, to which patients flocked from all parts of the world, Dr. Bourchier made special reference to the statistics of Carlo Ruata for Italy, where the treatment only applied to inhabitants of the country, and Carlo Ruata made this statement as regards the average number of deaths from hydrophobia before and after the treatment was introduced :— Bantock, 14790-7.

"Before the method, the mean of the deaths from rabies was about sixty. After the method—*i.e.*, from to 1887 to 1900—the total of the deaths is 1,193, giving a mean annual rate of eighty-five." 14793.

But apart altogether from statistics, which apparently can always be readily marshalled to prove or disprove any system of treatment, I have never, as previously stated, been able to see that there was the slightest analogy between Jenner's discovery and

Pasteur's method of anti-rabic inoculation on which it was admittedly based. In small-pox, and cow-pox or vaccinia, the virus in the form of lymph is visible and tangible, whereas in rabies it is supposed to be concentrated in the spinal cord just because the nervous system is especially affected, but it is excreted into the bite by the rabid dog's saliva. Pasteur's method was based on the most painful and frightful experiments on dogs and rabbits, and I have always felt convinced that had he been a fully trained medical man, instead of being a distinguished chemist, he would never have evolved such an irrational method of treatment. Briefly, the method of cultivating the supposed virus and inoculating it into patients is as follows:—The virus, represented by a portion of the spinal cord of a rabid dog, or of a rabbit which has been made rabid, is injected on to the brains of other rabbits by boring a hole in the skull, and can be passed on from rabbit to rabbit in continuous series. The cords of these rabbits are dried for varying numbers of days, and the virus which they are believed to contain can thus be attenuated according to the length of exposure. The cords are then pounded up, mixed in sterilized broth, bottled up, and labelled, so that they can be used on patients by commencing with those most attenuated by the longest exposure. The treatment consists of a series of fifteen injections which are made under the skin at intervals of two days, commencing with cords dried for thirteen to fourteen days, and continued until a series of fifteen injections are completed with cords dried for only five days. But it is the dead nervous tissues of rabbits that are being injected all through the series, and it is contended, and I believe with justice, that fatal results sometimes ensue, not always due to hydrophobia. For, though the injections are supposed to counteract, or induce a certain degree of resistance to the poison which is believed to be slowly incubating in the system of the patient after being bitten, Pasteur carefully screened his statistics, after some untoward deaths had occurred during treatment or immediately after, by ruling that all deaths should be excluded from the statistical returns which occurred either during the treatment, or within fifteen days after the last injection. This rule he endeavoured to justify, on the extraordinary hypothesis, that though the incubation period of the disease may extend from fifteen days to seven or eight months, or longer, its active development is assumed to have commenced just before the treatment has been begun, in all of these fatal cases which are excluded from the statistics. There is, therefore, the usual proviso, the earlier the treatment, the greater the chance of successful results.

But it is in accordance with this most extraordinary rule, that the percentage of deaths, in all Pasteur Institutes, works out at such a low figure. Thus, in the Report on the Kasauli Institute for 1910, already referred to, Major Harvey commences his comments on the statistics of the year as follows:—

> "In this year 2,073 persons bitten or licked by rabid or suspected rabid animals, were treated"

—yielding a percentage of failures of 0·19. This percentage Major Harvey explains in these words:

> "There were twenty-six deaths from hydrophobia. Of these fourteen died during the treatment, eight within fifteen days of completion of treatment, and four later than fifteen days after completion of treatment. Only the last four are accounted as failures of the treatment according to the usual definition of a failure, and it is on this number that the percentage failure rate is calculated. Besides the number actually treated, there were 328 individuals, who, under advice, did not receive treatment, or who when fuller information was available, discontinued treatment. These comprised persons licked, in whom the saliva did not come in contact with any recent cut or abrasion, persons who treated the licked spot at once with an efficient antiseptic, persons whose dog was still alive ten days after licking or biting them, and other similar cases in which the possibility of infection was precluded. There is a tendency amongst the general public and also amongst medical men to magnify the likelihood of infection, which leads to persons arriving at Kasauli or being sent there when a saner view of the probabilities involved would have negatived the idea of any danger."

Further on he makes the following frank statement concerning the very slight risks which many persons, who have been licked or bitten, run, but who, nevertheless are so terror stricken that they flock to Kasauli for treatment, only to be assured that treatment is not necessary:

> "A journey in a railway train carries with it the possibility of death in a collision, and yet the fact does not affect our actions to any greater extent than to make some take out an insurance policy. I believe that the probabilities of infection are no greater than the probabilities of a collision for some individuals who undertake a long railway journey in order to undergo treatment."

But a large number of deaths do occur, mostly after a considerable time has elapsed after treatment. If it be true, therefore, as was claimed by Pasteur, and is maintained by his followers, that on dogs his experimental treatment was invariably successful, and if, as is also claimed, the analogy between the experiments on dogs and the treatment of human beings is complete, the deaths from hydrophobia which occur during or after treatment must all be classed as failures, and I satisfied myself on careful enquiry, as far back as 1898, that the number of these deaths had then amounted up to 373. I contend, therefore, that this number of deaths, and others which may have escaped notice, merely represented the number bitten by rabid animals, who would have succumbed to the disease had there been no anti-rabic treatment, and that the thousands who have been treated and survive ran no risk of dying from hydrophobia had they never been subjected to treatment. The only risk which it appears they run, and this I grant is a very remote one, is that they may suffer severely, or die from the injection of these broth emulsions of dead rabbit nervous tissue, but not from the injection of the real virus of rabies. The diseased conditions which are induced in the rabbits which furnish the cords, present none of the features of "mad dog" rabies. It is called dumb-rabies, because its most prominent feature is paresis, or paralysis of the hind quarters, a sequela of severe illness which is often seen in dogs or other animals dying of distemper, or from other serious ailments.

I venture to submit that the whole Pasteurian theory, and the treatment based upon it, are fraught with error from beginning to end. The method of diagnosing the disease by injecting a portion of the spinal cord of a presumed mad dog into a rabbit's brain is obviously liable to error, and indeed, has been largely supplanted at Kasauli by Negri's method; the assumption that a dog is mad when it runs about wildly, may frequently be erroneous; the assumption that the actual virus is contained in the portions of the rabbit cords used for injection rests on experiments of the most inconclusive kind; while the tabulation of cases as cures which survive treatment, and almost all of the cases do survive, though by no means a novelty in the statistics of disease, is certainly open to more than question. Nevertheless, Pasteur's system of anti-rabic inoculation, with various modifications to suit the climate, receives not only the official recognition, but the actual support of the Government of India, by the establishment of institutions in which the treatment can be carried out.

Sleeping Sickness.

As the micro-organism stated to be the causal agent of this disease is protozoal, and not bacterial, it lies outside my prescribed range of criticism, but it may be noted as stated in the Report (Paragraph 54) that in order to clear up interesting points in the diagnosis of the disease, healthy, as well as sick, natives were submitted to needle punctures in the spinal canal for the purpose of ascertaining if the *trypanosome* was contained in the cerebro spinal fluid, but whether the healthy natives submitted voluntarily to this kind of research, does not appear.

As the *trypanosome*, which is believed to be the causal agent, is a protozoon, and not a bacterium, no vaccine or serum has been, or can be exhibited for the prophylaxis or cure of the disease, but an arsenical preparation called atoxyl, was at one time largely boomed, and strongly recommended by Koch and others as a remedy, until somewhat unfortunate results brought its use into discredit. An insect of the tsetse fly species is stated to be the carrier or intermediary host, and some bacteriologists have contended as a result of their research work, that the organism is conveyed by the fly from wild animals, though this is open to doubt. Whatever may be the natural history of the disease, it is especially prevalent in swampy, or lake side districts in certain tropical regions where tsetse flies swarm abundantly, and therefore the practical preventive measures which appear to be indicated, are either to clear, or as the great Livingstone tersely put it: "Keep clear of the fly belt." As regards the doubts and difficulties which have cropped up in respect to investigating this disease, and the amenities which sometimes characterize the discussion of differences between rival authorities, the following examination of Sir David Bruce by Sir William Collins is very instructive :—

Q. (*Sir William Collins.*) Then as regards sleeping sickness, I think you mentioned that before the discovery of the trypanosome, a claim had been set up for a streptococcus as the cause?—*A.* Yes, by the Portuguese, and by the first Royal Society's Commission. Bruce, 14424.

Q. That was very stoutly supported at one time, was it not?—*A.* I never believed in it. 14425.

14426. *Q.* Did not Castellani support it?—*A.* I was a Fellow of the Royal Society and a member of the Sleeping Sickness Committee when his paper came home giving this coccus as the cause, and I said to the Committee that there was absolutely nothing in the paper which amounted to anything like proof. It wanted animal experimentation. I wondered why he had not been able to give this sleeping sickness, by means of his particular coccus, to some animal. He showed no signs of ever having tried to do so.

Bruce, 14427. *Q.* Were no experiments performed by the Portuguese Commission ?—*A.* I think so, but I must say that I have not read the Report of that Commission very thoroughly, because it is written in Portuguese.

14428. *Q.* And did they point to this streptococcus as the cause ?—*A.* Yes. Castellani also pointed to it.

14429. *Q.* And Castellani afterwards changed his mind, did he not ?—*A.* He changed his mind after I wrote my paper.

14430. *Q.* No doubt the influence of the divine gift ?—*A.* In that particular case I should say that it was great luck my having worked for two years at a similar disease. I gave Castellani the paper describing the observations made by us during the three weeks he remained in Uganda after my arrival. I gave him that paper to take with him to London, in order that he should not go back empty-handed. In the interests of scientific history he ought to have associated my name with his, or at least described in the text the part I took ; but he did neither.

14431. *Q.* You point out the importance of differentiating and avoiding fallacies. I am anxious to know whether besides this streptococcus, there was not at one time another cause asserted very strongly, and held by a great many as the cause of sleeping sickness ?—*A.* *Filaria perstans* was one.

14432. *Q.* It was found in a negro in London ?—*A.* Yes, I think so.

14433. *Q.* And then found in many others ?—*A.* I should say, judging from the natives that I had to deal with in Uganda, that if you looked sufficiently long you would find it in 100 per cent, in the same number as you find the trypanosome, showing the difficulty of distinguishing between two parasites. The one is evidently a harmless parasite, living in the blood of man, just as Nagana trypanosomes live in the blood of wild animals. Here you have a parasite, and 100 per cent of the natives have it in their blood.

14434. *Q.* Healthy natives ?—*A.* No, many of these natives had sleeping sickness, and we used to find the *perstans* in them almost as regularly as the trypanosome.

14435. *Q.* Was it also found in healthy natives, and in the Indians in America ?—*A.* Yes, and it is found also in natives living outside the sleeping sickness areas, and in certain sleeping sickness areas where there was no *perstans* found, the trypanosome was found.

14436. *Q.* Do you know Sir Patrick Manson's work on tropical diseases ?—*A.* Yes.

14437. *Q.* He says : "In dealing with newly-discovered parasites, our experience with *Filaria perstans* should serve as a warning against precipitancy in drawing conclusions from the mere fact of concurrence." Do you agree with that ?—*A.* Certainly.

14438. *Q.* And he says : "Moreover, what we already know about some African trypanosomes should also make us hesitate in definitely committing ourselves to a trypanosome as the cause of sleeping sickness." You do not agree with that ?—*A.* Well, you see, I look upon Sir Patrick Manson as what we call an armchair naturalist. He lives here in London, and is very clever at reading papers and prophesying what is going to happen, but he has not the practical knowledge; he does not go and work at the thing practically on the spot ; and if a man is going to write a text book he cannot expect every word in the text book to be true, or even to be fairly reasonably true. There must be a lot of padding and nonsense in a text book. A text book is the lowest form of literature that I know of.

14439. *Q.* I take it that you do not agree with Sir Patrick Manson when he says : "Experimental evidence, therefore, so valuable in the settlement of such questions, has in this matter, to say the least of it, been far from conclusive." That is referring to sleeping sickness ?—*A.* Is that five or six years ago ?

14440. *Q.* It is the year before last.—*A.* I was opening a discussion on sleeping sickness some three years ago in Oxford, at the meeting there of the British Medical Association, and I quoted Sir Patrick Manson on sleeping sickness, he having opened a discussion on the same question, I think, the year before. I put down all the conclusions that he had come to, and I said : "Every conclusion of a year ago is now proved to be false except one, and that is that medicinal treatment has no effect." Unfortunately, that still remains true. All the other conclusions were absolutely wrong. I put down in the same way, under separate headings, some ten or twelve statements, and I asked the British Medical Association to bring these up a few years later, and find out how many of them remained true. I think at the present time all mine remain true.

'4441. *Q.* How long ago was that ?—*A.* 1904, three years ago.

14442. *Q.* What about the medical treatment ? Does not Koch claim that a subcutaneous injection of arsenic is beneficial ?—*A.* Yes, but Robert Koch is getting an old man, and he is much frequented by interviewers, and it is very difficult to get an interviewer to repeat exactly what you say.

14443. *Q.* You do not accept the crocodile ?—*A.* No, I do not, and I do not think Koch himself does. He has never himself said that atoxyl will cure. He said that he had every faith in it, and so he gave the lay mind to understand that he was curing them by the hundred, but I have never believed it, and I do not think that any person who knows the subject has believed it ; and I think he has gone back to Berlin very disappointed that the atoxyl treatment has been useless.

14444. *Q.* You think that attention should be directed rather to destroying the haunts of the tsetse fly than to an onslaught on the crocodile ?—*A.* That is the broad way. I think myself that in a place like Uganda, especially after the population has been so reduced, if the Government and the chiefs made a big effort, they could take the whole of the natives out of the sleeping sickness areas, and put them in healthy places, and if they could do that, theoretically in forty-eight hours there would be no more sleeping sickness; there would be no fresh cases of sleeping sickness in Uganda. The tsetse fly only carries infection for forty-eight hours, and if you could remove the reservoir of the disease, in forty-eight hours every Glossina palpalis in Uganda would be harmless, and if you did not expose affected people to the tsetse fly again, then sleeping sickness would be blotted out by a wave of the magician's wand sort of business.

Q. If you were made plenipotentiary there would be no fear of its spreading in East Africa or down the Congo or the Nile; is that so?—*A.* It is mainly a question of money. If you know the distribution of the fly, and you know the natural history of the disease, you can do a great deal. The great thing in all these cases is to know the natural history from beginning to end, and not till then to try to do anything. You require to keep the people away from the sleeping sickness areas until the healthy-looking ones have developed the disease; you require to keep them away in the interior of the country. Those who are affected by sleeping sickness must, of course, never go back to the fly area. If at the end of forty-eight hours you sent back the population to the lake shore you would not be able to distinguish between the healthy and the unhealthy; you would therefore require to keep the sleeping sickness area population away from the tsetse fly for several months. Bruce 14445.

Diphtheria.

In respect to this common infectious disease which appears next in the Report (Paragraph 55), I desire to be more particularly explicit, because, as previously pointed out, it was stated, and quite correctly, that I was one of the few medical men in this country "who uphold the uselessness of anti-toxin in the treatment of diphtheria." In once more intruding the personal note, I may be permitted to emphasize this statement by adding that I not only entertained the gravest doubts concerning the efficacy of the treatment when it was first introduced, but I also questioned the value of the bacteriological examination of swabs from infected throats as a valuable aid to diagnosis, and made it known to my professional brethren practising in the several urban and rural districts for which I acted as Medical Officer of Health, that though bacteriological examination was placed at their disposal by the County Council free of charge, I attached far more importance to careful clinical examination of cases, and would accept all notifications of throats as diphtheritic which showed signs of patchy or septic conditions, apart from any which might be pronounced to be diphtheritic as a result of bacteriological examination. This course I felt justified in pursuing, because I believed that all such throats are infectious, indeed they have all along been notified as cases of diphtheria, in order to be able to take prompt precautions, and also because it saved the delay of waiting for the bacteriological report, a copy of which was always sent to me direct.

If it be asked, how with my sceptical views both as regards the bacteriological diagnosis of the disease, and its treatment, by anti-toxin, I could conscientiously, and therefore efficiently, discharge my duties as a public official, I may be permitted to explain that my views were perfectly well known to my medical brethren, with whom I always strove to act harmoniously; that I made them known to the several sanitary authorities whom I served by reports and personal statements at meetings; and that through my reports, and also by direct explanatory report, I made them explicitly known to the Local Government Board. I was not in private practice, therefore I did not treat cases; nor did I act as Medical Officer to any of the isolation hospitals in my district, therefore I did not interfere in any way with the treatment of the patients. Briefly, my position was this—that while I could not conscientiously recommend, I had no right to oppose, and of course did not oppose, any facilities which were afforded either for bacteriological diagnosis, or for anti-toxin treatment. I need not say, however, that I took the deepest interest in this new departure, which was hailed by the medical profession generally, and especially by the medical staffs of the Metropolitan hospitals, with an enthusiasm which almost equalled that attending the introduction of Koch's tuberculin. But I believed then, and am convinced still, that if diphtheria had been a disease which affects adults as phthisis does, and not young children on whom its incidence almost exclusively falls, the anti-toxin treatment of diphtheria would have received as short a shrift as did Koch's tuberculin treatment for tuberculosis. But children are unable to explain the symptoms, more or less painful and disturbing, which follow injection of anti-toxin, and so the new treatment, which was adopted in all good faith, I admit, made such headway that practitioners generally were compelled to fall into line, or be dubbed old-fashioned or unscientific. I could always understand, or believed I did, why the injection of anti-toxin assisted in the loosening and removal of the diphtheritic membrane, inasmuch as the reaction following the injection has the effect of causing the minute blood-vessels underlying the membrane to expand and contract alternately, and thus exert a mechanical effect, so to speak, in loosening the membrane, while the sickness or vomiting, which was also frequently induced, further assisted in this direction. Another factor, which, I believed, operated largely in exploiting the treatment was this :—that the medical staffs of the Metropolitan hospitals were so greatly impressed with the success of the treatment as reported from the Continent, that they not only implicitly believed in it, but trusting in a sheet anchor on which they believed they could fully rely, they operated with greater promptitude, and with larger hope of success, in severe cases when tracheotomy or intubation became necessary, and as other means of treatment were not neglected, these were naturally applied with a keener discrimination and more timely application. Patients were watched more carefully in observing the effects of the treatment, and indirectly nursed more carefully, than they were in the days of somewhat hopeless despair in respect to severe cases which prevailed during the pre-anti-toxin period.

But while I could readily admit all this I could never admit the *rationale* of the treatment, nor the specificity of the micro-organism associated with the disease from which the serum treatment of the disease is evolved. For, in the first place, the *Lœffler bacillus*, as it was called, had been discovered in varying proportions as living and thriving in healthy throats, and under these conditions was playing the part of an ordinary saprophytic, or "scavenging," organism. Secondly, it was found abundantly in the nostrils and fauces of patients suffering from rhinitis, a disease which is somewhat prevalent among children, but not regarded as infectious, and certainly not diphtheritic. Thirdly, it was sometimes detected on the healthy mucous surfaces of other parts of the body, and as Dr. Bantock stated in his evidence, in the healthy discharges from abdominal wounds. Fourthly, its presence was often found in the throats of persons long after they had recovered from an attack of diphtheria, and after all signs of the disease had completely disappeared. Further, the bacteriologists sometimes failed to find the organism in swabs obtained from membranous throats which presented all the characteristics of true diphtheria. As stated in the Report (Paragraph 55) Dr. Sims Woodhead, of Cambridge University, explained this on the hypothesis that the diphtheria bacilli—the assumed causal agents of the disease—are crowded out by other organisms of a pyogenic character, so that the disease assumes the character of a mixed infection. It was known, too, that it was sometimes difficult to distinguish the *Lœffler bacillus* from other pseudo-bacilli, such as the *Hoffman bacillus*. Indeed, recent investigations have shown that the bacillus belongs to a large group of allied organisms, all of which resemble it so closely under the microscope, that it is difficult to distinguish them. These organisms have a wide distribution in nature, and have been obtained from the mouth, nose, and skin of healthy persons, and associated with certain diseased conditions such as coryza, leprosy, and even general paralysis of the insane. As the *Lœffler bacillus* can be varied enormously in virulence by cultivation from non-virulence or slight virulence to intense virulence, and as these similar bacilli also vary greatly, the difficulties attending bacteriological diagnosis are not only apparently great, but I contend there is no warrant for assuming that the *Lœffler bacillus* is the essential *causa causans* of diphtheria. The disease is admittedly a mixed local infection, because numerous other micro-organisms, such as *micro-cocci* and *staphylococci*, are also found in or around the membrane. It is admitted, too, that in cultivating the bacillus to produce toxins for injection into the horse used for anti-toxin serum, it must be rendered exceptionally virulent, and strange to say the serum when prepared makes an excellent culture medium for the bacillus. On what logical grounds, therefore, can its specificity be contended to rest ?

In respect to the serum, I have now to state that although great progress had already been made in research work on the wonderful phagocytic and other properties of blood serum, I was unable to accept the view that immunising power could be imparted to the serum of an animal by injecting into that animal the cultivated bacilli, or, as in the case of diphtheria, the toxin obtained by cultivation of the bacilli of the disease with which they were found to be associated. It could easily be understood why the serum of an animal suffering from an infectious disease, could, when injected into another animal, produce a real, or, possibly, a mild or modified attack of the same disease, and thereby confer a pronounced degree of immunity. But neither the injections of the toxins of diphtheria bacilli nor the bacilli themselves did produce the typical features of the disease in an animal, and the horse used for the production of the serum, beyond experiencing a series of reactions when injected, was none the worse for the injection, at least, so it was contended. It was assumed that on every reaction after injection of the toxin certain anti-toxic or protective properties were imparted to the serum which enabled the animal to receive gradually increased doses until a high degree of immunity was believed to be conferred. The serum obtained at this stage, when tested on guinea-pigs, if found to be sufficiently anti-toxic on mixing the toxin and anti-toxin before injection, was then treated with a small percentage of carbolic acid to prevent it from becoming putrid, and was labelled as fit for use. It is therefore seen that underlying the whole theory is the doctrine of immunity previously referred to. But as the horse, when finally bled, had ceased to react, and was assumed to be in perfect health, it was difficult to understand why the serum should possess any immunising or prophylactic properties which could be of value as a cure for the disease. And this sceptical attitude of mind was, so I contend, fully justified by the fact, also previously referred to, that the serum of a successfully vaccinated calf when injected into an unvaccinated calf confers no tangible degree of immunity against vaccination, in other words, the second calf can be successfully vaccinated. Further, in respect to the degree of protection which could be conferred experimentally by anti-sera against plague and typhoid fever, as well as diphtheria, my scepticism was supported by research work which Dr. Klein had been carrying out on behalf of the Local Government Board during the early days of serum therapy, concerning which Sir Richard Thorne Thorne, then Medical Officer of the Board, made the following comments in his Report for 1895–96 :—

> "Each several serum employed by Dr. Klein served to protect his experimental animals against the diphtheria for a limited period only, a period which in no case exceeded one week. Furthermore, the amount of serum required for inducing in rodents this condition of very temporary protection was relatively so large as practically to forbid anticipation of useful results from an application of the method to the human subject."

But Dr. Klein, and the Medical Staff of the Local Government Board, though Dr. Klein had proved experimentally that useful prophylactic results were practically impossible so far as the human subject was concerned, speedily fell into line when the serum was exploited on the Continent as a cure. The preparation of anti-toxin serum was soon begun by the Board under Dr. Klein's supervision, and the aid of a special grant as previously stated, and, according to Sir W. H. Power, it was offered for free distribution to Sanitary Authorities. But whether it was distributed without being tested or not, or whether it was insufficiently advertised, the demand soon fell off, and private manufacturers on the Continent, in America, and in this country speedily usurped the field, and swamped the market with sera for every disease which was suspected of being traceable to a specific micro-organism. It is a matter for serious reflection, and no less serious criticism, that almost all these much vaunted sera have become more or less discredited and have fallen into disuse, though there were often many different brands procurable for the treatment of the same disease. Only the diphtheria anti-toxin and the tetanus anti-toxin are left to hold the field, and the reputation of these is beginning to wane; indeed, the latter, though much exploited as a cure at one time, is now only used as a prophylactic, while the former, though still believed to be of use as a remedy, has become discredited as a prophylactic.

But apart altogether from any specific properties which the inoculation of specific bacilli or their toxins might confer on the serum of the inoculated animal, it has always appeared to me to be a matter beyond question that the serum of a large animal, such as an old horse, must in itself be toxic, because, in addition to the plasma containing the nutrient elements of the food, which the blood stream distributes for appropriation by the tissue cells throughout the body, it also contains its quota of the effete or used up products of the tissues which are being conveyed by the blood stream in order to be excreted by the kidneys, skin, and lungs. This toxicity of normal horse serum has for some time back been placed beyond dispute, and it is reasonable to infer that its assumed anti-toxic properties in relation to the disease do not lessen in any degree, but may even increase, its actual toxicity, because the horse after a prolonged series of inoculations and reactions may not be in quite the best condition of health when he is freely bled for his last quota of anti-toxic serum.

It is not matter for surprise, therefore that on injecting the serum of one animal into another, as the injection of horse serum into a child, somewhat unpleasant results may follow. Indeed, these results represent a condition now known as "anaphylaxis," that is a condition of "hypersensitation," the opposite of prophylaxis. This term has been devised to explain the fact that if an animal or human being is injected with a serum, and a second dose of the serum of the same species of animal, such as a horse, is injected after an interval of ten or twelve days, or even weeks, months or years, serious symptoms may ensue. Thus, there have been cases of medical men who, honestly believing in the prophylactic value of diphtheria anti-toxin, have injected themselves a second time in order to secure protection against the risk of infection on the occurrence of a fresh outbreak of the disease, and have suffered severely from rashes, fevers, and other painful symptoms. If again, in any outbreaks of diphtheria, contacts are injected with the view of protecting them from infection, and if they should subsequently contract the disease, they may also suffer from what is now known as "serum disease." Indeed, these risks attending "anaphylaxis" are now recognized as being so serious that, as already stated, the use of the serum as a prophylactic has fallen into disrepute, notwithstanding all the parade made of the successful results following the prophylactic use of the serum in connection with the diphtheria outbreaks at Cambridge and Colchester, referred to in the evidence.

Further, when anti-toxin serum is used in the treatment of diphtheria, rashes and other serious symptoms do occur in a considerable proportion of cases after varying intervals succeeding the first injection, and to substantiate these risks, as well as to illustrate the haphazard way in which practitioners are advised to use the serum as early as possible, and not wait for the bacteriological report, even if they happen to be in doubt as to the nature of the disease, I will now quote some replies to questions put by myself which were given by Dr. Martin, Director of the Lister Institute, in the course of his evidence :—

Martin, 11985. *Q.* (*Dr. Wilson.*) Now I come to the much vexed question of the serum treatment of diphtheria. Of course, the anti-diphtheric serum is believed to be the most successful of all sera in treatment, is it not? —*A.* Certainly.

11986. *Q.* And it is not quite a harmless product, is it?—*A.* It is not quite harmless.

11987. *Q.* It produces rashes in a great many cases?—*A.* Yes.

11988. *Q.* And other disagreeable symptoms?—*A.* The most disagreeable symptoms are the rashes. They keep the child awake at night.

11989. *Q.* Are not medical men encouraged to use it in all suspected cases of throat illness, and not to wait for the results of the bacteriological testing for the bacillus?—*A.* I think that is quite a wise procedure.

11990. *Q.* But if the bacteriological testing fails to confirm the suspicions of the medical attendant, and the case does not turn out to be one of diphtheria, would you not call this somewhat haphazard treatment and scarcely fair to the patient?—*A.* No. I should consider it had been done in the best interests of the patient, for the reasons given in my evidence as regards the necessity of the earliest possible treatment by all antitoxins.

11991. *Q.* If it does not turn out to be diphtheria?—*A.* No harm is done.

11992. *Q.* But you admit that it causes rashes?—*A.* The patient does not die of the rashes.

11993. *Q.* How long does it generally take before the medical man receives the bacteriological report, after forwarding a swab from the throat of a suspected case?—*A.* Fourteen or fifteen hours.

11994. *Q.* But supposing you have to send to the country by post?—*A.* You can telegraph the result.

11995. *Q.* But the swab has to be sent?—*A.* Say from fourteen to twenty-four hours, in this country.

11996. *Q.* But as there are several other bacilli closely allied to Loeffler's bacillus, can the bacteriological testing be regarded as absolutely reliable without having recourse to the guinea pig?—*A.* Not absolutely; but it is a near approximation to the truth, which is of great value for practical purposes.

11997. *Q.* How long would it take to complete a test for injecting a guinea pig?—*A.* About four days.

11998. *Q.* In relying upon the bacteriological testing, is not the medical attendant placed in a somewhat invidious position; does not the delay in waiting for the report rather militate against the chances of cure?—*A.* I have just previously stated that I thought the injection of the serum in the case of a patient that was suspected to have diphtheria was the best treatment in his interest.

11999. *Q.* But then that would be dispensing with the testing altogether?—*A.* No, because you can inject one dose. The earlier you can treat with antitoxin the less is necessary. You can give a moderate dose of antitoxin and then if the diagnosis is confirmed on the morrow by bacteriological examination, you can give more. If it is not confirmed you would give no more.

12000. *Q.* But then you must admit that this waiting for the report of the bacteriological testing does militate somewhat against the chances of cure?—*A.* I advise not to wait.

It will be seen from the above replies to questions that in insisting on the prompt use of the serum, even in suspected cases, bacteriologists attach very little importance to the risks which I have already indicated, and which they themselves admit, and that, in my opinion, is in a large measure attributable to the fact that they have no practical experience of the treatment, of the disease. As in all serum treatment they insist on the earliest possible use of the serum, because the period of prophylaxis which is believed to be induced after the injection of the anti-toxin, is admittedly so very short. In respect to diphtheria, for example, Behring, one of the great exploiters of the anti-diphtheritic serum, maintains that if a dose of diphtheria toxin, which will ultimately prove fatal, is injected into a guinea-pig, the guinea-pig will survive if an appropriate dose of anti-toxin is injected within a period of two days; but if the anti-toxin injection is delayed beyond two days, it will die. Dr. Martin, on the other hand, states that three days is the prophylactic limit. In the face of these experiments it would appear to be useless to inject the serum into patients after the third day of the disease, but in the case of patients, bacteriologists assume that the toxin is being manufactured locally in or around the membrane, and distributed throughout the system. In mild cases, with little or no membrane, and only slight patches, what right is there to assume that because the bacilli may be found on examination, they are continuously producing this toxin, and what about the numerous cases in which *Lœffler's bacilli* are found plentifully in the throat after all symptoms of the disease have entirely disappeared? In my opinion it is all so inconsequent and irrational that I may be excused for repeating Koch's famous statement which he made in respect to his tuberculin fiasco:—"An experiment on an animal gives no certain indication of the result of the same experiment on a human being."

Dealing now with statistical results, I attach very great importance to those advanced by Mr. Coleridge, referred to in the Report (Paragraph 55), namely, that the introduction of the anti-toxin treatment of diphtheria in 1895 had not been followed by any decrease in the total mortality from diphtheria per million of the population, as shown by the Returns of the Registrar-General; indeed, during several periods there was a slight actual increase. While it may be admitted that these statistics do not in themselves afford any proof or disproof of the success of the treatment, my long and extended experience of numerous localized outbreaks of the disease, both before and after 1895, warrants me in submitting it as my opinion that, had it not been for the use of the serum all over the country, the total, or general, mortality from the disease would have been reduced. So far as my experience goes, the type of the disease has certainly become milder, just as it has become milder in scarlet fever, though not in so marked a degree, and this change in type, I believe to be attributable in large measure to improved sanitary conditions of homes, and especially of the outside offices of elementary schools, as well as greater cleanliness of class rooms and the disuse of slates and slate pencils. In the earlier outbreaks of the disease, severe cases generally proved fatal within a few days or a week; they still prove fatal within that period, but after the introduction of the serum treatment I observed, in the death returns forwarded to me, a certain increase in the number of deaths from heart failure and paralysis following diphtheria, after intervals of two or three weeks or longer, and this increase I attributed to the use of the serum which weakened the constitution and the resisting powers of the patient. Indeed, the injection of the serum has such a debilitating effect on the heart, that the medical attendant gives imperative instructions that the patient be kept in the recumbent position for some considerable time after each injection. It was admitted, both by Sir W. H. Power and Dr. Martin that diphtherial paralysis had become more frequent since the introduction of the serum, and the latter contended that had it not been for the serum these paralytic cases would have proved fatal. My contention, on the contrary, is that their increase is directly attributable to the use of the serum, and that more heart and paralytic cases prove fatal since the introduction of anti-toxin than formerly. Martin. 12011, 12013.

So much for the total mortality rate; statistics, if they do not disprove the success of the treatment from the mere statistical point of view, certainly cannot be quoted in support of it. As regards the numerous other tables, bearing more especially on the case mortality, which Dr. Martin had collected from various countries in Europe, as well as from America, they impressed me as being all so much of the same pattern, that they appeared to me to be tabulated to prove two main doctrines or theories, namely, the earlier the treatment the less the mortality, and whatever the total mortality might be, there was a marked and steady fall in the case mortality. With respect to the former, I will only refer briefly to the statistics of the hospitals of the Metropolitan Asylum's Board, quoted in the Report (Paragraph 55), and first I would submit that these cases, taken in the aggregate, were all cases which had not been treated with serum until after admission into hospital; secondly, that the duration of the disease, first day, second day, third day, and so on, before the serum was used, could only have been arrived at on very vague information; thirdly, that those tabulated as being treated on the first day strongly suggested the fact that they belonged to anxious and careful parents who immediately sent for a medical man who advised their prompt removal to hospital, and that therefore they came from better class and healthier homes, and were consequently in a fitter condition to resist any ill effects of the serum; that those treated on the second day came from slightly poorer homes, had less anxious parents, and therefore possessed slightly lowered powers of resistance; and that those treated on the later days represented *pari passu* a greater reduction of home comforts and food, and greater lack of parental care, and therefore greater deterioration of bodily condition. On the other hand, there are these further considerations to be taken into account, that the earlier the treatment, the earlier the advantages of careful hospital nursing, medical attendance, other kinds of appropriate treatment, and all the favouring influences arising from removal to hospital. These advantages would tell less and less the later the day of admission and treatment, apart from the inference that the patients were physically in a much worse condition the longer they remained at home, ill, poorly fed, and more or less neglected, until the parents could no longer postpone the duty of calling in a medical man. If, however, as I contend the serum exercises an injurious effect on the constitution, however slight, it is legitimate to conclude that the weaker the condition of the patient the more pronounced would any ill effects from serum become. It is also to be noted that these hospital cases *en masse* are in a measure not representative of all the cases of diphtheria which occur, because in some cases the disease is so virulent that the patients are too ill to be removed and die at home, while others are so ill or moribund when admitted into hospital, that serum treatment is not given, or if given, and death immediately ensues, they probably would not be included in the statistics. For these reasons I cannot attach much weight to this table.

As regards the other table applying to the gradual reduction of the case mortality in successive years; that table, if it proves anything at all, only serves to prove that the type of the disease must have become milder after the introduction of the serum treatment because the serum factor is constant throughout, though it might be construed as likewise indicating that in the earlier years the results claimed for the treatment were not so marked as later on. Further, as stated in the Report (Paragraph 55), both Sir Henry Power and Dr. Martin admitted that the wider recognition of mild or atypical cases by bacteriological examination had the effect of increasing the number of cases notified and admitted into hospital, and whether this had any estimable influence in the case-mortality rate of the later years or not, it no doubt
Smith, S. F. 13249. operated to some considerable extent in diminishing the rates of the anti-toxin years when compared with the rates of previous years. Then too, as pointed out in Mr. S. F. Smith's carefully prepared *précis*, it is clear that the anti-toxin treatment was introduced at a time when, both in this country and abroad the mortality rate of diphtheria was on the down-grade, and this would tell, as it did tell, as shown by Mr. Smith's tables in favour of the statistical results applying to the anti-toxin period. I may also be permitted to say in passing that I was greatly impresesd with the destructive criticism of the assumed *rationale* of the anti-toxin treatment
13229–60. of diphtheria contained in Mr. Smith's *précis*, and the well-ordered collection of facts and figures on which he based his exposure of the abounding fallacies which permeate alike the theory and the practice of serum treatment, in all of which I fully concur.

Other witnesses such as Dr. Arabella Kenealy also expressed themselves strongly antagonistic to the serum treatment, but I feel bound to admit that the evidence of almost all the eminent professional witnesses who appeared before us was as uncompromisingly in support of it as was that of Dr. Martin, who represented the consensus of bacteriological opinion. Some bacteriologists go so far as to recommend that when the throat conditions represent a mixed infection, as they often do, a polyvalent, or "blunderbus" micrococcal serum should be used in addition to the anti-toxin serum.

That the incidence or severity of the disease varies greatly with time and locality, and further that the case mortality is not a reliable index of the assumed success of the serum treatment even in different parts of this country, are statements which I venture to think are sufficiently substantiated by the following statistical details submitted by myself to Dr. Martin for his opinion during examination, as a set-off against his own tables, and the conclusions which he based upon them :—

Martin, 12015. *Q.* (*Dr. Wilson.*) Though the general death-rate from the disease has gone up, the case mortality rate in the county of London, according to the statistics which you furnish, has gone down from over 20 per cent. in the pre-anti-toxin days to about 10 per cent. in the last few years. That is correct, is it not ?—*A*. That is correct.

Q. I see also that you refer to the statistics collected by Dr. Armstrong, Medical Officer of Health Martin,
for Newcastle, in various towns in the north, centre, and south of England. You give that table ?—*A*. Yes. 12016.

Q. These statistics show, do they not, that although there has been a reduction in the case mortality rate, 12017.
it is not nearly so marked as it is in London ?—*A*. That is so.

Q. And you attribute this, do you not, to the fact, or to the assumption, rather, that the anti-toxin 12018.
was not so freely used in these other towns as in London ?—*A*. I think that is a probable interpretation.

Q. But is it not the fact that almost all medical men now use the anti-toxin ?—*A*. I think the majority 12019.
of medical men use the anti-toxin. But of that I have no way of judging except by the amount of anti-toxin which is sold.

Q. Now I have here a very short extract from a paper recently published, which was read at a ses- 12020.
sional meeting of the Sanitary Institute by Dr. Hibbert, who is Dr. Armstrong's deputy in Newcastle, and is also physician to the isolation hospital. In that paper he deals with the statistics which you have collected, but groups them in a different form. For example, during the five years from 1900 to 1904, in eighteen northern towns the average case mortality rate was 24·7 per cent, in five midland towns it was 15·7 per cent, and in eight southern towns 13·1 per cent; and in London you say it is now 10 per cent ?—*A*. Yes.

Q. That table shows, does it not, that even during the years 1900 to 1904 the case mortality rate of 12021.
diphtheria was even higher in the eighteen northern towns than it was in London before the serum treatment was introduced. For example, before the serum treatment was commenced in London it was about 24 per cent ?—*A*. Yes, 20 to 24 per cent.

Q. And in the eighteen northern towns during these recent years, 1900 to 1904, it was as high as 12022.
24·7 ?—*A*. No, not quite; 18 in 1904 and 22·7 in 1900.

Q. Now he also makes these remarks in his paper :—" I may add that at the City hospital here " (of which 12023.
he is physician), "where anti-toxin in doses of from 6,000 to 12,000 units is given as a routine treatment, Dr. Harris and myself have not had evidence that anti-toxin is the definite curative agent which it has apparently proved itself to be in the hospitals of the Metropolitan Asylums Board. There is, however, this fallacy as regards diphtheria statistics, that at least one southern town " (he does not name the town) "includes amongst its cases of diphtheria those 'contacts' in whose throats the bacillus is found, but who do not show any clinical symptoms of the disease." Does not this statement somewhat qualify your inference that the much higher case mortality in the northern towns is probably due to the fact that anti-toxin is less used there. You see that they use it freely ?—*A*. I am unable to say unless I have more data.

Q. I am quoting from this paper ?—*A*. I am not complaining of the data that you have given me. 12024.
do not think the statement of this gentleman necessitates a modification of opinion on my part.

Q. But no medical practitioner, so far as I know, dare neglect the use of serum nowadays ?—*A*. I 12025.
am very glad to hear it.

As a remarkable instance of the fact that even the free distribution of diphtheria anti-toxin serum among the medical practitioners of a large town may be followed by an increase in the mortality rate per 1,000 of the population instead of a decrease, is shown by the prevalence of the disease, some ten years ago in the city of Hull. It was brought forward in evidence during the examination of Sir W. H. Power that in consequence of the prevalence of diphtheria in the city during 1901, the Corporation voted the free distribution of serum among medical men in order to check the spread of the disease, but instead of the death-rate per 1,000 of the population being lowered,
it was doubled. Thus in 1901, the death-rate per 1,000 was 0·15; in 1902, when the Power,
serum was freely distributed, it mounted up to 0·34 per 1,000; while for succeeding years, 4606.
it varied as follows :—0·30 per 1,000 in 1903; 0·24 in 1904; and 0·27 in 1905. Although 4608.
it would be unfair to suggest that in this instance the enormous increase in the death-rate was directly due to the freer use of the serum, it cannot be said that it was followed by any beneficial results. The real explanation no doubt is that the disease assumed increased virulence, but I cannot help concluding that the free use of the serum was also in some measure a contributing factor.

With reference to the general adoption of the serum treatment, not only in this country, but all over Europe and America, it may be admitted that though it might have been tentative at first amongst a small minority of the profession, it was hailed with enthusiasm, and the most honest expectancy as regards results, by almost all the hospital staffs throughout the world within a very short period after its introduction, and even general practitioners had, with very few exceptions, fallen into line long before 1900. It was therefore extremely difficult to obtain statistics from any isolation hospital in which a fair proportion of unselected cases were treated without anti-toxin which could be regarded as controls in comparing results with those of serum-treated cases. But from a continuous study of reports and other sources of information bearing on the subject, I obtained knowledge of the existence of at least one hospital in which some cases were treated with anti-toxin and others were not, in accordance with the diverse opinions as regards serum treatment which were held by the medical men who notified the cases for removal, and therefore the cases were in the aggregate, unselected. The hospital to which I refer was the Toronto Hospital, Canada. I accordingly wrote to the Medical Officer of Health of Toronto City for information on the subject, and he kindly supplied me with a copy of his annual report for 1904. In my further examination of Dr. Martin, I submitted excerpts from this report which appear in the evidence, but which he very properly declined to accept without examination. I deem them, however, to be of such importance that I feel justified in giving them here, merely premising that the few interpellations connecting the several excerpts from the report are mine, but I vouch for the accuracy of the excerpts. The excerpts are as follows :—

Martin, 12026. "Since 1894, when the first dose of antitoxin was given at the hospital, the mortality results have almost always been unfavourable. In every year, save one, the percentage of deaths in anti-toxin cases has even been higher than the ordinary hospital rate, which is of course influenced by the increased anti-toxin mortality. In 1894 the figures were respectively 21·4, against 14·07, and this state of things continued without much improvement until 1902, when the anti-toxin was about 1 per cent under the gross hospital rate." Then he gives the statistics for 1903 :—

	Cases.	Deaths.	Per Cent.
Total hospital admissions - -	565	65	11·5
Anti-toxin treatment - - -	228	37	16·2
Ordinary treatment - - -	337	28	8·3

But then he goes on to observe : "The statistics of the last decade, which can now be presented, embrace a sufficient number of cases on which to base a fair opinion of the value of anti-toxin in hospital practice.

	Cases.	Deaths.	Per Cent.
Total hospital admissions - -	5,100	667	13·0
Anti-toxin treatment - - -	1,132	181	16·0
Ordinary treatment - -	3,968	486	12·2."

And then he says: "In a report of this kind any speculations as to the cause of the pronounced failure of a remedy from which so much was expected would probably be deemed out of place. A bare statement of fact is therefore presented, and this can, if necessary, be supplemented or substantiated by a large accumulation of original and classified evidence of unquestionable reliability." Do you accept that report ?—Not without examination.

It will be seen from the above extracts that while doubts were entertained by the staff of the Newcastle Isolation Hospital as to the relative value of the serum treatment, the experience of the Toronto Hospital during the decade succeeding the introduction of the treatment goes to prove that in the aggregate patients suffering from diphtheria would have a better chance of recovery were no serum used.

12028. In reply to other questions, Dr. Martin stated that he would always recommend its use as a prophylactic in every household in which a case of diphtheria cropped up, and
12356. he further stated it as his opinion that if a patient in the early stage of the disease, received a dose of anti-toxin, no matter of what intensity the infection might be, the case would not prove fatal. Surely no experiments on animals could ever inspire a larger hope or more assured confidence than is contained in these two statements.

Tetanus.

This disease as stated in the Report (Paragraph 56) is admittedly an infectious disease under certain conditions, and as the usual habitat of the assumed casual agent, the *tetanus bacillus*, is polluted soil, and especially soil polluted with horse-dung, or road dust containing particles of horse-dung, the disease constitutes a remarkable exception to the theory that infectious diseases do not originate *de novo*. It is noteworthy too, that though wounds are common, and abrasions from falls on dusty roads are common, cases of tetanus are extremely rare. As already stated, the serum has fallen into disrepute as a remedy, but before it was given up altogether, there were some surgeons who possessed the reckless daring of trephining the skull of the patient, and injecting it under the *dura mater* into, or on to, the brain substance just because the treatment had failed when the serum was injected subcutaneously. And, here, I may be permitted to say, in passing, that this surely is a very significant instance of Professor Starling's famous dictum,—"The final experiment must always be made on man."

But though the serum has failed as a remedy, the bacteriologists soon discovered a way out by stating that it had not the same chance of success as in diphtheria, because the tetanus toxin seriously affected the nervous tissues before the symptoms of the disease declare themselves. But why it may be fairly asked was not this forcibly pointed out before, instead of encouraging the profession to exploit the remedy on what turned out to be a very haphazard basis? Having failed as a remedy, it is nevertheless still vaunted as a prophylactic, and, as shown by the evidence of Sir Henry Morris, chiefly in America.

So far as this country is concerned, Dr. Martin admitted in evidence that large Martin, quantities of the serum were shipped out to South Africa during the late war, but 11981. as only three cases of tetanus occurred throughout the war, it was not required, and 11979. had to be thrown away. Clean surgery, he admitted, prevented the occurrence of tetanus, notwithstanding the enormous number of wounded, and I therefore contend that the prompt cleansing or washing of all wounds is the only natural prophylactic. As already pointed out, it is strongly advocated in veterinary practice, and Mr. Stockman, of the Board of Agriculture, has such implicit faith in its prophylactic value, that he stated he would recommend its use in all serious cases of broken or cut knees of horses, and also before the castration of colts, while to get over the admitted defect of the short prophylaxis which it confers, he further stated that he would recommend its use in repeated doses till the wounds were healed.

But it does not follow that because either a man or a horse, when suffering from a wound or abrasion, escapes an attack of tetanus, after being injected with anti-tetanic serum, he would suffer from tetanus if he were not injected. As it failed as a remedy, I contend that on the same grounds, it is valueless as a prophylactic.

Dysentery.

This disease, which appears next in the Report (Paragraph 57), demands only brief reference. Although the so-called specific bacillus was discovered as far back as 1898 by Shiga, in Japan, and the usual horse serum, on bacteriological lines, manufactured and put upon the market, it has only been tried to a limited extent in Japan and Manila. Dr. Martin 12031. admitted in evidence that it had not been used to any extent in India, where dysentery is so prevalent, and the very fact that it failed to appeal to the Army medical men and experts there, who have shown no lack of trust in all kinds of vaccines and sera, is, in my opinion, sufficient evidence in itself of the grave doubts which are entertained by bacteriologists as to its usefulness. As in diarrhœa, dysenteric stools must contain numerous other bacteria apart from Shiga's bacillus, and in that respect must partake more of the character of a so-called mixed infection.

Malta Fever.

The practical eradication of this fever from the garrison in Malta has been so persistently paraded as one of the greatest triumphs of experimental research, that it may appear to be more than presumptuous to question either the specificity of the *micro-coccus militensis*, the assumed causal agent of the disease, or the *rationale* of the experiments on which preventive measures were based. But, as expressed in a question, Bruce, which I put to Sir David Bruce in the course of his examination, it does appear to me to 14523. be a very exceptional bacteriological discovery that a pathogenic bacterium, not a protozoon, should be excreted in the milk of an apparently perfectly healthy animal, as the goat yielding the milk is admitted to be, and that this micro-organism should be labelled as the sole *causa causans* of Malta fever. The conditions of its habitat are so utterly unlike those attaching to the tubercle bacillus, so far as its assumed pathogenicity when detected in cow's milk is concerned, that there are ample grounds for doubting not only its specificity, but that goat's milk, as milk, plays any part in the causation of the disease. In the case of tuberculous milk, as has been previously pointed out, the bacillus is believed to be excreted in the milk either because the cow is suffering from some kind of generalised tuberculosis, or tuberculosis of the udder or teats, but in the case of the Maltese goat, there is no disease of udder or teat, nor does the goat suffer from any febrile disturbance or other ailment. The only other instance of a pathogenic bacterium being excreted by a healthy, or presumed healthy animal, which was brought forward in evidence, was that of the rat-flea excreting the plague bacillus already referred to. In this case, however, the flea is stated to be playing the part of an intermediary host, and that likewise is altogether exceptional, so far as the

rôle of these so-called pathogenic micro-organisms of vegetable origin are concerned. Then, too, it seems very singular that the cultures of this bacterium produced no specific effects when injected into any other laboratory animal except the monkey. These experiments of injecting the *micro-coccus militensis* cultures into monkeys, after having discovered it in the tissues or secretions of patients dying of Malta fever, were made as far back as 1885, and according to Sir David Bruce, proved, on the basis of Koch's famous postulates already referred to, that this was the *causa causans* of the disease. But though the causal agent was believed to be discovered, preventive measures remained at a dead-lock for nearly a decade. The mosquito was at one time
Bruce, strongly suspected, then the late Captain Hughes, after a large number of careful
14409. investigations, came to the conclusion that it was due to a specific micro-coccus emanating during hot weather from a saprophytic existence in soil polluted with the fæces and urine of those suffering from the disease.

In order to investigate all the factors conducing to the propagation of the disease, an experienced officer from the Medical Department of the Local Government Board, was also sent out to Malta, and though he spent six months hard work on the inquiry, during which, as stated by Sir David Bruce, he lost four stone in weight, he
14261. came to the conclusion that milk, at least, was not a contributory factor. Others were sent out by the Army Medical Department, and among them Colonel Davis, who also agreed that milk had nothing to do with the disease, and Sir David Bruce laid great stress on these failures as stated in the Report (Paragraph 58) as proving the difficulty of discovering the cause otherwise than by experiments on animals. Ultimately, by a sort of accidental inspiration, so it would seem, it was proved that on injecting cultures of the micro-coccus into goats, it could be found in the blood and milk, and finally, on testing other goats as controls, it was also found in their blood and milk, though it could not be said that they were suffering from any diseased condition, not even Malta fever. The udders were healthy, and the animals appeared sleek and healthy, and yet in large numbers of them the specific organism was found to swarm in their blood and milk. Monkeys were fed on the milk, and Sir David Bruce maintained that they presented all the appearances of suffering from the disease. The supply of goat's milk was stopped as the most appropriate preventive measure, and preserved milk issued instead, and so the disease was eradiated ten years after Sir David Bruce discovered the causal agent, and ten years before he finally reported that he had conclusively proved that it was contained
14407. in goat's milk—though Captain Hughes advanced the claim of priority in discovering that the milk was to blame.

While I admit that it is quite possible that goats' milk may induce febrile symptoms on account of its containing a considerable quantity of filth particles, because Sir David Bruce stated that the large bags of the goat's udders almost trailed on the ground, I am unable to accept his theory in spite of his experimental work on the etiology of the disease, for several reasons :—Firstly, there is the exceptional *rôle* played by the healthy goat in secreting milk which is believed to be productive of a specific fever. Secondly, if goat's milk had been the principal factor in the propagation of the disease, I am strongly of opinion that it would at all events have been laid under suspicion by the Local Government Board sanitary expert who was sent out to investigate, or subsequently by Colonel Davis and the other army men who were instructed to make further independent inquiry. Thirdly, Sir David Bruce admitted that almost up to the date of his final investigations, Malta harbour was the receiving basin for the drainage and offal scourings of the town, garrison, and men-of-war, and must have been in a filthy condition, giving off foul effluvia. Fourthly, he also admitted that while there was much looseness in diagnosing typhoid fever, relapsing or continued fever, and Malta fever, there was a gradual fall in all three, and a marked reduction more especially in continued fever cases, concurrently with the abrupt disappearance of Malta fever cases. Fifthly, Sir David Bruce submitted in confirmation of his theory, that the disease was propagated by a number of Maltese goats which were shipped, first to Antwerp, there detained for a time, and finally transhipped to America, but on examination by Sir William Collins, he also admitted that there were several of the crew on board the "Joshua Nicholson," on her voyage to Antwerp, who
14401. were not affected, and that though the milk was partaken of freely by the staff of the quarantine station and others during the five days the goats were interned in Antwerp, and was subsequently drunk by the crew of the vessel which conveyed them to
14404. America, there was no positive evidence of the occurrence of cases of Malta fever either at the quarantine station, or during the voyage.

In addition to these reasons for being unable to accept Sir David Bruce's theory, there is this further consideration to be taken into account, that goat's milk is largely used at Gibraltar, Kimberley, in Manila, in India, and in other parts of the world, and Sir David Bruce admitted that he had made no special inquiries as to the prevalence of Malta fever elsewhere.

Even at the risk of being classed among the unscientific, I am constrained to come to the conclusion that Malta fever is in large measure, and notwithstanding its association with the *micro-coccus militensis*, a filth disease, whether generated by the foul effluvia given off by the harbour, or the ground air of polluted soil, or by both, than by the milk of apparently healthy goats. Fortunately for the garrison in Malta, no vaccine or serum has been successfully exploited from cultures of the micro-coccus, either as a prophylactic or cure for the disease, but as stated in the Report (Paragraph 58), they have been tried and failed.

Snake Poison.

As there is no specific micro-organism in snake poison, though different snakes eject in their bites specifically characteristic venom, it is difficult to conceive even on scientific grounds, why the serum of an animal which has been bitten should, on injection into another bitten animal, impart any degree of immunity or protection, yet in the earlier days of experimentation this was believed to have been clearly established. Now, it is conceded that the serum of an animal bitten by a particular kind of snake can only confer a limited degree of immunity against the venom of a snake of the same species. But admitting that this was proved beyond question, such a serum or series of sera would not be of the slightest practical use, because they would none of them be available when a person is bitten, and there might be some doubt as to the species of the biting snake, as well as to the serum which should be used. Even a polyvalent or "blunderbus" serum would not be of the slightest avail, nor will the lancet and permanganate of potash antidote devised by Sir Lauder Brunton be always ready at hand, seeing that about 20,000 deaths occur from snake bite in India annually, and probably more than double that number of natives are bitten. The usefulness of that antidote, however, was questioned by Dr. Martin on the authority of Wall, as stated in the Report (Paragraph 59). One can quite understand that immediate tight ligature above the bite, and excision of the tissue surrounding the bite whenever possible, might be of service, but these scientific remedies can only be ready at hand and available for use in the animal laboratory.

Work of Public Health Authorities.

Taking the above as the next heading in the Report, (Paragraph 60), there is no need to supplement the comments which I have already ventured to submit concerning the research work entailing experiments on animals carried on by direction, and under the authority, of the Local Government Board, beyond repeating that in my opinion the results have on the whole been very disappointing. Although Dr. Klein himself, like his co-workers, was employed all along on the "piece-work" system, as Sir W. H. Power phrased it, there can be no doubt he was the guiding expert on whose advice successive Medical Officers of the Board relied as to the kind of experiments which were from time to time deemed necessary for investigatory, prophylactic, or administrative, purposes. Those experiments were naturally for the most part inoculation experiments, and as it was well-known that Dr. Klein had expressed views before the 1875 Commission concerning the infliction of pain which placed him under the suspicion of those opposed to experiments on animals, it need hardly be said that the details of his experimental work were closely scrutinised as they appeared in the annual reports of the Medical Officers of the Board, or elsewhere, and some of them were quoted in evidence before us as having been exceedingly painful. I need not, however, dwell further on the responsibility which rested on the Board, as well as on the Home Office, in this connection, and will only refer to the remarks on the subject which are contained in the Report (Paragraph 29) and which I may here fitly repeat :—

> "we have no means of knowing whether Dr. Klein still adheres to his earlier views ; but it appears to us that to grant a licence or certificates to any person holding such views as those formerly expressed by Dr. Klein, and as those entertained by Dr. Pembrey, is calculated to create serious misgiving in the minds of the public."

But apart altogether from this phase of the subject, it must be conceded that if Dr. Klein and his co-workers did not make any epoch-making discoveries, they led the van in bacteriological research in this country, while following in the wake of advance on the Continent, and more particularly in respect to the bacteriological diagnosis of infectious diseases, and vague attempts at evolving prophylactic or serum cures. It was through the lead of the Local Government Board that the anti-toxin serum treatment of diphtheria, which was so enthusiastically adopted by almost all the Medical Staffs in the Hospitals of the Metropolitan Asylums' Board, was speedily welcomed throughout the provinces, and was encouraged by the free distribution of serum by the Board to Sanitary Authorities on application. The Board's influence too, in inducing Sanitary Authorities throughout the country to make arrangements for the bacteriological diagnosis of disease, as well as examination of milk, and other suspected articles of food, and water, was naturally of great force, and Borough County Councils, as well as Administrative County Councils, began either to appoint bacteriologists of their own, or arrange with recognised bacteriologists attached to universities and other institutions. Granted that all these arrangements constituted a move in the right direction, I venture to contend that the Board's Medical Staff should have been strengthened by the appointment of a bacteriologist of recognised position, such as Dr. Klein himself, who could have issued some instructions for the guidance of bacteriologists in testing samples of milk for example for the presence of tubercle bacilli, or testing oysters from beds suspected of sewage pollution, or in testing sputum, in respect to all of which, as already pointed out, there is admittedly great liability to error. It is well-known that there are often considerable discrepancies in the simple chemical testing of milk as to whether it is whole or skimmed milk, and whether it has been watered, but when it comes to testing samples of milk for the presence of tubercle bacilli, which necessitates the guinea-pig test, as Professor Lorrain Smith admitted, then the problem becomes very much more complicated. Indeed, I have no hesitation in stating it as my opinion from a continuous scrutiny of the results published from time to time of milk samples, that the enormous discrepancies which have appeared in different parts of the country and at different times, as to the percentages of samples found to contain tubercle bacilli afford evidence not of various percentages of actual specific contamination, but point to unreliability of diagnosis, whether in respect to method or interpretation of results, or both. I do not for a moment question good faith, and an earnest desire to be accurate on the part of the bacteriologist; I only wish to lay stress on the difficulties with which he is beset, and which in my opinion have led him more frequently to erroneous conclusions, especially in the earlier years of investigation, than to correct judgments. The guinea-pig test is by no means infallible, because so much depends on the personal factor, but unless it is resorted to, and bacilli are diagnosed solely morphologically and by staining, not much reliance can be placed on the accuracy of the certificate.

In less difficult testing, such as examining swabs for diphtheria bacilli, or specimens of blood for testing Widal's reaction in cases of suspected typhoid fever, the bacteriologist generally takes care to safeguard his interpretation of results to some extent by putting a whole series of questions which are printed on the test cases supplied for forwarding specimens, and the replies to which furnish a tolerably complete history of the patient's illness. As I have already pointed out, I attach far more value to careful and experienced clinical diagnosis on the part of the medical attendant, than I do to the verdict of the bacteriologist, because in respect to diphtheria, he is not always sure of his bacillus, even if he finds it, and the Widal test for typhoid fever, apart from the lengthy period of the illness which must elapse before it becomes operative, is going out of fashion. But why all this scientific parade about diagnosing these two diseases? The bacteriologist is unable to render the slightest assistance in diagnosing small-pox, chicken-pox, measles, whooping-cough, scarlet-fever, typhus fever, or mumps. One can quite understand that bacteriology must be a very fascinating study, but as an aid to pathological research, it fortunately has its limits, otherwise the pharmaceutical market would become flooded with such a multiplicity of vaccines and sera, that the much-tried common sense of the public would speedily revolt, and make short shrift of both vaccine and serum therapy.

Although the Local Government Board soon discontinued the manufacture of diphtheria anti-toxin serum, because other brands, some of which were manufactured in Germany, America, or in this country, became available; Sir W. H. Power stated that the Board were then engaged in preparing and experimenting with a new plague prophylactic from which they expected good results. In respect to diphtheria anti-toxin, however, many large Borough Councils became so impressed with its advantages as a remedy that they made arrangements to distribute it among medical practitioners free of charge for the use of patients who were too poor, or who were believed to be too poor, to pay for it, and the Board has recently granted permission to the smaller urban and rural sanitary authorities throughout the country to supply it under similar conditions. No doubt, it is a very expensive remedy; indeed, Doctor Martin, the Director of the Lister Institute, which prepares large quantities of all kinds of sera and vaccines, admitted in reply to a question put by myself, that the cost of the serum for the treatment of cases of diphtheria would, on the average, amount to 25s. or 30s. per patient. Martin, 11843.

To what extent Sanitary Authorities may be influenced to provide serum on economical, as well as public health, grounds, can only be surmised, but there are strong grounds for believing that in providing serum free, some of them naturally hoped to be relieved from the obligation of providing, or extending, hospital accommodation. In any case, the distribution of serum free of charge among medical men in general practice throws upon them a serious responsibility in their selection of cases for free treatment.

In contrast with the "piece-work" method of research carried out by the Local Government Board, the Board of Agriculture have appointed an experienced bacteriologist as the official head of the scientific and veterinary department, namely, Mr. Stockman, who gave evidence before us. He stated that not only was he a whole-time officer, but that he held a licence and certificate under the Act of 1876, which enabled him to experiment, or carry out investigations, either at the principal office at Whitehall, or at the experimental farm at Sudbury, or at any place where he deemed it necessary to investigate animal diseases. In addition to advising the Board in respect to all matters connected with the prevention of communicable diseases among animals, both he and his assistant conduct the bacteriological or scientific diagnosis of any cases of imported animal diseases which may be suspected by the Board's officers at the principal ports of entry, and of any suspected material of an infectious or communicable nature which may be forwarded from other parts of the country. If a dog is suspected of rabies, and dies, or is killed, it frequently devolves upon the Board's officers to test, by injecting a small portion of the spinal cord into the brain of a rabbit, as to whether it is presumably a case of rabies or not. But according to Mr. Stockman, as the test generally takes between thirty or forty days before a conclusion can be arrived at, it could only be of value for forming a decision as to whether the disease actually existed; it could be of no value in respect to the treatment by Pasteur's prophylactic of persons bitten. Mr. Stockman, in submitting his *précis* of evidence before the Commission, insisted very strongly on the value of accurate diagnosis, but, as in many instances such diagnosis can only be determined by resort to experiments on animals, the same liability to error, previously alluded to, obtains—with this difference, however, that the bacteriologists who conduct the investigations are responsible officials of the Board. It did not appear in evidence that the Board undertook the preparation of any kind of serum or vaccine, though Mr. Stockman, from his previous experience in India, and as Principal Veterinary Officer to the Transvaal Government, is a very enthusiastic supporter of vaccine and serum therapy in the preventive treatment of certain animal diseases. He admitted, however, in reply to questions put by myself that Koch and his assistants utterly failed in their preventive inoculations, in respect more particularly to horse-sickness and Coast Fever in South Africa, though the investigation cost the enormous sum of £20,000.

Stockman, 2424–38.

3125–38.

3342–8.

Rinderpest.

Taking the several diseases in their sequence, as the evidence in respect to them is summarised in the Report (Paragraph 62), the first commented on is rinderpest. As no specific micro-organism has been discovered in this disease, there are good grounds for entertaining doubts as to the success of any prophylactic serum, although Mr. Stockman maintained that after discarding a system of inoculation recommended by Koch as valueless, a rinderpest serum had proved eminently successful in South Africa and elsewhere. Fortunately this country has been clear of the disease for a good many years. 2491–3331.

Pleuro-Pneumonia.

This disease, too, has fortunately not been prevalent in this country for several years, but, as stated in the Report (Paragraph 63) a Departmental Committee appointed in 1888, under the Chairmanship of Lord Cranbrook, came to the conclusion that

"inoculation could not be recommended for eradicating the disease, nor as practicable under existing conditions."

Up to that date, although several micro-organisms had been discovered by Lustig, Arloing and others, which Mr. Stockman, in reply to questions put to him by Sir William Collins, admitted, were subsequently pronounced to be non-specific, a very minute 2701–32.

organism has been more recently discovered by Professor Nocard, which is believed to be the causal agent, and a virus has been prepared by him which has been largely used on the Continent, and is said to be protective against the disease.

But with regard to all these vaccines and sera which have been used as prophylactics in the prevention of animal diseases, I venture to raise the same objections which have been previously advanced in respect to human diseases, and for similar reasons, and need only refer to Mr. Stockman's lengthy examination by Sir William Collins (*Qs.* 2685–2787) and to questions put by myself (*Qs.* 3051–3350) in support of my objections. I desire, however, to refer more particularly to only a few of the other diseases dealt with in the Report, namely, anthrax, tuberculosis, glanders, and braxy.

Anthrax.

It was for the prevention of this disease that Pasteur's famous virus was first put upon the market, and was used so very extensively in France, and with such tremendous success, so it was stated, that few dared to question a method of inoculation which
Stockman, 2742–6. Pasteur himself based on vaccination as a protection against small-pox. But Pasteur's method was subsequently disputed by Professor Müller, of the Royal Veterinary College of Berlin, and also by Roszhageyi, in Hungary, and certainly it has never been used in this country. It is notorious that the cases of the disease which are reported in this country have for years back been scattered, or sporadic, cases, which I venture to think point either to erroneous diagnosis, or to the inference that if the cases are genuine, the disease, like tetanus, is frequently generated *de novo.* But Mr. Stockman
2670–1. admitted that there was often considerable doubt as to accurate diagnosis of the disease, and made the further striking statement that shortly after the death of an animal, the *anthrax bacillus* cannot be discovered with any degree of certainty because it speedily becomes crowded out by the other bacilli of putrefaction, especially during hot weather.
3120-4. But if this crowding out does take place in the *cadaver*, it appears to me to indicate that Pasteur's theory that the spores of the bacillus are so resistant that they can contaminate pastures for years, is open to great doubt. Koch disproved his theory that ground worms often brought up the spores of the bacillus from carcases buried under the surface; yet this dread of spores is the basis of all the precautions which are taken under the direction of the Board of Agriculture not to open the carcase, or permit a *post-mortem*, and either to cremate it, or bury it deeply in quick-lime. Why should not the Board put this spore theory of persistent contamination of pastures to the test? Then again, there is this curious anomaly about the disease, that while it is so rapidly fatal in animals, and usually presents symptoms of a generalised septicœmia, in man it may appear as a malignant pustule, which is generally curable, or as a fatal pneumonia as in woolsorter's disease, and yet, in all these three the *causa causans* of each is contended to be the anthrax bacillus, though the types of disease are so essentially different. No serum has been used for the cure of the disease in animals, but Sclavo's serum has been tried with what is claimed as a degree of success in man. Yet, according to a Report of the Medical Inspector of Factories, cases of anthrax do occur in man in which no
2669. anthrax bacilli are found, and he therefore insists upon the importance of clinical diagnosis.

In my opinion there are many problems connected with this disease, which persistent experimentation on animals during the last quarter of a century has failed to solve, alike in respect to diagnosis, etiology, and prophylaxis or cure. It seems to be such a convenient theory to be always assuming that the origin of these scattered cases which keep cropping up all over the country must be imported in some ingredient of food. That applies to cattle, whereas in respect to woolsorter's disease, there can be no question that the exciting cause is the dust or dirt from foreign wool inhaled into the lungs, and in respect to malignant pustule, it is contamination of a surface wound or abrasion by foul matter either from foreign hides or from animal carcases.

The bacillus has been the scientific mainstay of the bacteriologist ever since Pasteur's time, and though it is admitted that sporulation, or formation of spores, does not occur in the living animal body, and that the bacilli themselves are readily killed by the gastric juice, the spores are very resistant, and have long been used as tests for germicides. While some animals are very susceptible to inoculation with cultures of the bacillus, such as the ox, sheep, pig, and guinea-pig, others such as the carnivora, birds, and amphibia, are very resistant. The brown rat is susceptible, while the white rat is immune. Anthrax with its bacilli and spores has perhaps been studied more closely and continuously on laboratory experimental lines than any other bacterial disease. Why not devote more attention to the disease as it occurs under natural conditions? It may yet be discovered that the bacillus is an anærobic saprophytic, or soil, organism, and that the disease is a form of septicæmia.

Tuberculosis.

It has always appeared to me to be very singular that Koch's tuberculin, which failed so lamentably as a cure for phthisis, was speedily exploited, not as a test for tuberculosis in man, but as a test for tuberlosis in cattle, and perhaps no one did more to popularize this test among veterinarians than did Professor Bang, of Copenhagen. By the continuous testing of dairy cows, he contended that he could isolate the sick from the healthy, and with such success as to gradually stamp out the disease from dairies, and by adopting these measures there is no doubt that directly or indirectly he assisted very largely in booming the Danish butter trade a good many years ago. Indeed, so widely extended was the interest taken in Bang's methods, that many municipal committees visited Denmark to inquire and report, and the first Tuberculosis Commission also sent some of their members to investigate on the spot. In my examination of Mr. Stockman, I submitted extracts from the Report of that Commission for his opinions which I now beg to quote, together with some other questions and replies bearing on the tuberculin test, which I venture to think are very instructive :—

Stockman.

Q. (*Dr. Wilson.*) Is it within your knowledge that many deputations from public bodies went over from this country to Denmark to see the precautions which were so religiously taken ?—*A.* I believe that is so. 3207.

Q. Do you know whether a deputation from the Tuberculosis Commission went over too ?—*A.* I can- remember that. 3208.

Q. I have their report, I think. May I first read this and ask your opinion ? This is the Report of the Royal Commission on Tuberculosis, 1898. It says: " The process pursued has been as follows :—All the bulls, cows and calves are kept under one roof, an extensive building, stalled across its breadth, with roomy gangways before and behind each row of stalls. At the time of our visit (May 4) none of the animals had been out of the building since the preceding October, though the season was approaching when they would be turned out to pasture day and night. It must be admitted that, in spite of its large extent and scrupulous cleanliness, the ventilation of this great byre was far from exemplary. The temperature was kept very high, probably to induce the liberal secretion of milk; the cubic space to each animal seemed insufficient (it was stated to be about 300 cubic feet per animal), and swarms of common house flies on the side of the building furthest from the entrance doors seemed to indicate that a high temperature had been maintained throughout the winter. If this was the case on a spring morning, with the doors all open, the condition of things must be very much worse in winter. The stock, however, looked exceedingly well and blooming. Although, as we have said, they were all under one roof, the building was divided transversely by a movable wooden partition, without a door in it. This was put up to divide those animals which did not react from those which did. Each year, as the proportion of sound animals has increased (as shown in the subjoined table)," with which I will not trouble you. " The partition has been moved further on, until, at present, the reacting animals occupy the smaller portion of the building. On the night previous to our visit the sound part of the herd had been injected with tuberculin, and when we arrived, the staff, assisted by a number of schoolboys from the village, were taking and registering the temperature " ?—*A.* Yes, I remember that part of the Report now. 3209.

Q. " With the result that out of 155 cattle and calves tested only six reacted " ?—*A.* That is Bang's process of eliminating tuberculosis from a herd. 3210.

Q. By a movable partition ?—*A.* Yes, that is one method. He preferred to have them removed to another stable altogether. 3211.

Q. Now as regards this extract, the cubic space was miserably insufficient, was it not ?—300 cubic feet ?—*A.* I think 300 cubic feet is a small amount, but I do not want to give the impression to the Commission that I think cubic space has anything to do with tuberculosis, unless you introduce tuberculosis into such a stable. 3212.

Q. Would you think a moveable partition and separating the reactors from the herd a sufficient means ?—*A.* No, not unless it prevented all communication. I do not think a wooden partition is sufficient but you could make a partition like a sufficient watertight bulkhead. 3213.

Q. You would not regard it as affording efficient isolation ?—*A.* No, I would not regard it as affording efficient isolation. 3214.

Q. Nor would the temperature and flies meet with your approval also—a very high temperature ?—*A.* No but I may say that in a dairy you cannot expect to get a supply of milk unless you keep the temperature up to a certain height, and dairymen do it. 3215.

Q. And as to a number of schoolboys assisting the staff in taking the temperatures, would you not conclude that that fact alone would rather tend to interfere with the accuracy of the results ?—*A.* No, I do not think so, because I know in our laboratory in Pretoria, for labour we had to use Kaffirs, but they did not read the thermometers; they took the temperatures and then white men came along and read them, and I should think that would be what was done by the children. 3216.

Stockman, 3217. *Q.* Those were Kaffir men ?—*A.* Yes.

3218. *Q.* But these are boys ?—*A.* I expect they simply held the thermometers, I think that is allowable. It is the reading of the thermometers that is the main thing.

3219. *Q.* I think you said that the test is mainly used for testing animals for exportation—stock animals for sending abroad ?—*A.* No, not mainly; it is used very largely for that purpose.

3220 *Q.* But that test would not be regarded as a fair test on animals landing ?—*A.* No, if they have to test them on arrival they keep them for some time in a stable. And lately, I may say, they have kept them as long as forty days in one country.

3221. *Q.* Then the certificate or testing on this side is accepted as sufficient, I suppose, on the other side ?—*A.* It depends on the country. In our own Colonies it is accepted, or rather may be accepted. But on account of one or two unfortunate transactions, in the Argentine, for instance, it has not been accepted of late. They suspected, to put it plainly, dealers on this side of trickery.

3222. *Q.* But all the certificates are paid for on this side—the veterinary surgeons are paid for them ?—*A.* I think that is so—even for our Colonies. I know they are often paid for on this side.

3223. *Q.* Then do you think it is possible to stamp out tuberculosis among cattle by a free and judicious use of tuberculin, so long as cows are kept tied up in byres for months together ?—*A.* Medically speaking, —do you mean, for I know the financial question is gigantic ?

3224. *Q.* But theoretically speaking or practically speaking ?—*A.* Yes, I think it is possible with slaughter —tuberculinisation, slaughter, and isolation—to stamp out tuberculosis in cattle.

What struck me most in that Report was the exceedingly small cubic space per head, only 300 feet, and the general lack of proper ventilation in the Danish byres. If cows are continuously stalled for milking, a cubic space of 800 feet is small enough even with good ventilation, because in my opinion overcrowding and lack of proper ventilation in cowsheds have always contributed largely in the propagation of bovine tuberculosis. Another regrettable admission was that in using the test on animals for export, dealers on this side were suspected of trickery (*Q.* 3221). But apart from any temptation in that direction, I have all along ventured to contend that this much-vaunted test for tuberculosis in cattle, is not a specific test at all, and only possesses relative value, because tuberculosis is the disease which of all others is most liable to affect cows or cattle stalled in byres or cowsheds, and even a non-tuberculous cow, if her health is out of condition, will react. For example, Mr. Stockman admitted that tuberculin could not be used in testing cattle at markets, shows, or when landed on
3159. importation, because the animals being in an unstable condition, would all be more or less
3185. liable to react, even if none of them were tuberculous. He admitted too that a cow, if suffering from a slight cold, would in all probability react; that even a diseased animal, if injected repeatedly, would cease to react; and that an animal in the advanced stage of the disease would very likely not react at all. Tuberculin is a toxin which on injection into a normal animal will produce a reaction or rise in temperature if the dose is large enough. Indeed, the limitations to the utility and reliability of the test are so considerable, that, as Mr. Stockman admitted, the testing should only be entrusted to very experienced veterinary surgeons, and that the temperature must always be carefully noted both before and after the test. It is noteworthy, too, that when tuberculin was first adopted as a test for tuberculosis in animals, it could not be used on man on account of the disagreeable symptoms which were induced, and though a different preparation is now being tried, as already stated, in a tentative fashion, not only as a test, but as a cure for human tuberculosis, I still contend that its assumed curative properties, as well as its properties as a test, should have been first established on animals such as cows or monkeys known to be suffering from the natural disease. Mr. Stockman further admitted, as shown in the above extract, that it is only possible to stamp out the disease among cattle by slaughter, in addition to tuberculinisation and isolation. In spite of all the obscure theories concerning the production of anti-bodies, precipitins, agglutinins, and opsonins, in which the tuberculinisation of man or cow is enshrined, in my opinion tuberculin, no matter what the brand used may be, is a "broken reed to lean upon," whether as a test, or cure.

Glanders.

As stated in the Report (Paragraph 71) mallein, which is a preparation made from cultures of the bacillus of glanders, is recommended, and has been largely used as a test for that disease when it is suspected to affect horses in the "occult" form. But as the same careful precautions have to be taken as to temperature of the horse, and risk of catching cold in applying the test, which have already been explained in the "tuberculinisation" of cows, there attaches to its use the same element of unreliability.

Mr. Stockman admitted that "reactors," as they are called, have been freely slaughtered, Stockman,
just because they reacted and had occupied the same stable in which other horses were 3241–5.
found to be suffering from the disease. In some stables this surely would become a
somewhat expensive, and, I venture to think, would be an altogether unnecessary, preven-
tive measure. He also admitted that though great care must be taken in applying the 3248.
mallein, as well as the tuberculin, test, the Board of Agriculture had not issued any
instructions in respect to the use of either of them. He further stated that though
mallein had been tried as a cure for the disease, it had utterly failed, and that 3258–61.
neither it nor any other vaccine or serum had proved the slightest use as a prophylac-
tic. But if mallein has failed as a cure for glanders, what grounds are there for hoping
that its allied product tuberculin can be relied on as a cure for tuberculosis in man or
cow, no matter what the modifications in the methods of its preparation may be.

Tetanus.

I have already submitted my reservations regarding the use of anti-tetanic serum in
respect to tetanus as it occurs in man, and will merely state here that the same line of
criticism applies to its use in animal tetanus. But though it has admittedly failed as a
cure in human tetanus, Mr. Stockman would still advocate its use as a remedy for
the disease in animals, and is so enthusiastic regarding its *rôle* as a prophylactic in any
horse suffering from broken knees, or wounds, after castration or docking, or any other
operation, that he would go on using it till the wounds were healed, because the period of
prophylaxis is so very short; indeed, according to Mr. Stockman, it only lasts ten days. 3302.
But, as previously stated, it does not follow that because a horse with broken knees, or a
wound after an operation, escapes tetanus after being injected with the serum, he
would contract the disease had he not been injected. Mr. Stockman was unable to
produce any statistics, applying to the disease in this country, which would furnish
any reliable comparisons between the prevalence of the disease before and after the 3289.
introduction of serum. The statistics on which reliance is placed are chiefly foreign
statistics, and I certainly would not be prepared to accept them without the closest
scrutiny, and also inquiry as to how the remuneration for injecting the serum is met
and expended.

In respect to testing or protecting animals sold for export, it appears that different countries have different regulations, and I will content myself with quoting the following examination of Mr. Stockman by Sir William Collins, as showing the risks attending these protective inoculations in respect to swine fever and also in respect to stock animals sent abroad :—

Q. (*Sir William Collins.*) Has slaughter or isolation no effect upon swine erysipelas ?—*A.* It has some 2754.
effect; but I do not think it has any very great effect, because the cause is a microbe of the soil. Given an outbreak of the disease on one farm, you must not allow the animals to go on to another farm. But that would not stop the spread of swine erysipelas.

Q. Have untoward results accompanied the protective inoculations for swine erysipelas ?—*A.* Yes; there 2755.
have been accidents.

Q. Are you aware of instances in which 5 per cent of the vaccinated died, or something approaching 2756.
that percentage ?—*A.* Yes, that is so. But I may say that my evidence is based on the average, and taking those accidents into account.

Q. Then you spoke of the hopeful prospect of being able to immunise all animals, especially those 2757.
introduced into new countries, against these various diseases ?—*A.* Yes; these tropical diseases.

Q. How many of these preventive inoculations would be necessary ?—*A.* For each case, do you mean ? 2758.

Q. Yes, for each animal such as cattle or sheep ?—*A.* You mean before you send an animal out, how 2759.
many diseases would you want to immunise him against ?

Q. I understand that, on the theory of giving artificial immunity to various diseases to which these 2760.
animals may be liable in those infected countries, you advocate wholesale immunising antecedent to their going there ?—*A.* I say that it may be possible to immunise them to an extent which will enable them to resist infection, so that a large proportion of them will live.

Q. That will mean that every animal will be submitted to a series of protective inoculations ?—*A.* Not 2761.
necessarily.

Q. Will you explain what you mean then ?—*A.* If I understand your question aright, you ask if the 2762.
animal would want to be immunised against half-a-dozen diseases.

Q. I am asking for information. I gathered that that was the result of your evidence. If I am wrong, 2763.
I shall be glad to be corrected ?—*A.* No; an animal will not necessarily have to be immunised against several diseases. What I mean is this: If we take one example, a large number of these imported animals—these pedigree animals we found in the Transvaal—died of redwater. We could inoculate them out there against

redwater; but, with that inoculation, a considerable number still died as the result of the inoculation. These were newly imported, and had come off the ship, and probably could not withstand it very well. But one can inoculate an animal with redwater in this country and of this country, and, so far as I can see at present, with very little chance of fatal results. I say so far as I can see at present, because the thing is under investigation. It is hoped that when these animals are sent out, put on to these pastures which are infected with redwater (where redwater is the disease killing the animals), they will live and reproduce themselves. That is exactly what I mean.

I do not know what results have attended Mr. Stockman's experiments to protect against redwater fever by inoculating animals before exportation, but it is safe to predict that as inoculation, after landing, failed to protect some animals when exposed to infection, protective inoculation before they are shipped, will also prove a failure.

Louping-Ill and Braxy.

These two diseases of sheep are the last animal diseases dealt with in the Report (Paragraph 72), and as the natural history of both, as well as the assumed causative bacilli have been fully described, my comments need only be very brief. The late Professor
Hamilton, 20143–54. Hamilton, of Aberdeen, who submitted special evidence in respect to these diseases, admitted that there is a family resemblance between louping-ill, braxy, black quarter, malignant œdema, and even anthrax, and that there was considerable difficulty in distinguishing them. But braxy differs greatly from anthrax in this respect:—that while the braxy bacillus is not found in the blood during life, it is found in the blood after
20254. 20256. death, and braxy is not primarily a blood disease. Although numerous experiments had been carried out by the Committee appointed by the Board of Agriculture, and by Dr. Hamilton, as Chairman of that Committee, and his assistants, it did not appear that any discovery of importance had been made beyond this—that as cultures of the bacillus, when injected, killed a good many of the sheep, it was decided to drench them with cultures instead, in order to induce immunity, but with very questionable results. Indeed, this method appeared to me to be so exceptional, and altogether so irrational, that I feel warranted in regarding it as a complete failure. I venture also to lay emphasis on the fact that while abundant scope for inquiry, and carrying out every kind of experiment which was deemed necessary for elucidating the etiology of these diseases, as well as methods of prophylaxis or cure, was provided, the Committee could only make public this unsatisfying statement:—

20163. "The problem of the prevention of the contagious diseases of the sheep is a large one, and will most likely require years of patient observation and experiment before it reaches perfection."

At the same time, Professor Hamilton held out the hope, which I have no doubt he most fully entertained, that, to quote his own words—

"It may turn out that the sheep can be immunised to several of these diseases at the same time."

That, however, I venture to contend, is an altogether vain hope, alike on theoretical and practical grounds, and apart altogether from the inference that it implies the preparation of a polyvalent vaccine or serum—the most illogical and futile of all prophylactics.

Public Recognition of the Value of Experiments.

This is the next heading in the Report (Paragraph 73), and while I need not say that I fully agree that there is such public recognition, I am of opinion that it is largely based on an exaggerated over-estimation of the usefulness of results obtained from experiments on animals. But before referring to the three instances of recognition given in the Report, I desire to direct attention to what I may term the evolution of professional and public
Horsley, 15783, 15973. opinion in respect to animal laboratory research work. This I endeavoured to elucidate in some measure by questions which I put to Sir Victor Horsley on the scholarships and annual grants for research work voted by the British Medical Association. In addition to these, there are a considerable number of other scholarships connected with universities and medical schools throughout the country, all of them admittedly the best and most approved incentives to earnest work. They are competed for and won by our ablest and best students, many of whom proceed to continental laboratories to pursue research work there. They naturally become imbued with the doctrines and creeds, physiological, bacteriological, or therapeutical, which they are taught, and when they return to this country, many of them eventually become professors or teachers in our Medical Schools while some of them take to expert bacteriological work. They contribute almost all the articles on the scientific side of medicine which appear in the medical press, and they inspire, if they do not personally indite, almost all the microbial scare articles which appear in the public press. But the ablest of them do not take up the practice of their

profession; they continue to pursue their laboratory work, and remain faithful to their laboratory creeds, while the practising members of the profession, whether consultants or ordinary practitioners, accept their doctrines, and share in the hopes which they themselves entertain, and so confidently inspire, that they will eventually discover a prophylactic or cure for every infectious disease.

The other contributory factors to this evolution of public opinion have been partly the outcome of wholesome national rivalry. France, after her most distinguished chemist, Pasteur, had evolved his prophylactic for anthrax, and his so-called cure for rabies, elevated him to the highest niche in her temple of science; then Germany, not to be behindhand, similarly elevated Koch on his great tuberculin discovery; and finally, this country honoured and elevated Lord Lister, who of the three has undoubtedly proved himself to be by far the greatest benefactor to suffering humanity, though solely on account of his surgical work. But all three made themselves famous as earnest apostles of experimental creeds involving the vivisection or inoculation of living animals. And all through this evolution of medical, and so-called scientific, public opinion, the most earnest and enthusiastic workers have been the physiologist in his incessant quest for the control of function in man by hunting for it in the animal, and the bacteriologist, who is so absorbed in the vagaries of his microbe when he injects it into an animal, that he is too apt to misconstrue its relation to man, and his many microbial ailments. I venture to think that both physiologist and bacteriologist might escape many of the fallacies and failures which characterise animal laboratory research work if they cultivated a broader outlook, and devoted a little more attention to man and the influences of his heritage and environment.

The three instances of public recognition of the values of experiments given in the Report are:—

1. *The Foundation of Schools of Tropical Medicine* and their financial aid from the Colonial Government, as well as the equipment of expeditions to investigate diseases on the spot. No one can cavil at this, so long as special attention is devoted to the natural history of the diseases which are to be investigated, whether in man or animals, and the local or natural causes are not neglected. I admit that excellent work has been carried out in the investigation of protozoal, as distinguished from bacterial, diseases, but I cannot help entertaining the gravest doubts in respect to any degree of success ever attending experimental work on animals to elucidate human diseases which are classed as bacterial, and in my opinion only failure can be predicted when the use of prophylatic vaccines or sera is attempted. The history of the experimental work on yellow fever, and the outcome of it all, as well as Koch's lamentable inoculation failure in South Africa, should serve as object lessons for all time.

2. *The Foundation of an Imperial Research Fund for the purposes of investigating Cancer.*—While one can appreciate in the highest sense the motives which prompted the foundation of this fund, and the earnestness of all who are interested in carrying out its objects, it is to be sincerely hoped that experimentation on animals is not made one of the conditions of the endowment. Cancer was one of the diseases on which a considerable amount of evidence was received, and I feel bound to agree in large measure with the evidence of Dr. Herbert Snow, who was for many years Surgeon to the Snow, 2098. Cancer Hospital, Brompton, and has therefore had an intimate and extended experience of the disease. He not only disputed that the tumours which were transplanted from mouse to mouse, in experimental research work, were identical with cancer as it occurs in the human subject, but he maintained strongly that cancer cannot be investigated in the animal laboratory, and that in short all experiments, whether to elucidate the etiology of this terrible disease, or evolve a prophylactic or cure, will continue to be utterly futile.

For years back experiments have been carried on in numerous laboratories on the Continent, in America, and in a few in this country, and they are still being carried on, but the vast majority of them must obviously partake of the nature of repeat experiments. Surely research workers themselves must begin to realise that it is time to call a halt in order that they may take stock of their work, and consider whether there is any justification for proceeding with further inoculation or transplanting experiments on animals. There is ample scope for research on other and more rational lines.

As additional evidence of the continued barrenness of results in cancer research work, I may here quote some remarks from an introductory address delivered at the London

School of Medicine for Women by the late Sir Henry Butlin, Ex-President of the Royal College of Surgeons, and published in the *Lancet* of October 7th, 1911. It is well known that Sir Henry Butlin had perhaps as wide an experience of cancer, and had operated on as many cases as any other living surgeon of the day, and this is what he said :—

"I have never engaged, personally, in experimental investigation on account of the difficulty of doing so thirty years ago. But I have been associated with the Imperial Cancer Research, and in touch with its staff from the foundation of the Research and have been a member of the Publication Committee of all its scientific reports. It has done nothing on the lines in which observation has been so useful. It has not unfolded the life-history of a single variety of cancer so that we can base our operations on the information. It has not even discovered whether spontaneous cancer of a particular part of the body in the rat or mouse runs a similar course to spontaneous cancer of the same part of the body in the human subject. These problems are not suited for experimental investigation : they are determined by observation."

The above authoritative statement may, I venture to think, fitly conclude my dissentient and somewhat discursive, survey of the beneficial results from experiments on animals, which were so strenuously claimed by the many distinguished experts and medical men who appeared before us.

3. *The Appointment of the Royal Commission on Tuberculosis.*—As reference was made to the work of this Commission when evidence was taken, I have already dealt with it under the heading of *Tuberculosis* in man.

Conclusions.

Under this last heading (Paragraph 74), I may briefly submit that I agree unreservedly with the statement that the " weight of medical and scientific authority is against the opponents of vivisection." I agree also that useful knowledge has been acquired by experiments on animals with regard to the vital functions, and in other directions. But while paying due respect to the weight of professional opinion, I feel bound to associate myself largely with the evidence of the comparatively few medical, and other, witnesses who questioned the utility of many of these experiments, and more especially in respect to the etiology, and prophylaxis or cure, of diseases associated with a so-called specific microbe or *bacterium*. I admit further that experiments on animals may still be of limited use in assisting in the diagnosis of disease, and in certain branches of test-work, as well as in other directions. But I still contend, and have endeavoured to prove, that the useful results which have been claimed, or may still be claimed, have been enormously over-estimated. I can therefore subscribe to the carefully drawn up conclusions which end this most important section of the Report, but feel bound to qualify them with certain reservations which are printed in italics. The conclusions of the Commission, together with my reservations, are as follows, and I lay special emphasis on No. 1, because, in my opinion, it dominates and largely discounts all the others :—

1. That certain results, claimed from time to time to have been proved by experiments upon living animals and alleged to have been beneficial in preventing or curing disease, have, on further investigation and experience, been found to be fallacious or useless ; *indeed the fallacies and failures are in my opinion far more conspicuous than the successful results.*

2. That, notwithstanding such failures, valuable knowledge has been acquired in regard to physiological processes and the causation of disease, and that *some* methods for the prevention, cure and treatment of certain diseases, *other than bacterial*, have resulted from experimental investigations upon living animals.

3. That, as far as we can judge, it is highly improbable that without experiments made on animals, mankind would by now have been in possession of such knowledge.

4. That, in so far as disease has been successfully prevented or its mortality reduced, suffering has been diminished in man and in lower animals.

5. That there is ground for believing that similar methods of investigation if pursued in the future will be attended with similar results ;—*failures plentiful enough still, but successful results fewer and fewer, as the field of legitimate research must become gradually more and more restricted.*

Concluding Remarks.

I have not presumed to deal with the moral or ethical side of the subject ; indeed, I think it is a matter for regret that the question of morality should ever be introduced into vivisection controversies or discussions, because it can only bear a personal, or relative, significance. I can quite understand the position of those who maintain that it is morally wrong to experiment on animals, because, in their opinion, we have no right to exact this kind of vicarious sacrifice ; but I can also understand the position of those who contend that it is morally right, for the reason, as they believe, that such experiments have contributed to the relief of human suffering, and the prevention or cure of disease. But the argument becomes very complicated, inasmuch as on the scientific side, the commercial factor creeps in. I allude to this under a sense of compulsion, and chiefly because Sir George Kekewich quoted some remarks of mine to the effect that inoculation Kekewich, experiments were steeped in commercialism. When I made that statement in a medical 20494. address, which I delivered some few years ago, I alluded to the sums made by Koch for his tuberculin, and Behring by his diphtheria anti-toxin serum. Medical men in this country are not permitted to patent or commercialise prophylactics or cures; but the servant is worthy of his hire in all walks of life, and when I say, as I do say, with all due deference, though with some reluctance,—that good appointments may be the award of earnest research in animal laboratories, whether as teachers, or as pathologists in medical schools; that there are good bacteriological appointments to be obtained connected with county councils or municipalities; that in the army, a man's chance of extra emolument and promotion is en-enhanced if he devotes himself to research work; that in respect to tropical work, there are other chances of success; and that at home an expert pharmacologist or bacteriologist may be employed on remunerative terms by large pharmaceutical companies to test medicines or advise as to the preparation of vaccines and sera, it must, I venture to think, be conceded that commercialism does exercise a perfectly legitimate, but potent, influence on the exploitation of experimentation on animals. As an ultimate award to original research, and perhaps the most distinguished of all, I ventured to allude, in a question to Dr. Thane, to the blue ribbon of the Royal Society. A good many men have earned it, and justly earned it, solely on experimental researches on living animals, and their influence in the councils of the Society, and on public opinion must always count.

And so the whole question of experimentation on animals becomes more and more complicated by confusing issues. While I deprecate the indiscriminate denunciation of those engaged in animal laboratory work as lacking in humanity and indifferent to animal suffering, I equally deprecate the charges of ignorance, and indifference to human suffering which are sometimes hurled against those who are opposed to vivisection. Indeed, I venture to think that the only sane position which can be maintained in respect to these interminable controversies is this:—that experiments on animals, no matter with what prospective gain to humanity, are repellent to the ethical sense, and that those who persistently advocate them as beneficial to human or animal life, must justify their claims by results. That is the view which I hold. I am not an anti-vivisectionist, but I dislike vivisection or inoculation experiments; and I feel convinced that far more pain is inflicted in some inoculation experiments than in vivisection experiments under Certificate B., when the animal is allowed to recover. Moreover, I am always face to face with this distressing conviction that even admitting that experiments on animals have contributed to the relief of human suffering, such measure of relief is infinitesimal compared with the pain which has been inflicted on animals to secure it. But I must not expatiate further, and will only indicate briefly some outspoken support to those who oppose vivisection, whether on ethical or other grounds, by quoting from the evidence of Mr. Stephen Coleridge. After instancing Shakespeare, and Dr. Johnson, as opponents of vivisection, he went on to say:—

"And the cause of anti-vivisection counts, and has counted, among its supporters, living and dead, Coleridge, 10263. Cardinal Manning, Lord Tennyson, Robert Browning, John Ruskin, Thomas Carlyle, James Anthony Froude Freeman, the historian, the great Lord Shaftesbury, who was the first president of the society, and filled that office till he died, George Meredith, member of the Order of Merit, General Booth, the Lord Chancellor, Mr. John Morley, member of the Order of Merit, Cardinal Gibbons, James Martineau, Spurgeon, Lord Brampton, Wagner, Leslie Stephen, Sir Edwin Arnold, Mark Twain, Tolstoy, Victor Hugo, Dean Stanley, and a host of distinguished men and women in every walk of life whose opinions upon a matter of conduct cannot be disregarded, and Her late Majesty Queen Victoria, who was in favour of the total abolition of vivisection."

In concluding this Memorandum, I beg to apologise for its somewhat discursive style and inordinate length, but I feel altogether unequal to the task of re-arranging or re-writing it. I am pleased, however, to be able to state that in spite of its length, its preparation has not entailed any delay in the presentation of the Report. While I am fully conscious that it is lacking in form and finish, it is certainly not lacking in earnest endeavour to express and substantiate impartial convictions on many of the most important issues of experimentation on animals. But whatever criticism it may evoke, and especially on the part of my own profession, I may be permitted to say that it has been prepared solely under an abiding sense of duty and deep responsibility.

GEORGE WILSON.