

that we had to deal with, we gave chloroform to the cats before the performance of the operation, and then when the effect of the chloroform went off, tested the presence or loss of sensation in the parts supplied by the nerves that had been divided.

4645. You have not only been for some years professor of physiology, but you were for a long time I think one of the demonstrators in the Great Anatomical School of Edinburgh, were you not?—Yes.

4646. And you must have had an opportunity of seeing a great deal of what is done there?—Yes.

4647. Now did it ever come to your knowledge that a person pursuing an experimental inquiry would, in such an alternative as I have just placed before you, hesitate for a moment as to what he would do?—I do not believe he would.

4648. At any rate he would not justify it if he did?—He would not. It is perfectly possible that he might have something to do which he thought would give so little pain, that he might deal with it in the same way as we surgeons deal with cases every day of our lives, as I have often dealt myself in operating on living human beings, namely, take into consideration whether the amount of pain that was to be given was worse to bear than the giving of the anæsthetic. I am quite sure that anything that could be by any twisting of the word called torturing an animal would not be done without giving an anæsthetic. That is my own opinion.

4649. Have you seen any of the proposals that have been brought forward for the regulation of vivisection for scientific purposes?—I have not.

4650. (*Mr. Hutton.*) As to demonstrative experiments generally, has your attention been called to this Handbook of Physiology edited by Dr. Burdon-Sanderson, that has been recently published?—Yes, I am familiar with it.

4651. It contains a great number of painful experiments. Would you regard the larger number of those experiments as fit to show a class by way of demonstration?—If I were lecturing myself, and exercising my own judgment, it would be a very select class indeed to which I should show a very great number of those experiments; and that for the reason which I have already mentioned, that the great majority of students have their time taken up so much with other things. But if I were bent upon sending out thorough physiologists, I think I could have little doubt in considering that handbook as a very useful one to go by.

4652. And that even for purposes of demonstration and teaching; not for purposes, of inquiry, but for those of demonstration?—Yes; that they might see, and not take things on hearsay.

4653. Did I not understand you to say that you had studied under Ludwig, at Leipzig?—I said that I had seen their method of pursuing studies there. I have not myself studied under Ludwig.

4654. There, I suppose, a very much larger number of painful experiments are used in demonstration than are generally used in England and Scotland?—The

majority of the experiments that are performed in the Leipsic establishment are for purposes of inquiry, for purposes of research, but they do also illustrate before a class.

4655. And you are in favour of extending the system, at least for classes of physiologists as distinguished from medical men, I understand, of a much larger demonstration of painful experiments?—I am in favour of every professor of physiology having nothing but physiology to teach; and where he has nothing else to teach I am sure that he would be better able to demonstrate it by vivisection than I am able to do; but from various motives the chairs of anatomy and physiology are combined in the Queen's Colleges.

4656. That you consider a great evil, as I understand?—I think it is too much for any one man to teach.

4657. But if you had a separate class, for separately educating physiologists as distinguished from medical students, you would not scruple to use a great number of painful experiments by way of demonstration?—I should certainly consider it my duty to perform certain experiments.

4658. (*Chairman.*) Did I rightly understand you to say, with regard to the frog, that though you did not administer anæsthetics, you either what is called pithed the frog, or removed the brain, or took away the sensibility of the frog in some manner of that sort?—Yes. I did not understand the question about anæsthetics to reach to that; I do not say but what the word may be used as applying to these procedures, but it did not occur to me at the time. I thought of giving chloroform, and so on.

4659. It did occur to you, however, as I understand, to remove whatever sensibility there may be in the frog by removing the brain?—Yes.

4660. You have just been asked whether you would, if you were a teacher of physiology only, perform a considerable number of experiments of a painful nature; but I want to know whether the consciousness of the mammal would not be removed by chloroform, or some other anæsthetic, before the experiment was performed?—Certainly, wherever that could be done; but I think there are painful experiments that may be performed even on mammals. I am not prepared to limit even the performing of painful experiments; I would not lay down a rule; I think it is for each physiologist to judge for himself. I think that the more educated physiologists are the better they will be able to perform experiments in such a way as to get the knowledge with as little pain to the animal as possible. It is always an object to damage the animal as little as possible in an experiment, and I think it is quite possible that in the present, I may call it transition stage of physiology in this country, there is many an animal treated with less skill than it would be if physiology were as well studied as it is in a number of Continental schools.

The witness withdrew.

Adjourned to to-morrow at 2 o'clock.

Wednesday, 3rd November 1875.

PRESENT:

THE RIGHT HON. VISCOUNT CARDWELL, IN THE CHAIR.

The Right Hon. Lord WINMARLEIGH.  
Sir J. B. KARSLAKE, M.P.  
THOMAS HENRY HUXLEY, Esq.

JOHN ERIC ERICHSN, Esq.  
RICHARD HOLT HUTTON, Esq.  
N. BAKER, Esq., Secretary.

Mr. CHARLES DARWIN called in and examined.

4661. (*Chairman.*) We are very sensible of your kindness in coming at some sacrifice to yourself to express your opinions to the Commission. We attribute it to the great interest which we know you

take in the subject referred to us, both on the score of science and also on the score of humanity?—Yes, I have felt a great interest in it.

4662. I think you took part in preparing the reso-

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*Dr.*  
*J. Cleland,*  
*M.D.*  
2 Nov. 1875.

*Mr.*  
*C. Darwin.*  
3 Nov. 1875.

Mr.  
C. Darwin.

3 Nov. 1875.

Intions of the British Association at their meeting in Edinburgh in 1871?—No; I had nothing to do with that. I was very glad to see them, and approved of them; but I had nothing to do with the framing of those resolutions; I did not attend the meeting.

4663. But you signed a petition which embodied them?—When they were sent to me I may have done so. I do not remember it; but if my signature is attached I must have given it; I had forgotten it.

4664. But you cordially approved of them?—I cordially approved of them. I had occasion to read them over lately at the time when this subject was beginning to be agitated. I read them over with care and highly approved of them then.

4665. I think you took some part in the preparation of a Bill which was ultimately laid before the House of Commons by Dr. Lyon Playfair?—In the steps preparatory to that Bill, but the Bill itself did not exactly express the conclusions at which after consultation with several physiologists we arrived; I apprehend that it was accidentally altered.

4666. But in the main you were an approving party?—In the main.

4667. You have never, I think, yourself, either directly or indirectly been connected with the practice of trying experiments upon living animals?—Never.

4668. Will you have the kindness to state to us the views which you desire to lay before the Commission in connexion with it?—The first thing that I would say is, that I am fully convinced that physiology can progress only by the aid of experiments on living animals. I cannot think of any one step which has been made in physiology without that aid. No doubt many surmises with regard to the circulation of the blood could be formed from the position of the valves in the veins, and so forth, but certainty such as is required for the progress of any science can be arrived at in the case of physiology only by means of experiments on living animals.

The witness withdrew.

Mr. F. Sibson,  
M.D.

Mr. FRANCIS SIBSON, M.D., called in and examined.

4673. (*Chairman.*) Are you consulting physician to St. Mary's Hospital?—Yes.

4674. And a member of the Senate of the University of London?—Yes.

4675. And a Fellow of the Royal Society?—Yes.

4676. Now you have paid a great deal of attention, I think, to the subject which has been referred to us?—I have paid a certain amount of attention to it. Many years ago I did myself perform a few experiments, and within the last 12 years I have performed one short series.

4677. What is your opinion as to the necessity of such experiments?—I would say, as a physician and physiologist, that it does not admit of opinion, but that it is a certainty that experiments are absolutely necessary for the progress of medicine, including all its branches.

4678. The introduction of anæsthetics, I presume, has made a very great difference in the degree of pain to which animals are subjected?—A very great difference. The greater number of experiments that would be performed for the purposes of inquiring into the nature of disease, and the treatment of disease, would be performed under the action of anæsthetics; and I would say the same of those experiments, a great number at all events of those experiments, that are performed in pursuit of pure physiology; but of course in certain circumstances to use an anæsthetic would defeat the object of the experiment.

4679. Are those occasions rare?—Well, you see, I have never been a systematic physiologist, and therefore I should be speaking out of my province if I were to say whether they are rare or common. I should not feel myself competent to give an answer to that question. Most certainly in those points that come into my own cognizance every experiment which we as physicians can desire to perform to throw light

4669. Then I need hardly ask you what your opinion is as to the notion of prohibiting them altogether?—In my opinion it would be a very great evil, because many reasons, mostly general, but some special, may be assigned for a full conviction that hereafter physiology cannot fail to confer the highest benefits on mankind. Many grounds, I think, can be assigned for this conviction.

4670. Is it your opinion that most of the experiments can be performed while the animal is entirely insensible to pain?—That is my belief; but I ought to state that I have no claim to rank as a physiologist. I have, during many years, read largely on the subject, both general treatises and special papers, and in that respect I have gained some general knowledge, but as I have said, I have no claim to be called a physiologist, and I have had nothing to do in teaching physiology; but from all I can learn, the exceptions are extremely few in which an animal could not be experimented on in a state of entire insensibility.

4671. Then to hesitate to perform experiments, though painful in their nature, when the animal was rendered insensible, would not be, in your opinion, a judicious course to recommend to the Queen and Parliament?—Certainly not. It is unintelligible to me how anybody could object to such experiments. I can understand a Hindoo, who would object to an animal being slaughtered for food, disapproving of such experiments, but it is absolutely unintelligible to me on what ground the objection is made in this country.

4672. Now with regard to trying a painful experiment without anæsthetics, when the same experiment could be made with anæsthetics, or, in short, inflicting any pain that was not absolutely necessary upon any animal, what would be your view on that subject?—It deserves detestation and abhorrence.

upon disease, and the treatment of disease, can either be done under the influence of anæsthetics, or, so to speak, the experiment would be so trivial in its nature as not to require any agency so serious.

4680. That is to say, that the inoculation of an animal for the purpose of communicating to it a disease may no more deserve the use of chloroform than the vaccination of a child?—Certainly.

4681. But in that case the pain and inconvenience is consequent inconvenience, that is to say, being affected with the malady?—Yes, being affected with the malady.

4682. Now when animals are so affected for this purpose, is it necessary to keep them in distress for a long period of time?—It would depend entirely upon the nature of the inquiry, and the object. Of course if it is to inquire into a disease, let us take the instance of the cattle plague; the disease must run its course. If it is to inquire into diphtheria, erysipelas, small-pox, cow-pox, and the various diseases of that class, that have been inquired into, clearly each of them must run its course. But I need not tell your Lordship that the process of a disease is not one of constant pain, but very much the reverse in most instances; and as a physician I can say that in walking round the wards of a hospital, I very rarely see now, or did see when I was in the daily habit of going there, any trace of suffering on the face of any patient, particularly of a fever patient; because, so to speak, there is an anæsthetic within them in the very poison that saturates their blood and solid tissues and nerves; therefore they are lying there scarcely more than just conscious. Therefore I would say that that which applies to mankind applies also to animals; and that they are anæsthetised by the very malady that is produced, as a rule; I do not say in every instance.

# REPORT

OF THE

## ROYAL COMMISSION

ON THE

PRACTICE OF SUBJECTING LIVE ANIMALS TO  
EXPERIMENTS FOR SCIENTIFIC PURPOSES;

WITH

MINUTES OF EVIDENCE

AND

APPENDIX.

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

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