Vivisection : being short comments on certain parts of the evidence given before the Royal Commission.

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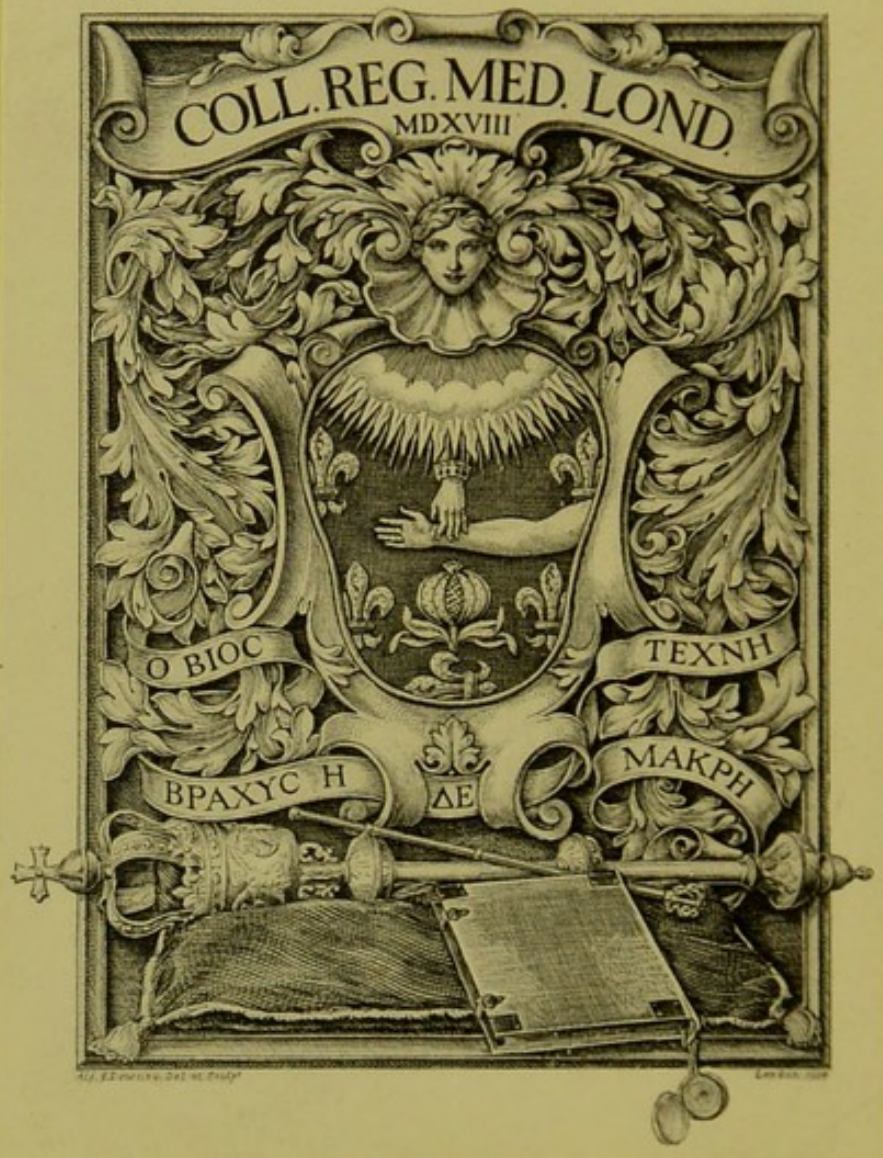
VIVISECTION

MACILWAIN



VIVISECTION.

BEING SHORT COMMENTS ON

CERTAIN PARTS OF THE EVIDENCE

GIVEN BEFORE

THE ROYAL COMMISSION.

BY

GEORGE MACILWAIN, F.R.C.S.

AUTHOR OF "MEDICINE AND SURGERY, ONE INDUCTIVE SCIENCE,"

ETC., ETC., ETC.

"*Quidquid enim justum sit, id etiam utile esse censent, itemque quod honestum*
"*idem justum, ex quo efficitur ut quidquid honestum sit, idem sit, utile.*"
CICERO, *de officiis.*

LONDON:

HATCHARDS, 187, PICCADILLY.

1877.

ANTHONY G. JONES, Esq.

Hatherley, Gloucester.

Dear Sir,

Should anything beneficial result from the discussion which took place on the question of Vivisection, at the numerous meeting, held in Gloucester, on the 24th of October last, a large portion of it will have been derived from your kind assistance, and the efficient manner in which it was supplied. As the mutual courtesies, necessarily observed by those who have to address public assemblies, oblige them to limit the time they severally occupy, I have endeavoured to supplement the few remarks which I ventured on making, by the little book of which I now beg your acceptance.

As I would also desire to express, as emphatically as may be in my power, the sense I entertain of the service you then rendered to a cause equally important to science as to humanity, I would beg you to accept the dedication of the little volume as an earnest of the cordiality with which

I am, dear Sir,

Yours faithfully,

G. MACILWAIN.

INTRODUCTION.

I NEED not detain the reader who may honour this Commentary by a perusal by any remarks as an apology or in extenuation of its imperfections.

Many years ago I offered to undertake an analysis of the so-called scientific claims of Vivisection in an elaborate course of Lectures, which would have necessitated at least a sketch extending over two thousand years. I have never ceased to regret that the offer was not accepted, when I had time and strength to undertake so much labour.

Not being able to do what I formerly desired, I have nevertheless yielded to the wishes of others in consenting to offer some remarks on the evidence given before the Royal Commission on Vivisection. It is an additional mite in aid of legitimate modes of research, as contrasted with that which I contend is as absurd as it is unprofitable. Again, it may tend to keep before the public a question in which, either directly or indirectly, every man, woman, and child

has a personal interest; and it gives me one more opportunity of emphasizing the sustained interest which I have always felt in anything calculated to confer a more logical and philosophical character on our professional studies. The reader must not suppose that I presume in the smallest degree to prescribe what should be done by the Legislature on the strength or weakness of the evidence offered to the Royal Commission. My object is simply to offer such remarks to the public and the Profession, as may induce them to determine whether the evidence constituted so full, so fair, so impartial, or so comprehensive a representation of the subject as that with which (as a basis for legislation) the Government ought to have been supplied.

As the time, labour, and expense oblige me to forego attempting a complete analysis of the voluminous evidence, I have selected such points as appeared to me to be chiefly insisted on by Vivisectors in justification of that mode of research.

CHAPTER I.

THE QUESTION STATED.

For many centuries there have always been men amongst anatomists and physiologists who, distrusting or disliking other modes of study, have sought to discover the laws governing the lives of animals by dissecting their bodies whilst the animals were yet alive. The consequences of this habit have been that thousands, it is very possible even millions, of animals have been subjected to every conceivable species of torture; that this suffering has been in many cases prolonged beyond the infliction of the first injury for hours, for days, and even in some cases for weeks; more recently the invention of certain anæsthetics, as chloroform, æther, etc., have enabled the experimenter so far to render the animal torpid as greatly to diminish, and in some cases it may be to suspend, anything approaching pain; but these proceedings, besides placing the animal in an artificial state, are not applicable to all experiments, and especially those which are addressed to the nervous system, which of course are amongst the most painful. This mode of enquiry has at all times been extremely shocking and repulsive to the greater part of mankind;

but those who have made their objections known have been always met by the Vivisectors with the allegation that such experiments were necessary to the discovery of the knowledge and treatment of disease, and that thus the sufferings inflicted on animals were condoned by the benefits which accrued to mankind. On the other hand, a great number of persons have demurred to the foregoing reasoning, and they have denied the accuracy of the statements on which it is founded. They deny that Vivisection has been the source of any useful discovery, with this reservation only—that where anything useful has resulted from it, it was something which was easily deducible from clearer sources, and which, besides being superior in their scientific character, have been entirely unobjectionable.

They also allege that many of the assertions of the Vivisectionists, with regard to the sources of particular discoveries, are demonstrably incorrect.

It is further contended that Vivisection, in being a fallacious method of research, has led to very serious practical errors, and in no hands more palpably and more extensively mischievous than in those of the most eminent of the Profession at the time.

Again, it is contended that if Vivisection be thus questionable, the substitution of it for higher and better modes of enquiry into the nature and treatment of disease, only intensifies its scientific objections and its alleged criminality, by making it gratuitously subservient to an avowed selfishness.

Now, when the interest in this question had been so much extended as to induce Her Majesty to order

a Commission for investigating it; it will be conceded that the selection of the individuals composing it should have combined gentlemen thoroughly acquainted not only with such branches of science as are more or less indispensable to physiological researches, not only with the more familiar elements of what is called pure physiology, but especially *au fait* with all that had been done by pathological enquiries, and with the bearings of these enquiries on the alleged necessity of Vivisection; and not only as regards these successes, but others which were necessary but hitherto unfruitful, from an obvious and declared neglect of them—a neglect admitted by no one more *than some Vivisectors*. Again, the examiner should have been as far as possible unbiassed by foregone conclusions; that at least avowed Vivisectors should have been admitted with great caution; that the examination of witnesses should have been carefully conducted, and their answers not encumbered or warped by leading questions; that opinions should not be allowed as substitutes for facts; and that facts themselves should not be allowed to rest on mere assertion where proof could be obtained by historical authority. How far these conditions were fulfilled the reader may form some idea even from the remarks I am about to submit to him. It may possibly induce him to consult the ponderous Blue Book itself, when he will see how much stronger the case may be made, than in the fragmentary comment I have only time or space to offer. Whilst we should avoid entering into any of those features of Vivisection which, from their sensational tendency, might deflect our attention from

a calm examination of its alleged utility, we must in justice consider some of those features which are inseparable from this mode of research. The Vivisectionist would have us believe that the knowledge of the nature and treatment of disease, and the practice of a Profession which walks forth with a mission of beneficence, chartered as it were by the Highest authority, can only be acquired by proceedings which, lulling our sensibility, and dwarfing for the time the most blessed attribute we have, lead us to inflict countless sufferings on animals—beings which minister largely to our comforts, our necessities, our pleasures, and, in their relations to surrounding nature, present subjects of our most elevating and delightful studies. Proceedings thus alleged to be necessary are further of a character which, in the abstract, are most repulsive to the majority of mankind, and for a purpose acknowledged to be selfish, viz., the removal of evils which are almost entirely the results of the vices, the follies, or the ignorance of mankind. All this may be so; there are no doubt many things which our limited faculties do not lead us to understand, and which are "not dreamt of in our philosophy." But I think everyone must concede that such extraordinary and, one would think, irreconcilable arrangements should not be taken on trust on the statements of mere men, and that the alleged necessity ought to be so clearly proved as to admit of no question or ambiguity. It should be a subject of more than mere scientific interest to consider how far it is possible that Vivisection, if it prove to be a wrong method, can end in a mere negative, and abandonment of it.

I have no idea of any proceeding in science ending in a mere negative; time at all events is wasted, if not ill employed. As curses are said to fly homeward, it may possibly occur to some that a calm contemplation of the actual state of medical science may, as an abstract proposition, suggest doubts whether we are quite in the right way, and that although Vivisection be not the *only*, it may prove the most misleading, exemplification of it. For my part, I do not dissemble my regret at seeing so beautiful a branch of science, suggesting the highest qualities of head and heart, so largely charged with conjecture; so encumbered, as I think, with such an army of parasites, the potency and grasp so narrowed or distracted by endless specialisms; and its high station too often marred by connections suggestive of commercial appetency, which should be as unknown as it is unworthy a great Profession; which, however, has a Palladium which condones its shortcomings, in ministering gratuitously more largely to suffering humanity than any other class of mankind; and which, I trust, will continue to preside over its earnest and too often thankless labours, and protect it from desecration, by the misapplication of talents, however well-intentioned they may be, until a time when a more philosophical cultivation of it shall indefinitely extend its unquestioned utility, and raise it to the high place in philosophy which becomes its exalted functions, and which it once enjoyed.

SIR THOMAS WATSON.

In perusing the very voluminous evidence on a mode of research declared by those who practise it to be essential to the cultivation of physiology and the progress of medical science, we are somewhat unprepared, if not surprised, at the evidence of the first witness examined. What there is in Sir Thomas Watson's evidence in favour of a measured and somewhat humanely restrictive admission of Vivisection, we shall have occasion to discuss when the experiments of Drs. Hope and Marshall Hall are to be considered. In the meantime, it is not a little significant in relation to the alleged claims of this mode of enquiry, to learn that so eminent a physician, who has arrived at the *summi honores* of his Profession, who is the author of an approved work on the Practice of Medicine, and, what is still more interesting, has practised his Profession with honour and success for fifty years in the largest metropolis in Europe, has never instituted himself, nor witnessed by the hands of others, a single Vivisectional experiment. This is the more instructive because it is well known that Sir Thomas Watson has been an industrious student of his Profession and a teacher of the Practice of Physic. This did not seem a very auspicious opening for such members of the Commission who might be favourable to the practice of Vivisection. Accordingly, with some other questions of a *quasi* leading character, Sir Thomas was asked the following

Question.—Although you have never performed any experiments, nor witnessed them, *you have used* the results of the experiments of others, *have you not*, as the *basis* for the advance of your professional knowledge?

Answer.—I have made myself acquainted with the experiments and their results, and have turned them to such uses as I could.

Could any answer convey a more measured recognition of a mode of study in reply to the question whether he had not made it a *basis* for the advancement of his professional knowledge? Could anything be more vague or unsatisfactory? Why was so experienced a witness not requested to favour the Commission with some of the details of so vast an experience? Why not requested to state in *what* cases he had turned it to account, and how far it had or had not answered his expectations? I must think that allowing so experienced a witness to limit his information to such a vague generality was losing or *not seeking* practical information from one who, of all others, may be reasonably supposed to have been most able to supply it; and therefore suggestive of a very unsatisfactory and by no means painstaking examination.

SIR GEORGE BURROWS.

I know nothing which is more calculated to fog scientific enquiries than that which is termed opinion. We cannot dispense with the word, nor can it be too closely examined. Founded on facts, truthfully stated, and flowing from a reasonable and intelligible relation to such facts, it is a necessary step—a species of scaffolding essential to the construction of scientific generalisation. If simply enunciated without these conditions, it is more justly regarded, when held as a *quasi* imposition on your credulity; or, as one who would wish you to take on trust that which he knows or should know he ought to prove.

I do not believe that anyone is disposed to undervalue the discoveries of Sir Charles Bell. It is an honour to his memory that, though a teacher of anatomy, he saw the fallacy of Vivisection as a means of discovery; he was, however, so far unfortunate as to live in a time (which yet lingers on) when he could not make his discoveries credible except through the medium of the external senses. This tendency to trust to sight, hearing, etc., things, which can only be discerned by the intellect, has always more or less attended physiological research. It seems to have resulted from regarding the functions of the body too exclusively from an anatomical point of view—looking too much at the mechanism and too little for the law exemplified in its action; just as a chemist would err who bestowed too much time on the apparatus and too little on its use—that is, on the combinations

and their effects which his apparatus were the means of his being able to exemplify or demonstrate.

Sir C. Bell denied that he deduced his discovery from Vivisection; he asserts that he resorted to it only to convince others after he had failed to do so by other means; he says he deduced his discovery from anatomy, and adds that experiments (on animals) have never been the source of discovery. Most people will admit that an investigator is the best judge of the means whence he has made a discovery; but the advocates of that mode of research still persist in saying that it resulted from Vivisection. Before we conclude this commentary, other examples will occur where the statement af the witnesses are equally irreconcilable with the facts of the subject to which they refer.

Vivisectors certainly exemplify the common tendency of us mortals in the *humanum est errare*, for now we have to guard, as I think, from having Sir Charles's discovery wrested to purposes beyond those which it can, in a practical sense, justify. I have already referred to the tendency to sensuous evidence. Lord Bacon warned us against it; "*sensus fallunt*," he says; and Abernethy used to ridicule the idea of inferring the structure of a part from what you could *see* as contrasted with the evidence afforded by physiological and pathological phenomena. I have not much to remark on the evidence of Sir George Burrows, but he "follows suit" in reference to the discovery of Sir C. Bell. It had been known for very many centuries that sometimes parts of the body lost sensation while power of motion remained; and, *vice*

versâ, that sometimes the power of moving was lost whilst there was still sensation. This leads us to another dominant character in Vivisection; I mean, first imagining a thing contrary to analogy, and then dissecting an animal to see if the idea be true. Why should they have thought that the same nerve should serve both for motion and sensation? People do not smell by the optic, or see by the auditory nerve; and if we do not disbelieve Sir Charles Bell's own account, he did not endeavour to solve the question by Vivisection. I cannot help regretting that there is so much of opinion in the evidence of Sir George Burrows; I cannot say that his recognition of Sir Charles Bell's or Dr. M. Hall's labours (of which I shall treat in another section) are such as, in a practical sense, I can endorse; nor is the reference to Bernard's experiments so given, as to satisfy anyone who thinks himself entitled to judge of evidence by the ordinary rules, or who is not content to form his conclusions on opinion. I will illustrate what I intend to convey by referring to what Sir George says in reference to Sir Charles Bell. After alluding generally to the value of diagnosis in affections of the nerves of the face, Sir George says: "A person may have a paralysis "of one side of the face, and it is simply a paralysis of "the facial nerve, and we can tell that person, 'Well, "'*this is a trifling affection;* there is some little pressure "'on your nerve, which can *easily be removed.*' We go "to another person, and find the same expression of "countenance, but at the same time there is loss of "sensation, and we say. 'This is serious; there is "'some mischief going on in the central portions of one

"'hemisphere of the brain. This is a thing attended "'with danger, and must be *treated on totally different* "'*principles.*'" Now there is something so ungracious in criticising the practical opinion of a professional brother, that I hardly know how to treat the foregoing passage. I have put in italics those parts to which I would direct the attention of the professional reader, and will finish my remarks on this reference to the interesting discovery of Sir Charles, by saying (with the exception of *surgical* cases, in which tumours or other bodies are found pressing on the *portio dura*), that after a practice of nearly or quite half a century in London, there is not a conclusion in the statement quoted from which my own experience does not oblige me to dissent. Sir George, whilst he thinks some Vivisectional experiments necessary, does not object to some restrictions as to the kind of experiment, and in favour of securing proper qualifications in the experimenter.

As a cautious enquirer into the real relations of the liver as tested by chemical and pathological data, I cannot accept the answers given to questions 187, 188, 189, without much more explanation and proof. They are much too vague and general to reason on safely, as to the so-called discovery, or its practical value.

We consulting surgeons, from our familiarity with pathological proceedings of various kinds, used to be called conceited, and accused of thinking that we knew more of pathology than the physicians. Well, that may have been said, and I cannot here contest the matter; but I must say that I believe I have made out, in a practical sense, the relation which the liver

bears to the *elements* of sugar, to an extent which has served me in very good stead by the bedside; and enabled me not only to form a diagnosis where the *symptoms* were misleading, but also a prognosis with relation to subsequent occurences, both early and remote, which have been confirmed respectively by cases which have recovered, as well as those wherein death has allowed examination; and not one iota of which information has resulted from any Vivisection whatever. But I shall have occasion to speak of the liver in another section, to which also I must refer my remarks on Dr. Marshall Hall.

In the meantime, I think I may safely challenge Bernard, or all the Vivisectionists in Europe, to obtain from any living animal whatever, by Vivisection, such a knowledge of the liver as may be deduced from the contemplation of man; provided the investigation be conducted in conformity to the rules necessary to pathological research, and with the patience and circumspection which are essential to all branches of knowledge, and which pathology like them demands.

ON THE DISCOVERY OF THE CIRCULATION OF THE BLOOD, AND THE ERROR IN ATTRIBUTING IT TO VIVISECTION.

The discovery of the circulation of the blood has been, in the evidence presented to the Royal Commission, repeatedly referred to Vivisection. This is a striking example of one kind of error which was presented to that body, and an instructive illustration of the sources whence others have arisen;

very significant of the little care with which such sources have been examined. I cannot but think that if anything like the care required had been observed, the confusion of ideas of which I have seen so many evidences, might have been avoided.

When anything has been discovered by one who had been occupied in Vivisection, the discovery has been at once referred to it, and in cases wherein Vivisection had nothing to do with the matter. John Hunter is a remarkable example of one whose useful works have been thus falsely ascribed to Vivisection, of which we shall treat in its proper place. In treating of the circulation of the blood, it is important to a clear view of the subject that we should so far digress as to give some idea of the mechanical or hydraulic arrangement by which the blood is distributed in man. Perhaps the most simple view is to say that there is a series of continuous tubes, commencing in some of large size, which are divided and subdivided into very numerous smaller and smaller tubes as they reach the various parts of the body; having become infinitely small, they gradually coalesce into larger and larger tubes, until they return to the part whence they set out (the heart). The first-named vessels or tubes are called arteries, and are connected with the left side of the heart; the returning tubes are called veins, and deliver their blood to the right side of the heart. From this part the blood is sent through the lungs, again returned to the left side of the heart, whence it is distributed as before to all parts of the body. The arteries are of somewhat different structure from the

veins, being firm and elastic; the veins being thinner and readily collapsing when empty. In certain parts valves are placed, allowing the blood to pass only in one direction. These valves are placed on both sides of the heart, and also at the commencement of the arteries issuing from it. There are also valves in the veins, but chiefly, if not entirely, in those which are superficial and which are conceivably more subject to pressure from without. Now, all this can be demonstrated in the dead body by the most ordinary dissection, and in an intelligible manner on the dead body only. All? Why not exactly? The valves show you in what direction the blood must travel; but you cannot demonstrate the terminations of the arteries in the veins, or capillaries leading to them, unless you inject the vessels by the arteries with coloured fluid, which will return by the veins; and now if you dissolve some wax, or other material, in the fluid, which will render it solid on cooling, you have all that is necessary to demonstrate the fact in question. Now, I am utterly at a loss to conceive how that which can be so easily and simply demonstrated on the dead, can be demonstrated at all on the living. I can by no means accept this as a fact, nor do I find that Harvey stated that he had done so.

I have in my time dissected and examined a great number of the bodies of the dead, and I have witnessed, and performed myself, a great number of operations on the living, in a long practice of my profession, always in London, and combining with private practice that of public institutions with which I was officially connected.

On the very threshold of the Vivisectional enquiry, the wound would be so obscured by blood, as to create nothing but doubt and confusion, and this would not be rendered less puzzling by the blood from arteries and veins being of different colours. In the most ordinary operations in surgery, as tying arteries, we are obliged to have an assistant sponging our way (as it were), that we may not include anything, which we should not, in the ligature we are about to use. In operations which require more time, and where that which we may desire to remove has often more or less complicated connections with important parts, this sponging is still more necessary. Now, it is impossible to clear this state of things in a living animal, so as to do anything to convey even a proximate idea of the circulation. By inspecting the vessels by the microscope, in the frog for example, a man previously informed might get some idea of the matter; but then he would substitute a doubtful analogy, not for a moment comparable with the simple demonstration afforded by dissection of the vessels of the human subject.

The effect of the interposition of valves in tubes conveying fluid could not be mistaken, and the existence of these was not unknown; moreover, the valves in the superficial veins had been discovered; and now, I think, we begin to see our way to the truth in relation to Harvey. These valves—I mean the valves of the superficial veins—were discovered by Hieronymus Fabricius, of Aquapendente, in Italy, who was *Harvey's Teacher*, and this seems to have been the last point whence emanated the instructive and intel-

ligible light over the labours of Harvey. The importance of the solution deducible from it was unperceived by Fabricius, but was at once recognised by his illustrious pupil.

This view, I think, independently of all other circumstances, is constructively deducible from the writings of Harvey himself. We will take the very passage which the pro-Vivisection witness himself selected. Let us see, then, what Harvey himself says. "When I first gave my mind to Vivisection as a "means to discovering the motions and uses of the "heart, and sought to discover them by *actual inspec-* "*tion*, and not from the writings of others, I found the "task so truly arduous, so full of difficulties, that I "was almost tempted to think with Fracastorius, that "the motion of the heart was only to be comprehended "by God; for I could neither rightly perceive at first "when the *systole*, and when the *diastole*, took place; "nor when and where dilatation and contraction "occurred, by reason of the rapidity of the motion, "which in many animals is accomplished in the twink- "ling of an eye, coming and going like a flash of light- "ning, so that the *systole* presented itself now from this "point, now from that. The *diastole* the same. Then "everything was reversed; the motions occurring, as it "seemed, variously and confusedly together. My mind "was therefore greatly unsettled; nor did I know what "I should myself conclude, nor what *believe* from "others. I was not surprised that Andreas Lauren- "tius should have said that the motion of the heart "was as perplexing as the flux and reflux of the "Euripus had appeared to Aristotle."

Now, I beg the reader to consider this statement by Harvey himself, in connection with the actual condition and the extreme simplicity of arrangement of the mechanism it was his object to examine, for I can hardly conceive anything which is more strongly suggestive of a man having actually puzzled himself by his Vivisection. If the reader does not participate in this view of the passage, let him invent a passage which he forms for the express purpose of exhibiting an investigator so puzzled, and then compare the two, as a severe test of the feasibility of the conclusion which I have ventured on drawing.

But, before we quit this subject, let us apply something more authoritative than the variable manner in which this passage may be construed. A very striking and significant character in Vivisection is that this sensuous mode of investigation, which is to render things so clear, seems on every hand to present nothing more frequently than the variety of conclusions to which it has led. This brings us to a quotation very *apropos* to the point we are discussing, and the more as the passage comes from a writer whom some of the witnesses have quoted rather too boldly as a Vivisector, and with especial reference to a series of experiments in which there was no Vivisection at all. I mean Dr. Hope, of whom we have yet to speak. Dr. Hope, in commenting on some erroneous conclusions of that inveterate Vivisector (M. Majendie), says: "If, "says M. Magendie, the heart of a living animal is "denuded, we easily see that the auricles and ventricles "*contract* and *dilate* alternately. These movements are "so arranged that the *contraction* of the auricles takes

" place simultaneously with the dilatation of the ven-
" tricles; and, *vice versa*, that the contraction of the
" ventricles coincides with the dilatation of the auricles,
" etc." (Quoted by Bouillaud, Traité 1, p. 57.) Now, hear Dr. Hope. " The great defect of this view is
" that it leaves no interval of repose. It is *easy* to see
" *how* M. Majendie has been misled, viz., *by operating*
" *on living animals;* for I have always found that when
" an animal unfortunately *retained or regained* the
" slightest degree of sensibility, the action of the
" heart was so violent, convulsive, and rapid, as to
" prevent the appearance of *alternate* action described
" by M. Majendie." Again, on another statement of M. Majendie in relation to the action of the auricles and ventricles, Dr. Hope observes, " This is monstrous!
" Its physiological impossibility is palpable." Again of Majendie, Dr. Hope observes, " Fortunately his high
" authority is opposed by that of Harvey and Haller.
" M. Bouillaud appears to follow Majendie (Bouillaud
" Traité, p. 87), and this error has betrayed him into
" several others respecting physical signs. Dr. Bostock
" also follows Majendie, but evidently from inad-
" vertence, etc."

Before I leave this subject, I would quote a passage from Dr. Willis' Life of Harvey, which seems entirely to confirm my view that Harvey discovered the circulation as a necessary result of his examination of what (for shortness) I may call the mechanism of the circulation, viz., the valves and the perceptible relation of them to the current in which they were interposed. Dr. Willis, after some other very interesting observations, says: " For Harvey, it must be here observed, left the

"doctrine of the circulation as an *inference* or induction "only, not as a sensible demonstration. He adduced "certain circumstances, and quoted various *anatomical* "facts, which made a continuous transit of the blood "from the arteries into the veins, and from the veins "into the arteries, a *necessary* consequence; but he "never *saw* the transit. His idea of the way in which "it was accomplished was even defective; he had no "notion of the one order of sanguiferous vessels ending "by uninterrupted continuity, or by an intermediate "vascular network in the other order. This was the "demonstration of a later day, and of one who first saw "the light (Malpighi) in the course of the very year "when Harvey's work on the heart was published." Surely nothing can be more convincing, that he founded his reasoning on the *anatomical* facts, showing that the very nature of the valves of the heart determined the course of the blood in that organ; whilst the only course permitted by the superficial veins completed the conclusion. These *anatomical* facts were not, and could not, be *demonstrated* (as I have said) on a living bleeding animal.

I must now conclude all I have space for on this part of the subject. As I told Lord Cardwell in reference thereto, Harvey did not discover the circulation of the blood by Vivisection; and I added that I did not see how it was possible to demonstrate it in a living animal—neither do I now; and if that which I have stated here does not convince the reader of the impossibility of doing so, and of the marvellous superiority of the dissection of the dead on such a subject as the circulation (too certainly marred

by circumstances inseparable from the dissection of the living, I must respectfully leave it to his own reflections.

DR. HOPE.

The experiments of Dr. Hope were referred to by some of the witnesses before the Royal Commission, in proof of the advantages of Vivisection. These we will now consider.

The industry displayed by Dr. Hope in his investigation of diseases of the heart invites our respectful recognition. Whatever be our mode of study, industry is, after all, the material—the indispensable requisition to all useful discovery. Nevertheless, I cannot perceive that the mode which Dr. Hope adopted, in the least degree superseded the necessity for other modes of enquiry. And further, that these are not only as accessible as are Vivisections, but infinitely superior, in their logical and scientific claims, to that mode of study, and for which it is impossible to regard experiments on living animals as an acceptable, or even reasonable substitute. It may occur to some, in reference to the experiments *chiefly made* use of by the witnesses, whether Dr. Hope can be cited as a Vivisector at all; but, not to digress, let us proceed.

Affections of the heart—comprising in that term not only those which exhibit changes of structure, but those also which we understand as functional—are very common; and the former, sooner or later, are too frequently fatal. The result of this is, that observed with the care and circumspection necessary to patho-

logical research, there are no diseases which are more instructive. Examined as to all their antecedents, and especially by the light of that correlation in the action of organs on which John Hunter so insisted, no diseases afford us more warning, none which enable us to form that prophetic prognosis (so to speak), which thus gives us oppportunity to prevent or indefinitely postpone the supervention of conditions which we are unable to cure. Were it the habit to examine diseases, as all cases should be, and I begin to think some are; their antecedents carefully recorded, their probably remote as well as their proximate causes and dependencies ; a single year, from the combined efforts of the Profession, would have afforded, not only opportunities of acquiring very accurate observations on the human subject, whilst living, but also many instances in which the interpretation of phenomena on the living, might have been tested by examination of the dead. It is the absence of tracing all the links in a chain of pathological phenomena, that has led men to that sort of confusion which confounds morbid anatomy with pathology, of which it is of course only one (however important) element. I should not venture to say as much as this, but for two reasons: the one, that I continue to see examples of imperfect investigation; and because nothing would more effectually expose the uselessness and the inferiority of Vivisection than that more careful observation and record of disease, which is still in a general sense wanting, although now not entirely without some exceptions. Many years ago, when insisting on the necessity of this, I constructed

tables to expedite and facilitate more comprehensive records of phenomena. The absence of this, and the mischief resulting from it (as the reader will see), is acknowledged by some of our most industrious Vivisectors, as condoning or excusing investigation by Vivisection.

The fact is, as I have often said, Vivisection has too commonly arisen from the investigator not having had, as the case may have been, either the pathological knowledge, the opportunity, or the industry, necessary to really philosophical pathology. Dr. Hope did certainly perform experiments on living animals; but it may be remarked that the experiments, which were commenced by stunning the animal by a blow on the head with a hammer, scarcely come fairly within what is intended by Vivisection; and I think there is something of special pleading in quoting them before a Royal Commission, with a reticence which suppressed the real nature of these experiments. No doubt Dr. Hope performed other experiments; but, as I have observed, those which were chiefly relied on were such as the following. "An ass, of which the pulse and "impulse were forty-eight in a minute, was instantly "deprived of sensation by a smart blow on the "head. The trachea was opened, a large bellows pipe "introduced, and artificial respiration maintained; "while, at the same time, the left ribs were sawn through "near the sternum, and forcibly bent back and broken "(in order to prevent hæmorrhage from the intercostal "vessels), so as widely and completely to expose the "heart, immediately behind the left shoulder; the "whole was accomplished in less than five minutes.

"The pericardium was next opened, and the auricles "and ventricles fully displayed."

They now observed the heart as it lay beating. Other experiments of the same kind were made. Various auscultatory and manipulatory measures were now instituted; such as listening for sounds, pinching certain parts of the organ, inserting hooks to raise the valves, thrusting a knuckle from one cavity of the heart towards another, applying the stethoscope on the heart, and similar more or less varied proceedings, by one or more of those who were witnessing the experiment. The object of all this was to ascertain to what particular part or structure of the heart certain sounds were to be respectively referred, and in this way to assist us in recognising the seat and sources of any or similar sounds emitted by the human heart during our investigation of its diseases. My first objection to this mode of proceeding is its manifest inferiority to opportunities afforded by the human subject, were they investigated by careful examinations and a faithful record; in short, by anything deserving the name of really philosophical or pathological research. It assumes in the first place that the sounds given out by the heart of the ass are identical with those of the human heart; that may be, or may not; at all events, the assumption belongs to the mode, and not to the matter. Then again, the heart of the ass was sound, not diseased. Again, the animal was placed in circumstances having no resemblance to those under which we investigate the heart in the human subject. Then supposing that by this experiment we ascertain that the sound we

hear results from the causes to which we refer it, it is (thus obtained) a very barren result. It is a mere abstraction, with not one of those instructive accompaniments which pathological investigation in the human subject never fails to disclose, and hardly ever, in my experience, without some contribution towards the ascertainment of the causes from which the disease has arisen, or from the phenomena by which it has been accompanied. Here, again, we observe those discordant interpretations of these sensuous enquiries, which so puzzlingly exemplify those endless discrepancies of opinion inseparable from this kind of investigation.

I give an example or two. Dr. Hope says: "Now, the ventricular diastole (relaxation), says "Majendie, is synchronous with the auricular systole "(contraction); consequently the auricles, after their "systole, must remain in the state of spasmodic con- "traction for the period of a second and a quarter, "waiting for the next contraction of the ventricles, "which are unexcited by the stimulus of distension. "This is monstrous; its physiological impossibility "is palpable, etc." (Hope, p. 62.)

Again, Dr. Hope observes: "It is easy to see how "M. Majendie has been misled *by operating on living* "*animals*, for I have always found that when the "animal *unfortunately retained the slightest degree of* "*sensibility*, the action of the heart was so *violent*, "*convulsive*, and *rapid*, as to present the appearance "of alternate action described by M. Majendie. In "small animals also, as rabbits, whose pulse beats "from 150 to 200 in a minute, the same appearance

" is generally presented, although they have been " completely killed; for the interval of repose is too " short to be appreciated by *the eye*. Nay, in asses " poisoned by woorora, much the same appearance is " presented whenever the pulse is accelerated twenty " or thirty beats above the natural standard of forty " or fifty. The contraction of the auricle thus be- " coming more active and extensive, and encroaching " so much on the interval of repose, as to render it " indistinct to an unpractised eye." (Hope, p. 61.) In other parts of Dr. Hope's work, we find other proofs of the differences of opinion amongst Vivisectors, at pp. 39, 40, etc., in relation to other theories, which were formerly broached by himself, " *but now discarded.*" He adds, in relation to another theory of M. Majendie's: " This theory is " completely refuted by my two original experiments " on the ass, 1830, proving that the sounds were " perfect when the sternum and ribs were removed." It is worthy of remark, too, that here one of the reasons assigned is from what happens in " *Dropsy* of " the *Pericardium;*" thus appealing, as Vivisectors are often forced to do, to the superior authority of pathological phenomena in the human subject. In allusion also to some conclusions of another physician, Dr. Hope says they proceed on an assumption which is doubtful, at least, if not entirely erroneous. I feel no interest in multiplying, even if I had space, examples of discrepancies of opinions amongst Vivisectors. Many years ago I cited examples from the works of those who have been most distinguished in this mode of enquiry; for, after all, however such differences

may suggest that it is not an auspicious mode of research, it is no proof that it must necessarily, on that ground only, prove unproductive. There is, too, no need of anything like special pleading, to show that Vivisection is a mistake—that it is wholly unproductive; or where it tends to prove anything useful, the necessity of it is superseded by superior modes of investigation. I must not close the few remarks which I have to offer on Dr. Hope, without some reference to the treatment. Now, on this subject, there are many judicious observations, and I do not perceive (had I seen the cases) that I should have withheld my general concurrence; but now comes the test. I do not see one single point of real *practical* value, which any well-informed surgeon would not have arrived at without any Vivisection, or which could stand instead of more unequivocal information otherwise required, and by more legitimate means. In serious diseases we like large comprehensive views, whence alone we derive the real value of details. In such, it matters little that a man should say, "No, "Sir, that which you consider the second sound arises "from the valves, and not from the source to which you "refer it." This suggests to me what Abernethy once replied to a man, who said, "I don't know, but I "think, Sir, you have got a slight touch of argina." "Very well," said Abernethy; "but I should thank "you much more if you would inform me how I am to "get rid of it." I need scarcely add that in Dr. Hope's book, the cases in the *human* subject which he gives are as interesting, instructive, and honourable evidence of careful observation, as his experiments on animals were inconclusive or unnecessary.

DR. MARSHALL HALL.

The labours of this industrious physician and his work on the Nervous System have been cited in proof of the advantages of Vivisection. Let us consider the subject, and especially in relation to disease, whence Dr. Marshall Hall would deduce its chief claims to attention. We must not confound his many practical remarks, and the greater attention he would exact to the causes of disease, with his Vivisections. The former represent things which are still too little attended to; the latter lead me (in which I think the reader will participate) to a somewhat different conclusion. He will probably with me regret that so much valuable time was wasted, or not otherwise employed, in eliciting a still more practical view from the facts which it was Dr. Marshall Hall's object to discover. Those who are, or have been, constantly engaged at the bedside, will instinctively apply the question, *cui bono?* to everything alleged as new; and this not only in an absolute sense, but also (and especially in relation to elaborate experiments) in a comparative sense, viz., How far this or that discovery might not be more securely deduced from more scientific modes of enquiry.

Viewed abstractedly, the Nervous System no doubt presents many phenomena more or less puzzling; but many of these become very intelligible in their practical application, when we regard them as occurring in the *living human body* in health and disease. Thus examined, we are able to arrive at generalisations, or

axiomatic approaches thereto, which render many curious phenomena very simple and intelligible. Dr. Marshall Hall's experiments were chiefly intended to show that there were actions emanating from the Spinal marrow, induced by impressions on the more or less distant peripheral branches of nerves which were independent of the brain, and thus not under the control of volition. I do not stop to remark on the frogs and turtles which were the subjects of some of his researches, because he also experimented on other creatures more nearly allied to man. Now, we are early reminded by Dr. Marshall Hall's proceedings, as in some other Vivisectional matters, how true it is, as Professor Whewell has said; and to which I endeavoured to direct attention many years ago; that discoveries for the most part result from the philosophical interpretation of common and even familiar phenomena. Dr. Marshall Hall, when he wishes to impress the fact of what he terms reflex action, cites the phenomena of tickling; and a very good illustration it is, of the violence with which muscles will sometimes act independently of volition, and in some exceptional cases, he might have added, in rather an undesirable degree. In following out these and similar phenomena, Dr. Marshall Hall arrives at some conclusions with regard to epilepsy, which are in many points interesting. We should, indeed, hesitate to adopt some of his conclusions, were it not that he accompanies them with judicious cautions, by which they are brought within the range of the ordinary resources of good surgery—but which, on the same grounds, appear to me to have

rendered his experiments altogether unnecessary; this we may more plainly show as we proceed. I wish I had space or time to make an analysis of Dr. Marshall Hall's works as they deserve; but I have over and over again declared that nothing less than an elaborate course of lectures would enable any one fully to demonstrate how unnecessary is Vivisection.

I cannot forego stating one case of Dr. Marshall Hall's. Having removed the head of a frog, and suspended the animal by means of a ligature round the toes, the animal remained motionless "until he "pinched the skin in various parts, which produced "forcible muscular action, and then the elongated "form was resumed as before." He then performed the same experiment on a snake, with "precisely "similar results. We see thus," he proceeds, "that "sensation is extinct in all parts of an animal whose "spinal marrow has been divided, situated below the "point of division; the excitomotory phenomena "remaining. But we have still more positive facts "in cases of *Injury of the Spinal Marrow in the human* "*subject*." He then relates a case sent him by Mr. W. F. Barlow, of Writtle, Essex, in proof. "A young "fellow (John Bright, page 63) fell from a walnut "tree. He was taken up in a cold and pulseless con- "dition. Besides other symptoms, the lower half of "his body and inferior extremities were entirely devoid "of sensation, and they were not in the slightest "degree under the influence of the will. Sometimes "the patient had cold shiverings; and whilst the "muscles of that part of the body supplied with "nervous energy from above the seat of injury, were

" observed to shake, those deriving their nerves from " below that spot were perfectly motionless. Never- " theless, when the integuments of the legs were " pinched, or more particularly when the sole of the " foot was tickled, the extremities were retracted with " considerable force. A little cold water dashed upon " the surface produced the same effect, though there " was no feeling of coldness. One leg was constantly " in a flexed position, and, if straightened, immediately " recovered itself. After death the spinal marrow " was found to be nearly severed in the neck." But now what was the use, much less necessity, for his Vivisections, when there were still more positive facts in cases of injury in the human subject?

Dr. M. Hall cites other cases equally instructive from the same superior source.

The most valuable part of Dr. M. Hall's remarks appear to me to relate to the necessity which exists for more careful examination and record of the phenomena in the living body, which he justly styles LIVING *pathology*. In " relation to which," he says, very truly, " it is not yet known what may be accom- " plished for the *epileptic, by extreme and sustained* " *attention.*

1. " By diet and regimens, excluding all stimulants " and indigestible substances.
2. " To the secretions and excretions.
3. " To security against all emotion and excitement.
4. " Exercises and occupations, avoiding all effort and " fatigue.
5. " To clothing, and especially warmth and dryness " of the feet.

6. " To a raised posture of the body.
7. " I mean such a degree of attention to diet and
" regimen, to the secretions and excretions, *as*
" *has never been attempted before.*

" The great difficulty in the treatment of epilepsy in " private practice, is the impossibility of securing the " necessary degree of attention to all the regimen, " etc."

In all this I perfectly agree. Whatever he has said about epilepsy, I believe I could exemplify as the consequence of that penetrative painstaking, at which he seems to point, which is more than any other thing the great requisition in bedside work—the absence of which is the complaint of himself, Dr. Jones, and similar ardent enquirers, and lies at the root of that sensuous philosophy, which, impatient in waiting for a recurrence of long neglected opportunities, vainly seeks a substitute in Vivisection. Now, not one of the requisitions so sensibly and forcibly urged by Marshall Hall, has the smallest necessary reference to his experiments. The fact is, that those experiments constituted a circuitous and unnecessary mode of arriving at a conclusion, which represented only a part of a subject which, in its more integral form, is deducible from pathology, and with most important results, not, so far as I know, yet made public. The experiments have led to contracted views; to a practice which, culminating, it seems, on the spinal marrow, referred its means chiefly to that part of the nervous system, and has led to a lamentably imperfect practice. I have seen this in the employment of remedies addressed to the muscular system, or in long continued

courses of remedies equally empirically applied, to the neglect of further enquiry into those parts of the œconomy whence the mischief *really* proceeded. Now, I do not believe that Marshall Hall was led into this error. He was too circumspect to be blind to the various influences which it was necessary to keep in view; and if his views were more or less restricted, which I do not assert, they resulted only from his having resorted to a mode of research which was unnecessary, and *very inferior* to that which, had it occurred to him, he was so capable of pursuing. Many years ago, I endeavoured to impress the importance and practical advantage of following out more fully those correlations of function so insisted on by John Hunter; and I showed that those relations were not restricted to particular parts or to *particular directions;* that although the more ordinary course might be from A. to B., yet it might be from B. to A., or any other direction. Now this was, I contend, a much larger view of the matter, and derived from physiological and pathological facts, observed in the living human body. This led me to very important results, which, as they were really legitimate outgrowths from my studies of John Hunter, I shall more fully exemplify when I treat of that author; but one case seems so pertinent to the subject treated of by Marshall Hall, that I will mention it. The precepts of inductive philosophy, instruct us to enquire and to look very much farther than the immediate object before us. Thus, supposing the subject to be muscular spasm; why, then we consider under spasm every variety we can discover, no matter how distant,

how unlike, the occurrence may be, either as regards the degree, the patient, or any other accompaniment; because, in pursuing a subject to obtain an insight into a *common* character, we sometimes find that in one case the circumstances may appear very mysterious, whilst in another they are very simple. Now in aid of illustration, I will endeavour to chip off, as it were, a piece from a great subject, and say a few words about that most afflicting action of muscular structures, and the practical relation of the case to the nervous system (Tetanus, or locked jaw). A man had a small abscess on his finger, of which he took no notice, until he was somewhat suddenly seized with tetanus. This induced Dr. Golding Bird (at that time my colleague) to refer him to me. Dr. Bird expected that I should at once have opened the abscess, but this I declined to do, for the following reasons: I thought it probable that the man might have had many trifling accidents to his fingers before, but he had never had Tetanus. It appeared clear that there must be some link between the abscess and the tetanus, which was not represented by either. Many common cases, as worms, etc., occur in children, and others, of disorders of the voluntary muscles, in which the symptoms are clearly referable to disturbances in organs more or less under the influence of the sympathetic system. I enquired particularly about the patient's antecedents, his employments, his habits, the diseases he might have had, and what he might know of his blood relations; and all the other points suggested in my Tables of induction, which I constructed many years ago. The result of

this induction led me to suspect that the organ most at fault was his liver, though he had not any (what are usually regarded as) "symptoms." I instituted a treatment which I thought best calculated to induce copious secretion from that organ; the only peculiarity of which was that, in addition to the medicine he took, I endeavoured, by all means I could, to protect his skin from any check, and to prevent the action of the medicine from irritating in the smallest degree the alimentary surfaces. The result of this was the occurrence of very copious discharges of biliary matters, with complete subsidence of the tetanus. Dr. G. Bird now said he supposed I would open the abscess; but this I declined to do, until we had waited a day or two to see if the tetanus returned. Of it, however, there was no recurrence. Now, I hold this case to be very important, and were I writing on the subject, I should have a great deal more to say on it. I shall be obliged to recur to these more extended views, when I show that the labours on which John Hunter's fame will mainly rest, had nothing to do with Vivisection.

I must not conclude my remarks on Dr. Marshall Hall without mentioning a very remarkable passage, strongly suggesting that, after all, he attached, as I do, greater importance to the study of the living human body as a whole.

After stating the necessity for more *accurate* observation and *treatment* in epilepsy, he says that "even when we find effusion, softening, etc., are these "the diseases or the *causes?* Possibly not; they may "be the effects of the violent congestions to which the

"nervous centres may have been subjected during the "paroxysm. Without attention to this *living* patho- "logy, even morbid anatomy may lead us to erroneous "conclusions." I know a case of a gentleman who has for years lost every useful power over his lower extremities, so that he cannot walk without support, and whose arms are so nearly helpless, that he has but very restricted use of them; who has been treated for years by bromide of potassium and other so-called remedies, without ever having had, so far as I can understand, any efficient attention, much less that analytical examination of his internal organs, so strongly suggested by a comprehensive view of his history, and who has accordingly obtained no result but that of slowly diminishing power. I believe, had this case been tested with more reference to the internal organs, and the correlations of the sympathetics with the spinal marrow, in which I believe Hall would have concurred, it might have resulted in the greatest benefit.

To conclude, I have endeavoured to show that the experiments of Dr. Marshall Hall were unnecessary, and how from his own work, as well as from my own observations, they are superseded by the superior advantages of carefully interpreting the phenomena of the nervous system on the living human subject; all which I hope to exemplify further when I have to consider the works of John Hunter.

DR. JONES.

In considering the evidence offered to the Royal Commission, nothing is more inexplicable than the absence of information as to other modes of research,

and the necessary tests they would have afforded to the value or the necessity of Vivisection. The most zealous advocates of Vivisection do not pretend that it is vindicable in cases open to other modes of enquiry.

Why were not these communicated to the Commission? Was this reticence the result of the blinding influence of foregone conclusion—the absence of positive knowledge of what has already been done—or of ignorance or inattention to that which is obviously still within the grasp of Philosophical Pathology?

The experiments of Dr. Jones were cited before the Royal Commission in proof of the advantages of Vivisection, and, had they been necessary, I should have not made any remark on them; but now let us see how the case really stands. It is clear that Dr. Jones's experiments were not instituted from any predilection in favour of Vivisection, as contrasted with more scientific modes of enquiry. He saw many circumstances which it was important thoroughly to understand, of which there have been *abundant opportunities of* understanding, but which opportunities had been either totally disregarded, or allowed to pass away without anything like studious or scientific cultivation by his predecessors.

I must not be misunderstood as deprecating the value of Dr. Jones's researches in the abstract; but the means he adopted were not the only nor the best. No man could extemporise a renewal of such neglected opportunities; they could of course only be obtained by being patiently waited for as they might occur.

Abstractedly, Dr. Jones's researches were made with great care and good sense; but, had the true

mode of physiological and pathological research applicable to the object desired been adopted, they would have been unnecessary. Now we will on this point see how far Dr. Jones himself sanctions the view I have just taken of the matter.

After some very sensible remarks on the danger of hæmorrhage, the anxiety of the surgeon, and the importance of the subject, he observes that "there "are only two modes by which we are enabled to "obtain any knowledge on these subjects—*first*, by "*patient observation on the human body;* and, *secondly*, "by direct experiments on brutes. *War*, *accidents*, "*and disease*, have never been wanting; and yet the "records of our profession afford us but few and "detached observations on the suppression of hæmor-"rhage, if we contrast the knowledge we possess with "the importance of the subject. *Rash conjecture* "and *idle hypothesis*, resting on partial observation, "have usurped the place of that truth, which could "only be discovered by a series of observations "through every stage of a process which, one would "think, claimed the strictest investigation from the "moment that surgery became an object of individual "pursuit. The author has endeavoured to supply that "deficiency by a series of experiments on brutes;" that is to say, a deficiency consequent on the neglect of careful observation and record of the opportunities which have never been wanting from wars, accidents, and disease; to which we may add, *par excellence*, surgical operations. He further states "that he does not presume that his experiments afford "an unerring guide to the corresponding changes in the

" human subject; but he has diligently sought after the " results of other eminent surgeons and physiologists, " and especially after such observations made on the " *human subject*, as corresponded with the results " of his experiments." The experiments were carefully made, and, as regards their object and result, judiciously and modestly put forth, in a tone which invites respectful attention. He showed the kind of ligature best adapted for tying an artery; that when an artery was so tied, the internal coat was divided; that from these divided surfaces lymph was poured out which, assisted by the formation of a clot of blood, constituted the means by which the further bleeding was prevented. He also showed that an artery, when about to be tied, should not be unnecessarily disturbed or separated from its surrounding connections; and that (when you had the choice) you should avoid tying an artery near a spot where any considerable branch was given off from it, as this had an obvious tendency to impede or prevent the formation of the clot, a useful accessary to the prevention of hæmorrhage. Now, the *whole* of this might not have been absolutely new in practice, but that is immaterial to our present object. Dr. Jones, in the work we are considering, corrected various erroneous suppositions which the imagination had so frequently substituted for facts, and which have so often supplied either the illegitimate grounds, or the inadmissible excuses for Vivisection.

But, after all, I think every philosophical pathologist who will really give his mind to the subject, and certainly one who combines with his study the practice

of operative surgery, cannot review the labours of Dr. Jones without something of regret, however inclined they may be to excuse the natural impatience of an ardent enquirer; his mode of research was not that of choice, but resulted from the deplorable neglect or tardigrade cultivation of the splendid opportunities afforded in the practice of surgery. This is the way in which Vivisection proves such a plague-spot in medical science. Write as one may, men will not give the time and pains which are really necessary, and then they try this short cut to knowledge, which is either unnecessary or mischievous. But let us again hear Dr. Jones, in proof. "I have (he says) diligently sought "in periodical and other records of surgery for cases "of divided arteries in the human subject, to illustrate "and confirm the doctrine of the natural means of "suppressing hæmorrhage, as deduced from my ex- "periments; but I have been *mortified* at finding "those records so barren of important cases, and dis- "appointed at the imperfect detail of the few that are "before the public. I have especially to regret the "*total want of observation* of the condition of the "artery itself, even where the opportunity of examin- "ing it had been offered. Four or five remarkable "cases of limbs torn off, without hæmorrhage, even "from the largest arteries, are recorded, but afford "not the least instruction as to the means by which "the hæmorrhage was prevented, *because the artery* "was not examined." (Jones, p. 79.) He then refers to some cases by Petit, Morand, Garengeot, and Gooch, which I have not space to quote *in extenso*, but in relation to which a word or two will show that

the general object was to assimilate those which he had observed to those described as occurring in the human subject. He says: "In justice to the accuracy "of Messrs. Petit and Morand, as a proof of the exact "agreement between these observations in the *human* "*subject* and those which have been made in the "course of these experiments on brutes, I have great "pleasure in giving the following interesting quota- "tions relating to the permanent means of suppressing "hæmorrhage, by the effusion of lymph, or the forma- "mation of a coagulum of lymph."

In contending that these experiments were unnecessary, I can add nothing to that which Dr. Jones has himself observed and regretted. The practice of surgery in the cases which Dr. Jones has himself mentioned, present frequent opportunities of ascertaining all the facts connected with the ligature of arteries. To these many more might be added, but scarcely any more teaching than amputation, whether successful or otherwise—especially, however, the latter, when necessarily, arteries of different sizes are usually the subject of ligature. It is to this that Gooch refers, in the passage quoted by Dr. Jones, constructively in support of his views, when he (Gooch) concludes by saying, "which work of nature is pretty "evident on the *stump*." (p. 78.) Had these and other cases been investigated, and their instructive phenomena recorded, they would in a single year have furnished to the united observations of English surgeons alone, facts, in number, variety, and grounds of safe conclusion, far more valuable in every scientific sense than any amount of experiments on animals whatever.

But I need not further enlarge, by any observations of my own, the instructive and significant commentary of Dr. Jones, on the absence of that assiduity, that careful and comprehensive observation of pathological phenomena, which is really the only foundation of any hope of rendering medicine and surgery a more positive science. To attempt to supply this by Vivisection is the most palpable of errors, if it be not the most egregious of follies; and I fear that the very natural repulsion at the sufferings Vivisection involves, has indirectly ministered to the practice, by allowing the Vivisector to blind the public by a plea, which the public are not competent, and too many of *the profession are afraid, to challenge or publicly examine.*

JOHN HUNTER.

In considering certain allegations made before the Royal Commission, it is not easy to imagine anything more calculated to mislead the lay members of the Commission, or anything which did more to dwarf our obligations to that most elaborate, most circumspect, and most teaching of our physiologists, or, as Abernethey would say, physio-pathologists, John Hunter. In the first place, the allegation that he derived his improvement in the treatment of aneurism from Vivisection is absolutely incorrect, and betrays either an entire ignorance, or a most culpably careless examination of the facts whence the improvement was deduced. Again, the actual reticence, or suppression of what he really did, is, in a sense which I will endeavour to explain, and in relation to his colossal labours, scarcely less than an atrocious calumny. We will consider the improvement

in the treatment of aneurism, in the first place. It is tedious to have to refute statements which have been so often refuted already; but I know not what is to be done, if persons are found who will recklessly continue to assert what is demonstrably untrue. An aneurism is a disease which occurs in one of the vessels called arteries, through which the blood is sent forth from the heart to all parts of the body. Now these vessels, or tubes, are subject to disease. The tube sometimes becomes enlarged; but that is not the disease I am about to describe. In what we regard as aneurism, the inner coat, or lining, of the tube is ruptured, or gives way, and then the blood, being impelled against the outer coat, gradually distends it, and thus forms a tumour. This tumour becomes gradually filled by layers of the more solid parts of the blood, and thus pressing on the artery, as well as any adjacent structure, in some degree diminishes the impetus of the blood through the vessel. This has a tendency to produce a natural cure, which sometimes actually takes place, thus: the blood, somewhat impeded in its usual course, flows more or less through collateral channels; and the diseased artery, rendered thus less pervious to the current of the blood, gradually becomes obliterated, and thus we have what is called the spontaneous, or natural, cure of an aneurism.

Affairs, however, do not often go on so desirably; on the contrary, the constant impetus of the blood produces a gradual enlargement of the tumour, which, if nothing be done, ultimately gives way, and the patient dies from hæmorrhage.

So much for the disease which, like all pathological

facts carefully interpreted, has suggested the most reasonable mode of relief. This gradual diminution of the impetus of the blood, is just what we endeavour to effect by our various operations, such as graduated and constant pressure, or passing a ligature around the artery. The old way was to open the tumour, tie the artery at the entrance and exit from it, and trust to the collateral branches to carry on the circulation. There were many objections to this mode of proceeding. It was serious, dangerous, and, too often, unsuccessful. John Hunter considered that the failures resulted, in many cases, from the artery being tied too near the disease, and where, the vessel being unsound, the reparative processes were not effected. You should tie the artery, said he, farther from the sac, where you probably will find that the artery is sound. This he proceeded to do; his anticipations were not long in being verified, and this forms the improvement he discovered in the treatment of this disease. The whole history of aneurism is very interesting, exemplifying how often it happens that the real study of disease in the living body suggests the best means of assisting nature in the cure of it. John Hunter's idea, that the artery was often diseased, was a fact ascertained by dissection *of the dead;* so was the fact that the diseased condition did not necessarily extend far above the aneurism; and that diseased parts were not endowed with those reparative powers, which are usually found in parts which are sound. Now, these were no random shots, so to speak, of the imagination, to be proved or disproved by experiments

on animals, but well ascertained facts from the pathology of aneurisms which occurred in the human body, and which are seldom, that I know of, ever seen in animals.

The treatment suggested by these facts was very simple, viz., to place the ligature higher up, that is, nearer the source of the circulation, where it might be reasonably hoped that the artery was sound. Now, there are very many people who do not reason on facts, as John Hunter did, and who found their opinions on their imaginations or their wishes. Accordingly, John Hunter had to undergo that sort of ordeal which every man has more or less to suffer, who dares to oppose conventional errors. One rather formidable class of opponents said that the disease was not restricted to the aneurismal artery, but that the arteries were *generally* affected; and, if a case were found in which there were diseased depositions in the arteries, which sometimes happens, there was the difficulty of showing that it had no *necessary* relation to aneurism. The question being whether, as a local aneurism, the disease was or was not extended to the neighbouring part of the tube. A somewhat disagreeable controversy followed; but at length the truth prevailed, and one of the greatest improvements in operative surgery became established.

Now, were John Hunter alive, I think nothing in the whole controversy would have annoyed him more than to hear it said that he discovered this improvement by experiments on living animals. Let us consider then what John Hunter did with regard to living animals, as a just preliminary to the recording of the greater

honour which results from his penetrative perception of the phenomena observed in the living human body.

We will discuss the subject of aneurism first. Some of his opponents alleged that aneurism resulted by a so-called weakness of the tube (artery). To disprove this, Hunter dissected off the external coats of an artery, so that the blood could be seen through the denuded inner coat; the wound was then closed in the usual manner. Now, when the animal was killed, everything was found healed and as sound as it was before. So much for the assertion that Hunter discovered his improvement of the treatment of aneurism by Vivisection. Now, his real Vivisections were unnecessary, but they show how much of that absurd proceeding results from the deficient study or the stupidity of others. When what is popularly called matter, or pus, was formed in any part, it was concluded that it resulted from a sort of melting or solution of the solid part; Hunter must have known better, because we have several diseases which prove that the idea was unfounded. Hunter, to assure others, produced inflammation and pus in certain parts of animals —in parts wherein he could most clearly have shown that there was none of that solution which had been supposed. But in those very parts, besides some others, the occurrence of disease had shown the same fact over and over again; so that his experiments were unnecessary. He also made experiments on arteries, but they were very different proceedings from those which represent the proceedings of some modern Vivisectors. Hunter wished to know how far the

contraction of the arteries was consequent on their vital force, and how far it resulted from mere elasticity. In aid of this enquiry he bled a horse to death, and then measured various arteries in different states of rest or artificial extension—First, when taken from the body; then he stretched them as far as he could, and measured them again; lastly, having allowed them to contract, he measured them a third time; and the difference between the last and that when first taken from the body, immediately after death, he considered the degree of contraction depending on their vital, or muscular power. Then he produced inflammation in a rabbit's ear, and, having killed the animal, he injected the vessel to show the enlargement of vessels in inflammation, by comparing the ear, which was the subject of experiment, with the other. Long ago, I observed on the uselessness of these experiments: Do we not see the vessels enlarge before our eyes in ophthalmia? Do we not see vessels now carrying red blood, which ordinarily only carry colourless fluids? Do we not feel the artery going to a whitlow beating with more violence than that in the opposite finger? Do we not see the veins more distended returning from an inflamed part? Do we not feel pulsations which did not affect our sensations before? Do we not see the part vividly red, there being no fluid in the body of that colour but the blood? If there be anything in medical science like demonstration, surely this is it. There is no possible objection to it, whilst, were we disposed to refine, the case of the rabbit's ear might perhaps be open to objection. The vessels might,

by death during inflammation, have been deprived of that contractile power which muscular parts exhibit after death, and of which they are, under certain circumstances, actually deprived; and thus the same force of an injecting syringe, which in the sound ear would only produce the ordinary dilatation of arteries, might easily, in vessels so affected, produce a greater distention. I do not say that I entertain this opinion, but no one can assert that it is impossible; whilst there is no objection of this kind attached to the phenomena which I have mentioned as observable in the ordinary development of inflammation in the human subject. Hunter has been seldom studied, so far as I know, with anything like the attention he deserves. He is in some points of view a difficult author; he is a very profound thinker, and there is a circumspect regard and perception of facts, bearing more or less on the subject he may be more immediately engaged in, which justifies that epithet of Abernethy's, which induced him so often to use the term—"That Argus-eyed man, "John Hunter." But we must recollect the state of things in John Hunter's time, before we can understand how such a mind could entertain experiments on animals as an auspicious source of investigation; but this would lead me far beyond my prescribed limits. Hunter rendered services to medical science which have represented *seeds*, which have germinated already so as to afford very important truths, and which hold out prospects of sooner or later conferring more of a positive character on medical science, than the labours of any writer with whom I am acquainted. Let us hasten then to those, and correct that idea,

erroneous as it is unjust to his memory, which would connect his investigations with Vivisection. John Hunter's enquiries were confined to no one department of nature, nor, perhaps, to any mode of research. His experiments on heat, and other subjects, involved very interesting researches, both in the vegetable as well as the animal kingdom. Few men have left such material monuments of indefatigable labour, or examples of more penetrative research. But John Hunter's elaborate and comprehensive observation of various phenomena was never more beneficially, nor, as I believe, more earnestly exerted, than in assisting him in his careful and discriminative interpretation of various but obvious facts occurring only in the living man. I allude to that correlation of parts which I have already mentioned, and which he called the sympathies of the body. Nothing can be more instructive, nothing more encouraging, than these long neglected labours. Abernethy used to say that when Hunter lectured on the sympathies of the body, one portion of his audience were seen to be "giggling," whilst another was disposed to go to sleep. There were, however, some of his contemporaries who knew better the value of all that he did, and none more so than Cline and Abernethy. Cline was an eminent surgeon, and with a very sound and extensive reputation. He fully appreciated John Hunter; and on the unfortunate and mysterious destruction of his papers, declared to Mr. Clift that nothing would have ever induced him to destroy a scrap of paper even, on which John Hunter had written anything. Abernethy was a different kind of man; he had a penetrative

perception, and an instinctive admiration of Hunter, which induced him to pay special attention to everything he wrote or did. He, therefore, fully appreciated what he said of the sympathies, and, with that rapid grasp of the more immediately useful portion of any research, said: "John Hunter proved that the whole "body sympathised with all its parts." And here we see the beginning of that important practical deduction of Abernethy's—"The Constitutional Origin of "Local Diseases."

Now, these so-called sympathies are what any of my readers may, in great part, observe for themselves. We all know that many things taken into the stomach, with—and very commonly without—any pain in the stomach, will produce headache. Well, John Hunter would simply say that the head sympathised with the stomach. He then went on to trace these relations between various parts, showing that sometimes the sympathising part or organ seemed to suffer more than the part whence the sympathy was excited. Now, every word of this, as we shall show, was convertible to most important purposes.

The extension of this kind of observation is a striking example of the manner in which the philosophical interpretation of simple and obvious phenomena has led to great results, and, as I think, even already, to important generalisations. I have already remarked on the relation between John Hunter's showing that the whole body sympathised with all its parts, and the "constitutional origin of local disease" of Abernethy; and here, I may state, lay the first germinations of that harvest, of which it is impossible as yet to predict

the ultimate results, but which has already produced what I shall contend is the real basis of scientific surgery—and not merely so, but the basis of a more positive medical science. If Abernethy's improvements have not yet been fully developed, it is in great measure because he has been misunderstood and, what is still worse, misrepresented; but truth has the property, in time, of penetrating the darkest recesses, and accordingly we see (very imperfect, it may be, but still practical) recognitions of those views so essential to all rational practice, whether of surgery or medicine, which it was Abernethy's object to enforce. Abernethy, however, grasping at a general result of Hunter's views, left many relations of those correlations of functions it was Hunter's object to expose, unapplied, and I do not observe that they had been carried out, or even observed, by Hunter, although they are the necessary outgrowths of that which he did discover and remark on. Some of his remarks, however, show that he had not as yet observed their more extended relations, which, I infer from his speaking of things as *quasi* exceptions, are undoubtedly more like, if not entirely the rule. In this there is a striking similarity in Mr. Boodle's first correspondence with Abernethy, and John Hunter's *quasi* exceptional examples. Mr. Boodle, in writing to Abernethy, observed that he had seen cases in which the organ principally disordered had not evinced it by any of the usual, or any symptoms, or something to that effect. John Hunter, as I have already remarked, said that the sympathiser was often more affected than the part with which it was sympa-

thising. Now, about these two simple and undeniable facts lay an important mass of consequences, which, as I contend, have been subsequently brought out by myself and, for aught I know to the contrary, by some others. I have very little doubt but that John Hunter was not wholly unaware of that for which I am about to contend, but his life was not long, his death was sudden, and his papers were burnt! Well, be that as it may, I hope some of us may obtain useful deductions from his *thoughts*, and show that the germs he left may from their prolific results show how absurd, as well as unjust and misleading, it is to associate his memory with the least important things he ever did, still less to falsely refer to such experiments on animals, his really useful improvement in the treatment of aneurism.

Now, the result of a cultivation of the phenomena of sympathy between various organs is no less than this—that the symptoms of disease in the commencement, and very often for a long time, are almost never the seat of the essential cause or origin of the malady. We are, of course, not speaking of accidental injuries, as wounds, fractures, etc. Multitudes of cases, in illustration, are even popularly familiar. No one now looks for the cause of gout in the part which is the seat of the pain, and other symptoms; neither is anyone, I suppose, misled in such cases as to the true interpretation of the occasional assertion of the patient that his health is perfectly good. Again, in diseases of the skin, I suppose very few (if any) look to that important organ as the seat of the cause; but there are many other more serious and more recondite disorders, where the true relations of the phenomena are

not so readily observed. I cannot exemplify this better than by referring to diseases, in the first place, to which some Vivisectors have directed their attention. I have already cited one case in relation to the spinal marrow, where the *symptoms* were wholly referred to that part, whilst the cause lay in an organ (the liver), the functions of which are under the influence of the sympathetic system. Again, in diseases of the heart, perhaps there is no organ in the body more frequently disturbed by conditions primarily occurring in other parts; nothing of this kind can be obtained by any number or variety of dissections of living animals. It is simply impossible, from such a source, to elicit by experiment those phenomena which disease and disorder—nay, even many phenomena in health—so beautifully, so teachingly, exhibit in the living human body. The study of the correlations of different organs, in the first place, enables us in various cases to warn and protect people from consequences which we cannot cure, and which furnish the proofs, the truth, and justice of those warnings—painfully, it is true—but teachingly, nevertheless, in cases wherein they are neglected. Again, in what we term cure of disease, we are enabled to do more in a few days, or it may be even less, by hitting on a *primarily* affected organ, than has been done in weeks—aye, I have known it even in months—by those who have been addressing themselves to symptoms, or what is termed routine, without the smallest benefit. There are very much more serious errors which arise from want of familiarity with these correlations. It is through these physiological relations that we arrive at the

true perception of those compensating actions, which, in their use, open new fields for more positive and intelligible practice, the superiority of which is never more clearly proved than by the calamitous results which too commonly, up to the present time, attend ignorance of or inattention to them. In some cases that neglect leads to the *excitement of organs*, when, so to speak, the *very essence* of the treatment, and the only ground of hope, is to keep those organs in the utmost practical condition of repose. This, again, is not all. The knowledge and familiarity with compensating actions, enables us to reflect light back in some most difficult cases in enquiring in abnormal conditions of excessive function, as to what function the œconomy, is thus supplementing by transferring its labours on another. I have seen a case, in which eight of the most eminent men in London had been consulted, in which only *one* had any idea of the organ primarily affected; and he did not differ, beyond saying that, although he admitted the correctness of the general opinion, yet he thought another organ "had something to do with it." This was a case in which the disease was referred to the kidney by the whole of the medical men, with the exception to which I have alluded. After the institution of proper enquiries after the plan suggested in my tables, I ventured on saying that the principal disease was not in the kidneys, but in another organ; and that when that was properly administered to, they would find, if I were correct, that the albumenuria would cease, and which accordingly it did. I could mention many more striking cases; but I must not forget that I am not writing a medical treatise, further

than is necessary to sketch some of the outgrowths of the thoughts of John Hunter, which have germinated in other soils, and in the hands of less distinguished cultivators; but which encourage the confidence and foster the hopes of less gifted enquirers, that the laws of nature lie open to that industry and common sense, of which the inductive philosophy is the directing grammar, and that, whilst the field is represented by a multitude of *facts* furnished by health and disease—the instruments by which they are to be interpreted are industry and common sense—the greatest obstacles, prejudice and Vivisection. Already that correlation of parts which lights up the cloud formed by a mere symptomatology, is opening new paths to the explanation of affections of the brain, in which the primary disturbance has arisen from the violation of physiological laws, and the disturbance of the brain from its sympathy with some other organ. Any one organ may so far disturb the brain, as to weaken its powers of resistance to any class of impressions, and thus lay it open to that worst of all inflictions, insanity; where the failure is referred to some recent impression or calamity, when the impotence to resist it lay in the previous diminution of its normal power, through the reflected disturbance of some bodily organ. I wrote of this nearly forty years ago. I have lived to see the general truth recognised, and never so emphatically as by a physician, whose practice was almost entirely devoted to the treatment of the insane. I have much more that I have worked out on the important consequences traceable to the thoughts of our great physio-pathologist (Hunter), but I hope I have said enough to convince

anyone who will study his works, how absurd it is to trace his labours to any dissection of living animals. I used the word "gross calumny;" well, that is rather strong, but, *scientifically* speaking, I think it a grievous injustice to a man's memory, to whom we owe such obligations, to associate his name with Vivisection, and to select as an example that which was *demonstrably* untrue. This, under many circumstances, would be unimportant, but before a Royal Commission a man's memory should not be so associated, and especially in aid of Vivisection, whilst our real obligations to him were not explained; and which not only exemplified better, more philosophical, and more fruitful modes of research, but which, more than anything that anyone has done, showed how superior his most matured mode of research was to that which he was only quoted to support, by the selection of a discovery falsely attributed to Vivisection, whilst the fact is that the discovery resulted from the study of phenomena which were alone to be found in the *human* body, and which were applied only through a careful study of the phenomena of health and disease. The thoughtful suggestions of John Hunter, in his short but laborious life, are so abundantly prolific of fruitful application — both immediate and remote — when carefully studied; that they suggest the symbol and contrast represented by the seed to the tree of the forest; whilst the endeavour to demonstrate the laws of life by dissecting living animals, seems nothing less than an attempt to supply, by the grossest species of sensuous curiosity, a substitute for the most exalted exercise of the intellectual faculties.

CHAPTER II.

VACCINATION.

One of the most remarkable statements in the mass of evidence given before the Royal Commission, is the following:—"The whole history of the discovery of "Vaccination—a discovery second to no other in the "benefits it has conferred on the human race—proves "that it is based on *experiments* on living animals, the "full meaning of which was interpreted by the acute "and philosophic mind of Jenner."

Had I been asked about this subject, I should rather have said that the whole history of Vaccination formed one of the most triumphant illustrations of the superiority of observation, as contrasted by what is intended by the phrase, "Experiments on Living "Animals;" and, as Charles Bell has said that physiology is a science of observation, I contend that, as careful observation and logical interpretation of obvious phenomena is never without some fruit, so Vaccination is a proof of the most fruitful results which have been wholly deduced from observation. In examining these two very different statements, we shall secure no small assistance from Mr. Turner's own statements. But first, I would protest against the latitude indulged in

as to the experiments which they call experiments on living animals. Always begging the reader to bear in mind the real objects of the Royal Commission, I objected to those experiments of Dr. Hope's being called Vivisectional, which were preceded by a blow on the brain with a hammer, which destroyed the sensibility of the animal; not, indeed, but that they were open to objection for other reasons, but not on the received sense of Vivisection. Further, let us hear Mr. Turner. "It is well known that the discovery of "Vaccination was made by Dr. Jenner from *observa-* "*tion* on cows, and those who milked cows, in the "dairy county of Gloucester. He *observed* that cows "had occasionally a pustular eruption on the udder; "that those who milked cows so affected contracted "pustules on their hands; and that such persons "enjoyed *an immunity from small-pox*." This is perfectly true; but where is there anything about experiments on living animals? The above statement was really *the basis*, and, with the exception of the actual inoculation of the boy Phipps, the *only* thing necessary to the whole success of his discovery. But, once more, to quote Mr. Turner. "From these *observations* he "proceeded to investigate carefully the whole subject, "and established the practice of Vaccination, which "has been of such *enormous* importance to humanity." Again, Mr. Turner says: "Jenner's *observations* were "not limited to cows, for there was another step in "the process, viz., the communication from the "horse to the cow of matter which occasioned in the "cow the pustular eruption. Jenner *observed* that "those cows which had their udders affected had

" been milked by persons who had been attending " horses having an affection of the hoof, called grease " of the hoof." Now, what is the meaning of Vaccination being "Based on experiments on animals?" Can anything justify this statement, especially when we consider the *nature of the proceedings* to which the investigation of the Royal Commission was *really* directed. The fact is, that the experiment here referred to, had nothing to do with the *essential* benefits conferred by Vaccination. Many circumstances had more than suggested that the disease in the cow might be the same as that known as grease in the horse—an affection chiefly of the heels (not hoofs), but which is not *necessarily* confined to feet or leg. This was a collateral circumstance not uninteresting in itself, but having nothing whatever to do, as I have just observed, with the discovery of Vaccination. Jenner did not even make the experiment in question, which, after all, was simply inoculating a cow with the matter of grease from a horse, and so producing a vaccine pustule. Jenner did not, it seems, think very much as to the necessity, however he might regard the experiment as expedient. And as Dr. Baron has been quoted in support of the general allegation as to the discovery of Vaccination, which cannot be supported, let us hear what Dr. Baron says on this collateral point: " Although Dr. Jenner's opinion respecting the " origin of cow pox is comparatively of *little moment*, " *when contrasted* with the important consequences " arising from the successful practice of vaccine in- " oculation, it is nevertheless necessary, while investi- " gating his character as a philosopher, to show that

" as this was considered a wild speculation, he pro-
" ceeded with his usual caution and discretion. The
" fact that the disorder in the cow originated from the
" horse had not been proved by direct experiment
" when he *published his enquiry;* yet the *evidence* on
" which this doctrince rested was so *complete*, as to
" entitle it to much attention. Jenner himself has
" stated that evidence, thus—

" 1st. From its being the fixed opinion of those who
" have been in the habit of attending to cows
" infected with this disease for a great number
" of years.

" 2nd. From its being a popular opinion in this great
" dairy country; and from the *cautions the*
" *farmer observes* when he has a horse with a
" sore heel.

" 3rd. From *observing* that in almost every instance,
" the appearance of cow pox at a farm was
" preceded by some disease of a horse at the
" same farm, which produced the discharge of
" some fluid from the skin.

" 4th. From having attempted, in vain, to give the
" small-pox to the son of a farrier, who had sores
" and a fever from dressing a diseased horse.

" 5th. And from the peculiar appearance of the pus-
" tule, and its disposition to run into an ulcer
" on the arm of the boy, who was inoculated
" with matter taken from the hand of a man who
" received the infection from dressing a slight
" spontaneous sore on a horse's heel."

In a note, Dr. Baron observes: "I cannot refer to
" this incident, without calling the reader's attention

"to the modest and diffident manner in which the "author speaks of a fact which was *well-nigh conclu-* "*sive* as to the truth of his doctrine. A beautiful "representation of the pustule, in an advanced stage, "is given on the second plate of the enquiry. The "character of the pustule is so correct, as to excite "some surprise that it has been so little attended to, "etc." Well, now we come to an experiment on this point: "Mr. Tanner inoculated a cow's teat with some "matter taken from the heel of the horse, and "produced a vaccine pustule." As a collateral fact this may not be without interest, but it had nothing to do with the discovery which was, as Jenner himself shows, based on observation. Now, by what latitude of interpretation this can be stated as the basis of Jenner's discovery, let the reader imagine. He will not find it in Jenner's "Enquiry," nor in Dr. Baron's life, nor anywhere else, that I know of, except in the evidence before the Royal Commission.

Dr. Jenner, in his evidence before a Committee in the House of Commons, in the year 1802, said—"My "enquiry into the nature of the cow pox commenced "upwards of twenty-five years ago. My attention to this "singular disease was first excited by observing that "among those whom I was frequently called on to in- "oculate, many *resisted* every attempt to give them "the small-pox. These patients, I found, had under- "gone a disease they called cow pox, contracted by "milking cows, affected with a peculiar eruption on "their teats. On enquiry, it appeared it had been "known among the dairies time immemorial, and that "a vague opinion prevailed that it was a preventive of "the small pox."

In his ensuing observations, he met with many circumstances of an exceedingly puzzling nature, but which he investigated with a diligence and penetrative sagacity of observation, which was rewarded by the discovery of facts, and suggestive of analogies between the small-pox and the vaccine, which was really a very proximate link in the chain which was to result in the discovery. Having remarked circumstances which led him to say, "Here the analogy between the "virus of small-pox and cow pox becomes remark-"ably conspicuous," he says: "During the investi-"gation of the casual cow pox, I was struck with the "idea that it might be practicable to propagate the "disease by inoculation, after the manner of the small-"pox, first from the cow, and finally from one human "being to another. I anxiously waited some time for "an opportunity of putting this theory to the test. At "length the period arrived. The *first* experiment was "made upon a lad by the name of Phipps, in the "spring of the year 1796, in whose arm a little of the "vaccine virus was inserted, taken *from the hand of a* "*young woman*, who had been accidentally infected by "a cow. Notwithstanding the resemblance which "the pustule thus excited in the boy's arm bore to "variolous inoculation, yet, as the indisposition attend-"ing it was barely perceptible, I could scarcely per-"suade myself that the patient was secure from the "small-pox. However, on his being inoculated some "months afterwards, it proved that he was secure. "This case inspired me with confidence, and, as soon "as I could again furnish myself with virus from the "cow, I made an arrangement for a series of inocula-

" tions. A number of children were inoculated in " succession, one from the other; and after several " months had elapsed, they were exposed to the infec- " tion of the small-pox, some by inoculation, others by " variolous effluvia, and some in both ways, but they " all resisted it." (See p. 2 of the Appendix to the Royal Commission of 1857.)

I think I have now said enough to show that what is *intended* by "Vivisection" had nothing whatever of influence in the *discovery* of Vaccination. Jenner trod the only path which is most auspicious for physiological or pathological discovery; that is, by the study of facts—whether of disease or other—in the living animal, and which has, as he demonstrated, not only the property of most probably leading you to that which you seek, but almost always to other facts of inestimable value. To have Jenner's labours brought forward as evidence of the advantage of dissection of living animals, to a Commission appointed specially to consider the claims of Vivisection, is something that I am unwilling to describe by terms which many might think the most appropriate; but this I will say, that like many other parts of the evidence, it is not only not borne out by the facts of the case, but is of an entirely misleading character. If a man makes a statement which is absolutely untrue, well, it may in time be contradicted, though the advocates of Vivisection have nevertheless repeated assertions which have been as repeatedly refuted. But the more mischievous error is that which has a mixture of truth, of which the evidence before the Commission also affords examples. This leads to confusion of ideas, which can only be

cleared, or disentangled, by formal and more or less lengthy statements, which not one person in ten, perhaps, will take the trouble to read, and of which many of the public find a difficulty in obtaining a clear idea. Add to this, the labour of an erroneous assertion is nothing, whilst the exposure of it may impose unnecessary expenditure of time, money, quotations, and much annoyance. I believe my limits will hardly allow of my saying much more on Jenner or Vaccination; but there is something so misleading in that philosophy of the eye—which is one element in Vivisection—that it tends to mar any comprehensive view of a subject, by the small field of vision which seems inseparable from that kind of study. It is mischievous in standing so much in the way of other modes of research. Now, with all that has brought small-pox so much to the front in the present century, I contend that the philosophy, so to speak, of that has not been utilised further than Jenner left it; yet I have found—and, as I think, proved, in my analysis of fever—that there is no one disease which so clearly assists you in detecting the real nature of fever as small-pox; not, indeed, as an isolated series of facts, but as leading up by a long chain of phenomena, deduced by a large induction from all sorts of fevers, European and others; and also those which occur in the practice of surgeons as well as physicians, to a most important generalization. Through such a series of facts, you arrive at a clear *definition* of what is called fever; and when you desire a crucial test (very important just now) of whatever is not, as well as what it is, there is nothing serves you so well, so

perfectly, as a reference to the known phenomena of small-pox. This I have shown in lectures on fever, and with every test that I could procure; but whilst the Americans thought the lectures worth reprinting, they have excited but little notice in this country. No doubt the labour of inductive research is very expensive as regards time. It is not otherwise laborious, so much as requiring some industry; and I fear the public are not sufficiently informed as to estimate, much less encourage, it. Still I would never replace it by a mode of research which is no substitute for it, which is never necessary, seldom fails to mislead, dwarfs that circumspection of phenomena necessary in all philosophical studies, and furnishes no comprehensive views of the field before you, nor any of those ulterior thoughts or suggestions, the encouraging accompaniments rarely absent from true paths of study. Few men were more tried as to patience or industry, or opposition, than Jenner; and while he is one of the ornaments of the country of his birth, he is an example of the right mode of study to those who have followed him.

DR. RUTHERFORD.

In the few remarks which I am offering on the evidence before the Commission, it is not my object to excite anything called sensational, because it diverts the attention from examining the facts under the light of common sense—in short, by the intellect; but in remarking on a practical subject, which has attracted the attention of Dr. Rutherford, I cannot bring the

matter fairly before the reader, unless I describe the experiment by which he proposed to investigate or illustrate it.

A living dog, having been opened in the abdomen, has his biliary duct dissected; being divided, a tube is inserted therein. An opening is now made into his bowels, and a solution of rhubarb thrown into the canal. A tube is also inserted into the windpipe, and, by means of a pair of bellows, air is blown in to keep up artificial respiration. The medicine having been injected into the alimentary canal, the wound in the abdomen is closed, and the animal wrapped up in cotton wadding, to prevent a fall of the temperature.

" Q. Then the actual operation would last about " half an hour?

" A. About half an hour.

" Q. In what state was the dog at the end of that " half hour?

" A. Simply paralyzed by curari—having artificial " respiration, by means of a pair of bellows, kept up— " having a tube in its common bile duct, with the bile " dropping from it.

" Q. And in that state it would go on for eight " hours?

" A. Not exactly; not *simply* in that state. The " wound in the abdominal wall being once or twice, " or sometimes three or four times, I dare say, opened, " and a substance injected into the bowels; the wound " was then closed again, and the animal wrapped up " in cotton wadding.

" Q. Then, supposing that curari does not deaden " pain, would there not be very great pain during that " eight hours?

"A. *I do not think so; certainly not very great* "*pain. I question if more than trivial pain.*

"Q. Does not the artificial respiration cause pain?

"A. No, I think not. It is impossible to say, "unless the animal could speak; but the conditions "are such that you could not reasonably suppose it "would cause pain, *wind being blown into its chest,* "and distending it *as in ordinary respiration.*"

And now, what does the reader (who for the first time hears of these proceedings) suppose is the object of this elaborate experiment?—To see if rhubarb acts upon the liver. Well, I am happy to see that Dr. Rutherford does not think it necessary to infer that rhubarb acts on man, as he considers his experiment a proof of its action on the dog; and further, that he leaves that to be determined by the medical practitioner. Now, it is very possible that there are many respectable old ladies, who might have put Dr. Rutherford in a better way of determining the question, the solution of which he sought by the foregoing experiment; but not usually trusting my conclusions to such authority any more than would Dr. Rutherford, I think it more respectful to a professional brother, to discuss the subject in another way. Lest, however, the reader should put a misconstruction on the phrase "old ladies," disrespectful to Dr. Rutherford, I only use it to emphasize the incalculable superiority of a man who looks to bedside work, or observation even from the most humble sources, to any more artificial mode of enquiry. I have known a sensible sister in an operation ward, able to give as good a prognosis of the result of a case as the surgeon. By observation, they obtain a great deal of

very useful knowledge as to remedies; by observation of secretions of one kind or other, which many a surgeon may often trust to report, instead of personal examination. Were Dr. Rutherford a practising physician, which I understand him not to be, I should be perfectly at a loss to conceive how he could have placed any reliance on the results of the experiment in question. The elements of fallacy are endless. In the first place, the unnatural condition of the dog renders it absolutely impossible to say what would happen. Distress and pain will very readily act on some secretion, but no one can predict what. The "ultima moriens" symptom in most animals, is usually accompanied by some secretion, apart from the mere relaxation of the sphincter muscles. Then again, when an organ is so disturbed, sometimes it is characterised by profuse secretion; at others, by a transfer of its function to some other organ.

Another obstacle to any correct conclusion, was the condition produced by curari. In answer to the question, " Could it not have been performed under " anæsthetics ?

" A. It could not have been performed, so far as " I know, under any other agent but curari, the object " being to keep the animal perfectly still; because " whenever an animal moves the muscles of its abdo- " men, it squeezes out bile, and the consequence is that " the flow is rendered irregular; therefore, it is abso- " lutely necessary that any movement which takes " place should be a regular movement; in consequence " of that, curari is given to prevent motion of the " limbs—to prevent motion of the muscles of the

" belly—and the only compression of the liver which " takes place is produced by the lungs being inflated, " by means of a pair of bellows, at regular intervals." Curari was not given to diminish pain, but to paralyze the muscles. The opinions of experimenters are divided on this point, one of those most extensively engaged in Vivisections (Bernard) being of the opinion that curari only paralyzes motion, without producing insensibility. Well, we have said enough of the mode of getting at the liver. Now let us consider some other mode. Before I retired from active practice, I was an early pedestrian in the park, where I would sometimes meet a professional brother, and very interesting conversations occasionally occurred. One morning, meeting one of the most eminent physicians in London, our conversation fell on the liver, and the best means of exciting its secretions. On his stating his views, I said, the difficulty not unfrequently results from the ease with which, in cases of torpid liver, some compensating function runs away with your medicine, whilst that for which you intend it offers no sign of its influence. I then detailed to him several cases in which calomel and other preparations of mercury had been given without producing material evidence of biliary secretion, and yet sometimes, with the very same remedy (otherwise managed), very copious—and, even, very remarkable—quantities had been procured. Without fatiguing my readers with a long narrative, as to how I had arrived at the conclusion, I said, Hence it is clear that you will almost always succeed best in exciting the liver if you so contrive matters, that your remedy pass *slowly* through

the duodenum (the bowel into which the liver pours the bile). This you may accomplish by one of two methods—either by giving your remedy by *very small* and repeated doses, so as to represent an interval of many hours before you obtain the action desired; or by combining mercurial or any other medicine you prefer, in larger doses, with *small* additions of some sedative, or larger additions of an *aromatic*. In consulting practice, I said, perhaps the small doses of aperients prove the more convenient, unless you are seeing the patient every day, because it is not always possible to hit at once the respective doses of sedative and the excitant in any one case; but the effect of the first prescription, if it does not answer your intention, will generally guide you as to the desired relative proportions. There are, however, a great many advantages arising out of this view. It will enable you, by the assistance of any sedative or aromatic, to convert almost any aperient into an excitant of the liver, and will thus, when sent for into the country, prove to be a very efficient addition to your means, by enabling you to employ what you may find in the cupboard or the family medicine chest, as the case may be. The investigations, however, to which I have been thus led, have brought out some very curious results, some of which have been published in my works, and which throw entirely new lights on the actions of mercury and other things, proving that they do not produce what are usually considered their *specific* effects, as mercury, etc., but in virtue of a *quasi* poison, suggesting a most important generalization, and which I shall not live long enough, I dare say, to work out, but the first step of

which might be readily made to appear by taking together the facts which I *have* published, with those brought out in a very interesting work, by M. Morel, entitled, "Sur les Degenerescesnes physiques et Morales de "l'espèce Humaine, et des causes qui produisent ces "variétés maladives," Paris, 1857. I cannot, of course, consistently with my present purpose, go into these matters; but I could easily show that no experiments on animals could ever produce results of such immense practical value as any of the facts to which I have above alluded, and which would (were it not for the *sufferings which Vivisections have involved*) have rendered the groping in the dark of Vivisectors all but amusing errors. I will give a result or two from my own facts. Salivation, held so characteristic of mercury, can be equally effected by many other things, as aloes for one; and this, again, has been first suggested to me by bedside observation of salivation, produced by diseased conditions, where no medicine of any kind had been taken. I have no opportunity of enlarging these remarks; but I will add my professional brother's concluding remark, on parting—"Well, you have given me, this "morning, one of the most useful lessons I have ever "received." To conclude, I have only to say that my experience induces me to regard the point from which the action of remedies may be most auspiciously investigated, is first to regard them as foreign substances, which are no sooner administered to the body, than they induce actions for their expulsion. That, ordinarily, organs evince certain tendencies to deal with certain matters; but that this tendency undergoes infinite modifications, by the varying state of different organs,

and the corresponding action thereby excited. That the safety of *almost* all medicines lies in the efficient, however gradual, expulsion of them; and unless this end be secured, minute doses may easily become sources of the most serious mischief. The facts leading me to this sort of (distant still, no doubt) approach to a generalization, have been slowly gathered from the study of an infinite number of phenomena in the living, and tested, whenever opportunity offered, by the dead, but never by a single Vivisection.

SIR WILLIAM GULL.

I have not space to say much on the evidence offered by Sir William Gull, and I the less regret it because for the most part it is either irrelevant, or such as most people, whether lay or professional, may be able to answer or estimate without any instruction from me. But not to pass over without some specimen of that which really relates to the subject of the enquiry, I will quote the following:

"Q. May I ask you, as a great physician, whether "you can enumerate to us any considerable number of "therapeutic remedies which have been discovered by "this *process of Vivisection?*

"A. The cases bristle around us everywhere; our "knowledge of dropsical affections, of pulmonary "apoplexy, of enlargement of the liver, and the whole "category of such affections, was due to Harvey's "discovery of the circulation.* We knew nothing "about them before; knowing, therefore, their causes,

* Which, as we have already shewn, was not due to Vivisection.

"we are able *in the same direction* to apply the "remedies. Then again, this same discovery of "Harvey's taught us the use of the transfusion of the "blood in cases of hæmorrhage, which is a cure for it. "Sir Astley Cooper's experiments on arteries shewed "the surgeons that they could cure* aneurisms of the "larger vessels, even to the extent of tying the aorta, "the largest vessel in the body. He tied the artery "in the dog, and the dog recovered. Hall, by his "experiments on the nervous system, pointed out to "us the *theory of all the spasmodic affections*, and how "to apply our treatment by removing the exciting "cause—it might be a tooth, a specula†; it might be "the irritation of a wound; all of which is essentially "therapeutic.

"Q. But have you any improved mode of healing "a wound which has resulted from these experiments?

"A. Yes. Suppose a man stuck his leg through "a window, by accident, and some glass got into it; "you would *twenty times more carefully* examine that "wound, to remove the particles of glass, with our "present knowledge of reflex action, than you would "have done before."

Well, Dr. William Gull is a physician, and may, without any disrespect, not be supposed to be a highly accomplished surgeon. It would have been well to have had this piece of evidence made the subject of that *quasi* cross-examination for which some of the Commission who were advocates for Vivisection

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* Equally inaccurate.

† Spiculum.

shewed so much inclination. I do not believe that there is a single statement in the two preceding answers which I cannot shew to be either absolutely incorrect or of a most misleading character. Of Harvey we have already shewn that the discovery of the circulation *was not* the result or the effect of Vivisection. So it is not necessary to consider the somewhat sweeping assertion that physicians before that time knew nothing of dropsical affections or enlargement of the liver. We have seen cases which suggest that there is a good deal yet to learn on that subject; transfusion is mentioned as a remedy for hæmorrhage, as if it were an ordinary and well-established fact. How many times has it proved successful?

We have shown that Marshall Hall took a circuitous route to establish what he himself showed could only be rendered certain by his own reference to the human subject; and we showed that the practice of surgery in removing parts—not teeth merely, but whole limbs—on the supposition that the *local mischief* was the cause, had ended in failures so often, as to be, if not abandoned, as in cases of tetanus, tic doloreux, etc., hardly ever instituted. I have exemplified, in my remarks on Marshall Hall, the success of the larger and more correct views, as deduced from pathological phenomena. Sir Astley Cooper certainly tied the artery in question in a dog, which recovered the operation; but we hear that he taught surgeons that they could cure "aneurism of the larger arteries, even to "the extent of tying the largest artery in the body." This statement may be excused from a physician, but

is not likely to mislead a surgeon so far as to place a ligature on the aorta, unless under some extraordinary complications of accident, which the utmost stretch of the imagination can hardly suppose to happen or indicate. Sir William does not seem well acquainted with the progress of the treatment of aneurism by ligature, and we have no space to insert it here. Shortly, the progress of tying larger arteries than Hunter had done, was due to Ramsden, Abernethy, Stevens, Cline, Cooper, Travers, and others, who respectively placed ligatures on the external iliac, the common iliac, common carotid, etc. But the statement that Sir Astley taught surgeons, etc., can, by no stretch of courtesy, be admitted. Here, as in every other subject, pathological data are alone the reliable teachers. If ever the aorta is successfully tied in man, or justifiably undertaken, it can only be on some complicated condition of things difficult to imagine, and on the evidence which exists that the artery has been, in some very rare instances of disease, found on dissection of the dead, nearly, or quite, impervious; and yet the body nourished by collateral channels. Here again, as we constantly show, lies the superiority of that teaching which every surgeon feels the essential basis of operative proceedings, viz., the observation of the living phenomena, and the inspection of the dead. I shall have occasion, in another place, to advert to some other proceedings of Sir Astley Cooper, which may well help to warn us from the folly of Vivisection. Sir William has some other ideas, in which we cannot participate. He seems to object to any legal enactments tending to place restrictions

on Vivisection. His reasons appear to be two-fold. He doubts, as becomes a stout believer in Vivisection, as to the abuse of this mode of enquiry; and we do not see any other witness who would leave everything more entirely to the will of the Vivisector. He seems to object to any restrictions, on account of the sort of slur which they would constitute on the honour of the profession. There are many arguments, the unsoundness or absurdity of which is best shown by following them out to their necessary consequences —something after the plan adopted by Paley, and which, practically, is a *reductio ad absurdum*. I would, however, observe that restrictions are not necessarily directed exclusively to the profession. We have gentlemen who are styled pure physiologists and biologists, and who, as I believe, are quite as ardent promoters of Vivisection as the comparatively few of the profession who practice it, and who add to that obscurity, which is inseparable from Vivisection, the additional cloud resulting from their not being expressedly students of pathology, one, not only of the most fruitful sources of knowledge, but the most valuable test of all knowledge in human physiology, however or whencesoever it may have been derived. But now as to the slur which Sir William Gull fears may be cast on the profession. Were any of our reverend friends to write a discourse on this ground of apprehension, we think he could hardly select a better text than that from St. Paul, in his letter to the Romans. "For rulers are "not a terror to good works, but to the evil; but if "thou do that which is *evil*, be afraid," etc. The fact, I apprehend, to be this; that if mankind were to

be deterred from legislating on the proper conduct of any classes of men, society would soon be in a state of moral anarchy. You cannot be allowed to place any restrictions on a certain mode of enquiry—which a large portion of mankind deprecate as morally unjustifiable—of which another large portion deny the necessity—and some of whom allege to be mischievous—and of which some examples have been condemned even by those who advocate Vivisection, on account of their useless barbarity—because it may possibly imply a slur on the medical profession, of which profession we are told, that in this country, to which the law would only apply, not more than twenty or thirty individuals practice the enquiry in question. If this reasoning be admitted, we do not see how any sort of mala praxis, whether social or professional, can be made the subject of legislation, since the very existence of a law necessarily implies the possible infraction of it. Take, for instance, the Army. We could have no regulations of any kind, the inspection of which implied conduct otherwise than that becoming officers and gentlemen, without casting a slur on that noble profession; and yet I do not believe that any class of men would have more claim to this pseudo sensitiveness than would the army. If we pursued this kind of reasoning, no body of men could have any rules relating to conduct without a foregone conclusion, constructive of slur or insult; so that we should soon arrive at the anti-climax of rendering all regulations obsolete, with the characteristic accompaniment of putting the Mutiny Act in the fire. As well might the members of the bar object to certain well-known rules regarding their relations to solicitors,

because an infringement of them would be regarded as a violation of that which is essential to the dignity of the bar. Now is it possible that any one, ridden as we are by various kinds of conventionalism, can be influenced by such reasoning as that to which I have referred, although it comes from a gentleman who is well-known as a popular physician and, as I believe, a well-intentioned and good man?

I am far from agreeing with Sir William Gull in these fears. I yield to no man in my admiration of our glorious profession, or in the general respect and regard which I bear to the many estimable men to whom its duties have been confided; but for that very reason, I would welcome any regulation which placed restrictions either on questionable modes of scientific enquiry, or in relation to the observance of conduct which, whether on professional or other relations, should not as strictly represent that of gentlemen, as it should be unmarred by any admixture of any *thing* conveyed by the term *esprit du corps*, or professional etiquette, which could not be identified with the highest principle in morals. We need not remark on what Sir William Gull says of Vaccination; we have already pointed out the mistakes of the evidence on that subject, in which Sir William Gull seems to follow suit. Nothing seems to me to be more teaching in Sir William Gull's evidence, than the example it affords of how far a man may be misled by a dominant idea, however opposed it may be to the ordinary rules and cautions necessary to scientific research; but the further remarks of Sir William we will not venture to discuss, more than by copying one part of the evidence, as anyone may, we should think, estimate its worth without assistance.

Asked if he said certain things, he said no; but he would explain what he did say, etc. Then, in the next question, he says: "I should quite be prepared to "maintain that science hereafter may show that in "our bodies there may be superfluous parts, relicta of "ancestral relations, which linger in us, and which "(though I do not know them now) may hereafter be "found to be useless, and that it is a conceivable object "of science to find them out, and it might even be a "conceivable object of *practice* to remove them." Asked, "Is it borne out that modifications caused by "these surgical operations are transmitted to future "generations?

"A. I did not affirm that they were; but what I "hinted was this. Pointing to my friends, the sur-"geons, I said: 'Should advancing knowledge show "'that we have superfluous parts, or organs, and "'especially if these are liable to disease, what a land "'of promise for operations!' But that was merely "hypothetical—it was no real suggestion that the sur-"geon was now to be called in.

"Q. It was a joke, was it?

"A. No, indeed, it was not a joke; it was an anti-"cipation!!!"

History, on all subjects, repeats itself more or less, and this is not the first occasion on which conclusions have been hinted at, as to the uselessness of parts; one of the most ridiculous exemplifications of that line of reasoning, which has sometimes assumed the superfluity of parts, because their real use could not be made out, I have elsewhere stated, but it is a little too technical for quotation here. I suspect that Sir William

Gull has not quite rightly estimated the value of what Marshall Hall did, either as regards its excellencies or its defects; but I have already remarked on Dr. Marshall Hall's works, and Sir William Gull's evidence, as much as I have space for on the present occasion. I may, perhaps, have at some other time another opportunity.

PROFESSOR HUMPHRY.

This gentleman is a warm supporter of Vivisection; and as he gave his evidence before a number of Commissioners—some of whom (without any disrespect) we may assume were not sufficiently informed on the subject to question or analyze the professional relations of his statements, and others who, however they might have or have not been able, evinced no disposition to question them—I have, in the difficulty of making some selections from such a huge mass of matter of evidence, thought it necessary not to allow one or two points of Professor Humphry's evidence to pass wholly without remark. It would have been satisfactory if Professor Humphry could have given us some better grounds for adjusting the pain which animals feel when under experiment—both absolutely and relatively—to that which is suffered by a human subject; especially as we have no opportunity, as a distinguished Professor suggested, of "hearing what animals have to say on "the subject."

Amongst many other questions, Dr. Humphry is asked—

"Q. There is a great deal of pathological informa-"tion obtained by seeing patients in hospitals?

"A. Certainly. But we cannot observe patho-
" logical processes in such patients; for instance, the
" most common of all morbid processes, viz., in-
" flammation—we only learn *what it is*, and how it
" is to be induced, and what agents will affect it, by
" observation on the *living* animal."

It seems to me that the foregoing answer is a series of the most unintelligible matter that any physiologist, pathologist, or surgeon, can have to consider. I should say—and I think I have long since proved—that in regard to all the *essential* characters of inflammation, there is no source comparable to that furnished by observation of the living human body, whence the true nature of what is called inflammation can be deduced. Everything regarding it, that I know of, as deduced from sight merely, has only tended to obscure the real nature of the process. Inflammation is itself a very good illustration of the fact. The ordinary definition of it as heat, redness, pain, etc., in the part, I showed, in a scientific sense, many years ago, is so incorrect, that not one of these characters is essential to it. Professor Humphry seems to attach great importance to certain mechanical relations between the contents and the vessels of the part, though he does not state the experiment. This, however, is of little consequence, because it is impossible to deduce the *laws* governing inflammation from any experiments on living animals, to be compared for one moment with the beautiful and demonstrative phenomena which may be observed in man. I stated formerly, when I was referring to the microscope and other phenomena observed by Kalten-

brunner, as to what was observable in the vessels of the inflamed part—"What we want to know is, why "the phenomena *occur at all; why the whole* process "is set up; not how this or that feature of it may be "produced."* The law of the integral process is the *desideratum*, I said, and that is not to be found in the part. I then proceeded to show, as I contend, several very important results from my observations. At one time, bleeding was almost universal in inflammation; then cases not appropriate for bleeding were referred to different morbid conditions; but I showed that, even in the most undoubted cases of inflammation, the propriety of bleeding, or not, depended on certain other conditions; and cited examples where inflammation had been, under the alleged conditions, successfully treated without any depletion; and to meet doubts (at that time very ready to be started) as to whether the cases might not have been mistaken, cited cases of active inflammations of the eye, accompanied by matter in the anterior chamber, which could not be open to mistake.

This, however, was not all; but I cannot pursue this matter further here. That kind of investigation which attaches so much importance to mechanical conditions of the minute vessels, may be interesting and curious, but it does nothing towards helping us to get at the *law* governing inflammation and other diseased processes. It may furnish curious objects for amusement by the microscope, but it affords in my view very little help or interest in lighting us to any generalization. There is a great deal in Dr. Humphry's

* Medicine and Surgery One Inductive Science.

remarks, which I should think would astonish some experienced pathologists; but as it chiefly rests on assertions, which are not supported by facts or reasoning, I cannot afford more space for them. One statement is somewhat discouraging to those who may participate in the views of Professor Humphry. He says: "Pathological experiments, I think, become more "and more necessary as civilization advances. Civili- "zation is the great engenderer of disease; and unless "the healing art is made to advance in proportion, "there will be, as the result of civilization, a distinct "degeneration of man—physical and moral. I think "there is no doubt of that; and, therefore, it is "necessary to take every means possible, most earnestly "and anxiously, to understand the nature of disease, "and to prevent it." I hardly dare venture on defining the real meaning of this passage, nor can I reconcile in any way, consistently with common sense, the apparent meaning of it. Of course, I do not know what may be Dr. Humphry's definition of civilization; it certainly does not convey to my mind the character that he appears so necessarily to combine with it.

It seems difficult to understand how those phenomena which, as a whole, constitute what are called diseases, and which, either directly or indirectly, result for the greater part from the follies, the vices, or the ignorance of mankind, must necessarily be multiplied, *parî passû*, with the progress of civilization. I can by no extension of the meaning of "civilization," arrive at anything like the view here stated. Were civilization to mean nothing but a life of luxury and sensual indulgence, or even mixed with an exaggerated form

of such sources of mischief, I should no longer think it civilization; but, on the contrary, that state of barbarism which Dr. Humphry amusingly prophecies as the result of the prohibition or abandonment of Vivisection. I cannot quite appreciate the point of view whence Dr. Humphry seems to regard those anticipative duties of our profession. It would seem (if his views of civilization were correct) that our duties were little better than to study how we could best minimize the evils resulting from that anticipated infraction of the physiological laws (which infractions are, truly enough, amongst the real engenderers of disease), by earnest, it might be, but most likely impotent, endeavours to impede the means by which the laws, sooner or later, vindicate themselves. I cannot say that there may not be men who take something like this view of our profession. This surely cannot be the real meaning of the passage in Dr. Humphry's evidence, which, however, it seems constructively to imply; when, instead of the study of causes by the legitimate paths of inductive research, we would endeavour to relieve the sufferings of intelligent beings, by minimizing these consequences, and by a mode of investigation, which would attempt the relief of sufferings evoked by ignorance or the folly of intelligent beings, by inflicting other and, probably, infinitely greater sufferings on unoffending animals.

Strong terms have been used to designate such a state of things, and figurative epithets borrowed from Pandemonium; but were such a state of things to occur, researches which inflict sufferings on animals—which blur the finest phases of humanity—throw a

cloud over the only secure path of physiological enquiry—might, indeed, conceivably represent a state of things in which the barbarities of Vivisection might constitute a mode of research, of all others, the only *characteristic* proceeding.

Another statement of Dr. Humphry's seems to imply that he has not been very observant of some things which are almost popularly familiar. He appears to think that animals have nothing like anticipation or retrospect in relation to pain. It is not my object to insert anything sensational which I can readily avoid, and, therefore, I will not adduce the many indications of both retrospect and anticipation, which are seen almost daily by persons who are observant of animals; and I will even omit a very remarkable example, which came to my knowledge through a medical man, in relation to some animals on whom experiments were in daily progress, near one of our hospitals. I also regret that it is not within my present plan to do more than remind Dr. Humphry (in relation to another assertion of his) of some very interesting phenomena. In stating his general view of Vivisection, Dr. Humphry observes: "That forasmuch "as a large part of the animal kingdom lives and "maintains its perfection by the death of other animals, "which is necessarily attended by *more or less* pain, it "is quite a justifiable thing for man to inflict death "and a *certain* amount of pain on other animals, when "there is a reasonable prospect of his condition being "benefited by it." This is a kind of jumble, if I may be allowed the expression, which would be, perhaps, most readily exposed by being put in the form of a syllogism.

The manner in which this statement is qualified, suggests some difficulties in arriving at what may be called a positive and clear definition of it. A *certain* amount of pain, when a man can see a *reasonable* hope of being benefited by it, gives something indefinite, but still not unopen to objection; but the statement that the necessary death of animals, and the pain they suffer, as a sort of warranty for the proceedings of the Vivisector, is not only as an argument unsound, but, to use no other term, is either unknowing or reticent, with regard to one of the most interesting series of phenomena of natural history; that is to say, of the numerous and varied forms of phenomena which, whatever other relations they may have, are most interesting. I refer to the circumstances observable alike in predaceous animals and those on whom they prey—facts which, as I have said, whatever other relations they may have, appear to have two, of which the proximate effect and general prevalence are of great interest, as evidently contributory to the painless and rapid death of the victim. These numerous phenomena it is not necessary that I should here detail. They may be easily deduced from observations of the habits of any animals, whether beasts, birds, fishes, or insects, that are predaceous. I have only to add that pathology must be cultivated by better and higher modes of study than by Vivisection; by a more enlarged and circumspective view of the various forces which act on living beings; by a far more methodical and complete annotation of the phenomena of the living, *and more accurate* and constant testing any conclusion by the examination of the dead; by a more

sedulous study and more diffused knowledge of hygienic laws; by observing the effects which the neglect of them never fails, sooner or later, to produce on mind and body; and the connection of these and their relations, not only on the whole body, but with the varying state of organs entering into its organisation; and that there is no shorter way to the acquisition of real pathological knowledge.

SIR JAMES PAGET.

It appears to me that the evidence of Sir James Paget has been very thoughtfully given, and invites as thoughtful consideration. I have endeavoured so to view it, and the impression I derive from it is that he is not a very ardent advocate for Vivisection. Still there are several points in which I cannot think that the facts of the cases justify the conclusions which he would draw from them. I scarcely need say much about the necessity of which Sir James speaks, of a student seeing a heart acting, but I have my doubts whether it would assist him in really understanding it; and, besides, it is not what is understood by Vivisection, to experiment on an animal previously decapitated. It appears to me, too, that there is something of a foregone conclusion, not justified by the opportunities available in medical research, in the following: Sir James, in reply to a question whether experiments on living animals is an optional question, says, "No." He is then asked:

"Q. What is your view of that subject?

"A. I think it may be said, generally, that medical

" science, being in a state of progress, is continually " coming in sight of things which can only be decided " by experiments, either upon man or upon some lower " animal."

Surely, when the word " experiment " is thus used —bearing in mind the object of the Royal Commission —there is something of a foregone conclusion in that word, as distinguished from pathological observations and enquiries.

" Q. 73. Supposing that a patient is brought to " you, having received some injury, for instance, which " requires your care, you either do know what to do " with him, or you do not. If you do, you proceed on " the knowledge already obtained. If you do not, " there is a necessity for an *experiment* in his case, is " there not?

" Yes."

If we are here to regard the word " experiment " in the sense entertained by the Commission, Sir James could hardly have so intended it; if he did, surely it was a foregone conclusion. In the margin of my copy of the evidence, I have put the words, " Let the case " be stated."

With regard to transfusion, I cannot accept the positive form in which the state of that subject is represented by Sir James; and, still less, that by one of the witnesses on whom I have already adverted. I have great difficulty in adopting the view suggested by James Paget, with regard to snake bites being auspicious subjects for experiments on animals; and when he states—what, I fear, is too true—that some thousands of lives are lost every year by venomous snake

bites, so far from this being a warranty for experiments on brutes, it occurs to me, as distinguished from the right mode of proceeding, very like the woodman cutting off the branch on which he was standing.

Now, it is impossible to exaggerate the gravity of this question. It is an astounding example of our ignorance on one of the most important subjects on which medical men could be engaged. No doubt it is beset with difficulties of various kinds; but those very difficulties should not only minimize, but at once dwarf, any consideration of the difficulty or complexities, financial or other, necessary to surmounting them. I, of course, would be understood as speaking cautiously with regard to measures of which the institution may involve other difficulties than those which occur to me. But I cannot help believing that, with such a field for experiments, in all respects unexampled, measures might be adopted, to which it would be unreasonable to attribute or compare any experiments on animals. I conceive that almost any number of supposed antidotes might be divided into any varieties of doses or combinations, and so distributed, that at least a very large number of persons might be provided with them, so that they could be taken without a moment's delay. In modifying and multiplying doses, the very fact that a man is to die in a time varying, perhaps, from an hour, or less, to a few hours, would soon give a latitude in practice, greater than in any other known enquiry. Besides which, it is conceivable that the Indian Government could devise means by which at least a great many, though perhaps not a majority, of the results might be

known and recorded. Then again, it is evident that some direction and perhaps varied directions, might be given by persons who may have had actual observation of the results already known, or from others who may have given attention to the scientific bearings of the subject. For example, we have already some phenomena which suggest that all injurious matters, if they are got rid of at all, are expelled or otherwise rendered innocuous coincidently with powerful excitement of some secretion; and even where this is not effected, we have many phenomena which suggest the endeavour. It is therefore, I conceive, one direction which might be given to such experiments on the snake-bitten patient, to furnish anyone so exposed with the antidote alone, and also combined with substances of great power as exciting secretion, making the vehicle alcohol, or any other matter having highly stimulating properties. I suggest these views with all possible diffidence, and merely as exemplifying one or two of the multifarious methods in which these experiments could be instituted on the instant, by persons furnished with the respective means.

With such opportunities on the human subject, I cannot conceive that experiments on animals can be otherwise than open to the objection of substituting for so abundant and practical field of enquiry that which must be admitted, however otherwise viewed, as an imperfect or questionable analogy. In concluding these few remarks on Sir James Paget's evidence, I cannot but think that the time will come, as in the case of the celebrated Müller, when he will lose his faith in Vivisection; for I infer, from the general tone of his

evidence, that were that more rigid annotation and record of all pathological phenomena, for which, amongst others, I have contended shall be observed; that what I regard as at present a very measured reliance, in relation to what is really intended by Vivisection, will not find a very earnest advocate in Sir James Paget.

MY OWN EVIDENCE.

I have only a few remarks to make on the evidence I had an opportunity of giving. I had hoped that the examination of the question would have taken a much more positive form. That when I avowed myself as one who thought Vivisection a fallacy, that some experiments would have been adduced by the examinators on which Vivisection was supposed to be justified, and answers required to oppose or sustain them; but nothing of this kind took place. I was simply called on to say what I thought, as other witnesses; but when I was proceeding to support the views I entertain, that Vivisection had not only been a fallacy, but productive of great practical evils, I had my evidence met by being courteously reminded that they were not a medical Committee. This was unfortunate, because I thought that it was emphatically a professional enquiry; and my idea of the somewhat imperfect constitution of the Commission, was that there were not a sufficient number of professional men, or at least pathologists, on it. Before the Commission commenced their proceedings, I was asked whether I thought it fair that Professor Huxley should be a member of the Commis-

sion, on the ground (as alleged) that he was known to be an advocate of Vivisection. I said certainly, for the reason that some one holding such alleged opinions would be there to put forth their strong reasons, and have them answered; but then I took it for granted that there should be other members who were capable of appreciating other views, or examining pathological or other relations to the questions which might be the subject of consideration. When I shall contend, at the close of my remarks, that the Commission was not happily constituted—I must not be understood as in any way presuming to express any dissatisfaction with the noble chairman, or the courtesy with which he conducted the enquiries; for there must have been, I presume, a very considerable reliance on his part, with regard to physiological matters, on the professional portion of the Commission.

Those who are opposed to Vivisection do not restrict their objections to alleging that it is useless, or as occupying the place of more philosophical modes of research, or the suffering it necessarily involves, however much that may be modified in some cases by anæsthetics; they also allege that experiments on living animals have been misleading, and so productive of great practical mischief. Of this last allegation there is not the smallest doubt, and this I must endeavour to show; not but that it has been done already, but those who advocate Vivisection seem to disregard anything that militates against the mode of study they advocate. The illustrations I select are from the works of Sir Astley Cooper and Mr. Travers, both men most popular in their day, and both surgeons of hospitals,

and both, with perhaps a single exception, men with the largest private practice in London. And I beg the reader's attention to the narrative, because, exclusive of the material facts of the case, the subjects, as a whole, are excellent illustrations of the faults which almost invariably attend Vivisectional enquiries, and which represent, as it appears to me, the foundation of Vivisectional errors. Physiological phenomena furnish abundant phenomena illustrating that to which I refer; where the mind, being strong in some foregone conclusion, is not only heedless even of very obvious facts, but entirely neglectful of that circumspection of surrounding relations, which is so essential to all philosophical enquiries. Sir Astley Cooper was a dexterous and able surgeon. He had a rapid progress to popularity and a large practice, which, when too early obtained, is somewhat unfavourable to culture of the philosophic mind. There is little time for that sustained exercise of thought which seems essential to effective study. Now, Sir Astley thought that when the neck of the thigh bone was fractured within the bag or capsule, which encloses the joint of the hip, repair by bony union was impracticable. He saw that the neck of the thigh bone at that part was (so to speak) somewhat isolated, when compared with other parts, or to their immediate and surrounding connections, and on this ground decided that fracture of it could not be repaired by bony union; in other words, that the union of such an injury was necessarily ligamentous. Now, the facts which would show the probable unsoundness of this reasoning were perfectly well known to Sir Astley, but he appears to have had

his attention confined to the apparently naked condition of the neck of the bone. That this accident was frequently repaired by ligamentous union was true, but it was not the only part where accidents were repaired by ligamentous union; nay, more, it was known that sometimes surgeons, after a while, purposely allowed some degree of motion in fractured bones, where they feared that the secretion of bone might be in inconvenient excess, and where ligamentous union took place. Besides, he knew that this fracture took place most commonly in persons advanced in life, when *unusual* care is necessary as regards the utmost quiet of the limb, so that no disturbance should occur in parts which it was essential to keep in apposition, and that various circumstances rendered this, in many cases, a matter of no small difficulty. Now, all this might be said to apply, more or less, to fractures in general, but it seems to have been lost sight of, or unappreciated, by Sir Astley. He had got the one idea of deficient reparative power, and seems to have referred failure to nothing else. Well, to prove this, as he thought, he made some experiments on animals; and here is another feature common in Vivisection. A supposition is started, contrary or irreconcileable with many known facts, or to some obvious analogy, and then an animal is experimented on to see if it be true. So that, in a vast number of cases, a man commences his experiment, as Sir Astley did, with the disadvantage of a foregone conclusion. He accordingly experimented on dogs; and finding that the fractures he made in the thighs of the dogs united only by ligament, he regarded that as a confirmation of his doctrine. Now, I

will venture to say that not one of the circumstances necessary to the proper repair of the fractured neck of the thigh bone in the human subject, could be accomplished in the dog, and especially that chief of all, the continually undisturbed condition of the injured parts. Now many surgeons, both here and on the Continent, took another view of the subject, and maintained that if the parts were kept perfectly still, and so maintained for the requisite time, that the fractured neck of the thigh bone would do just as well as others. Amongst these were Mr. Abernethy and Baron Larrey. But those cases which were successful, were for a time met by the allegation that the fractures were outside, or partially outside, of the capsule or bag of the joint; and as this could not be proved or disproved but by dissection, much time—even years—passed away, during which the subject was a matter of opinion. At length cases were so remarked and recorded, that opportunities occurred after death, and permission was obtained to examine the joint, when the practicability of bony union of the fracture in question was fully established. There were two or three examples occurred about the same time; but I think the first undoubted specimen occurred to a very good and industrious pathologist (Mr. Langstaff), and is possibly now in the College of Surgeons, as many of the most valuable of Mr. Langstaff's preparations were, I think, bought by the College at his decease. Now, if the reader is not fatigued with this narrative, I beg his particular attention to what follows. Sir Astley Cooper was surgeon to *one* of the largest hospitals, and a leading teacher of Surgery.

Concluding that bony union could not be obtained in the cases referred to, he recommended and adopted a practice which rendered it impossible. Thus, when the patient had been in bed for a fortnight or so, and the inflammation consequent on the injury had subsided, he recommended the patient to rise and use a crutch, which, as rendering bony union out of the question, necessarily involved lameness for life. The lamentable result of this practice of Sir Astley's, though not warranted by a careful view of all the practical facts, but which he concluded his experiments on dogs seemed to confirm, can only be estimated by considering what might have been the number of cases submitted to his care, besides those to such of his pupils of one of the largest classes in London, who would probably—for a time, at least—adopt the practice of their distinguished teacher.

MR. TRAVERS.

It is, of course, beyond my power, even were I ambitious so to do, to generalize the error which lies at the foundation of attempts to discover the laws governing organised beings, by dissecting them alive. Yet there is something very instructive in tracing, or endeavouring to trace, the sources of the various errors which seem inseparable from Vivisection, as the source of safe conclusions. Now Mr. Travers was, as I conceive, a different kind of man from Sir Astley Cooper. No doubt they both had industry and zeal in the prosecution of professional enquiries; but Mr. Travers appears to have had more of that contemplative

tendency which characterises the scientific mind, than his friend and colleague; but we shall see how entirely he missed the end, which, as we infer, was the *principal* object of his experiments on living animals. His experiments were made on living dogs, and consisted of a variety of injuries to the intestines of those animals, with a view to ascertain their powers of repair under different kinds of injury. It is probable that his object related to injuries of these parts, under whatever varieties of circumstances they might have been inflicted; but there is little doubt that his enquiries were directed in an especial degree to the treatment of strangulated hernia. To say nothing of the paramount importance of that subject, the great fatality by which it was too often characterized, the title of his book suggests the prominence that strangulated hernia held in his enquiries. This was the subject which I mentioned in my evidence before the Commission, when I was courteously reminded that it was not a medical Committee. Therefore, as I am now writing to the medical and general public in common, I must be excused from entering into the subject rather more minutely, perhaps, than my professional readers may think necessary. Now, what is usually intended by the word hernia (sometimes erroneously called rupture), is the escape of some portion of the contents of the abdomen through certain natural apertures. In the sense we are now intending, there is no necessity for any breaking through of parts, though that may happen; but it is not usually the case. The escape that we commonly mean takes place through certain natural apertures, and, in many cases, is easily

returned, or at once resumes its proper place when the patient lies down. Not unfrequently, however, the circumstances assume another—and a very serious—character. From a more than ordinary portion escaping, or some other cause, the aperture causes a constriction on the contents which have been thus protruded, and if these are any portion of the bowels, of course the passage of the canal becomes obstructed. Very soon a series of unpleasant and alarming symptoms supervene; these I need not particularly describe. It is sufficient to observe that they combine usually more or less sickness, pain, or tenderness of the part, and obstruction of the bowels. The object of the surgeon is, of course, to free the protruded parts from the constriction formed by the aperture through which they have been protruded. This he attempts either by such measures as he thinks may relax the parts, or by enlarging the aperture by the knife; and this constitutes what is called the operation for strangulated hernia. This operation, which is somewhat delicate in exceptional cases, is, on the whole, very simple, consisting in a *very careful* exposure of the parts protruded, and a slight enlargement of the aperture through which they have been so. Perhaps there is no subject on which there has been more mischievous writing than on this. Various measures have been at different periods recommended, scarcely any one of them entirely free from objection, especially as regards the delay to which they (more or less) have given rise. For it should be remembered that when any portion of the contents of the abdomen are thus strangulated, you can never be certain that even a single hour may

not give rise to changes in the parts, which increase the danger, and may possibly prevent recovery. As we are called on to operate in these cases at all periods, by persons uninstructed or unaware of the nature of the case, I have known the most undesirable changes occur in a very few hours, on the one hand, and I have, on the other, once or twice found the parts, after the most serious delay, still in a recoverable condition. We now do not waste time by the administration of so-called remedies; if we try some one or two, it may be, on which we have confidence, and yet cannot replace the part, we proceed to enlarge the opening through which it has escaped. This removes the essential cause of the symptoms. Before, however, we have had an opportunity of doing this, inflammation may have taken place in the membrane covering the intestines, and which also lines the cavity of the abdomen. This we call the peritoneum, and, if inflamed, we give it the name of peritonitis; this is indeed a dangerous malady, and that which, in fatal cases, is usually the cause of the fatality. The manner in which this serious affair was usually combated, was by bleeding, *purging*, calomel, and opium, etc., according to the views of the surgeon, and formed the principal source of his anxiety. Now we shall see how far from the right path Mr. Travers went, if not in consequence of, certainly notwithstanding, his experiments on animals.

In early life I was brought by circumstances in contact with many cases of hernia, and also called on to treat many cases of strangulated hernia, under my sole responsibility. I was for some time surgeon to the City of London Truss Society. I was also surgeon to

a large dispensary, in which the medical department was, as usual, conducted by two physicians, but only one surgeon. At this period I also gave clinical lectures on *surgery*, which, as far as I know, were the first *regular* clinical lectures on *surgery* in London. As for many years after this I continued to associate myself with the teachers and practice at Bartholomew's Hospital, I had full opportunities of appreciating the excellence of the lessons I received from Abernethy, and especially that which he seldom failed to recommend—to think for myself. Under these advantages I naturally enough tried to improve, so far as I could, anything which I had derived from these opportunities. Now, strangulated hernia was a particularly serious affair; too many of the cases in which, as it appeared to me and another surgeon—one afterwards surgeon to the hospital—the majority terminated unfavourably. Many of these were due to the late period at which the patients had applied; the protruded parts had already undergone changes, which involved dangers altogether apart from any necessary to the operation. Mr. Stanley, with whom I worked a good deal at that time (one of the most industrious surgeons about the hospital, and one of the most careful operators I had ever known) almost disliked being called to an operation for strangulated hernia; he had derived such discouraging impressions as to the results. Now at this period, the great anxiety of the surgeon, after he had relieved the bowels or other parts from constriction, was to procure the proof of it by the occurrence of a natural relief from the bowels. Accordingly, very soon after the operation—and sometimes almost immediately

—it was the practice to administer aperients for that purpose, and it was a long time before my attention was drawn to this as a most dangerous and, in fact, too fatal practice. Like all other things, the question was not free from difficulties, because if a patient applied immediately on the occurrence of the strangulation, and the surgeon, on failing to reduce the parts, did not delay the operation, then the bowels or other parts would probably be found in so nearly a natural state, that the relief from the bowels might occur spontaneously, or upon the administration of medicine, and yet no harm happen. So that until such cases were explained, it seemed difficult to understand the extreme fatality consequent on the use of aperients after the operation. Another difficulty in arriving at a clear view of the subject resulted from cases in which, when the operation had been performed, it was seen that the inflammation of the peritoneum had already become established; and this was regarded as a sufficient explanation of the fatal issue.

We cannot always tell the exact moment when we suggest an improvement in practice; but I recollect one case in which, having administered a mild aperient, after Mr. Abernethy's manner, that is in small doses, every three or four hours, until the effect desired, the patient, who was perfectly easy before, began after the first dose, as far as I remember, to be uneasy, when I instantly discontinued the medicine. Now, having cleared the cases of those which, from the healthy condition of the bowel or other parts, were calculated to obscure the subject, by furnishing instances where purgatives might have been administered

with impunity, it occurred to me as unreasonable to expect that an intestine just liberated from a ligature, which had been impeding the circulation in it, it might be, for hours—and sometimes, more or less, for even days—would be expected to resume its functions on the instant that the ligature was removed. And then I began to attach the phenomena to what I have enunciated as a general law, viz., that whenever you find mucous and serous membranes associated in the same organ, and the irritation of the mucous surface is accompanied by some obstacle which opposes the proper relief of the mucous (that is, by secretion), it is more than probable that you will have the irritation or its effects *transferred* to the serous membrane. I believe this to be an explanation of the fatality attending the use of purgative medicines in such cases, because it is general, whenever such structures are associated. These are things which are deduced from pathology in the human subject. I may mention a case or two before I conclude; but now to recur to Mr. Travers.

Mr. Travers, in the same book, says that the great danger of the operation for strangulated hernia is from the inflammation of the peritoneum (the membrane which lines the abdomen, and affords the outside covering to the bowels). That is true; but now hear the remedy he proposes. He says: "The great means to "combat this is by purgatives. If there is no peritonitis, "we give purgatives," he says, "to prevent it; and if there "is peritonitis, to cure it." Now no greater mistake, nor any more fatal, I believe, was ever made, and I do not see how it is possible to dissociate it from his experiments on the bowels of animals, and those reparative

processes which he exemplifies. There were other errors in the treatment of these cases; and one of these was an indiscriminate recourse to bleeding, by which a circumscribed inflammatory process may easily (without care and discrimination) be converted into the diffused inflammation of peritonitis. If Mr. Travers had wished to have ascertained the practical deductions least open to mistake, he should have imitated *all* the circumstances, so as to have rendered the parallelism of the two cases, as far as was possible, complete; but this, even though he had bled or purged his dogs, would still have left many particulars, rendering it impossible to complete the analogy. But not to anticipate a few words I have to add on the general subject, I will conclude this by adverting to two cases, exemplifying in two states, the one the most simple, the other most dangerous in its complexities, yet both showing the advantage of avoiding the dangerous practice which Mr. Travers seemed to have deduced, or supported, on such unsafe premises as his experiments on animals. I operated, one evening, on a large strangulated hernia in a woman, between fifty and sixty years of age; there was nothing extraordinary in the case; the intestine was deeply congested, but nothing suggesting an untoward issue. The next morning I called, and seeing her dressed and sitting by the fire, I did not recognise her, but enquired for the patient, when she said, "I am the patient, Sir." I immediately ordered her to go to bed until the parts were healed, etc. "The fact was," she said, "she really "felt quite well, and therefore she had got up." The other case was one of the most serious kind, and

occurred in the practice of Mr. Shillito, at Putney. This gentleman requested my advice in a case of strangulated hernia, the particulars of which I have not at hand at this moment, but the facts are very striking. The patient was subject to epileptic fits, and was labouring under one at the time that I was consulted. On the subsidence of this I performed the operation, and found that there had already occurred active inflammation of the peritoneum, puriform fluid and flakes of lymph issuing through the wound. On the completion of the operation the woman was put to bed, and ordered to be kept as entirely free from every disturbance, by medicine or food, as possible. Nothing could go on better. The next morning—serious as the case had been in all its aspects—I said to Mr. Shillito, on visiting our patient—"Now, Mr. Shillito, I have "often told you that if surgeons would only treat these "cases as I recommend they should be treated, they "would often find their patient so free from any symp- "toms the next morning, as to render it difficult, except "from their knowledge of the cases, to say that they "had anything the matter with them. Now, Sir," I added, "tell me if, apart from the wound, you can "find anything the matter with our patient." Mr. Shillito assented. Now, I claim to be the first person who *publicly* enunciated this great improvement in practice, viz., the abstention or relinquishment of purgatives in strangulated hernia; but as it certainly, I believe, saves a large number of lives, I must give everyone, so far as I know, any share he may have had in the improvement. When I first wrote on the subject, I was anxious to support a proposal so much against

the stream, so to speak, by some authority. I imagined —and I laboured to show—that the dependence on purgatives had not been by any means always warm or universal, but still there had been nothing like an abandonment of them anywhere. My attention, however, was drawn to some reports of the London Truss Society, where something like fifty-one or fifty-two operations had been reported as occurring since the formation of the society, with only one or two failures. My predecessor in that society was the senior Taunton, a man of some peculiarities of character, and who, on some subjects, appears to have been in advance of his day. I enquired from whatever authority I could find, and the impression conveyed to me was that he never gave purgatives in strangulated hernia, and that he combated peritonitis by severe blistering, etc.

This gave a new and very interesting reading to the word "operations," because, as this society relieves all sorts of prolapsus, besides hernia, their operations might possibly have included hydroceles and other things, in persons who apply under the impression that they have hernia, and which will readily occur to the professional reader; but when I heard that he never gave purgatives—although my traditional information was not such as I could, in the strictest sense, realize—yet my own success in the same cases rendered the account, as applicable to hernia, very probable. Be all this as it may, I believe that no more disastrous error ever proceeded from Vivisection than the one in question; and whether Mr. Travers' treatment proceeded from what he did in his operations, or from what he neglected to do, it still illustrates

the misleading character of Vivisection, to which there always seems more or less tendency, inseparable from that mode of investigation.

We have seen that the very measure which Mr. Travers was led to recommend to combat the peritonitis, was exactly that it was most dangerous to employ; and in order to help those who may think for themselves to be patient as to the reception of any new proposal, I will state further, that when I was working a good deal with Mr. Stanley, we visited a case of hernia, in the hospital, and on his proposing to give an aperient powder, I objected. He said, "You "did not use to be afraid of an aperient." "Ah!" I said, "but I am now," etc. Well, the powder was given. I need not pursue that case further; but some twenty years after this, Mr. Stanley, in urging the abstention from purgatives in strangulated hernia, said he had known "bushels of cases" destroyed by the peritonitis consequent on the drastic purge.

SIR WILLIAM FERGUSON.

It is not my object in these comments to bring forward the testimony of those who were, directly or indirectly, more or less opposed to the practice of Vivisection. That would not only exceed the limits I propose, but would make a volume so large, and a list so numerous, that, with the facts on which the claim to authority would rest, it would be only practicable, in order to do justice to the subject, in a course of lectures. I am unwilling, however, to pass over the evidence of Sir William Ferguson, because, although

I may not agree with him in everything, yet I fancy that there is really very little difference in our views, and especially in regard to what may or may not be, in the present state of affairs, practicable by the Government. It is no part of my plan to prescribe or suggest any form of legal enactment; but I am very well convinced that there is, at present, a good deal of difficulty, and that nothing will be more likely to mar or indefinitely postpone the wishes of those who impugn the claims of Vivisection, on scientific grounds, than the enactment of any law, or any restriction, which the Government *cannot enforce*. This difficulty, I am persuaded, will, by and by, be overcome; but there are certain steps towards it, in which, it seems to me, that Mr. Forster and Sir William Ferguson coincide, and which, I am disposed to think, constitute an auspicious mode of proceeding, and which, although they may not take the place of certain proposed restrictions, are calculated to aid in enforcing them.

1st. With regard to certain incorrect statements, on which I have already remarked, Sir William is asked in reference to certain things which have been referred to experiments on animals, the useless repetitions of them, etc.—

"Q. Can you give me any instances, in surgical "history, which would illustrate these positions?

"A. Such instances as I can think of, seem to me to "have been after the fact more than prior to the fact. "Some of the most striking experiments that have "been performed on the lower animals, with reference "to surgery, have really been already performed, not

" experimentally, but on the best judgment, on the " human subject, and proved on the human subject. " In recent times there has been more written and " said to catch the public mind than there used to be " on the subject; and I have observed that frequently " certain operations in surgery have been referred to " as having been developed in consequence of experi- " ments on the lower animals. Now, John Hunter, " one of our greatest physiologists, and allowed " to be one of our greatest surgeons also, who may be " said to this day to stand at the head of what may be " called scientific surgery in this country, is especially " celebrated for an operation which he devised on the " arteries. That operation stood for sixty or eighty years " as the most brilliant in surgery, and—in so far as I " have been able to make out (and I have enquired into " the subject)—Hunter's first experiment, if it might " be so called, was done on the human subject; and it " was long after he had repeated his operation on the " human subject, and others had repeated it, that the " fashion of tying arteries on the lower animals origi- " nated, or was developed. That fashion was quite " justifiable at the time—it is no longer now justifi- " able; but in regard to the surgical aspect of the " case, the experiment might have been left entirely " untouched, for Hunter had already experimented " and developed the fact on the human subject."

This is quite correct, as far as the *surgical* aspect of the case. The experiments of Dr. Jones, as I have already shown, were only expedient or necessary, from the *gross neglect* of opportunities on the human subject, as he himself has stated. They were therefore, in point of fact, *not* justifiable at all.

Of Professor Syme, who had made several experiments, Sir William is asked as to his (Professor Syme's) ultimate opinions.

"Q. But his ultimate authority was strongly on "the other side (that is to say, against Vivisection)?

"A. Strongly on the other side, as expressed in a "special report of his own, in association with some "gentlemen interested in veterinary surgery and physiology.

"Q. Have you a copy of that?

"A. Yes."

About the year 1867, Mr. Syme and other gentlemen had been asked to give their opinion in regard to the subject of Vivisection, and in the fortieth volume of the Veterinarian, 1867, there is the following report: "We, the Court of Examiners for Scotland of the "Royal College of Veterinary Surgeons, desire to "express our opinion that the performance of operations on living animals is altogether useless and "unnecessary for the purpose of causation.—James "Syme, Chairman; James Dunsmure, M.D., President "of the College of Surgeons in Edinburgh; I. Warburton Begbie, M.D.; John Lawson, President of "the Royal College of Veterinary Surgeons; B. "Cartledge, Member of the Council; William Cockburn, "Wm. Robertson, Charles Secker, James Cowie, "all M.R.C.V.S. I fully concur in the above.—John "Wilkinson, Principal Veterinary Surgeon to the "Forces."

Sir William Ferguson proceeds to say: "No man "has ever had more experience on the human subject "than Mr. Syme, and I believe, from knowledge that

" every man is acquainted with, that he investigated " by experiments on living lower animals, partly with " a view of developing features in reference to the " human subject, but more, in fact, with regard to " physiology than with regard to practical surgery; " and I, myself, have a strong opinion that such an " expression coming from Mr. Syme (and he must " have passed the middle period of life at that time) " *was* a mature and valuable opinion.

" Q. 1037. We have been told that, speaking " generally, experiments of this kind are performed " with the greatest possible consideration for the " animal, and with the greatest indisposition to inflict, " at least, protracted suffering. Do you believe that " to be the case ?

" A. Gentlemen may fancy that, but I do not think " that they fulfil that idea. Indeed, I have reason to " imagine that such sufferings, incidental to such " operations, are protracted in a very shocking " manner. I will give you an illustration of an " animal being crucified for several days, perhaps in- " troduced several times into a lecture room, for the " class to see how the experiment was going on.

" Q. In reference to the Society for the Suppres- " sion of Cruelty to Animals, are you aware that the " present Cruelty to Animals Act is not supposed to " apply to these experiments, because of the definition " of animal ?

" A. Yes, I am aware of that feature.

" Q. Do you see any objection to enlarging that " definition, so as to include wild animals ?

" A. It would be a great advantage, I think, so " to do.

"Q. So as to throw the *onus probandi*, to show "that he had a proper object, in the experimented?

"A. Quite so; and to make him aware that the "life of the wild animal is as precious to itself as the "life of a tame animal.

"Q. The fact that that might in some degree hit "the sportsman would not be an argument against it, "you think?

"A. No. That is the poor device of some people "to stop all these enquiries. I do not think it an "argument at all."

(This is one of the subterfuges, rather than arguments, to which the Vivisectionist is driven, and by which the public are misled. I hope to have space in my "Conclusion" for a few words on the subject, when I may show that—as Sir William says—"It is no argument at all.")

With regard to anæsthesia, as justifying experiments, and how necessarily inconclusive they are, Sir William observes: "I do not go in with that view, "which is very prevalent, that these experiments may "now be permitted, because we have got anæsthesia "to prevent the pain. The experiment is not of the "smallest value during its performance. You cannot "make a perfect experiment on the animal until it is "in its normal condition.

"Q. What you mean is, that if a man tries his "experiment, of course he hopes it will be a successful "one; whereas, you think that the anæsthetic may so "derange the animal as to prevent its being successful?

"A. It would be difficult for them to see what

"they want to see under anæsthesia, because the "animal is no longer itself. An experiment on the "human subject, for example, to whom you have "given an anæsthetic (chloroform, say), goes this length "—that the person is rendered insensible, and you "may do any kind of painful thing to the individual "for the time. This proves what I say; but, further "than that, the anæsthetic has no other value; because "when a person, having undergone an ordinary sur-"gical operation, recovers from it, then he suffers just "the same in every respect as if he had not had chlo-"roform at all during the performance of the opera-"tion." *

Passing to another very important subject, viz., the publicity of all Vivisectional proceedings, we arrive at that which, perhaps, is one over which Legislation has most power, and which will sooner mature the formation and influence of a sound public opinion than any other. I believe there is no one remedy more influential than publicity, be the end what it may. I was once speaking to one of the most distinguished police magistrates that ever adorned that position, and expressing my impression as to the happy combination of technical accuracy and discreet adaptation to the case, which appeared to characterize generally the administration of the cases brought before the police. "Ah!" said he, "it is a very good thing "for a man to know that whatever he does to-day "will appear in the 'Times' to-morrow morning."

Sir William is asked—

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* See Conclusion.

" Q. In fact, you would suggest that if the Com-
" mission did recommend any further proceedings,
" that they should take care not to exceed the limits
" of public opinion — not to go beyond what public
" might be expected to support ?

" A. Yes. It has struck me that it would be well
" —with a view to what may be the result of this
" investigation—if the attention of the Governors of
" Medical Schools were called more forcibly to the
" subject than at the present time. That there might
" be certain Governors in each School, who should
" take an interest in the matter, and see that there
" was no unnecessary cruelty in a part of the Institu-
" tion, where it is *admitted generally* that there must
" be a certain amount of cruelty for special purposes."

" Q. Has it ever occurred to you whether, as
" regards these large Institutions, it might not be well
" to put them under an obligation to make a public
" report of the experiments they were performing ?

" A. That has occurred to me. It might be a very
" good rule to make."

In relation to legitimate modes of research, Sir William is asked—

" Q. I suppose we may say this : That medicine is
" based upon physiological, but also on clinical obser-
" vation and pathology ?

" A. Yes.

" Q. And, in your opinion, is clinical observation
" and pathological observation of more service in
" practical surgery than experimental physiology ?

" A. Yes ; there is precision in the one, whilst the
" other is largely theoretical."

I have copied the foregoing extracts from Sir William Ferguson's evidence, because they go closer to the question that, as a practical advocate of scientific surgery, I have always endeavoured to hold.

Of late the advocates of Vivisection have, so to speak, taken new ground—at least, some of them—and instead of testing the value or the justification of experiments on living animals, by their alleged practical benefits in the study or treatment of disease, we have to hear those experiments justified by gentlemen who are termed biologists, or students of pure physiology; that is to say, having no necessary relation to the practice of medicine and surgery. Now, no one has ever contended that it is impossible to find out something from the dissection of a living animal; but the contention is that you cannot find out anything of any value, which may not be discovered from better and more certain modes of enquiry, and that Vivisection is not only unproductive, but, in fact, a great mistake. What Sir William Ferguson says of anæsthetics, as obscuring any question, I have long ago, over and over again, demonstrated by the plainest rules of common sense. It is no more reasonable to argue from the phenomena in an animal under an anæsthetic, than it would be to reason with a man who was intoxicated by alchohol, or narcotized by opium or tobacco; but to a man who does not at once see this, all reasoning is just as useless in the one case as in the other. He is plenarily absorbed by one idea, and (like the eagle, concentrating his attention on the carrion) does not see the trap on the contiguous rock, which has been placed there to catch him.

Sir William Ferguson appears to me to be quite right in regard to the question as to sporting. It is, indeed, "a poor device" to prevent enquiry.

Now, no doubt there are hundreds of gentlemen who are sportsmen, and I dare say very many sportsmen who may not in the humane sense be gentlemen; and I know of no proceeding, involving any suffering, that is not pursued with careful humanity on the one hand, or too little regard for it on the other. This cannot be denied in regard to sporting, nor, in my experience, to any other proceedings, by no means excepting that in which you would least, perhaps, expect it—I mean surgical operations. And I am far from feeling any of that sensitiveness which was evinced by one of the witnesses, if that surveillance, which Sir William Ferguson suggests, over experiments were extended to all other things, as far as practicable. But *apropos* of sporting. A man may breed pheasants to such an extent, that I have seen five hundred on one field. He may have seven or eight, or any other number, of battues, with any number of visitors with their guns, and in a few hours destroy a great many birds. He may thus, unintentionally, initiate a premium on poaching, and make an acceptable addition to the anomalous pursuits of tramps and of some half-a-dozen fellows who not seldom infest most villages, and, I dare say, help the logic of the poacher, who sees *no more* harm in sneaking about in the dusky evening and setting snares, than in "them gentlemen" killing such a "lot" of pheasants; but who calls this sporting? There is neither health nor exercise, nor the instructive obser-

vation of the wonderful properties of animals for capture or escape; none of the excitement of finding or seeking, of which killing is the last, and certainly the least, of the pleasure.

In any thing like sporting, the dog exercises his wonderful powers in a way, so far as we are capable of judging, in harmony with the purposes for which they were given. The bird is roused, is shot, or escapes. If shot, it is promptly picked up; if wounded, brought by the retriever, and, if not quite dead, instantly dispatched. The head and front of the offence is the death of the animal as quick, and sometimes more so, than that of the animal slaughtered by the butcher. The advocate for the practice of Vivisection thinks that if you lay any restrictions on that practice, you are unjust, unless you enact corresponding restrictions on the sportsman. That is to say, that if you do not make restrictions which it would be next to impossible to enforce, or that in which cruelty is no *necessary* part of the proceeding, you ought not to impose them on proceedings (Vivisection) wherein suffering is the necessary, the constant, and often sustained element; whether we trace it from that zero of the temporary suspension of the sensation of the animal, by so-called anæsthetics, or through an infinite variety, it may be, of gradation, up to the maximum of lingering agonies, voluntarily inflicted on the plea of scientific investigation.

There is something always obscuring in the reasoning of the Vivisector. It so happens that he has not hit the mark in regard to sport. There is, indeed, something like cruelty to animals in the *abuse* of sport;

but it is in the tendency to the demoralization of rural districts, not unfrequently incident on that ridiculous burlesque of sport, the usual accompaniment of those who indulge in large preserves. With regard to the argument of the Vivisector, nothing more strikingly suggests the class of mind which would incline to the philosophy of sense than this kind of special pleading, which attempts to identify sporting and Vivisection; such reasoning has seldom, I suppose, occurred in connection with scientific discussions, if it be not a solecism in language to call it reasoning at all. I must here close my remarks on Sir William Ferguson's evidence, referring my readers, who may desire more, to the Report. There is very much from other witnesses on which it would be interesting to remark; but anything claiming to be a thorough analysis of the various evidence, would require a book nearly as large as the Report itself. No man can, perhaps, have had a better or larger opportunity of observation than Sir William Ferguson, and he delivers his opinion with the moderation and calmness of matured experience. It is not to be expected that, however large the experience of men may be, they will agree in all particulars; but after so long an experience in practical surgery myself, I am gratified by observing that those of whose opportunities and industry I am best assured, impress me with the idea that the most ardent advocates of Vivisection are not to be found amongst the most distinguished consulting surgeons.

There are many other parts of the evidence on which it would be useful and interesting to remark, and which I trust will be—if not in my hands,

in some other — the subject of continued commentary. I believe they will severally exemplify the loose manner in which one of the subjects most important to mankind has been dealt with by some of the witnesses in the Royal Commission. The alleged discovery of the lymphatics by Aselli, by Vivisection, and the labours of M. Claude Bernard, must be made the subject of a careful analysis. People have no idea of the labour all these things require in order to ensure that correctness which has often been so wantonly disregarded in some of the evidence. I wish, however, that both the subjects of the History of the Lymphatics and the Labours of Claude Bernard, be regarded as subject of my comments still in abeyance. The former, because I desire to consult some further writers on the subject; and the latter, because he has experimented so largely, that the exposure of the fallacy of his investigations may, and will, require a somewhat long and elaborate analysis. I have now only an opportunity for a few remarks on either of these writers.

With regard to Aselli, I believe that his observation of the lymphatics might have occurred during his operation on a living dog for another purpose; but I am well convinced that if it had been done expressly for that purpose, it was not at all necessary, as the lymphatics may be observed under favourable conditions without any Vivisection at all, if an animal dies suddenly, or is killed during the process of digestion. But those who investigate physiology merely from an anatomical point of view, and who, as *anatomists*, naturally desire to make everything an object of sight, seem to rush to the "*non sequitur*" conclusion that

the animal must be alive, although his words do not necessarily convey the latter inference; there is perhaps something significant of it, when he says: "Sen- "suum veritas, et evidentia præ omni ratione est;" and, again, "Anatomicus non nisi oculis suis credere "debet."

It seems to me that Vivisectors of all times, and on all subjects, seldom fail to combine with any conclusion that is proximately sound, sufficient of that which is false, to convey the idea that Vivisection always tends to more or less mislead the investigator. M. Claude Bernard is regarded, I believe, as the most active Vivisector at this time, and it will be a very serviceable contribution to the whole subject, if a careful analysis of his *whole known* proceedings be published. This—if some of the large number who are opposed to Vivisection will adopt the plan I shall submit in the conclusion—may be easily done, as it will admit of an indefinite division of labour, and keep up not only the question itself before the public, but, what is still better, enable almost any person of ordinary education to form an opinion on the merits of the various cases. I here can only insert a few remarks, which may help the reader to form some idea of the style of reasoning adopted by so well-known an experimenter.

I remarked on the following experiment some years ago, in my reply to the French Commission. I do not think I can now select any one more striking of the questionable reasoning of a Vivisector. The experiment consists of dissecting down to the pancreas, or sweetbread, of a living animal (in this case a dog), to obtain the secretion from that gland, by means of the establishment of a fistulous opening. We are told

that "You may repeat the experiment several times "on the same dog without any serious inconvenience, "as the animal, *when properly selected*, 'does not "'suffer much,' notwithstanding the delicacy of the "organs wounded." We are not told to what property the "properly selected" refers. Again, we are informed that, "In consequence of the tendency of the "fistula to heal, if you want much of the secretion, "you must perform the experiment on the same dog "*several times*, or on many dogs at the same time."

M. Claude Bernard, remembering that he is only reasoning from analogy, and not a very close one either, asks—"Is the pancreatic juice found in man, "and that obtained from dogs, identical?" To this he answers: "I am prepared to answer in the affirma-"tive." I suspect that those who study physiology by the light of pathology in the human subject, will not be quite so prepared as is M. Bernard with this affirmative; but let us see how he meets the difficulty. He observes: "And if differences *have been observed*, "I strongly suspect, as in the preceding cases (two "cases of pancreatic fistulæ in the human subject), "they are to be attributed to the unhealthy condition "of the gland in the human subject in whom the "fistulæ existed." For he remarks—and I beg the reader's particular attention to the whole of this reasoning—"On making infusions of the pancreas "taken from *condemned criminals*, by allowing it to "macerate in tepid water, a liquid entirely *similar* to "the pancreatic juice in the canine species is obtained. "In the normal state, *therefore*, secretion in the man, "and, indeed, the dog, are the same." Another remark of M. Bernard, in regard to these artificial fistulous

openings, is—"In these experiments the results, by "various observers, do not agree;" and again, as if under no form could one detach elements of fallacy from this unphilosophical mode of research, M. Bernard observes, with a kind of "*sang froid*," not in itself insignificant, but much more valuable as coming from a Vivisector, that in "living animals, the use of "anæsthetics would appear convenient, but the liquid "thus obtained does not enjoy the *usual properties*." Now let us glance at this reasoning. The object is to ascertain the nature of the pancreatic secretion, for which too many opportunities have been offered in the human subject by wars, by accidents of travel, and various other occasions in persons who have been suddenly killed in perfect health. Now then, to revert to the proceedings of M. Bernard. 1st. The animal is a living dog, well selected (we are not told how), and a painful experiment is performed on him, it may be "several times," or you may experiment "on many "dogs at the same time." Here the obscuring elements, inseparable from so disturbed a condition of the nervous system, or on which system the secretion entirely depends, is so little regarded as to any influence it may exert on the matter secreted, that we are told that the experiment may be done several times on the same dog, or on several dogs at the same time; another assumption, viz., that all the dogs will produce the secretion you desire, without any qualification as to the kind, the ages, the sex, or any other of the numerous varieties observable in these animals, further multiplied as they are by domestication. We may learn the insecurity of this kind of assumption from the experiments of Tiedemann and Gmelin,

where, in their experiments on the pancreas, they describe the secretion thus obtained as being first acid, and then the very opposite, that is to say, alkaline.

Then, to judge of the identity of the secretion of the pancreas in dogs and the human subject, the influence of the experiment itself in disturbing the natural character of the secretion seems altogether disregarded; and with an equally bold assumption, the *normal* condition of the secretion in man is assumed in that of the secretion taken from the pancreas of a condemned criminal, and allowing it to macerate in tepid water. Now, whatever may be thought of Vivisection, either by those who regard it as a lamentable fallacy, or by those who still are inclined to think it not without its use, there can be, I think, but one impression as to the reasoning which I have just described. For my part, it is such a "hop, "step, and jump" kind of philosophy, that it appears to me scarcely less than a miraculous abnormity of mind, which can induce any man (on a subject which requires the greatest care, the most comprehensive circumspection, to avoid interfering influences), to reason in a manner which so easily disregards so many obvious sources of fallacy. It would be curious to hear what an analytical chemist would say, if he were shewn as a specimen of the normal secretion of a gland, that which had been macerated in warm water, and previously taken from the body of a condemned criminal. Surely it does not require a professional teacher to test the fallacy, or assumed value, of the conclusions in question.

CHAPTER III.

CONCLUSION.

I CANNOT at present further multiply these comments on the evidence before the Royal Commission on Vivisection.

If the question be subjected to the process that I have long recommended it should be, it would not be long before it engaged the intellectual examination of the public. This examination, carefully and calmly conducted, would soon be followed by a great diminution—if not an entire abandonment—of that reticence on the subject, which has been more or less observed by a very large proportion of the more reflective members of our great profession. No man, who knows how much suffering of mind and body, and, I may add, often of purse also, so invariably accompanies any opposition to a prevailing conventionalism, will be surprised, or impatient, on knowing how many professional men distrust the practice of Vivisection; how many cordially disapprove of it on scientific, no less than on moral grounds, and yet who do not think it necessary or expedient to promulgate their sentiments beyond the area of their respective circles.

There are, however, abundant indications to the more observing portion of the public, that the profession cannot be held as other than distrustful on the one hand, or disapproving on the other, of that mode of research, the pretensions of which it was the object and—I suppose I may add—the duty of the Royal Commission thoroughly to examine. Inclusive of those who are styled biologists or physiologists, who cultivate physiology without any immediate relation to the practice of medicine or surgery, we are told that there are not more than twenty or thirty active experimenters on living animals in this kingdom; so that in a profession composed of many thousand members, who must have not only a natural anxiety, but also a personal interest in relieving as many of those committed to their care, as effectually and as quickly as possible, there is only a portion of the small number above stated who evince any practical recognition of the mode of research in question. And yet this path of study is represented by its advocates as so high, so promising, so indispensable a mode of study, as not to be an object of choice, but of necessity; and one witness goes so far as to predict that the abandonment of it would be a relapse into barbarism, as a necessary condition incident on a "continued increase of civilization." I believe there is nothing which so successfully elicits the credulity of the public as bold assertions, especially if they have the advantage of not being easily intelligible. The very boldness of an assertion seems to produce a kind of senseless astonishment, and people fancy that must be true which they have not the power to examine. A well-known,

celebrated quack, who had invented a medicine, by the sale of which he had amassed a considerable sum of money, was asked by a medical man, who had known him in early life, how he had managed to persuade people of its efficacy, etc. He said, in his German accent—"Sir, nothing is more simple; you have "only to tell the people something that common "sense shews to be impossible, and you have the "secret at once."

There is no doubt that public ignorance is the real support of all fallacies. In my humble opinion, there is no mincing the matter. It is impossible for anyone, who examines the depth and the extent of the relations of physiology to body and mind, who has ever made any part of the relations of function to the various organs, either individually, or collectively in their combined phenomena in varying conditions of the body, and still more when these varying conditions imply the existence of disease—to accept the evidence before the Royal Commission as so full, so fair, so comprehensive a statement, as would be just to a benevolent Government or a great profession. Loose statements, often—I had almost said generally—unsupported by facts; masses of opinion, without any attempt to trace or teach them, so that they might be tested by the *lay* members; quotations of so-called facts, which were doubtful, unproved, or absolutely incorrect, are all exemplified in the evidence before the Royal Commission, in which, commensurately with the importance of the question, no one department was adequately represented. Now, I must explain here, that I do not presume to refer to the capacity or fitness of the *individuals* so

far as that number of them can suffice; but I do contend that the *number* was by no means satisfactory, as regards any branch of the great question submitted to them, and that the power of pathological investigation, as disclosing and testing the suggestions of the physiologist—the one the most *indispensable* of any—was least impressively represented of all.

One gentleman put a question, saying interrogatively, he supposed the witness thought we had pretty well exhausted the resources of the dead-house, or words to that effect. Why, who ever thought of finding the great lessons of pathological research in the dead-house, except those of whom there seems to be no dearth, viz., those who confound pathology with morbid anatomy? This is indeed an important *element* in pathological investigations, but it does not direct our enquiries, nor help us much (if at all) in the detection of causes, but it is, nevertheless, valuable in testing our conclusions. Here again is seen the tendency to attach so much interest to sight. Morbid anatomy teaches us the changes that organs undergo; but what the pathologist desires, is to have such a comprehensive record of the antecedent phenomena, as to help him to the causes of that change.

The questions, which are too often left to be solved by mere opinion, are just those which—lying at the root of the whole enquiry—should have been most fully and searchingly developed, as borne out by plain and incontestable facts. For example, it was sometimes asked in relation to some fact stated in favour of Vivisection, "and you are of opinion that that "could not have been obtained by other means?"

Thus implying that, *pro tanto*, Vivisection was necessary or vindicable on account of the alleged impossibility of arriving at the fact by any other means.

Now, the idea of taking so sweeping an assertion, which, at one jump of mere *opinion*, practically gauges, as it were, the multifarious resources of pathological physiology, and on a matter the subject of controversy, seems to me to be altogether irreconcileable with that elaborate accuracy which should characterize so important an enquiry. Now let us look at the matter in the light of inexorable facts. In all judicial enquiries, where a single life is concerned, nothing can exceed the care, the circumspection, or the reliance on *facts* alone. In regard to right or wrong methods of physiological or pathological investigation, it is no exaggeration to say that hundreds of lives may be sacrificed or saved, as the one or other path is chosen. Even these observations—fragmentary and imperfect as they are—have furnished proofs of that assertion. In further pursuit of this subject, evidence in favour of Vivisectional experiments had been given as derived from the works of those who, so far from pleading the necessity of experiments on brutes from any intrinsic difficulty in regard to the questions which they desire to solve, avowed and excused the necessity of their mode of research, not from any intrinsic difficulty, or from there not having been other means, but from the fact that, although superior opportunities had been offered in the human subject, they had been allowed to pass entirely unexamined—in fact, in a pathological sense, shamefully neglected. I am here referring not only, but chiefly, to the work of Dr. Jones on

hæmorrhage, and the important practical facts which he showed in relation to the ligature of arteries, in which, by the way, the pain inflicted could not be conceived as comparable to the sustained agony sometimes inflicted on animals. Dr. Jones, in expressing his regret at the want of more accurate observation, says how mortified he had been in consulting various sources of information, in periodical and other works, to find how many opportunities—from accidents and other sources—had occurred for investigation, and yet, in some of the most instructive cases, the artery had not even been examined. Now, why was this admirable concession to the true mode of investigation not elicited, whilst the alleged, the forced, or assumed absence of it is so often pleaded in favour of Vivisection? Why was it also not stated, if not to the professional, at least to the *lay* members of the Commission? But I do not see a word said on the subject.

If Dr. Jones was quoted in favour of a mode of research of which *thousands* doubt the use or propriety, why was what he said in favour of a mode, the entirely unobjectionable character and superior scientific claims of which *no one doubts*, suppressed, not elicited, or altogether disregarded?

The frequent employment of questions more or less of a leading character, cannot, I think, be held as entirely free from objection. We do not expect, of course, that gentlemen voluntarily offering their testimony on a scientific subject, should be cross-examined after the manner often expedient in courts of law; but it is reasonable to require that questions should be so

far guardedly framed, as to render anything like cross-examination unnecessary; and that, when deemed expedient, it should be from the too loose, too general, too sweeping, or too unsupported answer of the witnesses. It should also be borne in mind that the question of Vivisection has, like most controversies, excited a good deal of feeling, which, however natural on one hand, or explicable on the other, is practically antagonistic to the easy development of truth, and therefore requires, in the examination, unusual care and circumspection. When I first heard that a gentleman, who has felt greatly interested in the question of Vivisection, had applied to know if it would be allowed to employ counsel, I thought it unnecessary and inappropriate; but when I read the evidence, there were several questions, and answers too, which suggested that assistance, the expedience of which Mr. Jesse seems to have anticipated, either from counsel or, what would perhaps have been better, some competent pathologist. Another reason which has led men to look more readily to the dissection of living animals, is the habit of looking to the functions too exclusively from an anatomical point of view, and thus confounding the true relations of the result produced with the mechanism through which it is made known to us. In this way a number of very hasty and untenable conclusions have at times arisen, to which, for aught I know to the contrary, the discovery of the circulation may have indirectly contributed; for, although the discovery of the circulation was not necessary to show that all parts were supplied with blood or other nutritive fluid, yet it may have led to erroneous conclusions

with regard to structures, where the presence of blood could not be demonstrated to *the eye*. In my early days, it was not uncommon to hear it inferred that a part was not organised, because the vessels could not be demonstrated by injection. This Mr. Abernethy was accustomed to ridicule, saying that if the organisation of the part could not be demonstrated to the eye, it was luminously evident to the understanding. He meant from the changes which the part was capable of undergoing. In fact, the demonstration of structure (anatomy), which was, of course, a matter of sight, was applied to the solution of a series of phenomena, in regard to the real understanding of which sight was of no avail, because they could only be really appreciated by the intellect. This tendency to seek the real nature of function through the peculiarities of material structure is still seen, and perhaps never more so than in the very useful, but somewhat excessive attention to, or interest in, minute anatomy; but it does little or nothing to advance positive physiology; for it has no scientific relation to it, except in the sense that the apparatus of the chemist has to the demonstration of the questions in which he is engaged. Microscopic anatomy, however, has the negative property of disabusing people from conclusions which they are too ready to form from the unassisted eye. It is to the tendency to a too anatomical point of view that I attribute the character of the physiological lectures at the Royal Institution. They have been generally given by eminent men; but they have been, in my view, rather an anatomical narrative of certain detail in the adaptation of the parts to their

uses, simple platitudes, or the results of digestion, with some illustrations from comparative anatomy; all of which is very well in its way, but having no breadth or grasp to impart great and useful general views, which might be serviceable to mankind.

When I had the honour of being on the Board of Managers, I ventured to suggest that light, though *physiologically* considered, should be regarded at first with that larger grasp, which would glance at its more extended functions in the surrounding world of nature, in its relation to its physical and chemical functions or agencies, and the somewhat larger and circumspective view thus obtained should have its light focalized on the subject, the more immediate object of enquiry—the eye, its anatomy, and its functions; but I failed to impress the importance of such views, though they were not altogether without some indications of sympathy; but they appeared to be regarded as too difficult to secure a competent discussion of them. No physiologist, that I have ever heard, has discussed the subjects of light and vision, but in so restricted a manner as to exclude some of the most beautiful points on these interesting subjects. It is from these and similar narrowed areas, that men are still led to grasp so imperfectly the grand relations of this delightful science.

There are many points which lie, as it were, outside of the mere scientific question, which are nevertheless of great interest to the medical philosopher, and which furnish their contributions to the interminable fallacies of Vivisection. One of these is the notion that Vivisection is useful to the operating surgeon, and

that, in familiarizing him with the dissection of a living animal, it may enable him to acquire more coolness and self-possession in operating on the human subject. If this idea, when it first was presented to me, had not been from the published opinion of a surgeon, I should hardly have believed it could come from a professional man. I dare not express myself in the terms which appear to me alone applicable to this low, contemptible, and debasing idea of the attributes of an operating surgeon. In the first place, as an operating surgeon, I must reject the idea as a barbarous substitute. Vivisection may brutalize a man, so as to render him an unfeeling operator. That may be; but a man who has no better guard than this, will evince its inadequacy on the very first difficulty he has to encounter; he will either do something very wrong, or make some great mistake. I have seen as many operations as most people, and I speak from what I know; but it is a curious thing that the best illustration that I could willingly give, is from the practice of one of those who used the very argument of which I am endeavouring to show the fallacy. He was performing an operation—in itself one simple enough, provided that a man is prepared with the well-known cautions, which I have elsewhere pointed out—but, something occuring which was not necessary, he became confused, and the result was such (as alleged) an unwarrantable prolongation of the patient's suffering, that the case became the subject of an action at law.* The real sources of coolness and self-possession are very different from any

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* Surgical Commentaries on Lithotomy.

which can be derived from Vivisection. That the *voluntary* infliction of suffering on a defenceless animal may harden a man's feelings, and brutalize him in any degree, is a necessary result of the power of accomodation in a mind to whom such proceedings are acceptable. But to suppose that this is proper training for a surgeon who has a duty to perform, with so clear and definite an object as a real benefit to a fellow creature, shows to what absurd extremes the apologists for Vivisection are sometimes driven. You might as well assimilate to murder the man who saves a fellow creature from drowning, because he was obliged to wound him by the grappling iron; or a conscientious surgeon who brings his mind and his conscience to an operation, with a sincere desire to relieve a suffering brother, with a brute who would perform an operation for no higher purpose than to extend his reputation, or attract pupils. No doubt the surgeon has some painful duties; but the union of the most refined sympathy with the most successful skill has been often exemplified, but never better than in the celebrated Cheselden, who tells us that though he seldom slept the night previous to an operation, never found his hand tremble during its performance. No doubt a man may be an unfeeling surgeon as well as an unfeeling Vivisector; but in any way to confound, or criticise in any form, the necessary psychological relations of the two individuals, is to assimilate one of the lowest forms of mental research, wherein it is difficult to see—or even to imagine—any feeling, with a duty which should, and often does, combine the greatest self-possession with the most sincere humanity; than which,

in the execution of a painful duty, there cannot be a more dignifying combination in our or any other profession.

I have not much more to say. If spared, I may yet have another opportunity. With my views of the evidence offered to the Commission, with so much calculated to mislead, I think Mr. Cross must have had great difficulty in framing any Bill adequate to the requirements of the subject. The Bill may be regarded as an instalment, and it will be the fault of the public if it be not followed by more efficient enactments. This implies a delay, by which the public often expiate their long forbearance, or ignorant inattention to numerous conventionalisms. The public must help themselves by improving that ignorance, which is the real basis of most evils. The Government have no power of coercing public opinion, and they cannot act efficiently without it. If the public will not take the trouble of cultivating the easy and agreeable truths of sound elementary physiology, they must take such as the advocates of Vivisection regard as such. Public ignorance prevents many an ardent student from the study of the philosophy of disease as it should be studied, because the public know not how to appreciate it. As the rule, they think highly of a man who takes his fee and dismisses them in a few minutes; whilst the conscientious and patient examiner of a case is often thought slow and unattractive, if nothing else. All this is to be regretted, because it recoils on the public to the prejudice ultimately of their highest interests. With this conviction, I, many years ago, gave a gratuitous course of lectures, addressed alike to the public

and the profession. I have good reason to know that they were satisfactory to an audience, some of whom were distinguished critics. I have since heard the substance of one of those lectures given with great applause by another physiologist. My reward was being told, with what truth I cannot vouch, that the chief reason why I was not selected to a professorship —for which there were several other unsuccessful candidates—was, that I had given *popular* lectures on physiology; and yet twenty years afterwards, I found that one of the desiderata of the time was a popular knowledge of physiology, and this from the Chair of the Royal Society. Let the opposers of Vivisection be calm and patient, but persevering. The thin edge of the wedge is inserted. A cheap periodical calmly conducted, having for its object a description and scientific discussion of claims of every experiment, would in a few years show on which side the truth lies, and inaugurate that more careful observance, study, and interpretation of phenomena, of which some of us have for years pleaded in vain.

It is scarcely necessary to notice the language in which certain advocates of Vivisection think it proper to indulge—such as charging the humanitarian view of Vivisection with folly, fanaticism, and so forth. It is no part of my present plan to deal with those who think such language vindicable in a scientific matter; but it is a welcome sign of a cause not being overburthened with strength, when recourse is had to such questionable weapons. Having been asked to admit an experiment or two, in deprecation of this charge of so-called fanaticism, I said that I could not well put it in

the text in conformity with my avowed plan, but that I would allow it in an appendix, where they will be found. There are several collateral influences, which are already showing the small ripple that so often indicates a coming storm, or, it may be, a favourable breeze. People are just beginning to think—to examine the pretensions of those who assume the language of dictation—to test their force, not by the popularity or the position of the man, but by what he has really done in aid of a more improved or positive science. They also begin to perceive that although no one ever gets to the front, as it is called, without some kind of talent, this is not always shown by any positive additions to the science of his profession. Another thing which is rising up, is a doubt whether a man who inflicts, or sanctions, xperiments involving the torture of animals, is the best calculated to be the most genial recipient, or the most humane administrator, to the suffering patient. These, and many other agencies, are beginning to work; and if the Government allow *publicity* to be given to experiments, and they are calmly discussed as to their claims and purpose in a frequent and cheap periodical, a very few years, as I have said, will soon inaugurate an improved condition in physiological research, and less reserve as to the repulsion *already known to exist* in a large portion of the medical profession. The Vivisectionists express their fears that if Vivisection were forbidden, we should drive many experimenters to the continent. If that were so, it would emphasize the wish of many a scientific man for the abolition. Cosmopolite as I desire to be in everything regarding science, I do not desire to see any assimilation in

experimental physiology to our continental neighbours. I know of nothing which we have derived from them for many years, that I do not regret. Their absurd ministrations to some diseases, which are much more safely ministered to by scientific surgery, have only served to violate the delicacy with which one class of patients have always been treated, or to minister to the prurient and demoralizing appetencies of another. It is from these and similar views, that I believe more men have been led to entertain such imperfect views of the relations of the beautiful science—Physiology.

A science which, in its small and suggestive beginnings, presents us with facts so plain as to be intelligible and interesting to the least cultivated understanding, which are in close sympathy with our instincts, our feelings, and our necessities; which, by a beautiful and gentle progression, conducts us to other facts and relations which—without in any way impugning, much less dwarfing, the familiar lessons with which we commenced our studies.—invest them at every step with increasing interest and value; which, in further progress, flashes on us the startling fact that the most common of our bodily functions evince very striking connections, and exert unquestionable influences on the action of the mind itself. That, thus far, a science—which may have been cultivated with no higher view than the care and safety of our perishable bodies—should suggest that all the phenomena should be carefully reviewed. That new fields should now open to us, not only of bodily relations, but which require the ardent study of the philosophy of mind—and this, not on the basis of any mere metaphysical

abstraction or theory, but on the more secure ground, and as a necessary fluxion from plain, sequent, and inseparable phenomena. That what has been observed as to the outward and visible sign of disease in the body, as not representing with any security the seat of its *cause*, should, in harmony with the sublime comprehensiveness of natural laws, present us with its analogue in the mind itself; where we perceive that the outward and visible acts, whatever may be their intrinsic character, may be, and generally are, no expositions of the motive which led to them, and thus elevate us to a point of view, whence we have a larger and more positive field for the cultivation of psychological analysis, than we could obtain from any less elaborate mode of proceeding.

Now, to suppose that a science justifying such aspirations as these, built not on any mere theory, but on the close cohesion and inseparable relations of demonstrable facts, should be assisted, or even be an appropriate subject for the sensuous proceeding termed Vivisection, is, to my mind, a desecration of the highest objects to which the scientific mind can aspire, to the lowest and most barren modes of enquiry. Surely the idea of any great truth being developed in physiology by such means, is the very insanity of science—the very bathos of intellectual aberration. The subject of anæsthetics requires more examination than has yet been given to it, on the side of science. As to its influence in removing the scientific objections to Vivisection, it is acknowledged to be often obscuring, and sometimes inadmissible. It is, indeed, surprising how any rational being can suppose that he can educe any

natural phenomenon, or evidence, of the healthy actions of an animal, whilst its nervous system is thus narcotized. There are evils, too, not always inseparable from the more legitimate employment of those agencies. I have known, sometimes, the most desirable and unexpected light thrown on the reparative power of the œconomy, and the most splendid illustration of the superior views of Abernethy, consequent on a patient refusing—from the dread of pain—to submit to what was thought to be a necessary operation. Accidents of this kind, so to speak, must have occurred to most surgeons of experience, and with results obviously of a highly instructive character. Now, I fear that impatience on the part of the patient, or, it may be, of the surgeon, has sometimes, under the comforting assurance of an anæsthetic, led to operations, which anticipated unnecessarily the reparative processes of nature, and thus—as well as in obscuring from us the natural history of the disease—has been an unwelcome obstruction to a valuable scientific observation. I have thus glanced at a few of the evils of Vivisection, and the fallacies more or less connected with it.

I heartily wish that we were left to the study of a beautiful science, as beneficient in its objects as it is delightful in its legitimate study; and which revolts at unnecessary sufferings, whether in man, woman, or child, or any other animal, as contrary to its principles and its scientific relations, as it is to nature, science, and morality.

"Nunquam aliud natura, aliud sapentia dicit."

In the Appendix will be found a description of an experiment or two, to see where Vivisection may lead a man, and which are added at the desire of some who oppose Vivisection. I could hardly, consistently with my plan, insert them in the text; but I said I would put them in the Appendix.

APPENDIX.

An Experiment by M. BOUILLARD.

"I MADE an opening on each side of the forehead "of a young dog, and forced a red hot iron into each "of the anterior lobes of the brain. Immediately, the "animal, after howling violently, lay down, as if to "sleep. The dog slept occasionally for a short time, "and on awakening began its mournful cries; we tried "to keep it quiet by beating it, but it only cried the "more loudly. After some days," says M. Bouillard, "I was obliged to kill it, as its irrepressible cries "disturbed the neighbourhood." Another dog, similarly experimented on, which was described as lively and intelligent, was kept alive from the 28th of June to the 14th of August; and with what result? The experimenter says: "The subjects of these died "TOO SOON to allow me to draw any clear or definite "conclusion!!"

The following experiment was made by M. Brochet:—

"After inspiring strong aversion in a dog, by "plaguing it, and inflicting pain on it, first put *out its* "*eyes*, and then destroyed its hearing by piercing the

" drum of the ear, and filling up the cavity with wax." This was to know whether the animal would evince the same aversion as before ! ! !

Another experiment was—one of Magendie's—opening the body of a bitch with young, to know if the mother, in seeing them, in her dying state, would show parental feeling, which it appears she evinced by applying her tongue to them ! !

I have somewhat unwillingly admitted abstracts of these experiments, in concession to the wishes of others, because such narratives may divert the attention of the reader from the scientific bearings of the subject, which it has been more my object just now to consider; but I hope even the Vivisectionist will hardly call that " Fanaticism " which recoils from such horrors.

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