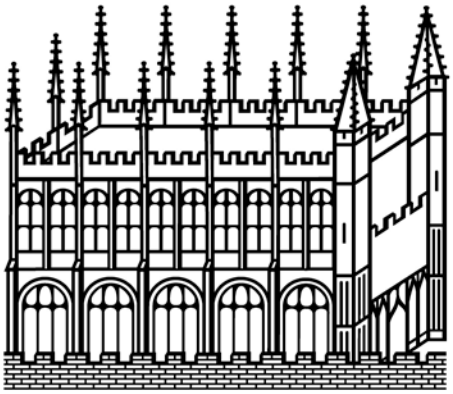

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VIVISECTION.

By ANNIE BESANT.

Sir Eardley Wilmot, M.P., has given notice that he will next year introduce a Bill "For the Total Suppression of Vivisection." Since the anti-vivisectionists will do their best during the autumn to stir up feeling on this subject, it may not be amiss to lay before our readers some reasons which may induce them to investigate the question before joining in the cry raised for the total suppression of experiments on animals. I am perfectly aware that in so doing I am laying myself open to furious attack, but as I am pretty well accustomed to be abused for speaking out for that which I believe to be true and useful, this probability does not seem so alarming to me as it might to many.

Under the general term "vivisection" are included all experiments on living animals, whether by instruments or by drugs; a Bill for the suppression of vivisection would stop all experiments causing pain to the subject. The question to be decided is: Would such a law as is proposed tend to the decrease of human and animal pain, using the word "animal" to denote living beings other than mankind? I answer distinctly in the negative, and I propose to show that such a law would enormously increase and perpetuate human and animal pain, and that the comparatively small amount of pain which would be prevented—small when contrasted with the vast mass of agony endured in the world from "natural causes," small when contrasted with the agony which Theists must regard as god-inflicted—would be purchased by a fearful price of preventible pain in the future.

Before considering (1) the necessity, and (2) proved value of vivisection, a few introductory remarks are necessary.

Vivisection should only be performed by well-skilled scientists, engaged in original investigation. No young student should engage in it, for the very simple reason that such a student would inflict unnecessary pain, and would obtain no new and useful results.

Vivisection should not be performed for needless re-demonstration of thoroughly well-ascertained facts. It was right that Harvey should vivisect while studying the move-

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ment of the blood; it would be wrong for a young student to repeat Harvey's experiments for the mere demonstration of the admitted fact that the blood circulates.

Vivisection, when practised by eminent scientists, should not be under State control. Only men far more learned than the leading men of science could decide on the utility, and therefore on the rightfulness, of experiments made by the latter. Such men do not exist, and to subject the learned physiologist to the control of the unlearned inspector is a mockery and an insult. A mockery, because the ignorant official might stop an experiment at the most critical period; an insult, because such inspection implies that the doctor or the scientist thus supervised may be guilty of needless cruelty. We trust our dearest into these men's hands for the most agonising operations; if we can trust them with these, surely we may trust them with the lower animals. The man who comes as the savior of life into the chamber of one beloved, albeit he comes with knife, and scalpel, and forceps, can scarcely turn into a devil of brutality when he crosses the threshold of his laboratory. Nay, the very delicacy and steadiness of touch, the firmness of nerve, the keen sensitiveness which recognises by "feel" the unseen tissue through which the knife is passing, the swiftness which means safety where delay or hesitation would mean death—all these which we so gladly and thankfully utilise in the hour of our need, all these have been learned in the long laboratory practice to which "zeal without knowledge" would put an end.

The infliction of pain is not the criterion of the cruelty of an action. Anti-vivisectionists circulate the most harrowing descriptions and the most revolting pictures of the experiments performed by scientists. But it would be the easiest thing in the world to outdo these by descriptions and pictures of operations within the walls of a hospital, which have resulted in the saving of life. A very harrowing description might be given of cutting out the mammary glands of a female dog, and anti-vivisectionists would use this as an argument against vivisection. But an equally harrowing description might be given of cutting off the breast of a woman, an operation not infrequently performed: ought this to be used as an argument against the operation? If not, why not? In both cases the ultimate result of the excision is the saving of pain and of life, but in both cases the means thereto is the infliction of very agonising pain. If an artist would go round any surgical museum, and paint

some of the preparations there displayed—say a partially performed operation of lithotomy, a cancer in the womb, and so on—some very horrible pictures might be made of human vivisection. If these yield an argument against surgery, then we had better go back to the day when cancer in the stomach was treated by the exhibition of the thumb of St. Ursula: an operation perfectly painless and perfectly —useless.

Religion is said to be against vivisection, and we hear a great deal of talk about "the great Father of all." If the argument is relevant in any way, it is certainly on the side of the vivisectors. The amount of pain inflicted is an insignificant portion of the sum total of the pain in the world. Animals starve by millions in woods and fields, in air and on land. They are tortured by the most loathsome diseases, by agonies long drawn out. If "the great Father of all" sees this objectless and useless suffering unmoved, nay, inflicts it as part of his scheme of creation, can he be supposed to object to the comparatively microscopic amount of pain inflicted in the laboratory? As excuse for the cruelty of nature, we are told that some good purpose underlies it all. The same answer applies to the work of the physiologist, and in far more forcible fashion; for no immediate nor even ultimate good is proved to result from the daily torture inflicted on myriads of sentient beings by nature—that is, according to Theists, by god—while, as will be shown, immediate and invaluable results accrue from the pain inflicted on units by physiologists.

It is not consistent to stop vivisection and leave untouched other sufferings inflicted on animals by man. Who that has heard a pig screaming in the death agony can join an anti-vivisection society and dine off pork? Who, remembering how many birds are winged and escape to die slowly, can touch game? How can the State stop vivisection and allow the coursing of hares to continue? Or the hunting of foxes and of stags? Or the shooting of pigeons? Or the angling for fish? The only answer to this argument is that the infliction of pain for the gratification of the palate or for the amusement of the idle is permissible, but for the advancement of knowledge and the curing of the sick is detestable. But those of us who are not blinded by prejudice think that anti-vivisectionists who swallow the camel need not strain so terribly over the gnat.

(1) *The necessity of Vivisection.* Sound knowledge of any natural phænomena can only be gained by experimental

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investigation. The botanist must pull his flowers to pieces, must make sections and must study tissues under the microscope. The chemist must analyse, test, decompose, recompose. The geologist must break rocks, analyse their composition. The anatomist must dissect, handle the bones, note the position of the organs. In a word, experiment must be used everywhere to discover new facts, to test doubtful assertions. The physiologist is not exempt from this law of progress. But since his work lies with function, and function of living organs can only be studied in the living, his dissection must be vivisection, his experiments must be on living beings, not on dead. The physiologist, the guide of the physician, must strive to discover the cause of disease and the remedy for disease. Without experiment, all medical science must be a matter of chance. Around us are countless drugs, some known, some still unknown. The effect of many known drugs on the human body has been recorded, but the record is based on the results of the administration of the drugs. Suppose a new drug discovered. It is impossible to predicate beforehand the result of the administration of that drug; the result cannot be known until the drug has been actually administered; it may be fatal, it may be injurious, it may be beneficial. Unless no progress is ever to be made that new drug must be tried—on someone. The only question is: On whom shall it be tried? Anti-vivisectionists answer: On man. Vivisectionists answer: On the lower animals. Professor Virchow states (see report in *Nature*, August 11) that "The animals' friends say to us, 'Then try the new medicine on yourselves.'" When I read this, I thought Professor Virchow had merely used a *reductio ad absurdum*, but to my astonishment, a day or two after, I read a letter in a public journal actually saying that physiologists should try the results of an unknown drug on themselves and not on animals. Could unreasoning cruelty go further? It is wicked to try an unknown drug on a rabbit; it is right to try it on a man! The man's life, the man's pain are of less value than the pain or life of a rabbit or a guinea-pig! Every physiologist knows how largely scientific men have experimented on themselves. The experiments of Hammond and Ranke on themselves are classical. If Dr. Hammond had fed a dog for nine days as he fed himself, the sentimentalists would have howled at him for cruelty. Dr. Hunter actually inoculated himself with the fearful poison of syphilis, in his noble ambition to win knowledge which should benefit his fellows. But all humane and

rational persons will prefer that experiments shall be performed on the lower animals rather than on man, and that doctors shall not thus sacrifice themselves. Besides, a rather awkward question arises if physicians are to try unknown drugs on themselves. Are they to administer them to each other, or each man to himself? If a physician administers an unknown drug to himself and the effect be fatal, he is scarcely in the position to record his experiment, while if it be administered to him by a friend the administrator would be arraigned for murder. If the drug were actively deleterious, although not fatal, the self-experimentor might often be incapacitated for accurate noting of the sequence of effects. But it is monstrous even to argue such a question. It would be as rational to bid an explorer go and offer himself to a lion for dinner, because the poor dear animal was so hungry.

If the anti-vivisectionists succeed, animals will be saved and men sacrificed. Doctors will be compelled to make involuntary experiments on their patients in the sick room instead of deliberate experiments on animals in the laboratory. Arsenic is a useful medicine in minute doses, and a poison in larger ones. Other now unknown substances may share this property. If Sir Eardley Wilmot succeeds, a number of men, women, and children may be killed with some such new drug before the quantity which will save instead of slay can be discovered. For myself, I should prefer that it should be tried on a few guinea-pigs. It may be said: If the drug kill, why not at once put it aside? First, it is impossible to say that it will kill until it is tried. Secondly, it may, as above said, be fatal in large doses and invaluable in small. Chloroform is often used to kill small animals. If it had been assumed that this fatal effect was its only effect, one of the greatest blessings to sufferers would be still unknown. Experiment, and experiment only, can decide on the value of drugs.

Nor is it only in the administration of drugs that experiment is necessary. Sound knowledge of function can only be based on vivisection. As everyone knows, Harvey was a great vivisectionist, and to vivisection he owed his priceless discovery of the circulation of the blood. But this brings me to my second point—

The proved value of vivisection. I cannot, in this brief space, give a tithe of the beneficial results which have accrued from vivisection, but even the few I mention are sufficient to prove its value. 1. The case of Harvey has already been mentioned. 2. By severing the anterior ar-

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posterior roots of the spinal nerves, Sir Charles Bell discovered that the sensory and motor fibres are distinct anatomically and functionally in the two roots. 3. By experiments, stigmatised as of special cruelty, Professor Ferrier discovered the portion of the brain most closely connected with the sense of smell, the "olfactory centre." One of his patients had for six years been deprived of the senses of smell and taste as the result of a fall on his head. The sense of taste was partially restored by iodide of potassium, but was lost again when the medicine was given up. Dr. Ferrier gave him no drugs, but applied an electric current to the part of the brain which his experiments had shown to be the olfactory centre. He permanently restored to his patient the sense of smell of which for six years he had been wholly deprived, as well as that of taste which had been temporarily recovered. 4. The effects of chloral, a remedy now used all over the world, "were discovered," Professor Virchow tells us, "and established experimentally by Herr O. Liebreich in my laboratory." 5. The innervation of the heart was discovered by Weber by experiments on the vagus nerve. 6. M. Villemin discovered that tubercular disease was contagious by inoculating animals with tubercular matter. It has been further shown that tubercular disease can be transmitted; that many cows suffer from tubercle and that their milk may carry contagion to human beings. A mother with a family of children can scarcely desire that her children shall be sacrificed to a horrible disease rather than that a few animals shall be experimented on in the laboratory. 7. Dr. Simon well points out the experiments referred to when "we think of guarding against the dangers of Asiatic cholera. On the one side there are the well-known *scientific* infection-experiments of Professor Thiersch, and others following him, performed on a certain number of mice. On the other hand, there are the equally well-known *popular* experiments which, during our two cholera epidemics of 1848-9 and 1853-4, were performed on half a million of human beings, dwelling in the southern districts of London, by certain commercial companies which supplied those districts with water. Both the Professor and the companies gave us valuable experimental teaching as to the manner in which cholera is spread." The anti-vivisectionists would save the few mice, and let the thousands of human beings be "destroyed through lack of knowledge." 8. The experiments of Pasteur have proved that certain forms of disease are due to minute organisms which repro-

duce themselves with marvellous rapidity. He "cruelly" inoculated animals with matter containing these germs. He "cultivated" the germs, and marked what rendered them specially mischievous or comparatively harmless. He has thus, by re-iterated experiments on living animals, discovered how to neutralise the infection, and the French farmers who had seen their fowls destroyed by "fowl's cholera," are now able, thanks to M. Pasteur, to guard their poultry yards against its ravages. A similar class of investigations has shown the real cause of "cattle disease," and unless the anti-vivisectionists triumph, English farmers will be saved from one of their most ruinous foes. 9. Mr. Lister—who has been compelled to leave England to carry on his investigations, in consequence of the "Cruelty to Animals Act," —has discovered by vivisection the antiseptic treatment of wounds, and has thus saved from agony and death thousands of human beings. The bitterest anti-vivisectionist would probably be only too glad to use Lister's discovery if he were suffering from an open sore, or compelled to undergo a surgical operation: yet anyone who benefits by the result has no right to complain of the means without which the result could not have accrued. 10. M. Toussaint, by inoculating animals, has discovered a protective against splenic fever. From the various researches of eminent vivisectionists the mass of diseases resulting from the presence of germs can now be dealt with, mitigated, and to some extent prevented. Yet the anti-vivisectionist clamor would put an end to all this beneficent activity, and condemn men and animals to preventible suffering and death. (On the whole of this see Dr. Simon's address, reported in *Nature*, August 18.) 11. The observations of the phænomena of starvation, by Mm. Chossat, Scheffer, Valentine, and Panum, have been most useful in revealing the relative values of food stuffs and in dealing with the phænomena of starvation in human beings. As Dr. Carpenter points out: "It is extremely important that the medical practitioner should be aware that many of the phænomena above described may be induced by the adoption of a system of too rigid abstinence in the treatment of various diseases; and that they have been frequently confounded with the symptoms of the malady itself and have led to an entirely erroneous method of treating it." He quotes from Dr. Copland some startling cases of mischief due to the misapprehension of symptoms which were really due to starvation. 12. The changes undergone by food in the alimentary canal, the

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work of the various secretions, the minutiæ of digestion, the knowledge of all of these has been acquired by the experiments of vivisectors. Such cases as those of Alexis St. Martin are too rare to be depended upon, and failing these such knowledge can only be won by the establishment of artificial fistulæ and certain other methods. Without this knowledge, however, the prevention or cure of enfeebled digestion is impossible.

These are but few of the cases which might be cited as reason for refusing to interfere with the prosecution of physiological enquiries by qualified observers. This paper does not pretend to be a complete argument in favor of experimental investigation: it only endeavors to persuade the indifferent to make themselves acquainted with the interests at stake before they join in swelling the senseless cry raised against scientific progress. The question is essentially one for the poor. If the laboratory be closed by the State, the hospital will become the chamber of involuntary experiment, and the poor will pay for the sentimental cry raised by the wealthy and the idle. The prolongation of preventible misery, the needless sufferings of man and beast, the continuation of disease and ill-health—these are the price to be paid for yielding to the demand of ill-regulated and ignorant sentiment. It is sometimes pretended that those who advocate vivisection are indifferent to animals. That is certainly not so in my own case, and I doubt if it is so in any. As I write, a favorite St. Bernard lies at my side, his big feathery tail waving gently if I speak or move; an English terrier is scampering round the room in full pursuit of an errant ball; a Dandie Dinmont puppy, exhausted with his endeavors to feed on the leg of the writing-table, lies asleep in my lap; a number of birds are twittering in a big cage that gives them space to use their wings; an aquarium, flower-surrounded, fills a large window; a squirrel is playing in the greenhouse outside. All my living brute friends are dear to me, and they gladden my otherwise lonely home; and just because they and their fellows are thus dear, and because dearer yet are those highest animals we call our brethren—because of this, I plead for freedom of science, benefactor of all that lives.

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THE MORAL ASPECTS

OF

VIVISECTION.

BY

FRANCES POWER COBBE.

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THE

MORAL ASPECTS OF VIVISECTION.

THE notion of the extreme tenderness and sensibility of early youth, especially in the male human creature, is almost as purely conventional and remote from experience as the poetic fiction of an English spring, all sunshine and flowers. That type of cruelty which comes of ignorance and recklessness, both of their own suffering and that of others, and wherein Curiosity, not Malice, is the prevailing motive, is at its worst in adolescence; and only as years go by, and observations multiply, and the experience of pain ploughs up the heart, does sympathy grow by slow degrees, till at last, as Sir Arthur Helps has pointed out, it may be predicted with certainty that a jury of old men will take the most merciful view of every case brought for their verdict.

On the larger scale of nations and of humanity, the same process of initiation into the mysteries of suffering and of sympathy goes forward, and we behold society so far emerged from the age of barbarism that an English gentleman would no more insert now-a-days in his account book (like the pious and charitable Alleyne) an item for "Whipping of ye Blind Beare" than the stream of traffic would proceed peacefully in 1882 over Westminster Bridge were a Fenian's head to be exhibited on the cornice. The influences of civilization, of religion, of cultivation—in short, of all kinds,

mental and moral—have softened, like the rain of heaven, the crust of our dry, hard world, and there is every reason to hope that, unless arrested or perverted, they will trickle downwards and permeate the whole soil of human society, till the "desert shall rejoice and blossom as the rose." When we think of what earth might become were the tiger passions within our race to be bred out at last, and the divine faculty of love and sympathy to attain its obviously intended development, it would seem as if efforts for the improvement of our physical or sanitary conditions, or for the advance of arts, science or laws, were scarcely worth making, in comparison of any step which should bring us nearer to such an age of joy.

But it is by no means an even and unbroken line of progress which we can flatter ourselves our race is pursuing towards a millennium of mercy. While the general stream of tendency is undoubtedly in that direction, and may indeed be dimly traced so to have been since the beginning of history, yet there are certain counter currents observable which are setting altogether in an opposite direction. The great wars which the gigantic armies of modern European statecraft have made possible, and the dire legacy of national hatred which such conflicts bequeath to unborn generations, present undoubtedly alarming obstacles in our road. It may excite surprise, perhaps ridicule, if I point to another and apparently comparatively insignificant feature of modern life, as no less threatening in another way. If, while a patient seems to be recovering from a long malady, a new and strange symptom should suddenly exhibit itself, the physician would unquestionably hold that there existed considerable latent danger. Much such a rapid development of peculiarly acrimonious moral disease appears to be taking place in that part of our social body which is just now the seat of highest vitality,

Science is undoubtedly at this hour the ruling passion

of the age. What the Chase, War, Art and Learning, have been in various past epochs, so is the pursuit of Physical Knowledge in our generation. The triumphs thereby achieved have dazzled us, as the people of France were dazzled by the victories of the first Napoleon; and even such of us as understand but very imperfectly wherein these boasted conquests consist, are ready, like our betters, to cast our palms in the way of the new Messiah and shout "Hosanna!" albeit we have too seldom reason to believe that he "cometh in the name of the Lord." If any men may claim to be more than others the representatives of the period, in the "foremost files of time," it is our men of science. Whether the rest of mankind will hereafter meekly follow in their mental track yet remains to be seen; but it is certain that no statesmen, no divines, no metaphysicians offer themselves at the present day with so high pretensions to become our Moses and Aarons, and to lead us—it may be into a Canaan, it may be into a wilderness. What is done, thought, felt, by the men of science is of almost incalculable weight in determining the proximate tendencies of thousands of lesser spirits—the direction to be taken by all those innumerable minds which have no motor force of their own, but follow the *Zeit Geist* whithersoever he goeth. A peculiar and abnormal manifestation of sentiment among the scientific class, or even of a certain small section of it,* is, therefore, quite otherwise significant than the rise of a silly or cruel

* Probably the great astronomers and geologists would be the very last to countenance such practices as those to which reference is made. Mrs. Somerville's expressions of abhorrence of them are repeated many times in her "Recollections;" and the late venerable Sir Charles Lyell, a short time before his death, answered the writer's inquiries as to his opinion with a shudder of disgust, and added: "I do not even like to think of all the *insects* I killed when I was a young man and made my entomological collection. Of course I did it with every precaution to save them pain, but I do not like to remember it now."

fashion among the *jeunesse dorée* of the clubs and the race-course, or the prevalence of an idle delusion in certain urban coteries.

Such manifestation is, I apprehend, actually observable in the very rapid extension of the practice of painful Experiments on Animals, to which some prominence has lately been given in public discussion. In the present paper I purpose studiously to avoid detailing, or even alluding specifically to, any of the multiform horrors which are classified under the name of Vivisections. But without harrowing the reader by similar descriptions, I shall merely point to such experiments as those narrated by Dr. Hoggan, and the singularly ingenious varieties of torture which fill the large volumes of French and English physiological Handbooks, and suggest to my readers the inquiry; Whether this sort of thing be not strangely at variance with the tone of thought and practice which at present prevail in other departments of human activity; and whether such books, for example, as these Catechisms of the Art of Torture, do not even stand unique in the literature of the world? While our legislation tends to an almost excessive lenity towards criminals; while our Art and our Letters become yearly more refined and fastidious; while our manners grow more uniformly courteous towards all classes; and while in a very special manner, we are beginning to take a new interest in the intelligence and affections of the lower animals, and to visit their cruel treatment with condign punishment—in the midst of all this humanising process we suddenly find a break, a pause, nay, a very decided retrograde movement. It is at least fitting that we should inquire into the meaning of this strange and startling phenomenon. Let us suppose to aid our imagination, that something analogous to vivisection were going on in some other department of modern activity. There are legends that *dilettante* sovereigns in the Cinque-cento age (when Art was supreme as Science is now), were so anxious

to aid the great painters at their work, that they beheaded men to serve for models for John the Baptist, and crucified boys to enable them to verify the details of Calvary. Were a similar expedient suggested in 1882 in the schools of the Royal Academy, can we conceive the tempest of public indignation which would gather round the head of the enthusiastic Art Director who deemed the end of producing a noble and religious picture so sacred that all means were lawful to attain it? Or suppose that, for the sanitary interests of the community, it were proposed to stamp out small-pox by administering poison to every person seized with the disease. Is it imaginable that such a scheme would obtain a hearing? Or (to come to closer analogies) let us fancy that in the progress of gastronomy, an experiment, to which we had not become hardened by custom, and no less cruel than the production of *foie gras*, or the old process for making white veal, were suddenly to be introduced from France; or that sportsmen adopted a fashion of merely mangling their game, or using red-hot or poisoned shot. How horrible and startling should we pronounce the novel indulgence of tastes so morbid and pastimes so atrocious!

Yet such indifference to suffering as we have imagined in our hypothetical cases of artists, or sanitary reformers, or books, or sportsmen, would, on the whole, be less monstrous and anomalous than the passion for vivisection among the men of science; and this for two noticeable reasons. In the first place, artists, sportsmen, and *bon-vivants*, know comparatively little of the nature and extent of the suffering caused by lacerations of the living tissues, or the production of morbid conditions, while the physiologists understand the matter to a nicety, and have the most perfect acquaintance with every pain which they cause—nay, whose causation is often the immediate object of their ingenious exertions. As the writer of a most admirable letter, bearing the well-known signature of "Lewis Carol," published in the *Pall Mall*

Gazette, Feb. 12th, expressed it : "What can teach the noble quality of mercy, of sensitiveness to every form of suffering, so powerfully as the knowledge of what suffering really is? Can the man who has once realised by minute study what the nerves are, what the brain is, and what waves of agony the one can convey to the other, go forth and wantonly inflict pain on any sentient being? A little while ago we should have confidently replied, 'He cannot do it.' In the light of modern revelations we must sorrowfully confess he can." Again, in a still more marked way the acts of the vivisectors are anomalous and out of character. It is the boast of the school of science to which they belong that it has exploded the old theory that Man was unique in creation, with a higher origin than the brutes, and a different destiny. They give us to understand that God has "made of one blood" at least all the *Mammalia* "upon earth."* Not merely our corporeal frames, but Thought, Memory, Love, Hate, Hope, Fear, and even some shadowy analogues of Conscience and Religion, have been traced by the great thinker and truly tender-hearted man at the head of this school, throughout the lower realms of life upon this planet ; and, in the eyes of most cultivated and thoughtful persons in these days, the claims of a dog, an elephant, a seal or a chimpanzee, to consideration and compassion, are at least as high as were those of a Negro, a century ago in the eyes of a Jamaica planter. To find a number of men of science—disciples, it is believed, almost without exception, of the doctrine of Evolution — themselves pursuing, and teaching their

* Or rather, perhaps, their views may be more accurately rendered according to the newest Genesis, which tells us that after "the Unknowable moved upon the Cosmos and evolved protoplasm ; by accretion and absorption came the radiata and mollusca ; and mollusca begat articulata, and articulata begat vertebrata. . . . And there followed the generation of the higher vertebrata in the cosmic period when the Unknowable evoluted the bipedal mammalia."

pupils to pursue, trains of physiological investigations involving unutterable suffering to these same "Poor Relations" of our human family, is an appalling phenomenon. That Pope Pius IX. should have refused Lord Odo Russell's request for permission to form in Rome a Society for Prevention of Cruelty to Animals, might, perhaps, be understood on the strange ground his response assumed—viz., that it was "a theological error to suppose that man owes any duty to the animal."* But that the disciples of Darwin should themselves be the teachers and leaders in a new development of most exquisite cruelty to the brutes

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* This expression has been perhaps scarcely rightly understood. His Eminence Cardinal Manning who has taken a lively interest in the subject and most importantly served the cause of anti-vivisection made the following observations as a Member of the Deputation to the Home Secretary from the Society for the Protection of Animals liable to Vivisection:

"I think it greatly to the honour of England that there is a law in the Statute-book punishing cruelty to animals. That law seems to express the great moral principle that people have no right to inflict needless pain. The plea of scientific inquiry and research appears to present the most refined pretext of cruelty in violation of that law. The infliction of needless pain is a moral wrong; and to say that we owe no moral obligations to the lower animals is simply odious and detestable, because a moral obligation is due to their Creator. Then comes the question whether, in the pursuit of science and the treatment of human disease, it is lawful to inflict pain for purposes of discovery. The pain is certain, but the scientific advantage of the vivisection uncertain, so that a limitation ought to be imposed, making it unlawful to inflict pain unless the certainty of some useful result could be attained. Cruelty in the streets comes under the notice of the public, but in lecture-rooms and private establishments any amount of pain may be inflicted on the lower animals without check or limit, and for that reason prompt legislative interference ought to take place. Let me know where cruelty ends and science begins, and when that line has been approximately drawn, let there be some public authority to take cognizance of these things, not only in public hospitals, but in private establishments, the great danger being in the latter, because they are beyond the knowledge and jurisdiction of public opinion and of law."

whom they believe to share our blood, our intelligence, and our affections, is indeed a portent of strange and threatening augury. It involves (as several writers in the daily press have not failed to remark) no less than the adoption of a moral theory of boundless application—namely, that the weak have absolutely no claims at all against the strong, but may be tortured *ad infinitum* even on the chance of discovering something interesting to the lordlier race; or for the purpose of better fixing an impression by the sight of their agonies than could be effected by the verbal description of a lecturer.* "We ask, bewildered," says a writer in the *Daily News*, "how far then will these apologists of vivisection go in approving of the sacrifice of the weak for the sake of the strong? If it be proper to torture a hundred affectionate dogs or intelligent chimpanzees to settle some curious problem about their brains, will they advocate doing the same to a score of Bosjesmen, to the idiots in our asylums, to criminals, to infants, to women?"

Truly this mournful spectacle of the perpetration of cruelty by those who best understand what is cruel, and of the contemptuous disregard of the claims of the brutes by those who have taught us that the brutes are only undeveloped men, is one to fill us with sorrowful forebodings for that future of our race which, from other quarters, seems to promise so fairly. "The simultaneous loss," writes one of the deepest and most observant thinkers of the day, "from the morals of our 'advanced' scientific men of all reverent sentiments towards being *above* them, as towards being *below*, is a curious and instructive phenomenon, highly significant of the process which their natures are undergoing *at both ends*."

Of course events, like the sudden development of physiological cruelties, do not take place without sufficient cause,

* Prof. Rutherford, at a meeting of the British Medical Association at Edinburgh, expressly defended vivisection on this ground.

and are not without some ostensible excuse on the part of those responsible for them. The common passion for science in general and for physiology in particular, and the prevalent materialistic belief that the secrets of Mind can best be explored in Matter, undoubtedly account in no small measure for the vehemence of the new pursuit of original physiological investigations. Then, for the instruction of students in agonizing experiments, other causes may readily be found. Young men at the age of ordinary medical students are, as I began by remarking, filled with curiosity and exceedingly empty of sympathy and pity. An eminent physiologist recently bore testimony to his surprise when a whole class of his pupils trooped out of his lecture-room, on purpose to see the assistant kill a creature which he had considerately intended should be despatched out of sight before dissection. "I remained alone in my chair," he observed, "a sadder and a wiser man." The same keenness of observation, or a memory of their own youthful insensibility, ought to teach all professors of physiology that they are indulging a maleficent tendency which already exists in their pupils' disposition, when they invite mere lads of the Bob Sawyer type to watch their frightful experiments—the more frightful, so much, alas; the more attractive.* And, further still, the proclivity of the time to

* Great indignation was expressed at the above remarks by Sir William Gull and Professor Ferrier before the Royal Commission. Sir William Gull said (Minutes of Evidence, 5502) that "he had never seen anything affording the remotest justification of the phraseology" of the passage and Professor Ferrier (ibid, 3350) thought it "a gross libel upon a class."

The opinions of two no less eminent men and more experienced teachers, Professor Rolleston of Oxford and Dr. Haughton were somewhat different. Professor Rolleston remarked:

"Kingsley speaks of 'the sleeping devil, that is in the heart of every man,' but you may say it is the lower nature which we possess in common with the Carnivora. It is just this, that the sight of a living, bleeding and quivering organism most undoubtedly does act in a particular way upon what Dr.

youthful independence and raw incredulity of the experience of others, adds strength to the desire of students to see with their own eyes the phenomena which their instructors might almost, or quite, as thoroughly convey to them by means of descriptions, and the extraordinarily perfect models and diagrams now available.* There is nothing intrinsically blameworthy in this wish, which is, perhaps, an integral part of the scientific temperament. But its claims to be indulged, when indulgence means for a sensitive creature exquisite torture, and for the student such satisfaction as he may find in watching it, is another question.

Of the argumentative defences of vivisection more must be said. The chief, I think, is a double-barrelled instrument, aimed at our selfishness (under the grandiloquent name of the Benefit of the Human Race) on the one side, and our bad conscience as regards various kinds of cruelty on the other. The latter, or *tu quoque* argument, which was set forth at large in a semi-jocose pamphlet by the assistant of M. Schiff, and published in Florence under the name of "Gli Animali Martiri," refers us with a sneer to the cruelties of the Chase and the Shambles, and asks us whether, in a world where such things are done from the very lowest motives, it is worth while to dispute a few victims for those sacred Altars of

Carpenter calls, the emotiono-motor nature in us. I know that many men are superior to it; but I beg to say that if we are talking of legislation, we are not to legislate for the good, but for the mass who I submit are not always good" (1287).

Dr. HAUGHTON said:

"I would shrink with horror from accustoming large classes of young men to the sight of animals under vivisection. I believe that many of them would become cruel and hardened, and would go away and repeat those experiments recklessly. Science would gain nothing, and the world would have let loose upon it *a set of young devils*" (1888).

* And which *are* so conveyed in other branches of study when their exhibition would cause any serious inconvenience. What chemist thinks it needful to blow up a room to show his pupils the qualities of a detonating powder?

Science which form the furniture of physiological laboratories? The answer to this appeal is not far to seek. One offence does not exculpate another, even if both be morally on the same level. But (as we have just seen) all other cruelties have some excuse in the ignorance or stupidity of those who inflict them, while those of the Physiologist alone bear the treble stigma of being done in the full light of knowledge by singularly able men, and with the calmest forethought and deliberation. And while every other kind of cruelty is falling into disrepute, if not into disuse, this alone is rising almost into the rank of a profession, like a superior sort of butchery. As to the argument that it "does not become people who eat animal flesh to demur to the torture of animals," it would have seemed as if no one with common sense could have employed it, had we not found it repeatedly brought forward by the pro-vivisectors as if it possessed withering force. The cattle we use for food exist on the condition that we shall take their lives when we need them; and in doing so in the ordinary, not unmerciful manner, we save them the far worse miseries of old age and starvation. To *kill* a creature quickly, is one thing. To cause it to suffer torture which shall make its existence a curse, is quite another matter.

Finally, for the tediously reiterated but more reasonable reproach, that the opponents of vivisection make no efforts to put down Field Sports, and count among their numbers many fox-hunters, deer-stalkers, fowlers, and anglers—what shall be answered? My reply is that the parallel between vivisection and Field Sports is about as just and accurate as if a tyrant, accused of racking his prisoners in his secret dungeons, were to turn round and open a discussion on the Lawfulness of War. That creatures who chase, and are chased all their days in fields and waters, should have an arch-enemy and pursuer in man, may be differently estimated as ill or well. But it is almost ludicrous to compare a fox-hunt (for example) with its free chances of escape and its

almost instantaneous termination in the annihilation of the poor fox when captured, with the slow, long-drawn agonies of an affectionate, trustful dog, fastened down limb by limb, and mangled on its torture trough.* An old-world passion, which had in its place and use in another form of society, is running to seed in the modern fashion of field sports, such as *battues* and pigeon matches. A new passion which scarcely had existence twenty years ago, is sprouting above ground and showing its bud in vivisection.

Of course the motive of the sportsman, being usually merely sport, contrasts much to his disadvantage with that which the vivisector requires us to believe is his actuating principle. The latter tells us that it is for the exalted purpose of alleviating the sufferings of mankind, which touch his tender heart to the quick, that he puts himself and his brute victims to the pain of the experiments; whereas the sportsman can only sometimes plead that he kills game for food or to clear the land of noxious creatures; and must usually confess that he hunts, or shoots, or angles for his own pleasure, health, and amusement.

So far as the present writer's opinion is concerned, these latter motives do *not* justify such pursuits when they entail the death of animals neither hurtful to man nor wanted for his food; nor do any field sports seem to harmonize with the highest type of cultivated and humane feeling. But the men who follow them may plead at least the excuse of custom and of partial ignorance. Turn we, on the other hand, to those boasted motives of lofty and far-sighted philanthrophy which are alleged to spur the vivisector to

* Left there sometimes *curarized* (and therefore doubly sensitive), when its wearied tormentors have gone to rest, having provided that their water-engine should continue to supply it with artificial respiration, on the chance that it might linger till morning. (See an instance in the *Archives de Physiologie*, described by the operator, M. Bert. The dog's pneumogastric and sciatic nerves had been dissected and irritated for ten hours.)

his ugly work in his laboratory, where no fern-brakes or heathery hills, no fresh breezes or dancing streams, such as throw enchantment round the pursuits of the sportsman, are present to cast any glamour over the process of torture; and where no chance of escape on the part of the brute, or risk to his own person, may stir his pulse with the manly struggle for victory.

In the first place I may remark that the mental constitution of a man must be somewhat exceptional who is enthusiastically anxious to relieve the sufferings of unseen and perhaps unborn, men and women, but who cares in comparison nothing at all for those agonies which are endured immediately under his eye by creatures who according to his philosophy, are only a step lower in the scale of being. It verges truly on the gigantic and Promethean to talk of such devotion to the interests of *Humanity in the abstract;* and when we behold a cultivated and gifted gentleman selecting freely for his life-work the daily mangling of dogs and cats, we are quite at a loss to qualify the grandeur of his voluntary martyrdom. Perhaps it is not very astonishing that homely people, who do not feel in their breasts the vocation for such sublime devotion, should treat the boast of these motives as just a little partaking of the character of moonshine; and suppose, in a matter-of-fact way, that either the vivisector is a perfectly callous man, whose horrid work never costs him a pang,* or that, if

* I am compelled to testify that in wading through a mass of this Dead Sea literature, I have never been refreshed by a single passing expression of commiseration for the animals, whose signs of agony are recorded merely as interesting features of the experiments; or of regret that the higher scientific objects in view necessitated the prolongation of their tortures. If such feelings exist in the hearts of the operators, I congratulate them on the signal success wherewith they eliminate the slightest trace of them from all their reports. Further, in perusing the books dedicated to the instruction of young students, I have looked equally in vain for any hint of caution, or recommendation to parsimony, in the use of the most excruciating experiments.

he have any lingering feelings of compassion, he puts them aside in favour of sentiments rather more common in the world than such Curtius-like self-sacrifice. As very few of us would purchase immunity from our own diseases at the cost of the torture of a hundred dogs, we may be pardoned for doubting whether the vivisector who cuts them up (as he assures us) for our sakes, is really more interested on our behalf than we are for ourselves.

I believe, then, that we may not unjustifiably fall back on the conclusion that the real motives of vivisectors are of one or other of two less exalted kinds. The better class we may credit with a sincere ardour for Science, and that passion which has been well named the Dilletantism of Discovery. And these belong precisely to that order of *hommes â grands desseins*, who are more than any others liable to overstep the bounds of justice and mercy, and who more than others need the invention of the social conscience to check their recklessness. For a lower class we must, I fear, take the word of a man who worked for four months among them, in a laboratory where from one to three dogs were sacrificed daily: "The idea of the good of humanity was simply out of the question, and would have been laughed at; the great aim being to keep up with or go ahead of our contemporaries in science, even at the price of an incalculable amount of needless torture to animals."*

But the motives which actually influence living vivisectors do not, of course, determine the ethical lawfulness of the practice of vivisection. Our real problem is, whether the highest end to which it *may* conduce, and which they *may* possibly contemplate,—viz., either the direct benefit of mankind by special discovery, or the indirect benefit by the general advancement of science—morally justifies the means whereby it is to be obtained? Does the Good of Man justify the torture of brutes?

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* Dr. Hoggan's letter to the *Morning Post*.

At this point we are commonly called upon to recognize with profound admiration and gratitude the immense value of discoveries said to be due to physiological experiment, and we are challenged to say whether, for example, Harvey's Circulation of the Blood, Bell's Double Function of the Nerves and Simpson's Chloroform, were not secrets worth buying at the price of a considerable amount of animal pain ? The first answer to this " tall talk " is, that not one of these great discoveries appears to have been really made by the aid of vivisection (see Dr. Macaulay's excellent " Plea for Mercy to Animals "); and that of the other reputed results of such experiments, it may be generally affirmed that they resemble the marvels said to have been wrought by the magicians of Pharaoh, who could *bring* the plagues upon Egypt, but remained quite powerless to *cure* them. Into such controversies, however, concerning the utility of vivisection, I, for one, refuse to enter. I am quite ready to admit that benefit has frequently resulted in all ages from a variety of evil deeds—from Rapine, Perjury, Infanticide, and especially from the sacrifice of " hecatombs " of women to spare " the smallest pain " (or self-restraint) of men. But not on account of such utility do I consider robbery and falsehood, the murder of infants or the prostitution of hapless women, right or lawful. Thus I refuse even to entertain the question, " Whether the torture of animals can be justified on the plea of benefit to humanity." And for this simple reason: I do not hold the Jesuit principle that " The End justifies the Means," and I am satisfied that the " Means " of Torture are morally forbidden and unlawful. Bishop Butler's grand axiom that every sentient creature has an indefeasible claim to be spared pain merely because it is sentient, involves the corollary that the claims of the humblest of such creatures must begin *somewhere*, and cannot be wholly and finally abrogated,—as they would be on the hypothesis that we may push our right to take their lives to the ultimate and indefinitely more remote point of

putting them to torture. To make of the existence of a creature such a misfortune and curse as that it should seem better it had never been born,—this is assuredly far beyond the exercise of any prerogative which man can claim for himself, either in virtue of any inherent superiority of his nature, or of any privilege he can conceive to have been granted to him by the Creator.

To affirm, then, as vivisectors are wont to do, that they would freely "sacrifice a hecatomb of dogs to save the smallest pain of a man," is merely an expression of contempt for the rights of beings feebler than themselves, and which are not yet advanced by evolution to the lordly class of "Bimana," or the genus "Homo." What are the moral grounds, we ask, for this astounding new principle of *Race Selfishness?* What is there in Man, either considered only as our fellow-bimanous animal, or as an immortal being whose body is but the garment of his soul, which should make his trifling pain so inexpressibly solemn a matter, and the agony of another animal, no less physically sensitive, insignificant by comparison? Of course we may naturally feel a little more spontaneous sympathy with a suffering man than with a suffering horse. But what is the ethical reason why we should prefer the pain of a thousand horses to that of a single man? Sir Henry Taylor has written noble lines on this matter, going deep into the heart of the question:—

> "Pain, terror, mortal agonies that scare
> Thy heart in man, to brutes thou wilt not spare:
> Are theirs less sad and real? *Pain in man*
> *Bears the high mission of the flail and fan;*
> *In brutes 'tis purely piteous.*"*

There is no sight in all the world, to a thoughtful mind, more suggestive of harrowing reflection, no line of the long "riddle of the painful earth" more confounding to the religious soul,

* Poems; Vol. III. "The Amphitheatre at Pozzuoli."

than the sufferings of creatures who have never sinned, and for whom (according to common belief) there will be no compensation for injustice in another life. While human pain has its plausible explanations and its possible beneficent results, animal pain seems (at least to our dim eyes) sheer unmitigated evil. I am at a loss then to conceive on what principle, deserving the name of moral, we are to speak and act as if such evil counted absolutely for nothing, while the aches and pains of men are to be so highly esteemed, that the most cruel sacrifices must not be spared, if a chance exist of alleviating them. When we remember who are the teachers who talk about the "hecatomb," and what is their view of the relationship of man to the lower animals, we discover (as above remarked) that the only intelligible principle on which they proceed is that very ancient one—*le droit du plus fort.* As the main work of civilization has been the vindication of the rights of the weak, it is not too much, I think, to insist that the practice of vivisection, in which this tyranny of strength culminates, is a retrograde step in the progress of our race, a backwater in the onward flowing stream of justice and mercy, no less portentous than deplorable.

But it is impossible to regard this snbject as if it were a mere abstract ethical problem. The vivisection of dull reptiles, and wild rats and rabbits, wherewith the elder generation of students contented themselves, is not alone in question, nor even that of the heavy beasts in our pastures. By some strange and sinister fatality, the chosen victims at present, are the most intelligent and friendly of our domestic favourites—the cats who purr in love and confidence as they sit beside us on the hearth, the dogs whose faithful hearts glow with an affection for us, truer and fonder than we may easily find in any human breast. To disregard all the beautiful and noble moral qualities which such animals exhibit, and coldly contemplate them as if their quivering frames were mere machines of bone and tissue which it might be

interesting and profitable to explore with forceps and scalpel, is to display heinous indifference to Love and Fidelity themselves, and surely to renounce the claim to be the object of such sentiments to brute or man. Our human race has for thousands of years trained these creatures to serve and trust us, till their natures are all bent towards us in love and confidence. So deeply rooted, indeed, is this faith in man in the case of the dogs that those who have witnessed the scenes in the laboratories of physiologists testify that the brutes can scarcely be made to understand that it is intended to hurt and kill them, but still try after hours of agony, to lick the hands of their tormentor, and plead with him for mercy with their beseeching eyes when their limbs are all fastened down and immovable on the operating table. Will any one contend that it is not the vilest, the most odious treachery to betray and mock such faith of the dumb creature, and torture him to death for our purposes, while he—poor brute, whom we despise!—would die freely to save us from fire or the waves, or perchance expire of grief upon our graves?

Nay, more; are we not altogether on a wrong track in arguing this question on the level to which we have descended? Are not Generosity, Self-sacrifice, the readiness to suffer, the very rudiments of all virtue and all nobility of character? Are we to go back to the condition of savages —nay rather of those

> "Dragons of the prime
> Which tare each other in their slime."

when we had boasted we had ascended to the rank of men, of Christians, of English Gentlemen? Is it a question for a man who aspires to be a brave or worthy, not to speak of a chivalrous or noble person, whether he *may*, within the limits of actual offence, spend his days in putting harmless animals on the rack for the benefit of himself and his kind? And is it our proper Teachers, those who are fit to guide and train

young minds, and direct the tendencies of future generations, who are striving to move us to condone and approve such deeds by cant about the "Glory of Science," and by appeals to our miserable, cowardly fears of disease, and our selfish willingness to save "the smallest pain of a man at the cost of the torture of a hecatomb of brutes?"

To me it appears, I avow, that all this reveals a backsliding in feeling and moral aim almost measureless in the depth of its descent. The whole notion of vivisection, as a legitimate exercise and mode of satisfying human desire of knowledge, seems to rest on a radically false conception of the proper ends of human life, and a no less erroneous idea of our relationship to those humbler tribes of creatures who are our fellow-lodgers in this planet-house of the Almighty. As life is more than meat, so are there better things to live for than Knowledge or escape from Pain; nor is any fact which Science can reveal worth acquiring at the price of selfishness and cruelty. The brutes are not mere toys and puzzles, put here by their Creator and ours that we may freely divert ourselves by breaking them to pieces to see how His wisdom has made them. They are *fellow-creatures* with ourselves—*sinless* fellow creatures, be it remembered, who have broken no Divine law and deserved no punishment. If the day comes (as it is our faith it will, hereafter) when all men should look back upon the deeds done upon earth, and behold them in their true colours, must it not be that in the agonies of remorse and self-abhorrence in the vivisector's soul will be meted out the measure of justice he has dealt to his victims?

Are we to sit down in despair and let this evil grow to full size, and allow first all the medical, and then all the ordinary, schools throughout the country to become Academies of Torture, with class-books abridged from the "Handbook of the Physiological Laboratory!" Shall we have the Royal Institution in Albemarle Street turned into

an exhibition, for the first year or two, of decapitated frogs, and then no doubt, by and by of vivisected rabbits and dogs? Shall our young ladies' boarding schools be entertained (like one now existing near Paris) by the spectacle of dying cats, poisoned to inspire the pupils with the vivid idea of the properties of a drug? Shall we have our hospitals employed in ingeniously proving Professor Ferrier's cerebral investigations and painful experiments on the brain of a dying patient who sought the shelter of that "Good Samaritan" institution?*

To restore the true moral perspective of acts of cruelty, it is needful that those who have looked on them so closely and familiarly as to have become blind to their enormity should learn how they appear to others whose eyes are yet fresh to the horrid spectacle, and who can take in from their remoter standpoint at once the vaunted bribe of relief to their own maladies, and the price which must be paid for it beforehand, in the pangs of innocent creatures. And as the lay conscience was needed to check the persecutions, inquisitions and Autosda-fe which the priesthood of Religion justified on the high plea of the eternal interests of mankind, so now the same lay conscience is needed to stop the scarcely less barbarous cruelties which that other Priesthood — the Scientific—justifies on the far inferior ground of our bodily welfare.

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* See *British Medical Journal*, May 23, 1874, p. 687; also *American Journal of the Medical Sciences*, April 1874, p. 308; also *Révue des Sciences Médicales*, Paris, Julliet, 1874. The woman's name was Mary Rafferty. She was admitted into the Good Samaritan Hospital, January 26, 1874, and there treated as described by Dr. Bartholow.

COMMENTS

MADE BY THE

Society Abolition Vivisection

AT THE

BIRMINGHAM MEDICAL INSTITUTE,

ON

"THE INFLUENCE OF VIVISECTION ON HUMAN SURGERY."

9TH MARCH, 1882.

FIFTH EDITION.

LONDON:
PICKERING & CO., 66 HAYMARKET, S.W.
BIRMINGHAM:
CORNISH BROS., 37 NEW STREET.

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"Publicity is the Soul of Justice."—BENTHAM.

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PRICE THREEPENCE.

MR. PRESIDENT AND GENTLEMEN,

For your courtesy in allowing me to be present at this discussion at your Institute, pray accept my acknowledgments.

As the founder of the Society for the *Abolition* of Vivisection—which obtained the appointment of the Royal Commission, and gave evidence on three days before it—pray permit me to make a few comments on the Address delivered by Mr. Gamgee.*

I have been engaged in personal controversy on this question with Professor Owen, Professor Ferrier, Dr. J. Crichton Browne, Dr. T. Lauder Brunton, Mr. T. Spencer Wells, Mr. Darwin, and other professional and scientific men

* On the 9th February, 1882, at the Birmingham Medical Institute.

of note. These controversies confirm the conclusions arrived at after much time given to the study of the moral right and true philosophy of painfully experimenting upon living animals for scientific purposes—conclusions opposed to the opinions advocated by Mr. Gamgee. According to the conviction of my mind, Vivisection has *not* materially advanced the noble art of surgery. Mr. Gamgee's Address, long as it was, and numerous as were the operations he touched upon, *proved* nothing. Not one demonstration did he, I think, give of surgery being, as he believes, founded upon Vivisection. In direct opposition to his opinion I will cite Sir William Fergusson's: "*In surgery I am not aware of any of these experiments on the lower animals having led to the mitigation of pain, or to improvement as regards surgical details.*"*

Mr. Gamgee not having endeavoured to prove his affirmative, it is not for his opponents to prove a negative. Nevertheless, I will, with your kind permission, comment on a few of the many points he mentioned. He stated, "the brightest ornaments and greatest practitioners" of human surgery "have been amongst the boldest and most indefatigable experimenters on living animals." Here again, Sir William Fergusson's testimony is on the other side. He said, "*No, I am not aware of any great surgeons having been very great experimenters on the lower animals.*" Again, Mr. Gamgee asserts that "experiments on brutes enlightened and nerved to action the practitioners of human surgery." While Sir William said, "*I am not aware that any very expert operator on the lower animals has made himself thereby an expert operator on the human subject.*"

Then as to Mr. Syme's experiments regarding the life and death and growth of bone. Mr. Gamgee is of opinion they have been of powerful assistance to surgery; but Sir William Fergusson said of them, "*I am very doubtful now whether these experiments had any special beneficial influence on the practice of surgery*"; and Sir William went on to state that Mr. Syme's ultimate authority was expressed "*strongly on the other side*"—that is, against the performance of experimental operations on living animals.

* See Sir William Fergusson's evidence before the Royal Commission on Vivisection, in 1875. Pages 48 to 55 of "Minutes of Evidence taken before the Royal Commission on the Practice of Subjecting Live Animals to Experiments for Scientific Purposes." Presented to both Houses of Parliament by command of Her Majesty. London: Printed by Eyre & Spottiswoode. 1876. Price 4*s.* 4*d.*

See also, as Index to the above Minutes, "Digest of Evidence taken before the Royal Commission on the Practice of Subjecting Live Animals to Experiments for Scientific Purposes"; with an Alphabetical List of Witnesses. Presented to both Houses of Parliament by command of Her Majesty. London: Printed by Eyre & Spottiswoode. 1876. Price 6*d.*

"*I have thought over it again and again, and have not been able to come to a conclusion in my own mind that there is any single operation in surgery which has been initiated by the performance of something like it on the lower animals.*" Whose words were these?—those of one of the most experienced and ablest surgeons in Europe, the late Sir William Fergusson.

Much has been made of Jones's experiments as to the kind of ligature for the arteries.* Now, setting aside that the controversy is still going on as to the best kind of ligature for arteries—whatever improvement is claimed for Dr. Jones, as to ligatures, was discovered independently of his experiments upon living animals, or of any such experiments. Dr. Veitch, M.D., in 1803, 1804, and 1805, by observation and reflection, in the Royal Naval Hospital at Plymouth, as to the danger of large flat ligatures then employed for the stoppage of bleeding, was led to try very small ones. The mode of arresting hemorrhage adopted by Veitch was carried into effect before Jones's book was published, and without any knowledge of his experiments. And Jones was indebted to Dr. J. Thomson for the knowledge that, when a ligature is applied to an artery without including the surrounding parts, the internal coat of a vessel is torn through by it. This fact, Thompson told Jones, had been originally noticed by Desault. Dr. Thomson even demonstrated to Jones on a portion of artery taken from the *human subject*, that the internal and middle coats are divided by the ligature. This, and the observations recorded by Petit and Morand, led Jones to make experiments on the arteries of dogs and horses. He made experiments—not to *discover*, but to confirm what had already been discovered. He admits that "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 hemorrhage." "I have," says Dr. Jones, "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 hemorrhage as deduced from my experiments; *but I have been mortified* at finding those records so barren of these important cases, and disappointed at the imperfect detail of the few that are before the public. *I have especially to regret the total want of*

* See the Evidence of Mr. William Sharpey, M.D., LL.D., F.R.S., before the Royal Commission on Vivisection. This gentleman was Professor of Physiology in University College, London, for more than 30 years, and been lauded by noted Vivisectors as "the Friend of Physiologists throughout the World," and "the Mainstay of Physiology in England." For about 20 years he was Secretary to the Royal Society.

observation of the condition of the artery itself, even where the opportunity of ascertaining it had offered. Four or five remarkable cases of limbs torn off without hemorrhage, even from the largest arteries, are recorded, but afford us not the least instruction as to the means by which the hemorrhage was prevented, because *the artery was not examined.*"

Dr. Jones's experiments were unnecessary. He himself refers to the accurate observations made in surgery by Petit, Morand, and Gooch, as being confirmed by his own experiments; and Jones candidly acknowledges that in regard to the temporary means by which the bleeding from a punctured artery is stopped, he has but little to add to what Petit has explained, in his third publication on hemorrhage. Jones said he was "under the highest obligations" to Petit for many observations. I think, gentlemen, many of you are of opinion that such observations in the practice of surgery in hospitals, in accidents, and in war—especially as to amputations—if noted down, as was done by Guthrie, Larrey, Syme, and others, by the Surgeons of Great Britain and Ireland, would effect more, when united and recorded, than any number of vivisectional experiments upon animals practised for any length of time, by a whole tribe of physiologists. Indeed, in this case, that is *proved* by the instance I have brought forward of Dr. Veitch. The advance due to the legitimate practice of the noble and humane art of surgery has been awarded, nevertheless, to the unnecessary and unphilosophical practice of Vivisection. Petit, Morand, Desault, Sharp, and Veitch, as well as other surgeons, are deprived of the honour due to them for their skill and power of observation in the legitimate practice of their profession on mankind. Even in remote ages the ligature was employed for the cure of aneurism and wounded arteries, and in a manner which modern experience has decided to be the best; but the Greek and Arabian physicians' employment of the ligature gave way to less scientific practice in later times, when, I suppose, Vivisection had become more general. How likely fallacy is to arise, by endeavouring to obtain conclusions from experiments on the arteries of animals, which may serve as sure guides in regard to those of man, is illustrated by the following observations:

"The artery of an animal bearing no analogy whatever to that of man, either in susceptibility of disease or in the powers of reparation after injury."

"Aneurism is a disease unknown among inferior animals; it cannot by any ingenuity of contrivance be artifically produced."

A still more unfortunate selection from the experiments of the Vivisector is that made as to Travers.

Those "experiments," by Benjamin Travers, on dogs and horses, gave birth to one of the most erroneous methods of treatment ever practised in surgery. In his book, detailing his "experiments" on dogs and horses (that is, stabbing, cutting them open, dividing their intestines, stitching up their wounds, &c.), he also published his directions for the treatment of strangulated hernia. *That treatment killed the patients.* Very many lives have been destroyed by the treatment he advised, viz., giving purgatives to prevent or cure peritonitis. That fatal error has since been corrected, and in the "Treatise on Hernia," recently written by John Birkett, Member of Council and Fellow of the Royal College of Surgeons, Consulting Surgeon to Guy's Hospital, &c., he says—"The introduction of every kind of purgative medicine into the stomach must be scrupulously avoided in all cases of strangulated bowel." I think, gentlemen, you will, one and all, coincide with that opinion.

Mr. Gamgee said, "the epoch-maker was John Hunter." So he was. Nevertheless, Mr. Gamgee went on to say, "I shall not *stop to inquire* if Hunter's operation for aneurism was the direct result of any *one* experiment." Why did he not *stop to inquire?* Surely the object of Mr. Gamgee's address is to prove that human surgery is *founded* upon experiments upon living animals. Now, in the opinion of some very able surgeons, Hunter was not indebted to Vivisection for his great improvement in the operation for aneurism. Fergusson, Macilwain, and Paget have expressed that judgment; and our controversy with Professor Owen has, we believe, utterly refuted the assertions of the great comparative anatomist. Of this you can judge, gentlemen, for yourselves, as we have forwarded a copy of the pamphlet to every member of this Institute, and published the whole correspondence in *The Times* and *The Athenæum.* *

The positive averment of Professor Owen that anastomosis was discovered by John Hunter—and as to the *thigh* of a *man*, on the *horn* of a *deer*—is an assertion that may be characterised as ASTOUNDING! A statement made by Mr. Gamgee that "amputation at the hip joint," was *proved* safe by Vivi-

* Correspondence with Professor Richard Owen, C.B., &c., on "John Hunter and Aneurism." Third Edition. London: Pickering & Co., 66 Haymarket, S.W. 1882. Price 3*d*.

section is not so startling, but yet leaves room for some surprise. "Amputation at the hip joint," we are told, "is the *direct* outcome of Vivisection." We think the history of the art of surgery—the surgical records of the wars of Napoleon—and of the hospitals of England and France utter forth another and opposite voice. Could the vivisection of every animal, from a mouse to an elephant, have enabled surgeons to add to the true triumphs of their great art—to achieve such a formidable and tremendous operation as amputation at the hip joint?

The foregoing comments on Mr. Gamgee's address were read to the meeting, and the President then interposed, saying that the time allowed to us had expired. We accordingly were obliged to conclude, which was done as follows:—"In conclusion, Gentlemen, I wish to state, that the placarding the hoardings of London and Edinburgh with illustrations, and descending to *The Police News*, the Society had no hand in. Its object is, and ever has been, to *argue* the question. If you can overcome us, we must sit down under it,—but we shall fight out the question to the last. I thank you again, Gentlemen."

We regret to be obliged to say that we cannot compliment the Midland Medical Institute on the amount of fair play which it accorded to us. After having read our manuscript comments for about two minutes, the President interposed, saying it was against the rules to read a paper. That I might *speak*, but not read. In reply, we observed that Mr. Gamgee had read *his* address. Some discussion then took place, and it was eventually ruled that we might read, but were not to exceed a quarter of an hour. Yet Mr. Gamgee's address on the 9th February occupied more than an hour in delivery. He was also allowed time to reply to us on the 9th March. Other members addressed the Meeting in his support,—and attacked us, but we were not permitted to attempt to refute their assertions.

Nor can we compliment some of the members of the British Medical Association on their courtesy to a guest present to take part in a scientific discussion in their own Institute. When we mentioned the late Sir William Fergusson as "one of the most experienced and ablest surgeons in Europe," several of the medical gentlemen laughed aloud with derision at his name. We were astonished, and expressed

our surprise, adding, "Do you not consider him a great surgeon?" "No, no, no," was the loud response. "Well," we replied, "He was Sergeant-Surgeon to the Queen." Again they derisively laughed. "Well, gentlemen," we rejoined, "we think there is not one of you present who would not be happy to take that appointment if he could obtain it." And that we consider a fair retort."

No more was said on the above point, but does it not indicate how barren was the land in facts and argument, when, instead of controverting the reasoning of an opponent and refuting his statements, recourse was had by those present to depreciating the ability of a man who had been their own chief as President of the Royal College of Surgeons, and of the British Medical Association; who was the inventor of numerous ingenious improvements in surgical instruments; the author of "A System of Practical Surgery," which has gone through many editions, held in the highest estimation by the profession; who had won many high honours in testimony of his distinguished merit; and was undoubtedly regarded as one of the very ablest operative surgeons in the world? The name of Fergusson is treated with contumely and received with scorn when it is quoted as adverse to Vivisection. This is the old story,—"No case,—abuse Plaintiff's attorney." But what testimony does Sir James Paget bear to the name of Fergusson? In the Hunterian Oration delivered by him in the presence of the Prince of Wales at the Royal College of Surgeons of England, he uttered this tribute to his memory: "Fergusson is gone: the greatest master of the art, the greatest practical surgeon of our time; and men can no longer watch the eyes that were so keen, or try to imitate the hands that were so strong, and yet so sensitive and swift and light; or wonder at the ready and clear knowledge, the prompt invention, the perfect calmness in the midst of danger."

No evidence or arguments emanated from the assembled members of the medical profession, refuting our statements as to the growth of bone; the ligature of arteries; the use of purgatives advocated by Travers in strangulated hernia; the discovery of anastomosis; and Hunter's improvement in the operation for aneurism. In place of this, one gentleman rose and warmly defended and justified the practice of Vivisection by asserting that "Nature herself is cruel." He read the following portion of a letter by us which appeared in *The British Medical Journal* of the 14th January last:

"We do not take your correspondent's view of nature; she is a beautiful, a kind mother. In the myriads of wild

animal life—and to which man is almost but a unit—there is, to all appearance, a preponderance of health, happiness, and enjoyment. The earth is wronged by man's oppression. Man does not learn injustice, tyranny, and cruelty from his mother." The gentleman then went on to say, "Mr. Jesse considers nature a kind mother, but is she not 'Red in tooth and claw?' Is not the ocean one vast slaughter-house below?" &c., &c. Had we been permitted to reply to our opponents, we should have asked, "What kind of *argument* is this? If nature is a *cruel* mother, is that a reason why *we* should be cruel? If the world is miserable, ought we therefore to render it more miserable? Are we to go from bad to worse, and render our beautiful habitation a hell?" "One cat," continued the same speaker, "causes more pain by torturing its prey than is inflicted by all the physiologists in Europe; or, at least by all in *this* country." Even if this were so, our answer to it is, that human beings are not cats. And we are inclined to believe that it is only well-fed and domestic cats who do play with their victims ere devouring them. *The light of nature demonstrates a Deity*, said Francis Bacon. According to the above deduction it demonstrates a demon, "so great a difference is there betwixt the spirit of man and the spirit of the universe."

Another medical man rose, and with rather an irritated air, said, "Mr. Jesse has disowned the placards and *The Police News*, but can he disown this statement which appeared in *The Times* during 1875?" He then produced a paper and read as follows from the manifesto of the Society for the Abolition of Vivisection:

"The hideous cruelty of dissecting living animals, or inflicting on them, though innocent and defenceless, multitudinous deaths of excruciating and protracted agony, has secretly grown up in this nation—a nation which for ages past has been nobly distinguished by the courageous and unsanguinary character of its people.

"This moral ulcer has spread widely, and (whether it be or not a dreadful form of insanity) become dangerous and demoralising to Society—a blot on Civilisation, a stigma on Christianity. The public has little idea what the horrors of Vivisection are; its crimes in studied, ingenious, refined, and appalling torture, in wantonness, uselessness, and wickedness, cannot be surpassed in the annals of the world. It therefore calls for extirpation by the Legislature, cruelty being not only the worst of vices in itself, but the most retributive to man-

kind, more especially when perpetrated by the refined and educated."

Of this we promptly avowed ourselves the author, and endorsed it in full, by crying "Hear, hear," at its conclusion.

The gentleman then proceeded to pay us, with considerable emphasis and apparently with some fretfulness, this compliment: "Mr. Jesse never has a controversy with anyone but he finishes off with—'SNUFFED OUT!'" These words were accompanied by uplifted arm and a forcibly petulant snap of finger and thumb, so expressive and so inimitably performed as to defy description.

Several other gentlemen addressed the meeting, the last of them being Mr. Gamgee. He receded from the position he had taken up at the Birmingham Medical Institute on the 9th February last, and in his pamphlet, since published, entitled "The Influence of Vivisection on Human Surgery."* On that occasion and in that pamphlet Mr. Gamgee asserted as follows: "*Without such experiments* (on living animals) *Scientific Surgery could not have been founded.*" That position, namely, that scientific surgery is founded upon Vivisection, Mr. Gamgee discovered was an untenable one; and had time been allowed us we would have endeavoured to demonstrate it in still more instances. For one example, the opposition of Liston to the operation for the removal of ovarian tumours was mentioned by Mr. Gamgee in a manner to credit experiments upon animals with the great improvements which have been made in ovariotomy. A more baseless and visionary belief than this could hardly be. That it is so we appeal to our controversy with Mr. T. Spencer Wells†—a controversy which surgeons have pronounced convincing and conclusive on the question, leaving no opening whatever for further debate. The first great apostle of ovariotomy in this country; the original hero of the operation; who did more than all other operators to place it on a sure foundation; who has

* "Vivisection and Human Surgery." By Sampson Gamgee, F.R.S.E. Second Edition, 8vo., 6*d.*, by post seven stamps. London: J. & A. Churchill, New Burlington Street, W.

"This eloquent address is very valuable on account of the precise and trustworthy manner in which it marshals important facts from the records of Surgery."—*Edinburgh Medical Journal.*

"A trenchant, vigorous, and irrefutable defence of the experimental method."—*Medical News.*

"Mr. Gamgee's address is full of surgical learning, finely touched in a literary respect . . . . We cordially recommend it."—*The Lancet.*

"We would strongly recommend our readers to purchase this essay, and study it themselves."—*British Medical Journal.* (The *First* Edition, Price One Shilling, is superior to the Second Edition. It contains *a Hymn!* which was afterwards omitted.)

† Correspondence with T. Spencer Wells, F.R.C.S., Member of Council of the Royal College of Surgeons, Surgeon to the Queen's Household, &c., on "Ovariotomy." Third Edition. London: Pickering & Co., 66 Haymarket, S.W. 1882. Price 1*s.*

operated in 400 cases and saved about 300 lives, Dr. Charles Clay, personally told us that he had never practised nor yet countenanced Vivisection. How then was "Scientific Surgery" "founded" upon Vivisection here? There is no such foundation. The discoveries credited to Vivisection are phantoms, which melt into air, fade away, and disappear before the eye of the candid and enquiring mind.

"The Alchemists wax old and die in hopes; the Magicians perform nothing that is permanent and profitable."—BACON.

Mr. Gamgee appeared to consider himself injured by the notice we had taken of his pamphlet. We understood him to complain to the meeting that he had sent us the first, or one of the first copies he received from the printer. We thanked him at the time, looked upon it as a gage of battle cast into the lists of controversy on a burning public question—and lifted it accordingly. Our desire is to fight the battle out, and with the courtesy due to the great profession which has had Sydenham, Cheselden, Percivall Pott, Abernethy, and Bell among its members.

"Great is Truth, and stronger than all things. It endureth, and is always strong: it liveth and conquereth for ever more."

GEORGE R. JESSE,
Honorary Secretary and Treasurer,
Society Abolition Vivisection.

Henbury, near Macclesfield, Cheshire,
March, 1882.

P.S.—Mr. Sampson Gamgee has a dismal admission in his pamphlet, an admission which indicates that disaster, not success, is the outcome of the practice of Vivisection. Mr. Gamgee (at page 25, First Edition) makes this lugubrious confession: "Every precaution is taken, every wise counsel sought, all possible skill employed, and yet *how often within a few hours the Undertaker follows the Surgeon.*" Here is an honest confession indeed! and from the writer who has deliberately asserted that "Scientific Surgery" is founded on Vivisection! (*See* page 3, First Edition.)

Mr. Gamgee quotes Dante, and applies a line from "The Inferno," to the kindly surgeon, which quotation appears to us appropriate only to Magendie and his disciples, who ruthlessly inflict agony upon the helpless and unoffending.

"I wept not: so all Stone I felt within."

Doubtless,—for they can too truly say,—"Tear-falling Pity dwells not in this eye."

The apologist for Vivisection then discharges a farrago and jumble of sentences on "Compromise," "Zeal," "Talleyrand," "The Old Puritan," "Index Expurgatorius," "Ecclesiastical Titles Act," "Love of Animals," "Alfort," "Smugglers," "Body-snatchers," "Advanced Thinkers," "Magdalenes," "Murderers," "Fools," "The Talmud," "Soldiers of the Cross," "The Bull 'Ineffabilis Deus,'" "The Apocalypse"!

Even the foregoing is surpassed by the finale,—by the grand climax at the concluding page of the *First* Edition. In the *First* Edition, (and that only, for Mr. Gamgee to our regret has eliminated his choicest eloquence from the Second!) is an attempt to paint a pathetic picture of a Vivisector,—"*a sorrowing Experimenter.*" "If," says the author of "The Influence of Vivisection on Human Surgery," "If a downcast man of Science, beaten in the struggle, sick, poor, and lonely, were very doubting, cold, unsympathetic, and inarticulate under the paralysing influence of his pent-up feelings, the abstract assertion of dogmatic truth might not find a ready listener. Then let a spiritual adviser arrange for bringing to the poor man's ear, from a Music Temple, a telephone singing in the accents of a Mozart, an Auber, or a Rossini, John Henry Newman's hymn—

Lead, kindly Light, amid the encircling gloom,
Lead Thou me on;
The night is dark, and I am far from home,
Lead Thou me on.
Keep Thou my feet; I do not ask to see
The distant scene, one step enough for me.

I was not ever thus, nor prayed that Thou
Shouldst lead me on;
I loved to choose and see my path, but now
Lead Thou me on.
I loved the garish day, and, spite of fears,
Pride ruled my will: remember not past years.

So long Thy power has blest me, sure it still
Will lead me on;
O'er moor and fen, o'er crag and torrent, till
The night is gone.
And with the morn those angel faces smile,
Which I have loved long since, and lost awhile.

"When told that such are some of the good and glorious means by which God reveals Himself through His servants, the priests, the musicians, and the men of science, the poor experimenter's nerve tension might relax, his tears flow, and the way be opened for those higher mysteries which bring that peace which passeth all understanding. He could then perceive how and why in religion, as in science, ALL is vouchsafed to no one man.

"Each must

. . . . not ask to see
The distant scene, one step enough for me.

"In the scientific laboratory and in the cloister, on the Rocky Mountains and the Jungfrau, on Olivet and Calvary, '*step by step*' is the marching order."

What is the nature of the mind which can unite the names of Olivet and Calvary with the Vivisector's laboratory? Which can compare the agony of Christ in the Garden, His sufferings on the Cross and Martyrdom, to those beings who have no pity and perpetrate the most cruel barbarities on the innocent creatures of their God?

Religion is being adapted to "science,"—so called. *A Hymn* is appropriated for the *Vivisectors*. John Henry Newman's Hymn is the Vivisector's Hymn! At the end of last December it was announced in London Medical Journals that a Sermon would be delivered on Sunday, the 8th of January, at St. Barnabas' Church, Stockwell, "in favour of Legitimate Vivisection," with "an Anthem suitable for the occasion." Seats were to be "set apart for the Medical Faculty." We then said in the Public Journals:

The English people may wonder what ANTHEM can be found "suitable" to the cruelties of Vivisection. In the *British Medical Journal* of 4th December, 1880, are details of certain "experiments" performed upon dogs by first poisoning them with strychnia—then playing melodies to them on flutes and violins, and noting down their heart beats at the carotid artery. What "Anthem" is "suitable" for the tortures of the damned inflicted on the faithful and affectionate dog?—or, for those, who "—— mangle the living dog that has loved them and fawned at their knee"?—*Tennyson.*

The Sermon and the Anthem were abandoned in London; but in Birmingham a *Hymn* "suitable to the occasion" *has* been found. In some respects the words *are* "suitable."

The night is dark, and I am far from home.

the Vivisector cries. True, most true, his intellectual night *is* "dark,"—and he *is very* "*far* from home."

. . . . I do not ask to see
The distant scene, one step enough for me.

It *is* "*distant*"—distant as it was a hundred thousand years ago,—and that "*step*," the knowledge of the mystery of life, the Vivisector will never achieve. The verses are desecrated. With what feelings will their author, John Henry Newman, regard the use now made of the utterances of his soul? utterances poured forth when his mind was passing through the shades of experience.

G. R. J.

APPENDIX.

The Medical Press and Circular, April 19, 1882.

That indefatigable enthusiast on anti-vivisection, Mr. G. R. Jesse, having considered it incumbent on him to attempt the annihilation of Mr. Gamgee's unassailable arguments in favour of Vivisection, has, it seems, ventured, with a courage that must command a certain admiration, to beard the lion in his den—in other words, to address a meeting of the Society before which Mr. Gamgee delivered his now famous oration.

. . . . . .

Taking them all in all, these anti's are a curious class of cranks, worthy of careful study on the part of some of our experts in mental diseases during the brief intervals in which they have no medico-legal case in hand.

The Lancet, April 15, 1882.

The fanaticism on the subject of anti-vivisection is showing signs of abatement, or rather there are indications that it has had its day with sensible people, and will soon be appraised at its real value.* When this happy day arrives we shall have to confess ourselves greatly indebted to the anti-vivisectionists themselves. The feebleness of their contributions to the great controversy is striking, even to those who had a sentimental leaning to their side. Those who wish to see a contrast will do well to read the argument for Vivisection from a surgical point of view by Mr. Sampson Gamgee, delivered before the Birmingham Medical Institute, and the reply to it by Mr. George R. Jesse, Hon. Secretary of the Society for the Abolition of Vivisection. The disproportion is either painful or amusing, according to the point of view taken. We have already noticed Mr. Gamgee's address, full of surgical learning, finely touched in a literary respect, gathering testimony from all countries and a great cloud of witnesses, and *withal breathing a spirit of tenderness to the whole animal creation.* Mr. Jesse, on the other hand, relies on himself and on Sir Wm. Fergusson, and is prepared against the whole medical profession to assume the responsibility of abolishing Vivisection.

* The Editor of *The Lancet* stated in a leading article on the 19th November, 1881, that "the part of physiological research which depends on Vivisection is being gradually stamped out."

Ten thousand cattle may be slaughtered in London in a week for the use of human beings. But a dog or a guinea-pig must not be submitted to experiment, however tenderly, even if it would advance the art of surgery, both human and veterinary.

Dr. J. J. GARTH WILKINSON, F.R.G.S.,

76, WIMPOLE STREET, CAVENDISH SQUARE,
LONDON, W. *April* 11, 1882.

DEAR MR. JESSE,

I thank you for your pamphlet on Surgery and Vivisection received yesterday. It is full of your quality; mastership of facts all round, and destructive deductions from them of the positions of the Vivisectors, &c.

Yours,
J. J. GARTH WILKINSON.

"THE BEGINNING OF THE END."

The succeeding advertisement, publishing abroad a fact so important and encouraging to the cause, and so striking a proof of the success of our crusade, was inserted in the *Morning Post* of September 13, 14, and 15, 1881; *Nature* of September 15; the *Standard* of September 15, 16, and 17; the *Athenæum* of September 17; the *Times* of September 20, 21, and 23; the *Saturday Review* of September 24; and *Galignani's Messenger* of September 19, 20, and 21.

VIVISECTION.—THE BEGINNING OF THE END. —The HOME SECRETARY has REFUSED CERTIFICATES to Professor Fraser, Dr. Lauder Brunton, and Professor Lister, for carrying on investigations which they declare to be of "the highest value to Medical Science."

GEORGE R. JESSE,
Hon. Sec. &c., Society for the Abolition of Vivisection.

Henbury, near Macclesfield, Cheshire,
10*th September*, 1881.

Advertisement which appeared in the *Daily Chronicle*, *The Standard*, and *The Times*, of January 11, 12, and 13, 1882.

From *The British Medical Journal*, 7th January, 1882.

"The authorities of MASON'S COLLEGE have refused at present to accede to the request of the Professor of Physiology, Dr. Haycraft, to take the necessary steps for obtaining a Licence for the College for Physiological Investigation under the EXPERIMENTS ON ANIMALS ACT."

Friends to the cause will be gratified to learn by the above extract that MASON'S COLLEGE, BIRMINGHAM, has followed the example of TRINITY COLLEGE, DUBLIN, in regard to the PROHIBITION OF ALL EXPERIMENTS ON LIVING ANIMALS.

May these examples be imitated by London, Cambridge, Edinburgh, and all Universities in the Kingdom, and a practice which is not merely unnecessary, but misleading, be soon universally regarded as one of the aberrations of the human mind from the path of reason and rectitude.

SOCIETY ABOLITION VIVISECTION.

GEORGE R. JESSE,
Honorary Secretary and Treasurer.

Henbury, near Macclesfield, Cheshire,
9th January, 1882.

"*The lame in the path outstrip the swift who wander from it.*"—BACON.

Spottiswoode & Co., Printers, New-street Square, London.

With the writer's compliments

NOTE

ON MR. LAWSON TAIT'S PAPER, "THE USELESSNESS OF VIVISECTION UPON ANIMALS AS A METHOD OF SCIENTIFIC RESEARCH."

BY

E. C. COPAS, B.A. (Oxon.), F.C.S.

Read before the Birmingham Philosophical Society, June 23, 1883, and Reprinted, by permission, from the Society's Transactions, with Additions.

BIRMINGHAM: HERALD PRESS, UNION STREET.

1883.

1516. e. 1 (4)

TO THE MEMBERS OF THE BIRMINGHAM PHILOSOPHICAL SOCIETY.

As I was unable, in my Note as it appears in the Society's Proceedings, to make any allusion to the recently issued edition of Mr. Tait's Paper, in which he has referred to my criticism, I have, with the consent of the Council, issued a reprint of my communication, to which I have made an addition containing a discussion of Mr. Tait's reply. Of course it will be understood that this addition has in no way appeared before the Society.

E. C. COPAS.

PORTLAND HOUSE, SOHO HILL,

BIRMINGHAM, 23RD OCTOBER, 1883.

XV.—*Note on Mr. Lawson Tait's Paper "On the Uselessness of Vivisection upon Animals as a Method of Research," published in the Proceedings of the Society (Vol. III., Part I., 1881-1882.*

By E. C. Copas, B.A. (Oxon), F.C.S.

[Read before the Society, June 23rd, 1883.]

I feel that I ought perhaps to explain how it is that I have taken upon myself to reopen this subject, after so long a time has elapsed since Mr. Tait's paper was brought before the Society. A short time ago I read a paper on Vivisection before another Society, and before doing so I looked through Mr. Tait's communication, as it appears in our Proceedings.

I at first felt inclined to wish that a paper in reply to Mr. Tait's might be read, in order that it might appear in our Proceedings; but this would have involved the infliction upon the Society of a discussion which would have been, to a very great extent, a repetition of what they had recently heard; and I felt that, even with the aid of all their philosophy, the members could have scarcely endured it.

But I think I may perhaps ask the indulgence of the Society for a few minutes while I draw attention to what seems to me a most important point in the paper, and which, as far as I remember, altogether escaped comment at the time of the discussion. It occurs at page 125, Vol. III., Part I. of our Proceedings, among Mr. Tait's statements as to Harvey's discovery of the circulation of the blood. It will be remembered that no part of Mr. Tait's paper was more severely criticised or more completely disposed of than that in which he referred to

Harvey's great discovery. His statements are simply assertions against Harvey's claim to the discovery, without any evidence and without quoting any authorities that can be examined, except upon one point, and it is to the authorities on that point that I wish particularly to refer. Mr. Tait says, "That he made any solid contribution to the facts of the case by vivisection is conclusively disproved, and this was practically admitted before the Commission by such good authorities as Dr. Acland and Dr. Lauder-Brunton." I was considerably surprised at this statement, as it was so entirely opposed to what I believed to be their opinions. I have, therefore, most carefully searched through their evidence in the Report of the Royal Commission. In Dr. Acland's evidence I can find only two references to Harvey, and only one to the discovery of the circulation of the blood. In his answer to Question 986, he states that the Royal Society's Transactions are full of experiments on living animals, at least as far back as Harvey, but in this answer he makes no allusion to the circulation. In answering Question 990, he describes the great impression made upon him when a youth by observing a heart, which he had removed from a freshly-caught trout, palpitating in his hand for at least half-an-hour. In the next question (991) he is asked:—"The circulation of the blood was discovered in exactly the way in which you have described your observing the heart of the fish, was it not?" It will be noticed that the *question* does not suggest Harvey as the discoverer. This is Dr. Acland's reply:—"It is not quite certain what argumentation led Harvey to that, whether it was the observation of the living structure or the contemplation of the dead structure." I cannot see that Dr. Acland makes any such admission as that attributed to him by Mr. Tait. My examination of Dr. Lauder-Brunton's evidence led to a still more startling result; for I was quite unable to find that he had made the slightest allusion either to Harvey or the discovery of the circulation of the blood. At my request a well-known member of this Society has also carefully examined the evidence and entirely confirms my impression that it is quite impossible

to find any ground for Mr. Tait's statement. I have here a copy of the Report, and I shall be glad if any member that can discover such evidence, will call my attention to it.

I think it may be well for the Society to hear what Dr. Acland and Dr. Brunton themselves say on this point, and I will therefore read from letters I have recently received from them.

Dr. Acland writes :—" In reply to your question as I understand it, whether I hold it to be 'disproved' that Harvey 'made any solid contribution to the facts of the circulation of the blood by vivisection,' I would reply I do not hold that opinion, and I never did; nor is there, as you rightly state, any expression of mine to be found which is intended to convey it. The answer to which you refer in my evidence (991) refers to Harvey's own statement that he derived his conclusions *both* from vivisections and from dissections on dead animals. No one who reads his writings would question this double origin of his data."

Dr. Lauder-Brunton says :—" There is absolutely no foundation whatever for the statement that I at any time admitted that Harvey made no solid contribution to the facts of the circulation by vivisection. I consider that Harvey knew how he had made the discovery, and he says himself that it was by vivisection. Without vivisection the idea of the circulation would have remained a mere idea, possibly true, possibly false, accepted by some, disbelieved by others. By vivisection Harvey removed the circulation from the realm of mere ideas, and gained it a place amongst established facts."

I find it impossible to resist the conclusion that Mr. Tait has fallen into a most unfortunate mistake, and I hope I shall be pardoned for taking up the time of the Society by calling attention to it.

I have thought it necessary to do so because Mr. Tait attributes opinions to scientific men which they emphatically repudiate. But I consider it of more importance because it is an erroneous statement against Harvey's claim to the discovery

of the circulation of the blood—a claim which has been generally admitted by the great authorities * on physiology, both English and foreign, since Harvey's time.

* There have been some (chiefly Italians) who have disputed Harvey's claim to this discovery, just as there have been some to dispute the claims of most great discoverers; but, with the exception of comparatively very few, those who can be considered competent to give an opinion on this question have, after an examination of the evidence, decided distinctly in Harvey's favour. The Royal Commission (1875-76), after hearing the evidence of the authorities on physiology of the United Kingdom, referring to Vivisection, say in their report:—"Harvey appears to have been almost entirely indebted to it for the ever-memorable discovery of the circulation of the blood." The report goes on to give a quotation from a translation of Harvey's immortal work, "De Motu Cordis et Sanguinis in Animalibus." I think it may be more interesting to give Harvey's own words, and I therefore quote from the original:—'Tandem majori indies, et disquisitione, et diligentia usus, multa frequenter, et varia animalia viva introspiciendo multisque observationibus collatis, rem attigisse, et ex hoc labyrintho me extricatum evasisse, simulque motum, et usum cordis et arteriarum quem desiderabam compertum habere me existimabam."

On this point, too, I will give the opinion of the late Professor Rolleston, who is universally admitted to have been one of the most learned scientific men of our time. No one who knew him would think him capable of giving a prejudiced opinion on the matter. In his Harveian oration of 1873 he says:—"Servetus had speculated, but rightly, as to the lesser circulation, so had Cæsalpinus, and on Harvey's own showing Realdus Columbus. . . . . What was left for Harvey to discover was nothing less than the *circulation* itself. His predecessors had but impinged, and that by guesswork, upon different segments of the circle, and then gone off at a tangent into outer darkness, whilst he worked and proved and demonstrated round its entire periphery. His demonstrations and direct proofs were all new, and his indirect arguments nearly all new. . . . . With regard, that is, to the circulation as a whole; with regard to the actual demonstration and exhibition of it as opposed to mere guessing about it; with regard to all, or nearly all, sound reasoning as to any large proportion of it, Harvey might have said with Lucretius:

'Avia Pieridum peragro loca nullius ante
Trita solo,'

or in the words of a poet of another country and a later age:

'We were the first that ever burst
Into that silent sea.'

I do not wish to assert that Harvey was wholly independent of the works of his predecessors. He himself would, as his repeated reference to them shows, have been the very last man to make any such claim for himself."

Further, I look upon it as important from another point of view. It will be remembered that Mr. Tait's argument is that the evidence that certain discoveries were made by vivisection will not bear the test of historical criticism. Now I know of no more necessary qualification for one who engages in historical research than accuracy, and I think we may take this as a fair test of the value of Mr. Tait's historical criticism on this question. Surely no one can complain that it is too severe a test. It is not a matter of very remote date—the Report was only published in 1876—and it is not a matter, respecting which there are many conflicting statements, from which the truth has to be sifted. It is simply a question of whether Drs. Acland and Lauder-Brunton made a certain statement before the Royal

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The great foreign physiologists are equally decided in their opinion. Milne-Edwards says: "La découverte de la circulation du sang date du xvii[e] siecle, et la gloire en appartient à Guillaume Harvey" (Leçons sur la Physiologie, tom. iii., page 1).

In J. Müller's well-known work (Baly's translation) I read:—"The circulation of the blood was discovered in the higher animals by Harvey in 1619."

Albert von Haller says:—"Postquam adeo et per varii generis experimenta, necessitas admittendi circuitus sanguinei ostensa est. . . Harveio inventi gloria debetur, homini modestissimo" (tom. i., p. 240).

Professor Virchow, of Berlin, in an address to the International Medical Congress held in London in 1881, said:—"The first great and distinctive example of successful vivisection which the history of medicine knows is that of William Harvey. The foundation of the doctrine of the circulation, which, in the main was experimental, has radically changed the whole direction of the thoughts of physicians. Had we this one example alone it would be sufficient to prove brilliantly the utility, yea, the indispensability of vivisection."

I could also quote Descartes, Kuss (Strasburg), Flint (New York), and many others. It is quite easy for anyone to satisfy himself that the most eminent foreign authorities have decided in Harvey's favour. The whole question is discussed in Dr. Willis's work, "William Harvey: a History of the Discovery of the Circulation of the Blood" (1878).

I cannot see how Mr. Tait's hypothesis, "that it is only our insular pride which has claimed for him (Harvey) the merit of the discovery," can be made to explain the fact that the great French, German, and American authorities have endorsed the opinion of the English physiologists, even if it be admitted that this hypothesis is a possible, although an extremely improbable, explanation of the practical unanimity of these latter on the subject.

Commission, and yet Mr. Tait's historical research on such a simple point will not bear examination. The Society will remember, too, that this is by no means the only mistake into which Mr. Tait has unfortunately fallen on this question of Vivisection. Now on such an important subject as this we may be quite sure that he has used every effort to be accurate in the statements he has published, and yet we find that he is so carried away by his most laudable sympathy with the lower animals that, in spite of his utmost care, the wish is so much the father to the thought, that he finds evidence in favour of his views where no one else can detect it; and utterly fails to see evidence on the other side, which is recognised and deemed entirely convincing by very nearly all the medical profession and all great living scientific authorities; just as a man affected with colour-blindness may see red in a part of the spectrum where nobody else can perceive it, and may completely fail to notice the green which is plain to almost all other observers.

Now I yield to no one in abhorrence of inflicting unjustifiable pain upon the lower animals, and I am daily giving evidence of my views upon this point. I have every reason to think that the Act now in force most effectually protects the lower animals. The report lately published shows that only a few experiments on animals (chiefly frogs) have taken place in England during the past year. I wish it could be shown that we could wisely dispense with them altogether; if this could be done I should be quite willing to sign a petition for their abolition, and so, I believe, from what I know of their opinions, would the high scientific authorities who now advocate them. But the evidence in their favour seems to me incontestable. Nothing perhaps has done more to confirm me in this opinion than my opportunities from time to time during the last few years of seeing my college friend Professor Thomas carrying out his investigation on the fluke disease in sheep, from which, as is well known, many farmers have lost the whole of their flocks. As members probably know, he was completely

successful in tracing the origin of the disease, and was able to point out to the farmers how their flocks could be best guarded against it. I need not dilate upon the importance of this research, and it is beyond dispute that without experiments on the lower animals it could not have been carried out.

And if I weigh the authorities on this question what do I find? In the one scale are Mr. Tait and a very few other medical men, and in the other all the rest of the medical profession and great scientific authorities, including such men as Charles Darwin, Professor Owen, Professor Huxley, Sir James Paget, and Sir William Gull. Now I cannot think that these latter men are less humane than Mr. Tait. I cannot persuade myself that they are less acquainted with the evidence, or less able to estimate it than he is, for they are our most learned authorities on the subject. I cannot, therefore, find any lingering doubt in my mind as to which scale kicks the beam.*

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* Mr. Tait has recently issued another edition of his paper, for a copy of which I am indebted to him, and in it he makes reference to my Note. He states that a criticism by "Philanthropos" and this Note are "the only criticisms of my paper. . . . which I have seen."

I think perhaps it would have been well to have made some reference here to the criticisms which the members of the Philosophical Society made at the time the paper was read. I am, of course, assuming that these criticisms were seen as much as mine were. I will just draw attention to the fact that during the two evenings' discussion of the paper all the members of the Society who spoke were opposed to Mr. Tait, and that the only speakers who supported him were two lady visitors, who came to the meeting specially to aid him.

Referring to my suggestion that he was mistaken as to the admission which he had attributed to Dr. Acland and Dr. Lauder Brunton, Mr. Tait says:—"It will be observed that I had cautiously used the words "practically admitted," though I am of opinion that I might have used a very much stronger expression and have said "actually admitted." Certainly this is the case with Dr. Acland.

The question of the discovery of the circulation of the blood has really nothing to do with the question of vivisection.

In the first place it is denied by many (and I made the denial years before I ever took part in the vivisection controversy) that the merit of this discovery is to be credited to Harvey. There are, at least, two other claimants for it, and as the elements of the controversy seem in every

instance to be more in support of national *prestige* than of individual merit, the discussion has become somewhat farcical. But even if Harvey be admitted as the successful claimant there remain the questions—Did he make any substantial contribution to his discovery by means of vivisection? and, if he did, does it follow that such a course of experimentation as the one he pursued (without any definite purpose at all as it seems to me) need be followed now? To answer the second question first, and dismiss it, everyone must say, No, emphatically. The circulation of the blood could neither be discovered nor demonstrated now by anything but a dead body and a syringe."

But I find that the great authorities say *most emphatically*, Yes. It is quite easy to convince oneself of this by referring to the Report of the Royal Commission. I will quote one of many witnesses who give an answer to this effect.

Charles Darwin says:—"(4668) The first thing that I would say is, that I am fully convinced that physiology can progress only by the aid of experiments on living animals. I cannot think of any one step which has been made in physiology without that aid. No doubt many surmises with regard to the circulation of the blood could be formed from the position of the valves in the veins, and so forth, but certainty such as is required for the progress of any science can be arrived at in the case of physiology only by means of experiments on living animals."

Mr. Tait goes on:—"To answer the second (*sic*) question, and to reply alike to Mr. Copas and 'Philanthropos,' it is necessary to go carefully over the greater part of the evidence published by the Royal Commission. The first four witnesses examined, Sir Thomas Watson, Sir George Burrows, Sir James Paget, and Professor Sharpey, went largely into the discoveries concerning the circulation alleged to have been made by vivisection research, and (with the exception of Sir James Paget) they all boldly claimed Harvey's alleged discovery as of the chiefest value. They gave their evidence on the 5th and 6th of July, 1875, and it may be summed up in a sentence taken from the answer to question 394, by Professor Sharpey: 'Then Harvey, as everyone knows, discovered the true motion of the blood. That was by vivisection.' A week elapsed, and by the 12th of July it was discovered that this was a statement which might easily be disputed, and could not be substantiated, and therefore Dr. Acland, on the same point (991), in answer to Lord Winmarleigh, said: 'It is not quite certain what argumentation led Harvey to that (the discovery of the circulation of the blood), whether it was the observation of the living structure, or the contemplation of the dead structure.' Now, I submit that on the part of Dr. Acland this answer is not only a practical but an actual admission such as I have claimed."

I find myself quite unable to adopt Mr. Tait's view. In the first place Mr. Tait's assumption,—that it was because it was found that Dr. Sharpey's answer "might easily be disputed and could not be substantiated" that Dr Acland gave a different answer,—is opposed to the evidence before us; for in the letter which I have quoted above Dr.

Acland says:—"The answer to which you refer in my evidence (991) refers to Harvey's own statement that he derived his conclusions *both* from vivisections and from dissections on dead animals. No one who reads his writings would question the double origin of his data." And if we look a little more closely into Sir T. Watson and Professor Sharpey's evidence we find they are substantially in accord with Professor Acland. Dr. Sharpey says:—"and established in fact the doctrine of the circulation of the blood as now received. That was done *largely* by vivisection." (The italics are mine.)

Sir T. Watson also:—"if the circulation of the blood had never been discovered (and its discovery was *aided and perfected* by innumerable vivisections, in the strict meaning of that word, made by Harvey himself)" (The italics are mine.)

But putting Mr. Tait's assumption and Dr. Acland's letter (which Mr. Tait had heard read when he wrote the paragraph containing this assumption) both aside, let us look at the statement as it stands in the Blue Book. Using symbols for the sake of brevity, the statement is:—"It is not quite certain whether it was A or B." Mr. Tait draws the conclusion that this is an admission that it was not A. I cannot tell by what method he arrives at this conclusion; certainly it is not an allowable method of reasoning. And Mr. Tait virtually assumes that it was B. Now let us consider for a moment whither this process of deduction would lead us. Suppose a trial of two men, A and B, for murder in which the evidence shows that it is not quite certain whether A or B committed the crime. According to this method of reasoning the jury would be justified in acquitting A and convicting B. I think we must be happy in the thought that justice is administered on somewhat less haphazard principles.

Mr. Tait goes on—"Concerning Dr. Lauder-Brunton, the evidence is less direct, but to my mind it is quite conclusive, and I adhere to my statement that he made 'a practical admission.' Dr. Lauder-Brunton's name had been introduced specially in the evidence concerning the circulation. After Dr. Acland's evidence not a single contravention of his correction of Dr. Sharpey occurs. Therefore I take it that all the subsequent witnesses agree with him. I might have taken any one or all of the twenty-four names, but for a special reason I picked out that of Dr. Brunton."

In reply I beg to say—(1) that I have been quite unable to find any mention of Dr. Brunton's name in the evidence as to the discovery of the circulation of the blood. Sir James Paget and Mr. A de Noé Walker refer to Dr. Brunton's experiments with nitrate of amyl as a remedy for angina pectoris, and Mr. J. Colam mentions his experiments as to the action of certain poisons on the heart; but I can find nothing that bears on the point in question.

(2.) On turning to the evidence of the witnesses, subsequent to Dr. Acland, I find on 26th October Professor Turner says:—"in order that the discovery of the circulation of the blood should be made, it was necessary that the living body should be examined." He then

quotes from Harvey's writings to show that he made his discovery by the aid of vivisections on various animals.

Sir W. Fergusson on 12th July, Dr. McKendrick on 28th October, Charles Darwin on 3rd November, and Sir William Gull on 6th November, also attributed Harvey's discovery to vivisection. If the evidence of these witnesses, *which is in agreement with that of Dr. Sharpey* on this point, be held, according to Mr. Tait, not to contain a single contravention of Dr. Acland's correction of Dr. Sharpey, it is difficult to see how Dr. Acland can be said to differ from Dr. Sharpey.

(3.) Mr. Tait's argument appears to me to fail from another point of view. Of course his argument would hold not only for Dr. Acland, but for all the other witnesses; and not only on this particular point but on all points of the evidence; so that each witness must be held to agree with all the evidence that has been taken, previous to his appearance before the Commission, unless he makes a distinct disclaimer. Therefore all the witnesses who opposed vivisection would endorse all the answers of those in favour of the practice, unless they gave evidence adverse to those answers; and, of course, those in favour of vivisection would be similarly responsible for the evidence of those against it. Now everyone knows that the witnesses only answer questions put to them, and that they do not necessarily know what evidence has already been given. Further, although some witnesses, as the Presidents of the Medical Colleges, and the Professors of the Universities, were asked to give evidence on the general question as to whether vivisection was defensible, and as to the best method of legislation, etc., others were asked for information only on special points on which they were considered to be authorities. The questions put to Dr. Brunton bore especial reference to experiments in pharmacology. He was asked what experiments of this particular nature had been productive of beneficial results, and he gave several; but no question was put to him as to the results which had flowed from other kinds of experiments. There is no allusion whatever to Harvey, or the discovery of the circulation of the blood, either in the questions put to him, or in his evidence. I cannot see the slightest reason for holding Dr. Brunton responsible for any evidence beyond that contained in his own answers.

I have most carefully considered Mr. Tait's reply, but I have completely failed to find any basis for the statement that Drs. Acland and Lauder-Brunton had made the admission attributed to them.

Further on, Mr. Tait says:—"We cannot blame Harvey for not being in advance of his time, and for being incapable of a reasoning power which has been granted to those who follow him in the nineteenth century." . . . He then gives some quotations from Harvey's works to show that vivisectional experiments are useless and misleading, and also that Harvey did not always see the logical conclusions of his facts.

But to what does this all amount? Would it not be possible to show that many other of our most eminent predecessors who have seen so much that others failed to see, have not *always* seen the logical conclusions of the facts before them? But would it therefore be wise to

suggest that they were incapable of a reasoning power granted to those of the nineteenth century? There is ample evidence in Harvey's works that he was by no means deficient in reasoning power, and I think that in drawing logical deductions some of us of the present day might learn something from him. I should like to know how many of our most eminent living physiologists would be prepared to say that if they had been placed in Harvey's position, with the same instruments and the same amount of instruction from contemporaries and predecessors that he had, they would have been likely to have accomplished more than he did, even granting them their nineteenth century reasoning power?

And if we allow that vivisections have sometimes been misleading and useless, how can that outweigh the long list of important discoveries, which, by the almost unanimous voice of those who can be considered at all competent to give an opinion on the subject, are attributed to these experiments?

It is much to be regretted that Mr. Tait cannot appreciate the greatness of Harvey, who has won the admiration of the scientific world. It is quite painful to me to see Mr. Tait writing in the most contemptuous terms of this great man.

It seems to me lamentable that Mr. Tait, in his desire to do justice to the lower animals, should have unintentionally done a grave injustice to one of the noblest of the higher ones.

Although Mr. Tait has failed to convince me, yet I think his reply will be considered very interesting; for he here gives us a demonstration of the methods by which he has succeeded in constructing for himself an almost unique position upon this question. In building operations of this kind it is laid down by the highest authorities that the only legitimate materials are *accurate premises*, and that you must use *logic* as your instrument of manipulation. Now it is clear from Mr. Tait's own showing that he has neglected these two essentials to good construction, although he is not conscious of this neglect even when it is pointed out to him. It cannot, therefore, be surprising that many of those who have examined his position have come to the conclusion that it lacks the elements of stability.

I think it is much to be desired that those taking part in this agitation for the total abolition of experiments upon the lower animals would reconsider their position. It is very gratifying to me to find that there is a probability of Mr. Tait's looking further into this question. In a letter quite recently published in *Nature* he says:— "The pamphlet . . . . does not deal with physiology, but with surgical questions. My efforts have not been directed so much against vivisection as against the . . . . statements which have been made concerning advances in surgery, alleged to be due to its practice. . . . . If physiologists can make out a case for themselves, I for one am prepared to give it the utmost attention . . . . . ." It seems to me greatly to be deplored that so much energy should be wasted in attacking a position which is defended by substantially all the authorities on the subject, when there are practically indefensible positions in our

midst that could be carried at once if those opposed to cruelty would only unite their forces. Is it not possible that pigeon shooting with all its horrors might now be a thing of the past if those agitating for the abolition of Vivisection had concentrated their strength for an advance against Hurlingham?

I cannot help thinking that if they could be induced to make a careful study of reliable authorities on this subject, they would be disposed to relinquish this agitation. I certainly cannot understand anyone who has carefully examined the Blue Book published by the Royal Commission (1875-6), going on with it. It seems to me difficult to wish for a better Commission. Viscount Cardwell, Baron Winmarleigh, Mr. W. E. Forster, Sir John Karslake, Professor Huxley, Mr. Erichsen, and Mr. Hutton appear to me to be men likely to get at the evidence and to be able to weigh it when it had been given. What was their opinion after hearing the evidence, not only of the authorities on physiology, but also of those who had been most prominent in the agitation for the abolition of the experiments? In their Report they say:—"Our conclusion, therefore, is, that it is impossible altogether to prevent the practice of making experiments upon living animals for the attainment of knowledge applicable to the mitigation of human suffering or the prolongation of human life; that the attempt to do so could only be followed by the evasion of the law or the flight of medical and physiological students from the United Kingdom to foreign schools and laboratories, and would, therefore, certainly result in no change favourable to the animals; that absolute prevention, even if it were possible, would not be reasonable; that the greatest mitigations of human suffering have been in part derived from such experiments; that by the use of anæsthetics in humane and skilful hands the pain which would otherwise be inflicted may, in the great majority of cases, be altogether prevented, and in the remaining cases greatly mitigated; that the infliction of severe and protracted agony is in any case to be avoided; that the abuse of the practice by inhuman or unskilful persons,—in short, the infliction upon animals of any unnecessary pain,—is justly abhorrent to the moral sense of your Majesty's subjects generally, not least so of the most distinguished physiologists and the most eminent surgeons and physicians; and that the support of these eminent persons, as well as of the general public, may be confidently expected for any reasonable measures intended to prevent abuse. . . . . We believe that by such a measure as we have now proposed the progress of medical knowledge may be made compatible with the just requirements of humanity."

October 23, 1883. E. C. C.

THE

USELESSNESS OF VIVISECTION

AS A METHOD OF SCIENTIFIC RESEARCH,

BY

LAWSON TAIT, F.R.C.S., ETC.

NEW EDITION, WITH NOTES.

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THE USELESSNESS OF VIVISECTION

AS A METHOD OF SCIENTIFIC RESEARCH.

I NEED not go into the general history of Vivisection, for it hardly bears upon the question to which I desire to limit myself; but I think it advisable to formulate a few preliminary conclusions before I come to my immediate subject, in order that I may clear the way for discussion, and show at once the grounds upon which I stand, for I find myself in a position adverse to the view adopted by the great majority of my professional brethren.

I dismiss at once the employment of experiments on living animals for the purpose of mere instruction, as absolutely unnecessary, and to be put an end to by legislation without any kind of reserve whatever. In my own education I went through the most complete course of instruction in the University of Edinburgh without ever witnessing a single experiment on a living animal. It has been my duty as a teacher to keep myself closely conversant with the progress of physiology until within the last four years, and up to that date I remained perfectly ignorant of any necessity for vivisection as a means of instructing pupils, and I can find no reason whatever for its introduction into English schools, save a desire for imitating what has been witnessed on the Continent by some of our most recent additions to physiological teaching. In Trinity College, Dublin, the practice has been wholly prevented, and on a recent visit to that

institution I could not find, after much careful inquiry, the slightest reason to believe that any detriment was being inflicted upon the teaching or upon those taught.

The position of vivisection as a method of scientific research stands alone amongst the infinite variety of roads for the discovery of Nature's secrets as being open to strong *prima facie* objection. No one can urge the slightest ground of objection against the astronomer, the chemist, the electrician, or the geologist in their ways of working; and the great commendation of all other workers is the comparative certainty of their results. But for the physiologist, working upon a living animal, there are the two strong objections: that he is violating a strong and widespread public sentiment, and that he tabulates results of the most uncertain and often quite contradictory kind.

I do not propose to deal with the sentimental side of the question at all, though no one can doubt it is a very strong element in the case as maintained by public opinion. I shall deal simply with the inquiry: Has this method of scientific research—Vivisection—contributed so much to the relief of suffering or to the advance of human knowledge as to justify its continuance in spite of the manifest objections to it? My own answer I shall try to give in the following pages, merely premising that an answer to justify vivisection must be clear and decisive, must be free from doubt of any kind, and, above all, it must not assume the protection of a "privileged mystery." This is a question, I maintain, which can be discussed by an educated layman just as well, perhaps better, than by a physician or a surgeon or a professional physiologist. It is a question chiefly of historical criticism, and we must have a conclusive answer concerning each advance which is quoted as an instance, how much of it has been due to vivisectional experiment and how much to other sources, and this amount must be clearly and accurately ascertained. It will not do, as has been the case in

many of the arguments, to draw such a picture as that of an amputation in the seventeenth century and one performed last year, and say that the change is due to vivisection. We might just as well point to the prisons of the Inquisition and then to one of our present convict establishments and claim all the credit of the change for the fact that our judges wear wigs. The real questions are: What advances in detail are due to vivisection? Could these advances have been made without vivisection? If vivisection *was* necessary for elementary and primitive research is it any longer necessary, seeing that we have such splendid and rapidly-developing methods in hundreds of other directions? Have we made complete and exhaustive use of all other available methods, not open to objection? And finally, are the advances based upon vivisection of animals capable of being adapted conclusively for mankind, for whose benefit they are professedly made?

It must be perfectly clear that to answer all these questions specific instances must be given, and that they must be analysed historically with great care. This has already been done in many instances, and I am bound to say, in every case known to me, to the utter disestablishment of the claims of vivisection.

Take the case of the alleged discovery of the circulation of the blood by Harvey, and it can be clearly shown that quite as much as Harvey knew was known before his time, and that it is only our insular pride which has claimed for him the merit of the discovery. That he made any solid contribution to the facts of the case by vivisection is conclusively disproved, and this was practically admitted before the Commission by such good authorities as Dr. Acland and Dr. Lauder-Brunton.* The cir-

* My attention has been drawn to a book which has just been published under the title of "Physiological Cruelty: or, Fact *v.* Fancy, by *Philanthropos.*" This book purports to be a scientific contribution to the much-discussed question of Vivisection, but its author departs from the usual custom of writers on

culation was not proved till Malpighi used the microscope, and though in that observation he used a vivisectional experiment his proceeding was wholly unnecessary, for he could have better and more easily have used the web of the frog's foot than its lung. It is, moreover, perfectly clear, that were it incumbent on any one to prove the circulation of the blood now as a new theme, it could not be done by any vivisectional process, but could at once be satisfactorily established by a dead body and an injecting syringe. In fact, I think I might almost say that the systemic circulation remained incompletely proved until the examination of injected tissues by the microscope had been made.

But supposing we grant, for the sake of argument, that such an important discovery had been made by vivisection, and by it alone, there still remains the all-important question, Is it necessary to use such mediæval methods for modern research? No one can doubt that the rude methods employed in Charles II.'s reign for obtaining evidence—the rack, the boot, the thumb-screw, and the burning match—were occasionally the means of accomplishing the ends of justice, but need we go back to them

scientific subjects in that his book is anonymous. There is, I think, sufficient evidence to reveal to any reader who is fairly well acquainted with the literature of this question the identity of the author, and if it would serve any good purpose I think I could name him. He is either ashamed of his book and his cause, or he has a cowardly nature, and for the present I shall let him accept which of the two stigmas he prefers.

He attacks me on several points, and I think it worth while to reply to him, the more that one of the points of his criticism has been enlarged upon in a paper by Mr. E. P. Copas, read before the Birmingham Philosophical Society on June 21st, 1883. This paper, together with the remarks of "Philanthropos," form the only adverse criticisms of my paper on the "Uselessness of Vivisection as a Method of Scientific Research" which I have seen, though it is now fifteen months since the paper was published. Mr. Copas did not make up his mind to criticise me for more than a year after the publication of the pamphlet, and after this long period of incubation his criticism was of the most trumpery kind. It was to the effect that I had misrepresented Dr. Acland of Oxford, and Dr. T. Lauder-Brunton, of London, when I said (p. 125)—"That Harvey made any solid contribution to the facts of the case (the discovery of the circulation of the blood) by Vivisection is conclusively disproved, and *this was practically*

now? The very necessity for ending them brought into use fresh and far less fallible methods, and I am inclined to make the claim for physiology, pathology, and the practice of medicine and surgery that the very retention of this cruel method of research is hindering real progress, that if it were uttery stopped the result would certainly be the search for, and the finding of, far better and more certain means of discovery. To urge its continuance on the ground that it was useful in the seventeenth century is just as reasonable as to ask the astronomer to go back to the cumbrous tackle by which Huyghens first worked his lenses.

If the method of obtaining evidence by torture was occasionally successful, there can be little doubt that as a rule it failed and led the inquirers astray. So I say it has been with vivisection as a method of research, it has constantly led those who have employed it into altogether erroneous conclusions, and the records teem with instances in which not only have animals been fruitlessly sacrificed, but human lives have been added to the list of victims by reason of its false light.

Those who have recently advocated vivisection seem to have

admitted before the Commission by such good authorities as Dr. Acland and Dr. Lauder-Brunton."

I have italicised the words objected to by Mr. Copas, who is a Bachelor of Arts of Oxford, and a Schoolmaster. His charge was, and it was supported by letters he had received from Dr. Acland and Dr. Lauder-Brunton, that they had made no such admission and entertained no such view.

It will be observed that I had cautiously used the words "practically admitted," though I am of opinion that I might have used a very much stronger expression and have said "actually admitted." Certainly this is the case with Dr. Acland.

The question of the discovery of the circulation of the blood has really nothing to do with the question of vivisection.

In the first place it is denied by many (and I made the denial years before I ever took part in the vivisection controversy) that the merit of this discovery is to be credited to Harvey. There are at least two other claimants for it, and as the elements of the controversy seem in every instance to be more in support of national *prestige* than of individual merit, the discussion has become somewhat farcical. But even if Harvey be admitted as the successful claimant there remain the questions,—Did he make any substantial contribution to his discovery by means of vivisection? and, if he did, does it follow that such a course of

forgotten or to have ignored this most fatal objection, and as a rule they have indulged in a line of argument which is little more than assertion. For the purpose of this paper I have gone carefully over a large mass of literature upon the subject, and find that the bulk of it is altogether beyond criticism, because it does not deal with fact. Thus in a recent address on the subject by Professor Humphry, of Cambridge, there is a long list of advances in medicine and surgery, every one of which is attributed to vivisection solely because some experiments were mixed up in the history of each instance; but not an effort was made to show that the advances were due to vivisection. The proper method for the discussion of this subject is to take up a number of special instances and to subject them to careful criticism, chiefly by historical evidence, and as soon as the advocates of vivisection do this successfully I am prepared to grant their case. But hitherto they have failed.

Serial literature during the last few months has been singularly fertile in articles on the question of vivisection, and one commanding attention as an editorial is to be found in *Nature* of March 9th.

experimentation as the one he pursued (without any definite purpose at all as it seems to me) need be followed now? To answer the second question first, and dismiss it, every one must say, No, emphatically. The circulation of the blood could neither be discovered nor demonstrated now by anything but a dead body and a syringe. In fact the only approach to satisfactory and scientific experimentation which I find in Harvey's work on the circulation is given in a letter to Paul Slegel, where he describes an experiment with a clyster bag on a dead body. If he had pursued this line of research, and left living animals alone, he would have anticipated many very important discoveries which have been made since, and would have left no doubt whatever as to his claim to the establishment of the scheme of the circulation.

To answer the second question, and to reply alike to Mr. Copas and "Philanthropos," it is necessary to go carefully over the greater part of the evidence published by the Royal Commission. The first four witnesses examined, Sir Thomas Watson, Sir George Burrows, Sir James Paget, and Professor Sharpey, went largely into the discoveries concerning the circulation alleged to have been made by vivisection research, and (with the exception of Sir James Paget) they all boldly claimed Harvey's alleged discovery as of the chiefest

There the *a priori* argument for vivisection is put in the familiar illustration that "it would be more reasonable to hope to make out the machinery of a watch by looking at it, than to hope to understand the mechanism of a living animal by mere contemplation." Unfortunately there is a fault in the analogy, and it may be far more truly put in the converse, that it would be wholly impossible to repair the damaged movements of a watch by experimenting with an upright pendulum clock. There is a perfectly parallel dissimilarity between the functions and the diseases of animals and those of man.

In the same article is a quotation from the article of Sir William Gull, to the effect that the experiments of Bernard, in baking living dogs to death in an oven, have opened the way to our understanding the pathology of fever. In zymotic diseases the elevated temperature is not a cause of the disease but its consequence, and the answer to the argument is that not a single contribution of any kind has yet been made to the cure of scarlet fever. Its course cannot be shortened by one hour. Medicine is powerless for the cure of zymotics, whilst hygiene is all-powerful in their prevention, and the medicine of the future lies

value. They gave their evidence on the 5th and 6th of July, 1875, and it may be summed up in a sentence taken from the answer to question 394, by Professor Sharpey: "Then Harvey, as every one knows, discovered the true motion of the blood. That was by vivisection." A week elapsed, and by the 12th of July it was discovered that this was a statement which might easily be disputed, and could not be substantiated, and therefore Dr. Acland on the same point (991), in answer to Lord Winmarleigh, said, "It is not quite certain what argumentation led Harvey to that (the discovery of the circulation of the blood), whether it was the observation of the living structure or the contemplation of the dead structure." Now I submit that on the part of Dr. Acland this answer is not only a practical but an actual admission such as I have claimed.

In the work of "Philanthropos," at p. 146, that critic says: "With great care, therefore, I turn to the proceedings of the Royal Commission, to seek for the admission of the highly respectable authorities named (Dr. Acland and Dr. Lauder-Brunton). And I find that neither of these gentlemen says one word on the subject, or even indirectly refers to it." All I can say is, that in the copy of the Report of the Royal Commission now before me, at page 47, there is the admission of Dr. Acland which I have quoted.

wholly in this direction. Drugs are impotent, but sanitary laws can and will banish all these diseases when they are completely understood and fulfilled.

The article continues that "between 1864 and 1867 seven new drugs were added to the Pharmacopœia, of which at least the two most useful, carbolic acid and physostigma, are due to vivisection." Upon the question of new drugs I can speak only with great reserve, for such a wholesome scepticism concerning drugs has been introduced by the medical schism of homœopathy, that I look upon all new drugs with great suspicion. Sir William Gull himself says he has not much belief in drugs. I fear most new drugs do more harm than good; some of them, such as chloral, most certainly have done so. I cannot learn that physostigma is of any practical service, and I have shown in my published writings that carbolic acid has done far more harm than good. Perhaps it would have been better if we had never heard of it. The question of the investigation of the actions of drugs by experiments on animals I have to confess is a very difficult one, because after we have found out what they do in one animal we find that in another the results are wholly

Concerning Dr. Lauder-Brunton, the evidence is less direct, but to my mind it is quite conclusive, and I adhere to my statement that he made "a practical admission." Dr. Lauder-Brunton's name had been introduced specially in the evidence concerning the circulation. After Dr. Acland's evidence not a single contravention of his correction of Dr. Sharpey occurs. Therefore I take it that all the subsequent witnesses agree with him. I might have taken any one or all of twenty-four names, but for a special reason I picked out that of Dr. Brunton. In a letter from Dr. Brunton, read by Mr. Copas in the papers I have quoted (the exact words of which I cannot give as it has not yet been published), Dr. Brunton intimated that he had never held the views concerning Harvey for which I claim his admission; but in his work "Pharmacology and Therapeutics" (Macmillan, 1880), pp. 120-121, are the following words: "Harvey *found valves in the veins*, and *therefore* thought it probable that the blood circulated, instead of oscillating backwards and forwards. * * * If the blood circulates, pressure on the veins must make them empty above and full below the point of constriction. * * * The experiment was tried,—the expected result followed, and thus the thoughts of Harvey passed from the region of speculation

different, and the process of investigation has to be repeated in man. Not only so, but in human individuals the actions of drugs in very many cases vary so much, that each fresh patient may form really a new research. Pharmacy forms, therefore, at least, a very shaky argument for vivisection.

Finally, the Editor of *Nature* deals with the argument of proportion, which is stated to the effect that the proportion of pain inflicted by vivisection bears but small ratio to the pain relieved by the discoveries effected in that way. But if this question be examined historically, as it must be for the sake of justness, it will be found that the argument is all the other way. To take the case of Ferrier's experiments, if the history of the point be examined, even from the period of Saucerotte till now, the number of experiments recorded is perfectly awful, and we can easily imagine that many more were performed and not put on record. Concerning the arteries this is still more true; and it is, to say the least of it, very doubtful if any permanent good has been done by them. What we do really know about both of these matters with certainty has been derived from the post-mortem examinations of our failures in human subjects, and not from vivisection experiments.

into that of ascertained fact." Possibly this is quite enough for Dr. Brunton. Harvey worked and wrote early in the seventeenth century, a time when the rights of human beings had but scant consideration, a time when men's thoughts were rough and rude on almost all questions, and when science was in its infancy. We cannot blame Harvey for not being in advance of his time, and for being incapable of a reasoning power which has been granted to those who follow him in the nineteenth century. But we must blame those of our own time who remain wilfully blind to the logical conclusions of the facts given by Harvey himself. When I say, as my general argument, that vivisectional experiments are useless and misleading, I merely say what Harvey himself proves in numerous instances. He did not see the logical conclusions of his facts, but he states the facts clearly enough. The following illustrations I take from his works almost at random. (Willis's Edition, Old Sydenham Society.)

P. 16. Speaking of Galen's most meaningless and utterly useless experiment of dividing the trachea of a living dog, forcibly distending the lungs with a pair of bellows, and then tying the trachea securely, he says: "Who, indeed, doubts

In a work published within the last few weeks by a distinguished member of this Society, Dr. George Gore, entitled "The Scientific Basis of National Progress," and at p. 80, will be found the following sentence: "The Antivivisection movement is but one of the phases of the ever-existing conflict between the advancing and retarding sections of mankind."

I do not know whether I belong to the antivivisection movement or not, but I certainly cannot rank myself with those who attribute to vivisection the merit which distinctly belongs to other causes. So far I am an antivivisectionist most thoroughly.

Similarly I do not know whether or not I am to be regarded as belonging to the "retarding section of mankind." If I am so classed I fear I shall be in company as strange to me as I shall be objectionable to it. But my relief is great as I read further in Dr. Gore's book and see upon what grounds he has built his conclusion. I have never heard that Dr. Gore has conducted any vivisection research himself, and therefore I assumed that he took his argument from some other source. He was kind enough to give me his reference for the following statement, which he makes at page 81: "Ferrier's comparatively

that, did he inflate the lungs of a subject in the dissecting-room, he would instantly see the air making its way by this route, were there actually any passage for it?" In this case Harvey was clearly of opinion that vivisection was useless.

P. 50. "This truth, indeed, presents itself obviously before us when we consider what happens in the dissection of living animals; the great artery need not be divided, but a very small branch only (as Galen even proves in regard to man), to have the whole of the blood in the body, as well that of the veins as of the arteries, drained away in the course of no long time—some half hour or less. Butchers are well aware of the fact, and can bear witness to it; for, cutting the throat of an ox and so dividing the vessels of the neck, in less than a quarter of an hour they have all the vessels bloodless. The same thing also occasionally occurs with great rapidity in performing amputations and removing tumours in the human subject." Here vivisection experiment was therefore wholly unnecessary, but Harvey did not see it.

P. 126. "The internal jugular vein of a live fallow deer having been exposed (many of the nobility and his most serene Majesty the King, my master, being

recent vivisection experiments have already enabled medical men to treat more successfully those formidable diseases, epilepsy and abscess of the brain." His authority is an anonymous article in the *British Medical Journal* of November 19th, 1881, in which a series of cases is given in support of this extraordinary statement. The purport of it is that the experiments of Ferrier have led to greater certainty in applying the trephine for the removal of depressed fractures, &c., which had produced serious symptoms, or for the relief of matter in cerebral abscesses.

I do not propose now to go into this very wide and difficult question, because I shall have a fuller opportunity on another occasion. I shall only say that Ferrier's first experiments were published in 1873, and that previous to that time a large number of cases are on record where the seat of injury was ascertained with perfect accuracy by simpler and less misleading methods—in one case by myself in 1868. The *a priori* difficulties in the application of Ferrier's conclusions are enormous and, as it seems to me, insuperable; and, after a most careful historical consideration of the illustration quoted by Dr. Gore, my verdict is most decidedly that of *not proven.*

present), was divided, but a few drops of blood were observed to escape from the lower orifice rising up from under the clavicle; whilst from the superior orifice of the vein, and coming down from the head, a round torrent of blood gushed forth. You may observe the same fact any day in practising phlebotomy: if with a finger you compress the vein a little below the orifice, the blood is immediately arrested; but, the pressure being removed, forthwith the flow returns as before."

The experiment alluded to here forms, so far as I can discover, the basis of Hannay's well-known picture, which affords the British public the proof of Harvey's claim as the discoverer of the circulation, very much as popular theology is drawn from Milton's "Paradise Lost." But Harvey admits in the last sentence that the experiment was wholly unnecessary.

Again, if we go into Harvey's mistakes we find further proof of the misleading nature of vivisectional research. Thus at p. 103 he says: "I can therefore boldly affirm, that there is neither any anastomosis of the venae portae with the cava, of the arteries with the veins," &c., &c. (concerning the liver); and we find his experiments giving conclusions wholly at variance with the facts displayed by the injecting syringe.

The application of the trephine for the treatment of epilepsy is of course absolutely limited to cases where the disease is the result of injury to the skull. No one has ever dreamed of applying it to other cases. I find that the first operation of this kind was performed in 1705, by Guillaume Mauquest de la Motte with partial success, and it was repeated with complete success by Mr. Birch of St. Thomas's Hospital, 1804. Between 1804 and 1865 there are 50 cases on record (collected by Dr. James Russell, *British Medical Journal*, 1865), and of these 44 recovered, the results being satisfactory in 39 of them. This paper of Dr. Russell's was published years before any of Ferrier's experiments were undertaken, and the results of trephining for epilepsy published since are not so good as those published by Dr. Russell. The most recent contribution to the subject is a paper by Mr. J. F. West, who asks the question "Are our indications in any given case, either of paralysis or epilepsy, sufficiently precise and well-marked to warrant us in recommending the use of the trefine at a particular point of the skull? and he answers it thus: "It will be a long time before it is definitely settled, but such cases as those alluded to give en-

What I said therefore originally on this subject I adhere to; nay, I say more, for I am convinced that the vague and, in many instances, unintelligible descriptions and conclusions of Harvey would have been simpler, clearer, and more correct if he had never made any vivisectional experiments at all.

"Philanthropos" criticises my views about Listerism and carbolic acid. He evidently knows nothing about surgery, or he would know that my views are now, with one exception, adopted by all those who practice my department of the art. The same thing is true about the use of catgut. Let him look at the preparation of a case of subclavian aneurism now in the Hunterian Museum, added to the collection last year, and study the question a little more.

Further, I think I may say without egotism, that my surgical practice is now one of the largest of its kind in the world. I have entirely dispensed with the use of carbolic acid or any other such substance, greatly to the beneft of my patients. Medical visitors come from all parts of the globe to see how it is done, and they go away satisfied that the days of Listerism are numbered.

Let him, or any of our readers, peruse my chapter on the "History of Ovariotomy" (Diseases of the Ovaries, 4th edition 1883), and then make a con-

couragement." This answer of a practical surgeon is very different from that of Dr. Gore.

Even if the conclusions which are attributed to Dr. Ferrier's researches were to be regarded as indisputable, my answer would be that they might have been arrived at, and certainly would soon be enormously extended, if our clinical research were conducted upon reasonable and scientific principles. The chief reason of the slow advance of the arts of medicine and surgery is the reckless waste of the material so plentifully supplied by disease, and the first remedy will consist in the sub-division of the labour, a remedy against which, unfortunately, the medical profession protests most vigorously.

It is of course perfectly impossible to deal with all of the illustrations in favour of vivisection which have recently been advanced in the limits of an ordinary paper, and I prefer to take those which deal with points of practical utility, rather than with such as have as yet only a possibility of being useful in the future. I shall deal, therefore, at present chiefly with the illustrations which have been gathered from the field of practical medicine and surgery, for in them, of course, the public see the strongest arguments. If it is publicly announced, as has been done of late very widely, that human diseases have been cured and human suffering lessened by experiments on the lower animals, the public must therein see a strong argument for vivisection. But such announcements are open to the test of his-

trast with the facts there given and the nonsense he writes about Mr. Spencer Wells on page 28 of his "Physiological Cruelty."

Finally, I have to complain that all through his book "Philanthropos" makes charges of ignorance and mendacity against his opponents to which we cannot retaliate unless we know exactly who he is. Particularly he throws doubt on an account given by me of an amputation of the leg of a dog. If "Philanthropos" will reveal himself, I shall then be able to reply to his insinuation. But it is quite impossible to reply to any utterance of this kind on the part of an anonymous writer.

July 24, 1883. L. T.

torical examination, and to this I propose to subject the most important of them. I am equally open to discuss in the same way those points of less apparent usefulness, the matters of mere physiological discovery, on some future occasion, if it should arise; but, as with these the only defence can be that some day they may prove of service, it is clearly best to deal first with those for which an actual and not merely a potential utility is claimed.

Those of my professional brethren who take the other side may probably complain that I have selected a lay audience for the discussion; but the answer is, that by the circulation of pamphlets, and by communicated paragraphs in newspapers, they have already taken the initiative, and I am but meeting them on their own ground.

I am quite well aware that I am one of a small minority of my profession in my view that vivisection is useless as a method of research, but the answer I am disposed to offer on this point is, that not one in a hundred of my professional brethren have ever seriously examined the question. Ninety-nine take for granted the statements of the hundredth, and he, in turn, has not gone into the matter upon that side from which alone a safe answer can be given—that of historical criticism.

The dispute, as I have already said, is not to be settled by mere statement of opinion, one way or the other; nor is it a question of authority. On the argument of authority a very singular answer has been given by the supporters of vivisection in the case of the late Sir William Fergusson, who stated in his evidence before the Royal Commission that in his opinion nothing had been gained for surgery by experiments on the lower animals—an opinion which I entirely endorse. During his lifetime, Sir William Fergusson had heaped upon him all the distinctions which his Queen, his country, and his profession had it in their power to bestow. He was the titular head of

his profession, its most successful operator, one of its greatest anatomists, its most widely employed practitioner, its most successful teacher, the author of its principal text-book on surgery —but now, when he is dead, we are told he was not a scientific surgeon, because he did not believe in vivisection. Nobody said this in his lifetime, and so late as 1873 he was elected President of the British Medical Association, over all the profoundly scientific surgeons of the Metropolis. I share Sir William's opinions concerning vivisection, and I am quite content to rank with him on that account as an unscientific surgeon.

A pamphlet has recently been published in this town on "The Influence of Vivisection on Human Surgery," by Mr. Samson Gamgee, in which the proposition is set forth that without experiments on living animals "scientific surgery could not have been founded, and its present humane and safe practice would have been impossible." Mr. Gamgee supports this proposition by a series of instances which we may presume are the best and strongest he could find. These I tabulate as follows, and I shall discuss them historically in this order.

I. Treatment of injuries of the head, and the theory of Contre-coup.
II. Amputation of the Hip-joint.
III. Paracentesis Thoracis.
IV. Sub-cutaneous Tenotomy.
V. Treatment of Aneurism, Ligature, and Torsion of Arteries.
VI. Transfusion.
VII. Abdominal Surgery.
VIII. Function of periosteum.
IX. The Ecraseur.
X. Detection of Poison.

Mr. Gamgee tells us that the Académie de Chirurgie gave

out the subject of contre-coup, and its influence in injuries of the head, as the subject for a prize competition, and that the prize was obtained in 1778 by M. Saucerotte, whose essay was based "on literary research, clinical observations, and twenty-one experiments on living dogs." * He omits, however, to make any estimate of the value of the experiments on the dogs, which seems to me to be absolutely nothing; and he quite forgets to mention that the theory of contre-coup had been completely established for nearly two centuries before, and had been particularly the subject of Paul Ammannus of Leipsic, who wrote a well-known work, "De resonitu seu contra fissura cranii," in 1674, in which trepanning is recommended at the point of contre-coup, as had been practised by Paul Barbette, of Amsterdam, thirteen years before that. The theory of contre-coup, and the fatal practises arising from it, are happily now buried in oblivion, in spite of Saucerotte's vivisection, and would never again have been alluded to but for Mr. Gamgee's unfortunate resurrection of them.

The modern verdict concerning fractures of the skull is given tersely in Mr. Flint South's words, "the less done as regards meddling with them the better," and "a knowledge of counter-fractures is quite uncertain." In fact nothing could be more unfortunate than the selection of M. Saucerotte's experiments as an illustration of the value of vivisection, for they were performed for a purpose which was long ago recognised as futile, and in support of a practice universally condemned.

M. Saucerotte says—"Pour établir le diagnostic des lésions des différentes parties du viscère, j'ai cru devoir prendre la voie de l'expérience et de l'observation. Ce ne sont point ici des conséquences hasardées, ce sont les resultats de faits pénible, que formeront, á ce que j'espère, un foyer lumineux, dont les

* Mémoire sur les Contre-coups dans les Lésions de la Tête, par M. Saucerotte (Couronné en 1768), Mem. Acad. de Chirurgie, tom. x. p. 327, *et seq.*

rayons répondront le plus grand jour sur la pratique." He anticipated many of Ferrier's experiments by more than a hundred years, and when he trephined the skulls of dogs and injured their brains on the right side, he found that they became somewhat feeble on their left sides, and *vice versa*, a fact that had been established by pathology long before. His idea of imitating the injury of contre-coup, was to pass a knife right through the substance of the brain, till it impinged on the inner surface of the skull opposite the trephine hole, a most absurd experiment, as the contre-coup injures at the opposite surface only, and not necessarily at all the intervening brain substance.

Reading his experiments, they seem so like Ferrier's that I fancy if Dr. Ferrier had known of the existence of this essay he would have found little need to repeat its work.

Many of the conclusions of Saucerotte's experiments are eminently absurd, and, save that of the decussation of the fibres, which was known before, I can find few that have been since accepted, and those that have been he candidly avows were previously observed in cases of disease. Finally, the conclusions concerning treatment of injuries of the head which he draws from his experiments are not such as would be listened to in modern surgery, and it is certain that if they were ever acted upon they must have had results almost uniformly disastrous.

The fact is, that the whole run of vivisectional experiments on the brains of animal, now extending over hundreds of years, have given no sort of assistance to the elucidation of the physiology of that wonderful organ, so contradictory have been the results. On this subject Dr. W. B. Carpenter, who curiously enough has recently appeared as an ardent supporter of vivisection, says, in the seventh edition of his standard work on the " Principles of Human Physiology," p. 645, " The results of partial mutilations are usually in the first instance a general disturbance of the cerebral functions; which subsequently, how-

ever, more or less quickly subsides, leaving but little apparent affection of the animal functions, except muscular weakness. The whole of one hemisphere has been removed in this way, without any evident consequence, save a temporary feebleness of the limbs on the opposite side of the body, and what was supposed to be a deficiency of sight through the opposite eye. * * * So far as any inferences can be safely drawn from them these experiments fully bear out the conclusion that the cerebrum is the organ of Intelligence," a conclusion which surely has never been doubted, since it was first the object of the then savage club to destroy the intelligence of a foe by cracking his skull. Continuing his researches on such experiments as those of Saucerotte and Ferrier, Dr. Carpenter tersely sums up the *prima facie* objections to them, objections which seem to him, as they seem to me, to be fatal to their utility: "It is obvious that much of the disturbance of the sensorial powers which is occasioned by this operation is fairly attributable to the laying open of the cranial cavity, to the disturbance of the normal vascular pressure, and to the injury necessarily done to the parts which are left by their severance from the cerebellum." Dr. Marshall Hall also pointed out long ago that injury to the-dura-mater is an important factor in the results obtained.

II.—Amputation of the Hip Joint.

At page 8 of his pamphlet, Mr. Gamgee makes the astonishing statement that this operation was only attempted after it was proved safe by vivisection. The authority he has been kind enough to give me for this is a brief sentence in the preface to the ninth volume of the "Mémoires de l'Académie de Chirurgie," written by the Secretary General and published in 1778.

But the first hint we get of amputation of the hip-joint is from a German surgeon named Vohler, who was in practice

about 1690. It is doubtful if he ever performed it on a living patient, but it is on record that he tried on the dead body. But it was performed by M. la Croix, of Orleans, in 1748, not only on one limb, but on both limbs of the same patient, the first operation being successful, and the second almost so. This was nearly thirty years before the publication of the vivisection of dogs; and there are many other cases of success previous to Mr. Gamgee's alleged origin of the operation, one being by the celebrated Ker of Northampton, in 1773; and, as Mr. Gamgee has published a large book on amputation of the hip-joint, it is surprising that he did not know something more about the history of the operation.

III.—Paracentesis Thoracis.

Mr. Gamgee makes another most unfortunate selection in the case of William Hewson, who based a theoretical operation for pneumothorax upon experiments on living dogs and rabbits so long ago as 1769. He made a wound in the side of the chest and admitted air into the pleura, where no air ought to be, and then he operated to get it out again. When such a condition is brought about in man, and no vital organ seriously injured, the patient gets perfectly well without any operation. I cannot learn that Hewson's operation for the removal of air has ever been performed on man. When pneumothorax occurs from disease it is generally associated with conditions necessarily fatal, for which no operation is advisable. On this point the greatest authority, Dr. Bowditch of New York, says, "I have operated once in pneumo-hydrothorax, with temporary relief and comparative ease for several days. Many theoretical objections may be urged against the operation in such a case; but as the operation can do no harm and may give much relief, I shall operate again in such a case." The proceeding is therefore doubtful, the conditions

are extremely rare, pure pneumothorax, such as Hewson invented his proceedings for, never needs it, and therefore his experiments on living dogs and rabbits were useless.

Finally, tapping for the removal of fluid in the chest was practised long before Hewson's time, and therefore his research was needless. Hewson really based his proposal on this well-known practice, but in this he was anticipated in the most favourable cases—those of wounds—for Anel, of Amsterdam, published quite the same proposal in 1707, and it has been uniformly condemned by every writer on military surgery since, because the removal of the air merely induces bleeding.* Anel devised a syringe for the purpose, which has been revived as the modern aspirator.† Had Mr. Gamgee known anything of Dominic Anel he would never have mentioned William Hewson.

IV.—Subcutaneous Tenotomy.

I have traced the history of the surgery of tendons, and I cannot see the slightest reason to attribute any of the advances in this department to the alleged vivisections of John Hunter. I cannot find any record of these experiments, beyond the allusions to them by Drewry Ottley, and Palmer in his life of Hunter.

The same accident which happened to Hunter in 1767 happened to the first Monro in 1726, and from the latter instance a very marked advance in surgical practice was at once made and a contrivance invented by Monro himself, for his own case is still in use and goes by his name. No such advance was made from Hunter's accident or from his vivisections. In their histories of the progress of orthopœdic surgery Little and Adams make no such claim for Hunter. Adams points out clearly, and with justice, that Hunter established the principles on which subcu-

* Flint South's edition of Chelms, vol. i. p. 452.

† L'Art de Sucer les Plaies sans le servir de la bouche d'un homme. Amsterdam, 1707.

taneous surgery is now conducted; but these he established from clinical observations, not from experiments upon animals. And in his lecture on "Ruptural Tendons" (vol. i. p. 436), Hunter says not one word about his vivisections, or any conclusions he derived from them as to the method of repair of tendons. If he ever made any such experiments he must have placed very little value upon them.

If we trace the development of tenotomy we find that Hunter's experiments had no influence upon it at all. They were performed, it is said, in 1767. But the first tenotomy was not performed till 1784, by Lorenz, at Frankfort, and then the conditions were absolutely in defiance of the principles of subcutaneous surgery. It was done by an open wound, and this practice was continued with hardly any modification till far on in this century. In fact, as Adams points out, it is from 1831 that the commencement of scientific tenotomy dates, at the hands of Stromeyer. If this is so, and Adams makes his case out most conclusively (Club-Foot, 1873), how utterly useless Hunter's experiments on dogs must have been, to lie forgotten and unnoticed till unearthed in Mr. Gamgee's pamphlet of 1882, one hundred and fifteen years after they were performed; or how singularly careless and inattentive to the teachings of vivisection the medical profession must be, that they should allow this immense discovery to lie neglected from 1767 till 1831.

To bring forward so rash an illustration as this for the value of vivisection is to cast a terrible slur at the profession of surgery, a slur which I do not think at all deserved if the true history of such advances is carefully investigated, and the moving causes of them properly credited.

V.—Treatment of Aneurism, Ligature, and Torsion of Arteries.

Mr. Gamgee alludes to the oft-quoted story of the Hunterian

operation for aneurism as a proof of the aid vivisection has given to surgery. This illustration has been so completely and so often destroyed, that it is absolutely unnecessary to allude to it further than to explain that Hunter modified Anel's operation merely because he found the artery near to the seat of disease would not hold the ligature, and the patients bled to death. As the arteries of animals never suffer from the disease in question experiments upon them could not have helped Hunter in any way whatever. Sir James Paget, who has lately appeared as an ardent advocate for vivisection, and, therefore, may be appealed to by me as a witness not biassed to my view, has recorded his opinion in the Hunterian oration given at the College of Surgeons in 1877, that Hunter's improvement in the treatment of aneurism "was not the result of any laborious physiological induction; it was mainly derived from facts very cautiously observed in the wards and deadhouse." In this opinion Sir James Paget is undoubtedly correct.

Concerning the tying and torsion of arteries I am in a position to speak with some authority, because I have myself performed experiments on living animals, and have found how futile they are, and how uncertain and untrustworthy are their results. Mr. Gamgee tells us that some local worthies, who were distinguished by early performances of serious operations, practised their 'prentice hands on living animals. This is not scientific experimentation, but culpable and wholly unnecessary cruelty. It is on the dissecting table that a surgeon prepares his hand for his work, and not on the bodies of living animals. I have never known nor heard of such an instance before, and I trust there are no more to be quoted. Any surgeon who did this now would, I am sure, receive a universal condemnation from his professional brethren.

Mr. Gamgee quotes Jones's experiments on the arteries of animals as an instance of a valuable contribution to surgical

progress by experiments on animals, and I do not think any more complete illustration could be quoted in support of the uselessness of vivisection as a method of scientific research than that of the history of the physiological and pathological processes to be observed in arteries. If we consider the question from what some would call the purely scientific side, that is apart altogether from any practical bearings it may have for the relief of human sufferings and the cure of human disease, it consists merely of a mass of observations in which each observer contradicts some other. Upon this subject I wrote as follows so long ago as 1865 :—

"John Hunter warned surgeons to avoid injuring any of the coats of an artery, and to this effect advised that the ligature should not be drawn so tight as to cut them; while many of his contemporaries and successors dreaded any injuries so much that they used all sorts of clumsy contrivances to avoid it—such as pads of lint and bits of cork inserted between the arteries and ligature. Again, Travers, in his experiments on ligatures of arteries, demonstrated that Jones was quite wrong when he insisted that it was necessary to divide the inner coats; and Mr. Dalrymple, of Norwich, proved by his experiments that while simple and continued contact of the parietes of a vessel, without the slightest wound of any of the coats, was sufficient to produce permanent adhesion and obliteration, yet that division of the internal and middle coats without continued coaptation invariably failed to produce adhesion. Hodgson says that he cannot substantiate Jones's statement that division of the coats is essential, and strongly supports the opinion that coaptation of the walls, without rupture of any of the coats, will produce occlusion. The theories of Dr. Jones were strongly supported by Professor Thompson, his teacher, but were strongly opposed by Sir Phillip Crampton, who insisted that the division of the coats not only was unnecessary, but that it frequently defeats its own object." —(*Medical Times and Gazette*, 1865.)

I quote this at length to show that fifteen years ago I found authorities differing so much on this scientific question that I thought it advisible to institute a new series of vivisectional ex-

periments to decide it. The experiments performed by myself only added to the confusion, though nobody saw that at the time. What we were working at was to get quit of the ligature altogether, and to secure arteries by a temporary compression of some kind without injuring the coats. Acupressure promised to accomplish this; but it failed, for reasons I need not enter into here. The desire to get quit of the ligature was due to the fact that after a vessel was tied one end of the ligature was cut off and the other left hanging out of the wound, where it remained for weeks, sometimes for months, and occasionally (as in Lord Nelson's case) for years.

The amazing thing is that with all the experiments made upon animals nobody ever thought of cutting both ends of the ligature quite short and closing the wound over it. As a matter of fact, from the time of Ambrose Parè to that of Simpson, an interval of over 300 years, we went bungling on with experiments on animals when the whole thing lay clear before us. It was the successful experiments of Baker Brown and Thomas Keith upon women suffering from ovarian tumours which showed us that if we use pure silk, cut the ends of the ligature short, and close the wound carefully over them, success will be certain. Yet, not content with this, we hear of fresh experiments on animals with carbolised catgut, chromicised catgut, kangaroo tendons and other novelties, which speedily die out when applied to human beings.

In the case of the arteries, therefore, experimentation on animals has proved to be "science, falsely so-called." What we have done in this direction is entirely the result of clinical experience, and that only.

VI.—Transfusion.

This operation was not initiated, as asserted by Mr. Gamgee, in the second half of the seventeenth century by Dr. Lower, of

Oxford, nor was it first proposed as a legitimate surgical operation at all. It was proposed, and in all probability was really practised, by the alchemists of the sixteenth century as an attempt to obtain for the wealthy aged a renewal of their lease of life, after the theory and legend of Faustus. Certain it is that allusions to it are frequent, though the first actual account of its performance is given by André Libavius, Professor of Medicine at Halle (Helmst. 1602), as having been performed by him in 1594, the blood of a young healthy man being transfused into a man aged and decrepit, but able and willing to pay for the supposed advantage. In the early part of the seventeenth century it was a good deal discussed from this point of view, forgotten for a while, and then after the Restoration it was reconsidered, and a great deal written about it in this country and on the Continent. An extremely interesting allusion to the experiments is to be found in the wonderful Diary of Samuel Pepys:—

"November 14th, 1666.—Dr. Croone told me, that at the Meeting at Gresham College to-night (which, it seems, they now have every Wednesday again) there was a pretty experiment of the blood of one dog let out (till he died) into the body of another on one side, while all his own run out on the other side. The first died upon the place, and the other is very well, and likely to do well. This did give occasion to many pretty wishes, as of the blood of a Quaker to be let into an Archbishop, and such like ; but, as Dr. Croone says, may, if it takes, be of mighty use to man's health, for the amending of bad blood by borrowing from a better body.

"16th.—This noon I met with Mr. Hooke, and he tells me the dog which was filled with another dog's blood at the College the other day is very well, and like to be so as ever, and doubt not it's being found of great use to men, and so does Dr. Whistler, who dined with us at the Tavern."

The scheme of transfusion in all the experiments of the seventeenth-century descriptions of which I have seen was to take arterial blood from an animal and pass it into the veins of another,

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VI.—Transfus

This operation was not initiated, as as
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[illegible] is not surprising. But this has never [illegible] times upon man. It certainly would [illegible] to interfere with a large artery—and [illegible]—in a man is always an ex- [illegible] Dr. Lower, who is Mr. Gamgee's authority, [illegible] injected or tried to inject arterial blood from a lamb into a man, but the operation was so badly done that I do not believe any blood really passed. If Pepys' idea could have been carried out, of transferring some of the peaceful blood from the veins of a member of the Society of Friends, for the replacement of the turbulent and brutal spirit of Archbishop Laud, some good might have been done, much of the terrible history of that time need not have been written, and I might not have appeared here as a critic of such experiments. But no such or any other good result was obtained. A large army of experimenters rushed into the field, a fierce controversy took place; but before the eighteenth century dawned the whole thing was discredited and forgotten. Mr. Flint South gives a succinct history of the matter, and tells us that it was revived by the plan of mediate transfusion in the early part of the present century. The former experiments were fruitlessly repeated and [illegible] The result is that the operation has a very insecure [illegible] physiological opinion. I have seen it performed seven [illegible] times in a single instance. I have twice been [illegible] and have declined, and both patients are now [illegible] hear a great deal of cases in which patients [illegible] transfusion has been performed, but we hear [illegible] Personally, I have no confidence

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foundation for the recent advances of abdominal surgery, which are attracting the admiration of the whole professional world, and the instances he quotes date so late as 1880. If Shipton's experiment has been so fertile, why has the crop been delayed for one hundred and seventy-seven years?

But even here Mr. Gamgee is wrong in his history. The whole progress of abdominal surgery dates from the first successful case of ovariotomy performed by Robert Houston in 1701. Failing to see the lesson taught by this, and led astray by vivisection, no further success was achieved till 1809, by Ephraim McDowell, and it was not till 1867 that any substantial gain was made. Disregarding all the conclusions of experiment, Baker Brown showed us how to bring our mortality of ovariotomy down to 10 per cent.; and again, in 1876, Keith proved that it might be still further reduced. The methods of this reduction were such as only experience on human patients could indicate; experiments on animals could and did teach nothing, for operations have been performed on thousands of animals every year for centuries, and nothing whatever has been learnt from this wholesale vivisection.

As soon as Keith's results were established abdominal surgery advanced so rapidly that now, only six years after, there is not a single organ in the abdomen that has not had numerous operations performed upon it successfully. I have had, as is well known, some share in this advance, and I say, without hesitation, that I have been led astray again and again by the published results of experiments on animals, and I have had to discard them entirely.

Speaking of some recent attempts which have been made to cases of cancer of the stomach, Mr. Gamgee says: as such cases do, the placing of cancer of the diseases curable by the knife, do they not also of dogs by Shipton and Travers, who, by

and that this was successful is not surprising. But this has never been attempted in modern times upon man. It certainly would not be justifiable; because, to interfere with a large artery—and a large artery would be required—in a man is always an extremely risky thing. Dr. Lower, who is Mr. Gamgee's authority, in 1667 injected or tried to inject arterial blood from a lamb into a man, but the operation was so badly done that I do not believe any blood really passed. If Pepys' idea could have been carried out, of transferring some of the peaceful blood from the arteries of a member of the Society of Friends, for the replacement of the turbulent and brutal spirit of Archbishop Laud, some good might have been done, much of the terrible history of that time need not have been written, and I might not have appeared here as a critic of such experiments. But no such or any other good result was obtained. A large army of experimenters rushed into the field, a fierce controversy took place; but before the eighteenth century dawned the whole thing was discredited and forgotten. Mr. Flint South gives a succinct history of the matter, and tells us that it was revived by the plan of mediate transfusion in the early part of the present century. The former experiments were fruitlessly repeated and others tried. The result is that the operation has a very insecure hold on professional opinion. I have seen it performed seven times without success in a single instance. I have twice been asked to do it, and have declined, and both patients are now alive and well. We hear a great deal of cases in which patients have survived after transfusion has been performed, but we hear little or nothing of its failures. Personally, I have no confidence in the proceeding.

VII.—Abdominal Surgery.

Mr. Gamgee alludes to a vivisection experiment made by John Shipton, and published in 1703, as having laid the

foundation for the recent advances of abdominal surgery, which are attracting the admiration of the whole professional world, and the instances he quotes date so late as 1880. If Shipton's experiment has been so fertile, why has the crop been delayed for one hundred and seventy-seven years?

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Speaking of some recent attempts which have been made to operate on cases of cancer of the stomach, Mr. Gamgee says: "Warranting, as such cases do, the placing of cancer of the stomach amongst diseases curable by the knife, do they not also justify the vivisection of dogs by Shipton and Travers, who, by

their experiments, laid the first scientific foundation of intra-abdominal surgery?" Such a statement as this must be so completely qualified as to be regarded as altogether inaccurate. No form of cancer is yet known ever to have been cured, either by operation or anything else. If removed it invariably returns, and in all these cases of cancer of the stomach quoted by Mr. Gamgee, save one, the disease speedily returned and killed the patients. The one exception has not yet been under trial long enough to enable us to give an opinion. Doubtless it will have the same end as the others.

VIII.—Function of Periosteum.

The history of the development of our knowledge of the formation and growth of bone is extremely interesting, because it shows how completely misleading are the conclusions based upon vivisectional experiments, and how perfectly the secrets of Nature may be unravelled by a careful and intelligent examination of her own experiments. No one can look now at a necrosed bone without seeing how completely the whole story is there written. The history also exemplifies the fact that it is not only the purely practical details of surgery which are independent of vivisection for their development, but what are called the more scientific developments of physiological knowledge are equally possible without its aid, and are often retarded by its misguidance.

The first real observer in this department was Jean Guichard Duverney, born in 1648, who achieved such distinction that Peyer, in a dedicatory epistle, says to him, "Sempiterna te (Duverneyum) quondam trophœa manebunt et Regi vestro, Academiæ Urbique gloriosum erit tantum aluisse civem." He studied closely, and wrote a great deal about the anatomy, physiology, and surgery of bones, and in his books* he fully

* Traité des Maladies des Os, 1751, Paris. Œuvres Anatomiques, Paris, 1761.

describes the method of growth and ossification of bone, its dependence for its nutrition and growth upon the periosteum; the only thing he lacks is the microscopical knowledge of modern times. He also performed vivisections, not upon the periosteum but on the medulla, and they led him into most erroneous conclusions. He cut through the thigh bone of a living animal, and repeatedly plunged a stilette into the medulla, and the animal gave evidence of great suffering. The marrow, he therefore concluded, received a great number of nerves, which passed through the canals in the bone, but which existed only in his imagination. As long as he kept to his clinical observations and anatomical dissections he reached exact conclusions, but as soon as he entered the arena of vivisection he went all astray.

The next author of note was Francois Hunauld, born in 1701, who published in 1730 "Recherches Anatomique sur les Os du crâne de l'homme," in which he describes with the utmost accuracy the ossification by the membranes, between which the cranial bones are developed. The only errors he made were hypothetical descriptions of things he could not have seen without a microscope, and that he evidently had not used.

Next comes Robert Nesbit, a Scotch surgeon, settled in London, who published in 1736 an essay, entitled "Human Osteogeny, explained in two lectures."

He was the first to demonstrate the construction of bone by the now familiar experiment of dissolving out the mineral matter, and leaving, as he most accurately says, a spongy substance altogether different from cartilage. Cartilage he referred to its proper function; but he describes it as vascular, in this showing the want of microscopical investigation; but concerning the process of ossification he had got quite as far as we have at the present day. He tells us that in the blood, or in a liquid separated from it, there is an ossifying fluid, a fluid containing the material out of which bone is built up, composed of parts which

are not sensible: that whenever Nature determines upon an ossification within a membrane, from which all bones are developed, or in a cartilage, she directs by some means, the nature of which we are ignorant of, a larger quantity of blood to the vessels of the membranes, so that they become distended and visible, whereas before they were invisible. He describes the process of ossification only with such errors as are due to the absence of the microscope, and says: "Thus the membranes (periosteum) and the cartilages are the reservoirs in which the osseous particles are deposited and moulded." He denied the existence (and quite correctly) of an internal periosteum which had become about that time a matter of great contention.

The celebrated discovery of the property of madder for staining growing bone, when used as food by animals, was published by John Belchier in the Philosophical Transactions for 1736, and he fully disclosed thereby the method of growth of bone from periosteum, and many other most interesting and valuable discoveries concerning bone.

Between 1739 and 1743 Henri Louis Duhamel-Dumonceau published eight memoirs on the growth and repair of bones, largely based on the suggestive discovery of Belchier. Up to this time the formation of callus was thought to be due to an effusion of osseous juice—a belief which pervaded the surgical teaching of a distinguished professor of the University of Edinburgh so late as my own student days—but Duhamel proved its real origin. He also completely established the fact that bones grow in thickness by the addition of osseous layers originating from the periosteum.

Duhamel performed many vivisections, but it is quite clear from his own descriptions that they were failures and did not help him. He says himself that his conclusions were based on sections which he made of specimens of fractures which were in the collections of Winslow, Moraud, and Hunauld. In fact, to

any intelligent observer who looks at a preparation of necrosis it is evident that no vivisection was needed to show the whole process and growth of repairs of bone; and, even if vivisection were necessary, history displays with certainty that Syme and Ollier, to whom Mr. Gamgee attributes the merit of these discoveries, were only uselessly repeating the attempts of Duhamel more than a century old, and were only attempting to establish what had long before been proved.

Since Duhamel's time thousands upon thousands of experiments upon animals are on record, some to prove that the periosteum has nothing whatever to do with the formation of bone or with the production of callus, and others to prove that we owe everything to the periosteum, and yet it has been settled absolutely only by the experiments of disease upon our own bodies, and not by experiments on animals. It would be really amusing to read the account of the researches of Sue, Bordenave, Delius, Dethleef, Fongeroux, Haller, and countless others, were not the humour of their mutual contradictions sadly marred by the accounts of the tortures they inflicted uselessly on myriads of animals.

The experiments of Dethleef of Göttingen in 1752 were far more scientific than those of Mr. Syme in 1837, and the conclusions of both seem to me to be equally erroneous. At any rate Mr. Syme did not help us one bit in advance of Duhamel and Fongeroux. Haller made numerous vivisectional experiments, and he was the most distinguished physiologist of his time, yet he records his conclusion that the periosteum has nothing whatever to do with the formation of bone, and as a proof of this he quotes the formation of exostoses on teeth. The fact is, that as long as dependence was placed on vivisection, so long did one experimenter investigate after another fruitlessly, and with conclusions absolutely contradictory. On pathological research alone has the true conclusion been established. Haller made a

long series of vivisectional experiments, published in two memoirs,* and triumphantly proved that the periosteum can have nothing to do with the formation of bone. He concluded from his vast array of experiments that bone grew from the middle and not from the outside, together with many other absurdities, only to be matched in the modern researches of Bennett and Rutherford on the function of the liver, also based on fallacious vivisections.

The whole of the physiology and pathology of bone have been laid bare by the accident of the pigs of the dyer with whom Belchier dined, by microscopic research, and the observations of disease. Yet Hunter and Stanley thought it necessary to confirm the conclusions of the madder stain by such a clumsy device as fixing a ring of metal round the growing bones of a young animal, letting the ring remain for months or years, and then examining to find—what? absolutely nothing, save that the ring had been more or less covered, just as it would have been on a tree, thus only repeating Duhamel's conclusions. Other observers bored holes in bones and filled them with metal plugs and shot to find only that the conclusions of disease, that long bones grow from the epiphyses, is absolutely correct. Then we come to Mr. Syme's paper in 1837, "On the power of the periosteum to produce new bone." Mr. Syme almost every week was in the habit of cutting through great thicknesses of new bone attached to and growing from the periosteum to get at dead old bone from which the periosteum had been separated; and the new bone, being between the periosteum and the old bone, must of necessity have grown from the periosteum: there was nothing else it could grow from. Therefore, if Mr. Syme found it necessary to cut up animals to find out what was constantly staring him in the face, he was a profoundly unscientific surgeon, whose researches were as badly conducted as they were useless.

* Sur la Formation des Os, Lausanne, 1758.

When Mr. Gamgee read his paper at the local Medical Society and quoted these experiments of Mr. Syme, I said that, as far as I could recollect, the fact was that their conclusions had been absolutely upset by Mr. Goodsir, who did not make experiments upon animals, but followed a far more scientific method of research—microscopic examination. On refreshing my memory I find this is the case. In a paper read before the Royal Society of Edinburgh* in answer to Mr. Syme, Mr. Goodsir shows that Mr. Syme's method of research was so bad that the experiments could not be performed accurately. Mr. Syme was pre-eminently an unscientific surgeon, for he knew nothing of the microscope ; in fact it may be doubted if he ever looked through one. Mr. Goodsir, on the contrary, may be looked upon as the father of modern histological research. He proves conclusively that Mr. Syme's experiments were absurd in their conception and futile in their application. Mr. Goodsir's conclusions are, on the contrary, uniformly accepted, and as to his method he says that they were made upon shafts of human bones which had died,—museum specimens, just as Duhamel's were. They showed that whilst the periosteum is the matrix and machine by which the new bone is made, the real agency is in the layer of osteal cells, and so he finally solved the riddle. He did this by microscopic and pathological research. He condemned the employment of vivisection as useless and misleading, and to him we owe the completion of Belcher's and Duhamel's research—a completion which was hindered for a century by the blunders of vivisectionists.

After this I need not stop to discuss the useless repetition of Mr. Syme's experiments, with variations by Ollier of Lyons, for that would be merely a waste of time.

IX.—The Ecraseur.

Mr. Gamgee quotes the introduction of the ecraseur as an instance of the influence of vivisection on the progress of human

* Trans. Roy. Soc. Edin., vol. xiv.

surgery. No more unfortunate instance could be quoted. The principle of the instrument is that it crushes and tears the tissues instead of cutting them as by the knife. The surgical aphorism that "torn arteries don't bleed" was in existence long before M. Chassaignac was born, and if he had based his employment on that alone he could have done all that his instrument has effected. But unfortunately he performed experiments upon animals, and immediately he was led astray. I once saw the leg of a favourite dog amputated at the hip-joint on account of disease, and when the limb was removed not a single vessel bled, and the main artery was tied only as a matter of precaution. In the human subject I have seen twelve or fifteen arteries tied in the same operation, for with us the smallest arteries bleed and require to be secured. Our arteries act in ways altogether different from those seen in the lower animals. Their pathology and physiology are absolutely different, as may be seen in the frequency of apoplexy and aneurism with us, and the almost complete immunity from them of all the lower animals, even in extreme old age. Hunter tried his best to induce aneurism to the lower animals, and failed. Injuries to arteries in the lower animals are repaired with the utmost certainty and readiness, but in man it is altogether different. It may be easily imagined, therefore, that M. Chassaignac's application of the ecraseur to the lower animals was found wholly misleading when man was the subject, and now in human surgery its utility is extremely limited; that is, it is entirely confined to operations where only very small arteries are divided. Speaking for my own practice, I may say that it might be dispensed with and never missed.

Mr. Gamgee's quotation of its application to the ovarian arteries of the cow is peculiarly unfortunate, seeing that when it was used for the same purpose in the human subject it had speedily to be given up on account of its failure.

X.—Detection of Poison.

A great deal has been made of the successful experiments recently performed by the medical experts for the conviction of Lamson, for that worst of all crimes, the most unpardonable, murder by poisoning. At first sight this does seem a case in which experiments upon animals may be justified. Certainly anything and everything ought to be done to convict a poisoner, and if nothing short of that would do, I would advocate the performance of a hecatomb rather than that such a scoundrel as Lamson should escape. So late as a few weeks ago I made a reservation on this point in my condemnation of vivisection as a method of research, but it seems to me, from a closer consideration of the facts of the case, that it forms really a very strong argument for the complete abolition of vivisection, and, at the same time, unfortunately it is a matter of grave reproach to modern science.

Fortunately the conviction of a poisoner is almost certain. If he is not a doctor he commits the crime so clumsily that he cannot escape. If he is a doctor he must have an interest in the victim's death, is almost certain to be in pecuniary difficulties, and is sure to have had a bad character previous to his great crime. The only difficulty lies in the proof of the presence of the poison. With all poisons but the alkaloids this is a matter of such ease that failure is impossible, and as the alkaloids are almost exclusively in the hands of chemists and doctors the limitation of their use is very close.

The most notorious case in which an alkaloid was used, or supposed to have been used, by a poisoner was that of Parsons Cook. The alkaloid was supposed to be strychnine, and I say supposed, because I rise from the perusal of that trial with much doubt as to whether Parsons Cook really died of strychnine poisoning. Certainly I cannot accept it as proved, and I think

if the trial were to occur now the same evidence which convicted Palmer would probably break down. I am perfectly satisfied, however, that Palmer received substantial justice.

In Palmer's case the principal witnesses for the prosecution were the late Dr. Alfred Swayne Taylor, and the late Sir Robert Christison, certainly the greatest toxicologists of this century. Strychnine was not discovered in the body of Cook, and Dr. Taylor had to admit that the best tests then known were insufficient to discover one fiftieth of a grain, and that even half a grain might remain undetected amongst food in the stomach. Palmer was sentenced to death upon the 27th of May, 1856, and in July of the same year a method of chemical analysis was published by Copney in the "Pharmaceutical Journal," by which one five hundred thousandth of a grain of strychnine could be detected with certainty after separation. In his evidence Dr. Taylor admitted that the experiments he had performed upon animals with strychnine were practically worthless for any application to man, and in the report of the Royal Commission of 1876 he condemned such experiments, particularly those which are directed towards the discovery of an antidote to snake bite.

Strychnine was discovered in 1818, and was first used as a poison in 1831, and again in the case of Mrs. Sergison Smith in 1847, and it was no new matter the toxicologist had to do with in the trial of Palmer. It must be regarded, therefore, as a matter for deep regret that it was not till after the trial and execution of Palmer that the chemistry of strychnine was exhaustively examined, and definite and certain tests for it obtained. At the trial there was a sort of competition among the vivisectionists, and Serjeant Shee actually urged as an argument for the defence that his witnesses had performed ten times more experiments to prove that there was no strychnine, than the witnesses for the prosecution had performed to prove what never was proved, that strychnine was used at all. Yet in two months

chemical processes were devised, without the slightest aid from vivisection, which detected half-a-millionth of a grain with certainty.

At the trial Professor Christison said that another alkaloid was known, of a deadly poisonous character, which it was impossible to detect, but under the judge's direction he refused to make its name known. There were really many alkaloids of a deadly poisonous character at that time quite well known, and aconitine was one. The first case to bring this poison under notice as a criminal agent was in 1841, and the notorious Prichard destroyed his victims with it in 1865. Dr. Penny of Glasgow resorted to experiments on animals in order to bring the crime home to Prichard, and succeeded. Yet I have looked in vain for any record of a research for a method which will detect aconitine with certainty by chemical analysis, as strychnine can be detected, and Dr. Stephenson admitted in evidence that there was no such test.

I daresay such a method will be shortly published, and what I I desire to point out is that this discovery ought to have been made long ago in the interest of public safety, not only with regard to aconitine, but with regard to many other alkaloids which may be used in the same way, and which cannot be discriminated from aconitine, even by experiments on animals. At present, when need arises, we must go back to the uncertain method of experimenting upon animals. But this is not science, if by that word we are to speak of exact knowledge. The very weakness of this method has led to a serious infraction of the principles of our judicial proceedings, for the Home Secretary announced in the House of Commons only a few nights ago that the Government, in a case such as Lamson's, could not allow the proceedings of the medical experts for the prosecution to be watched by other experts on behalf of the defence.

This is altogether unfair, for with such an uncertain and

inconclusive method as that of experimentation on animals two men, even if appointed by the Colleges of Physicians and Surgeons, and not by the Treasury, may be mistaken, whereas by chemical or spectroscopic analysis mistakes are extremely unlikely, and the more observers there are the better.

The general conclusion therefore is, that for such purposes experiments on animals should be entirely prohibited, and that an exhaustive research should at once be undertaken at the expense of the State, upon the spectrum and chemical analysis of all substances which may be used for criminal purposes. There is no known substance of constant character which has resisted the chemists' effort to identify it when it has been properly investigated.

If all these alkaloids had been subjected to an exhaustive investigation as strychnine was after Palmer's trial, there would have been no need to revert to vivisection in order to convict Lamson, and I do not think it would now be contended as necessary for the detection of a poisonous dose of strychnine that experiments on animals should be made. Vivisection in this case is therefore not the weapon of science, but is the refuge of incomplete work.

I have now gone over all the points urged in favour of vivisection as contributory to surgical advance as given in Mr. Gamgee's pamphlet, and with the result, to my mind, of proving that in every instance the claim is groundless. Had I time at my disposal I could examine in detail numerous other claims equally fallacious. So far, indeed, as I have already said, I have not met with a single case capable of substantiation, not even the most recent—that of Pasteur's discovery of the prevention of zymotic diseases in domesticated animals by inoculation of cultivated virus.

In the *Nineteenth Century* for March will be found an article by a well-known veterinary surgeon, Mr. Fleming, on this

subject. He describes the ravages of such diseases as anthrax, splenic fever, rinderpest, swine plague, &c., amongst the animals which form our food supply, and I admit the accuracy of his statements. Quite recently Mr. Pasteur has discovered, and his statements have been amply confirmed, that the specific organisms which form the poisons of these diseases may be so artificially cultivated as to be capable of producing by inoculation a mild form of the original disease, which mild form is largely protective from the severe and fatal form of the same malady. In fact there is a perfect analogy between this discovery of Pasteur and that of Jenner.

The argument is that by their inoculation the zymotics of domestic animals may be stamped out, and the claim is that it is a great advance brought about by vivisection. But on a little examination it seems to me that both argument and claim break completely down. If it is really an advance from vivisection, then those who benefit are the animals experimented upon, and that may be legitimate enough—they at least would share largely in the benefit.

But the case must be examined from another side. There are some twenty zymotics amongst our domestic animals to be provided against. Are we to have each of them inoculated some ten or twelve different times, each time for a different disease? The affirmative reply possesses a strong pecuniary interest for a veterinary surgoon, but a practical man will only smile at it.

But, to go deeper into the question, we find another and a much stronger objection. Such a process as protective inoculation must always be an inefficient and a temporary measure. To take the case of vaccination and small-pox, it is beyond dispute that vaccination protects the individual to a large extent from small-pox, but it does not protect the community—as may be seen from the ravages it is making at the present time in neighbouring towns and counties. The machinery of vaccination

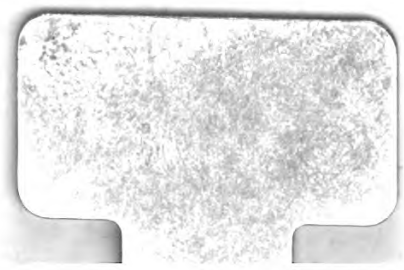
never can be so perfect as to stamp out the disease, and it must be regarded purely as a temporary expedient. The real agent for the stamping-out of small-pox is the machinery of a system of sanitary police, such as we have here; and even on the small scale in which we have had it for six years it has worked marvels. It will stamp out not only small-pox but every other zymotic at the same time, and by the same measures, and then we need not trouble about vaccination—certainly it need not be compulsory.

But the case is still stronger with the lower animals. With them, as with us, civilisation has introduced zymotic poisons, which are absolutely unknown to the wild animal, and the reasons are not far to seek. In my capacity as one of the managers of a large public institution, I had recently to investigate the cause of an endemic of swine plague, and I found a state of matters which had caused at the same time typhoid fever in a human patient.

Look at the arrangements of an ordinary British farmyard, and then believe that it is a matter of no wonder that rinderpest destroys the cattle, and diphtheria the farmer's children. The animals spend their lives in houses not lighted and not ventilated, or walk about in a mass of seething filth, on one side of which stands the farm house, every room reeking with the stench of the cattle-yard.

When it begins to dawn on the mind of the British public that all these diseases, both for man and animals, are absolutely preventible by the simple means of securing fresh air, pure water, and abundant light, they will be banished. Meantime inoculation may, and probably will, prevent individuals being attacked, but it will not stamp out the diseases, and it must be regarded as really a retrograde proposal when we have in our hands the means of complete prevention.

I hope I have thus made it clear that, deeply as I feel the strength of the objection to the practice of vivisection upon the

various grounds I indicated at the beginning of my paper, I urge against it a far stronger argument than these: that it has proved useless and misleading, and that in the interests of true science its employment should be stopped, so that the energy and skill of scientific investigators should be directed into better and safer channels. I hail with satisfaction the rousing which is evident in the public mind upon this question, and I feel confident that before long the alteration of opinion which I have had to confess in my own case, will spread widely amongst the members of my useful profession.

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