White (C. E.)

AN ANSWER

TO

DR. KEEN'S ADDRESS

ENTITLED

OUR RECENT DEBTS TO VIVISECTION.

BY

CAROLINE EARLE WHITE,
VICE-PRESIDENT OF THE AMERICAN SOCIETY FOR THE RESTRICTION OF VIVISECTION.

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"OUR RECENT DEBTS TO VIVISECTION."

To the Graduates at the Twenty-third Commencement of the Woman's Medical College of Pennsylvania :—

Having accidentally, a few days ago, been led to take up and read the address delivered before you by Professor Keen, on the occasion of your graduation, I feel a strong desire to state to you some of the facts on the other side, and to show you how illusory I, as well as those connected with me in the Anti-Vivisection agitation, consider his claim to a long list of benefits derived, within the last twenty-five years, from experiments upon animals.

I am not, it is true, a student or graduate of medicine; but the fact that I am, in the first place, one of your own sex, interested as you are in the advancement of true science, and in the progress of the human race; that, secondly, I was one of the first originators of the American Society for the Restriction of Vivisection (the only such Association in existence in the United States); and that, thirdly, for years I have given this subject of experimentation upon animals careful and serious study, reading without reserve both sides of the question, will, I hope, entitle what I now say to respectful consideration at your hands.

Dr. Keen begins with stating the axiom that medicine must either grow worse, stand still, or grow better, and that as we all naturally desire it to grow better we must perform experiments upon some living body; "that in many cases these experiments involve great risk "to life or health, and that as here they can not and must not be "tested first upon man, the only alternative we have is to try them "upon the lower animals; and that we should be most unwise, nay, "cruel, both to man and to animals, if we refused to pain or even to "slay, a few animals that thousands, both of men and of animals, might "live."

Here, at once, I begin to take issue with Dr. Keen, and to this premise I decidedly object: first, to its wording, because I do not consider it correctly stated; secondly, to its argument, which is, in my opinion, fallacious.

I think his expression, "to pain or even slay" a few animals, decidedly an inverted climax, and would substitute for it "to slay or even to torture," since we do not object to the slaying of animals, when by so doing we can derive undoubted benefit for mankind; but it is the torture, or as the Doctor terms it, the "pain," against which we protest, considering it a far greater evil than death, and in fact involving death, as a general thing, since we have scarcely ever heard of an animal which was made the subject of severe experimentation and then allowed to recover and live.

For Dr. Keen's expression of a "few" animals, I should like to substitute "millions."

It is a favorite euphemism with vivisectors and the advocates of vivisection to say a "few" animals. Is it not better, they almost invariably ask, that a *few* animals should suffer, than that the human race should be deprived of any advantage that it is possible to obtain? Allow me to give you some idea of what is implied in the term "*a few* animals."

Dr. Schiff, the eminent Vivisector, during his residence in Florence, of ten years, is generally stated to have killed in his experiments fourteen thousand dogs. The Vivisector, Flourens, in his own writings, says: "Magendie sacrificed 4000 dogs to prove the correctness of Sir Charles Bell's views with regard to the distinction of the sensitive and motor nerves; he then sacrificed 4000 more to prove these views erroneous. I took up the experiments in my turn, and demonstrated the first opinion to be the right one; in order to arrive at my results I also vivisected a great number of dogs."

If four vivisectors alone can thus be shown to have put to death about twenty-five thousand animals, some ten thousand of which were in one single series of experiments, and those some of the most terrible that can be imagined, since they were made on the nerves, is it too much to say that all that have been offered up on what are called by physiologists the "bloodless altars" of Science, since these experiments first began may correctly be represented by the term millions?

I take issue with Dr. Keen, in the second place, where he says "these experiments can not, nay, they must not, be tested first upon man." I assert, on the contrary, that in the majority of cases they must be tested first upon man, or not tested at all, because, no important deductions can ever be drawn, with any degree of certainty, from experiments upon animals, since in some inexplicable way their construction is so different from that of man. I can cite no better instance of this fact than that given by Lawson Tait, one of the most celebrated Eng-

lish surgeons, who claims that Vivisection has done more harm than good, at least to surgery, from the very fact of its leading to such erroneous inferences.

In a letter to the Birmingham *Daily Post*, of the 12th of December, 1881, he says:—

"Like every member of my profession, I was brought up in the belief that by vivisection had been obtained almost every important fact in physiology, and that many of our most valued means of saving life, and diminishing suffering, had resulted from experiments on the lower animals. I now know that nothing of the sort is true concerning the art of surgery, and not only do I not believe that Vivisection has helped the surgeon one bit, but I know that it has often led him astray.

"Very many years ago, at the request of my master, the late Sir James Simpson, I undertook a series of experiments on the lower animals, to determine a doubtful question in the method of closure of arteries after surgical operations. His restless genius had incited him to efforts for the improvement of our means of arresting hemorrhage, and he introduced what he termed 'acupressure,' to replace the ligature. My experiments were conducted under the advice and guidance of one of the most eminent physiologists, still alive. The poor animals, chiefly dogs, were operated upon under chloroform, and were spared as much suffering as possible. But, of course, they suffered, and I have often thought of those poor dogs with bitter regret.

"The conclusion of the experiments seemed absolutely perfect, and my observations were quoted far and wide, were translated into foreign languages, and everything looked as if 'acupressure' was to reform the art of surgery. It did not; it speedily died out, and has been, I think, almost forgotten. The explanation of this lay in the simple fact that the closure of a dog's artery is altogether a different process from that seen in the human vessel, and my experiments were not only needless, but they were absolutely misleading. Simpson's inquiry, as we all know now, was altogether in the wrong direction, and the perfection to which this part of our art has been brought has not been obtained by the aid of vivisection, but actually in spite of it.

"Here, then, is the great difficulty, an '*a priori*' one, but, so far as I can see, absolutely insurmountable. If we cannot apply the facts observed in so simple a matter as the closure of an artery, from the dog to man, how can we, in reason, apply facts from the same sources in so difficult and complicated a subject as the action of the brain, etc.?"

On another occasion he says, "I venture to prophesy that every unprejudiced practitioner of medicine and surgery, if he will investigate

the matter, will have an awakening of conscience such as took place in my case, and will begin to doubt the propriety of cruel experimentation upon animals."

Dr. Keen speaks at length of the value of experimenting upon animals to ascertain the effect of drugs and poisons, which surprises me, since in no respect are erroneous deductions more likely to be made than in these same experiments. It is true that the action of these substances upon men and the lower animals may sometimes be analogous, but no matter what experiments were made in the hydrochlorate of cocaine upon the animals mentioned by Dr. Keen, they were absolutely inconclusive as regards man until the crucial experiment was made upon the latter. Therefore, in trying the hydrochlorate of cocaine first upon dogs or cats, they did not avoid any risk to man, for they could not tell how man would be affected thereby. In their general effect upon the system, the action of powerful and poisonous drugs, moreover, is often so different in men and animals that experiments upon the latter, form a most misleading method of research. You have, doubtless, all heard of how many substances that are poisonous to man are innocuous to certain animals; how horses can take large quantities of antimony, dogs mercury, goats tobacco, mice hemlock, and rabbits belladonna, with perfect impunity.

Supposing that any physiologist, reasoning after Dr. Keen's fashion, and having tried belladonna upon rabbits or mercury upon dogs, should have given these substances, without any hesitation, to human beings, would not the consequences have been much more deplorable than trying the hydrochlorate of cocaine upon the eye of a man before testing it upon an animal?

There are other instances, not so well known, of the different action of certain poisons upon the system of men and animals.

One of the most remarkable is that mentioned by Dr. Livingstone, in his account of the Tse-tse fly, found in Central Africa.

"The bite of this poisonous insect," he says, "is certain death to the "ox, horse or dog. The *mule*, *ass* and goat enjoy the same immunity "from the Tse-tse as man and game. Many large tribes on the Zam- "besi can keep no domestic animals, except the goat, in consequence "of the scourge existing in their country. Our children were bitten, "yet suffered no harm, and we saw around us numbers of zebras, buffa- "loes, pigs, pallahs and other antelopes, feeding quietly in the very "habitat of the fly. There is not so much difference in the nature of "the horse and zebra, the buffalo and ox, the sheep and antelope, as to "afford any satisfactory explanation of the phenomenon."

Dr. Abiathar Wall, in his essay upon Vivisection—from which I

have made the extract—when speaking of this, says, "It seems strange that two members of the same family, though of different genera (*e. g.*, the horse and zebra), should be so unequally affected by the same poison—the one being certain to die, the other escaping scathless; but it serves to show that the difference of structure in genera of the same family, implies a difference of physiological action. Hence this affords one more illustration of the danger of generalizing from experiments made on animals, even if claiming a sort of kinship. If, then, animals presenting but slight differences between each other, behave so differently under the action of a poison, what must the gap be between them and man?

"Had the action of the Tse-tse poison been unknown in Africa, and had it been sent home to be experimented upon, with the result of fully establishing its lethal effect upon the horse and dog, we should have been told that it was, therefore, a poison to man! This would be an utterly wrong conclusion, but would amply demonstrate the folly of arguing from the lower animals to human beings."

How different is this incertitude and vagueness of results from the satisfaction that attends the performance of experiments upon man, when they can be properly and safely tried. Divine Providence has, we believe, ordained that in some of the many accidents which so often, unhappily, befall mankind, a legitimate means can be afforded for wresting the secrets of Nature from her grasp and increasing our knowledge of physiology. How much greater was the knowledge obtained of the digestive organs and the process of digestion by the accident which happened to Alexis St. Martin, than by all the experiments upon animals that had ever been made!

And yet, in the face of this fact, Dr. Keen exclaims: "But Nature's "experiments are rarely ever limited in area, or uncomplicated; they "are never systematic or exhaustive; it takes years to collect a fair "number of her clumsy experiments; and the knowledge is diffused "through many minds, instead of being centered in one that will sys-"tematize the results."

Next on the list of benefits arising from Vivisection, Dr. Keen mentions the improvement in the treatment of gunshot wounds in the abdomen, by making an opening therein, and treating, by appropriate surgical methods, the wounded places; sometimes even going so far as to remove a piece of the bowel, and uniting the cut ends.

This improvement he specifically attributes to the result of experiments which Dr. Parkes, of Chicago, made upon thirty-seven dogs, that were etherized, then wounded by shooting, the abdomen opened and the wounds treated in a proper manner. He does not, however, ap-

pear to regard it as at all material to his argument that similar experiments were made by Professor Gross over forty years ago, and the same deductions drawn therefrom.* Is not this fact enough to convince any one "in sana mente" of the necessity of some restriction of Vivisection?

This same claim which Dr. Keen makes, was advanced in an editorial published in the Philadelphia *Times* on the 28th of last July, and which I answered at that time, my answer being inserted in the *Times* of August 4th.

In it I urged that the experiments upon the dogs were not of the least use, since they, in common with many of the lower animals are not liable to peritonitis in cases where the peritoneum is cut or ruptured,† and that as the great danger to man in such cases arises from his extreme liability to peritonitis, no deductions of value could be drawn from experiments upon dogs. I said that Dr. Parkes, of Chicago, might have vivisected thirty-seven dogs, as reported, or thirty-seven thousand; the experiment when tried upon man for the first time was just as much of an experiment as if no dog had ever been touched, therefore, Dr. Parkes' experiments, like most of those in Vivisection, were entirely unnecessary, doubly unnecessary, I should now say, since, as I before mentioned, similar ones had already been performed by Dr. Gross. Dr. Henry J. Bigelow, the eminent surgeon of Boston, to whom I showed my answer, confirmed my argument and my statements in all respects, except that he thought I had overrated the immunity of dogs from peritonitis, as it is possible, he said, for them to have that disease, although it occurs very, very rarely. While there may be some physiologists who differ from this, the weight of evidence is incontestably on the side of the statement that dogs enjoy almost entire immunity from that painful disease.

The same argument applies exactly to the next claim of importance on Dr. Keen's list, which is that of the improvements in ovariotomy, owing to Sir Spencer Wells' experiments upon what Dr. Keen is pleased to call a "few dogs," but which were in reality a good many rabbits and guinea pigs. Dr. Abiathar Wall, in his essay upon Vivisection, when speaking of these experiments says: "Now, according to the

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* *Medical News*, May 3d, 1884.

† The peritoneum, the covering and enveloping membrane of all the organs in the body, is in man particularly liable to inflammation, even by slight puncture or abrasion; and its inflammation constitutes one of the most painful, dangerous and mortal accidents known to the medical profession. But in animals, and even in a creature so highly developed as the dog, the incision and violent rupture of this membrane is attended with no inflammatory danger.—Dr. Anna Kingsford, in the *Nineteenth Century*, February, 1882.

Lancet (September 29th, 1877), the animals used for these experiments 'enjoy immunity from severe peritonitis,'* hence it is open to doubt as to whether the results obtained could be made applicable to human beings, who are eminently liable to this form of inflammation."

Another fact that Dr. Wall mentions in disproof of the claim that Sir Spencer Wells' experiments resulted in any benefit is, that the mortality in his subsequent ovarian operations was scarcely lessened for several years. "In the first hundred cases the mortality was 34 per "cent., whereas in all previous cases that could be collected the mor- "tality was only 33.89 per cent."

In the first five years, according to the statistics of Mr. Spencer Wells' own cases, about one in three died; in the next ten, about one in four; in the next five years, about one in five; but in the last two years about one in ten. "From these data it is evident," says Dr. Wall, "that the diminution of mortality has been gradual, whereas, "had the knowledge obtained by Vivisection been true, we ought nat- "urally to have expected to have had an *immediate and rapid fall* in "the death rate. The first mortality is somewhere about 34 per cent., "the present is about 10 per cent., and it has taken twenty years to "produce this satisfactory result."

To refer again to Lawson Tait: In a speech made at the annual meeting of the Anti-Vivisection Society, in London, in the year 1882, he said: "I follow the line of practice in which Spencer Wells made "such a reputation. I remember the statement made by the Bishop "of Peterborough, in the House of Lords, that some of Spencer Wells' "experiments had contributed to medical advancement (alluding to "the same experiments of which Dr. Keen speaks), but that statement "was as childish and incorrect as anything could be. I am speaking "on matters of fact within the compass of my own daily life, matters "on which I am in the position to speak as authoritatively as any man "in this country, and I say that nothing could be more childish and "incorrect than that statement."

From this it appears that there exists a difference of opinion between Dr. Keen and Lawson Tait as to the value of Vivisection in the operations of Ovariotomy; which of the two is the better qualified to speak upon this subject, I will not attempt to decide.

The next debt which we owe to Vivisection, according to Dr. Keen, is the improved treatment of sunstroke, resulting from Dr. Horatio C.

*It is only fair to say that one physiologist, at least, differs from the London *Lancet* in the opinion that rabbits cannot have peritonitis.

Wood's experiments upon animals. As this was answered at the time by Dr. Owen Wister, most satisfactorily, I merely append his answer:

For the Public Ledger :—

MR. EDITOR :—Will you kindly give me space to say a few words in reply to some comments on Dr. Keen's address at the Commencement of the Woman's Medical College, which appeared in your issue of the 12th instant?

In speaking of the experiments made with heat on living animals in Philadelphia, Dr. Keen is stated to have said that the animals were exposed to a heat no greater than that to which outdoor laborers are subjected in Summer. Dr. Keen omitted to mention that in those experiments the heads of cats and rabbits were exposed, by means of a skull-cap, to a stream of water at a temperature of 175° to 190°. We should be glad to know under what circumstances similar conditions occur in nature. It is proper to state, in the interest of truth, that these investigations did not add one fact to our knowledge of the pathology of sunstroke, nor one suggestion to our method of treating it. In fact, there is no stronger argument against the usefulness of vivisection than the researches to which Dr. Keen refers.

March 12th, 1885. OWEN J. WISTER.*

Dr. Keen next mentions that in India alone twenty thousand human beings die annually from snake bites, and, as yet, no antidote has been discovered. "How can we search intelligently for an antidote," he asks, "until we know accurately the effects of the poison?"

The answer that suggests itself to me, is very different from the one which he makes. I should reply that, in order to find out the effects of the poison, and to search also for an antidote, the best plan would be for the experimenters to go to India, where they could find as large a field for investigation as they require, in the poor victims themselves. Here is an opportunity such as is not often afforded, of experimenting upon human beings, since, as they would infallibly die from the snake bites, there can be no objection to trying upon them every variety of antidote that can be discovered. Nothing seems to me less defensible than these experiments in the poison of snake bites upon animals, since it is the one case in which they could be observed with so much satisfaction and certainty upon men. It is true that it would require a journey to India, since the deaths from snake bites are generally all in

* There are two more letters in the correspondence, one from Dr. Keen in answer to the above, and another from Dr. Wister, but as they leave the subject very much in the same position that it is at the end of this first letter, we do not think it worth while to add them to an article so much longer than we at first intended.

that country; but should these benefactors of the human race, as Dr. Keen considers them, who have sacrificed for this end the God-given sentiments of pity and compassion for helpless brutes, hesitate to make the extra sacrifice of a temporary residence in India? Another reason, moreover, why these investigations should not be continued upon animals, is the extreme uncertainty as to whether any antidote will ever be found that will prove available, no matter upon whom or upon what the experiments are tried. Dr. Alfred Swayne Taylor, Fellow of the Royal College of Physicians, Lecturer on Medical Jurisprudence and Toxicology at Guy's Hospital, author of works on Poisons and Medical Jurisprudence, in his examination before the English Royal Commission to investigate the subject of vivisection, was asked the question, "Do you imagine that experiments are likely to do much good for that purpose" (*i. e.*, of obtaining an antidote for snake bites)? He replied, "No, I do not. "I have read them all with great care. Am- "monia has been recommended by Dr. Halford, in Australia, but this "has proved utterly inefficient when the experiments have been fairly "performed; and, in truth, if you consider for a moment the mode "of death from poison, you will see how difficult it is for any antidote "by injection to operate. The poison rapidly gets into the blood; "when in the blood it alters this fluid, and unless the remedy proposed "enters into the blood quite as quickly, and very soon after the poison "has entered, no good can be done. There may be some slowly ope- "rating poisons, but with regard to serpent poison, when it once enters "the blood, the effect is most extraordinary; the rapidity of death is "very great indeed."

Dr. Keen next speaks of anæsthetics (the most important of which, viz., ether and chloroform, have certainly *not* been discovered by means of experiments upon animals, but on human beings), and says that the ideal anæsthetic, which will abolish pain without abolishing consciousness, will surely be found, if experiments upon animals are continued. This prediction, referring to something in the dim future, is not, of course, susceptible of disproof, so I pass it by, as I should if Dr. Keen predicted that by means of these same experiments, the elixir of life would be discovered.

The next instance that he mentions is where Simon, of Heidelberg, wished to remove one of the kidneys from a woman under his charge, but feared to do so, as no one had ever removed a healthy kidney, Dr. Keen says, and no one had tested what was the best method of reaching the kidney, whether by the abdomen or the loin, etc., so he experimented upon a number of dogs, and then removed the kidney from his patient, and saved her life. It seems strange that he could find out

how to reach the kidney in a human being from trying the same operation upon a dog, when the construction of a quadruped and man is necessarily so different. Moreover, if, as Dr. Keen tells us, a diseased kidney had been removed from a human being several times, why should Simon, of Heidelberg, object to removing a healthy kidney? The risk would seem much less in the latter than in the former case, and if Dr. Keen has no better excuse than this to urge for the performance of experiments upon animals, he is even in a worse case than I thought him.

He next comes to Ferrier's experiments upon the brains of monkeys, "basing his conclusions on which, Dr. Hughes Bennett," he says, "about a year ago, diagnosticated a small tumor on the surface of the brain, involving the centre of motion for the muscles of the hand."

That the result of Ferrier's experiments are so entirely to be depended upon as Dr. Keen would have us believe, is not admitted by all scientists. In a paper published by an Italian Professor, Marcacci, in the second volume of "Archives Italiennes de Biologie," and announced to be the work of the laboratory of the Sorbonne, he sets himself to solve the following problem: "To find whether the "results obtained by experimenting on animals—especially by the abla- "tion of the motor zones—are in harmony with bedside observation, "and further to test whether these last are always identical, and "whether it be possible to draw a conclusion as to any definite localiza- "tion in the human brain. The record of Professor Marcacci's "observations is most elaborate, and occupies twenty pages. Cases "which ought to have corresponded with and verified Professor Fer- "rier's conclusions, respecting the localization of the motor centres of "the arms, face, leg and half body, are numerously cited from French, "English and American medical papers, and the following are speci- "mens of the results which Professor Marcacci reaches:—

"The cases of brachial monospasm hitherto reported do not serve "in the slightest degree to establish the localization of the motor centre "of the arms, especially if we accept Ferrier's scheme (p. 85).

"Here are two cases which, though reported by Ferrier, are simply "against him (p. 85).

"That it is possible to have facial paralysis without the lesion which "Ferrier indicated, we have already seen. . . . Here are two cases "in which the lesions occupied precisely the seat indicated by Ferrier, "but in which there was no paralysis of the face whatever (p. 89).

"After citing many more such failures, Professor Marcacci remarks, "We might continue thus indefinitely, and we should find in all cases

"as little exactitude. . . ." He proceeds to criticise Ferrier's doc-
"trine of the latent zone, and refers to 167 cases, of which the majority
"seem to refuse most positively to fit Professor Ferrier's theory, on any
"plea whatever. In conclusion (p. 100), Professor Marcacci lays
"down the following results of his exhaustive inquiries :—

"1. That pathological observations do not authorize us to admit
"motor centres in the human brain.

"2. That hemiplegia of cortical origin may arise from various
"lesions, and not only from that of the common centre.

"3. That lesions of the "latent zone" may occasion motor
"troubles, and lesions of the "motor zone" occasion nothing of the
"kind; in other words, that the "motor zone" may be latent and
"the "latent zone" be motor.*"

This view—that the so-called "motor zone" is not really motor—has recently been reaffirmed by Vulpian,† while it is well known that so eminent an authority as Brown-Séquard is quite opposed to the current tendency, to premature application of the localization theory to diagnosis of brain tumors.

Now, with regard to the removal of the brain tumor from the patient in the London Hospital for Epilepsy and Paralysis—which is announced with such a flourish of trumpets by Dr. Keen, as being due to the vivisectional experiments of Ferrier—if he had wished to come over to our side, and advocate the fallacy of conclusions drawn from these experiments, he could scarcely have done so more effectually than in citing this case, since the physicians who removed the tumor, *did not* find it where it was stated to be by Dr. Hughes Bennett, who made his diagnosis on the basis of Ferrier's experiments. The London *Lancet* of November 29th, 1884, says, when speaking of the case, "The tumor was diagnosed as lying in the upper part of the fissure of Rolando, but was subsequently found under the gray matter of the ascending frontal convolution," the one place being separated from the other by at least the whole breadth of the anterior ascending parietal convolution.

Moreover, the patient from whom the tumor was removed, soon afterward died, so that in no light can the affair be considered anything but an utter failure; yet this, we are told, is one of the debts that we owe to vivisection.

The editor of the Philadelphia *Medical Times*, when speaking of this case, in an editorial published about a year ago, says, "It "would be a bold surgeon who would operate in a case, with nothing

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* *Zoöphilist*, October, 1883. † *Medical Times*, June 27th, 1885.

"more to guide him than the information afforded by the study of "motor centres in the brain made by Hitzig and Ferrier—especially if "he were at all familiar with the writings of Brown-Séquard upon the "same subject."*

Dr. Keen next proceeds to speak of the labors of Koch, Pasteur and others, in the experimental study of the minute organisms called microbes, and of the theory of Koch, that consumption is caused by the "bacillus tuberculosis" and cholera by the "comma bacillus.' This last alleged discovery has been received with considerable doubt by the scientific world from the moment of its publication, and the investigations made during the late cholera epidemic in Europe, have tended to greatly weaken the little faith yet remaining in its probability. Dr. Keen himself says that Koch's views have met with the "opposition of prominent scientists." How, then, if there is such incertitude as to their reliability, can he claim them as one of "our recent debts to vivisection?" As to the "classic" experiments, as Dr. Keen calls them, of Thiersch, in 1853, in inoculating mice with choleraic discharges, what had they to do with the "comma bacillus," or of what use were they? Did not all physicians know, prior to that time, that if unwholesome and impure matter was taken into the system, the result would be either to sicken or to kill the organization receiving it, whether that of man or of the lower animals? But to return to the alleged discoveries of Koch; admitting them to be true, how little good have they accomplished. With regard to his well-known theory of the microbe called the bacillus tuberculosis, as the cause of consumption, all that even Dr. Keen himself can show, as already resulting from his discoveries, is that "they have enabled us to recognize, by the microscope, doubtful cases in their earlier and more remediable stages, and have made certain what was hitherto only a probability, viz., that consumption is distinctly contagious." That they have taught us any way of lessening the mortality from this dread disease, I have never seen stated. These results—admitting that they are reliable, do not seem to us of enough grandeur to compensate for a tithe of the sufferings inflicted upon the animals used for the experiments. The other great microbe discoverer, Pasteur, is open to severe criticism—or so, at least, his fellow-worker, Koch, seems to think, judging from an article which was written by him, and which appeared in the *Semaine Médicale* of the 21st and 28th of December, 1882. In this article, Koch, who is certainly likely to be better authority on the subject than any one else, accuses Pasteur of being a mere plagi-

* Phila, *Medical Times*, Vol. xv, p. 357.

arist—going from Congress to Congress and Academy to Academy, announcing new diseases and germs as his own discovery, which had previously been discovered by others and published long before Pasteur had done anything in the same direction. As a case in point, he instances that the working out of the causation of charbon (splenic fever), which Pasteur claims as his own, and for which he has received the highest honor, was really the work of Koch himself, who had completed and published the whole account in 1876, although Pasteur's first note on the subject was only published in 1877.* The article is very long, and contains a number of charges against Pasteur, all of which appear to be clearly proven; but I think that I have quoted enough to weaken your confidence considerably in anything that Pasteur can claim as one of his discoveries. It is true that if any real benefit is gained for the human race, it makes little difference by whom it is discovered; but the fact that Koch attacks Pasteur, and says that he has not made any discoveries of importance, and that other scientists, notably your own teacher, Dr. Formad, attack Koch, and say that *he* is mistaken in his theories, and that his conclusions are entirely incorrect, ought, I should think, to dispose you to reject what they both say, or to maintain, at least, that animals ought not to be tortured for the purpose of gaining results that are, after all, so vague and uncertain.

After having finished with the subject of microbes, Dr. Keen introduces that of inoculation, and cites the experiments upon guinea pigs with the virus of yellow fever, by Mr. Freire, of Rio Janeiro, and his subsequent experiments upon himself and a number of others, also on two hundred wharf laborers, by inoculation with the attenuated virus, obtained from the guinea pigs, which resulted most successfully. Dr. Keen thinks that if this method continue to prove efficacious, as he believes it will, a glorious result will follow during the next epidemic of yellow fever. I am as anxious as Dr. Keen that something should be found to check the ravages of yellow fever, but I will confess to being skeptical as to all these discoveries that involve the inoculation of animals and the passage of diseases through their bodies before communicating them to the human race. There are, as yet, but one or two well authenticated cases of Pasteur's alleged discovery of inoculation by the attenuated virus of rabies being applied to the human race, and it seems that this discovery of inoculation by the attenuated virus of yellow fever has not yet been proved to be a success, to

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* *Zoöphilist*, Feb. 1st, 1883.

more than an extremely limited extent. Does it not seem, then, a little premature, and therefore unfair, for Dr. Keen to cite these among the "debts that we owe to Vivisection?" In view, too, of his frequent sanguine predictions that glorious results will follow from certain real or alleged discoveries, I will cite to you the case of the eminent scientist, Professor Tyndall, and the mistake into which he fell.

"A few years ago, he announced in the London *Times*, that Dr. "Klein had just discovered the germs causing typhoid fever in the "pig, and prophesied with temerity that that disease would soon be "under control. The sequel to the story is that, after Klein's vaunted "researches had been published in the Royal Society's Transactions, "together with a plate of the morbid (sic) organisms, it was found out "that these organisms consisted only of albumen coagulated by alco-"hol, and Dr. Klein was compelled, by the exposure of the deceptive "nature of his germs, afterwards to publish in the 'Transactions' a "recantation and withdrawal of all he had previously published, and "Tyndall had trumpeted." *

Dr. Keen's list of great discoveries (?) does not continue much further. The only one which he enters into at length, and to which I propose to reply (as this article has already gone beyond the limits intended), is one with which you are all familiar, under the name of Listerism, or the antiseptic method. I will here make a last quotation from Lawson Tait. If I seem to have recourse often to his writings, it is because we could have no greater authority than he, in all matters relating to abdominal surgery. In the early part of the year 1882 he read a paper before the Surgical Society of Ireland, on "Listerism," so long that I can only make a few extracts from it, but these may serve to give you a pretty fair idea of what his opinion is of most of the vaunted discoveries of Sir Joseph Lister. In the first place he says:—

"Putting theory and private conviction aside, and influenced solely "by a verdict which seemed almost unanimous, and by the surgical "conscience which obliges us all to do everything we can, and to use "everything we know, for the welfare of our patients, I gave Mr. "Lister's method a trial, which extended over a series of abdominal "sections. This was composed of nearly a hundred operations, sixty "of which were for the removal of ovarian tumors, and the detailed "results of most of them, and many others, are given in the paper "I have already alluded to. The conclusions of the figures, which

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* Physiological Fallacies, No. 11.

"were not, and so far, have not been, challenged, were in every way "against Mr Lister's practice; and the influence which has been exer- "cised by this paper has been considerable, in modifying the views "of a large number of competent authorities upon this important "subject.

"I announced in that paper that, having come to the conclusion "that Mr. Lister's system, when completely used, was prejudicial to "my patients, not only in the question of mortality but in the speed "and evenness with which they recovered, I should further inquire "into the influence of the method, etc."

Lawson Tait goes on to say that, having begun his operations with the use of the carbolic spray, he soon came to the conclusion that it was absolutely injurious, and eventually rejected carbolic acid altogether, using only a spray of steam from ordinary hydrant water, which proved very successful. He then says, "This research occupied "nearly two years, and all through that time I was carefully on the "watch for either symptoms or results which would arrest me in my "experiment and show me that I was in error, and that I must retrace "my steps and reëstablish Listerism in my practice, but I found none. ". . . Suffice it to say that, since the time when I may be said to have "abandoned the practice of Listerism, I have performed 107 com- "pleted operations for the removal of ovarian tumors, and of these "there have been only three deaths, or a mortality of 2.08 per "cent."

You can now see what is thought by this great authority of the carbolic spray, at least, in ovariotomy operations.

But Dr. Keen says, as if resolved to meet every difficulty that may arise, "We may reject carbolic acid and the spray." Reject carbolic acid and the spray!!! Then we reject what many high authorities consider the essence of Listerism. Lawson Tait concludes by saying that Sir Joseph Lister has done much good by careful attention to details, but for a knowledge of the fact that this careful attention to details, was so vitally necessary to success, we are not indebted to experiments on animals. He gives him no such extravagant praise as is bestowed upon him by Dr. Keen, nor does he mention any of the benefits which Dr. Keen claims that we owe to his discoveries.

I will, in passing, say a few words in reply to what Dr. Keen mentions of Pasteur's experiments with the attenuated virus of rabies, to which I have already alluded. This discovery has just been applied to man for the first time, and it has not yet been positively demonstrated whether it will prove a success or not; but admitting that it may be, I will show you at what a cost of animal suffering the virus is

obtained.* Its efficacy lasts a very short time—only a few days—so that it is always necessary to have a succession of dogs inoculated with rabies, in order to obtain fresh virus. It is also necessary to have a constant succession of either rabbits or monkeys, with their skulls opened and the virus injected therein, in order to obtain the necessary attenuation. An adoring disciple of Pasteur, visiting his laboratory, gives this description of it: "Isolated in round and well-secured cages are the mad dogs. Some of them are already at the stage of furious madness—biting the bars, devouring hay and uttering those dismal howls which no one can forget who has once heard them. Other dogs are still in the incubating period, and still caressing, with soft eyes, imploring a kind look."†

Does not this seem too painful a method of obtaining exemption from hydrophobia for the human race, in view of the fact that the disease is so rare that it is said that one hundred deaths occur from lightning to one from hydrophobia, and of those who are bitten by mad dogs only twenty out of every hundred are likely to have the disease.

I have now answered in detail, one by one, all, or nearly all of the claims which Dr. Keen makes on behalf of vivisection, but I have not yet done. I now propose to reply to his main or general argument, by asking why, if such immense gains have been made during the last twenty-five years, as he alleges, is there so little diminution in the mortality of the principal diseases that afflict mankind?

In an article which appeared lately in *Lippincott's Magazine*, entitled "Vivisection, is it Useful or Justifiable?" by Dr. Alfred J. Leffingwell, he says:—

"If scientific evidence is worth anything, it points to the appalling conclusion that, notwithstanding all the researches of physiology, some of the chief forms of disease exhibit to-day, in England, a greater fatality than thirty years ago. In the following table I have indicated the average annual mortality, per million inhabitants, of certain diseases, *first*, for the period of five years from 1850 to 1854, and *secondly*, for the period twenty-five years later, from 1875 to 1879. The authority

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*It seems wonderful that sensible people can be so carried away by Pasteur's investigations in regard to hydrophobia, which have as yet amounted to no practical value, since he has not cured one case of declared hydrophobia. He claims to have prevented it in the persons whom he has inoculated, but who can show that they would ever have had the disease, since it had not manifested itself in any way, at the time they first applied to Pasteur. Moreover, the intelligence has just arrived, since the above was written, that his cure has failed when applied to a case where hydrophobia had begun to be developed. The *Zoöphilist* of Jan. 1st, 1886, mentions that a little girl of nine years of age, who was taken to him, and whom he inoculated, died not long afterward, of the disease.

†Paris *Figaro*, Jan. 26th, 1884.

is beyond question; the facts are collected from the report to Parliament of the Registrar-General of England:—

Average Annual Rate of Mortality in England, from Causes of Death, per One Million Inhabitants.

NAME OF DISEASE.	During Five Years, 1850-54.	During Five Years, 1875-79.
Gout,	12	25
Aneurism,	16	32
Diabetes,	23	41
Insanity,	29	57
Syphilis,	37	86
Epilepsy,	105	119
Bright's disease,	32	182
Kidney disease,	94	114
Brain disease,	192	281
Liver disease,	215	291
Heart disease,	651	1,335
Cancer,	302	491
Paralysis,	440	501
Apoplexy,	454	552
Tubercular diseases and diseases of the respiratory organs,	6,424	6,886
Mortality from above diseases,	9,026	10,994
Mortality from all causes whatsoever	22,299	21,250

"This is certainly a most startling exhibit, when we remember that from only these few causes about half of *all* the deaths in England annually occur, and that from them result the deaths of two-thirds of the persons, of both sexes, who reach the age of twenty years. What are the effects here discernible of Bernard's experiments upon diabetes Of Brown-Séquard's upon epilepsy and paralysis? of Flint's and Pavy's on diseases of the liver? of Ferrier's researches upon the functions of the brain? Let us appeal from the heated enthusiasm of the experimenter to the stern facts of the statistician. Why, so far from having obtained the least mastery over those malignant forces which seem forever to elude and baffle our art, they are actually gaining upon us; every one of these forms of disease is more fatal to-day in England than thirty years ago; during 1879 over sixty thousand *more* deaths resulted from these maladies alone than would have occurred had the rate of mortality from them been simply that which prevailed during the benighted period of 1850 to 1854! True, during the later period there has been a diminished mortality in England, but it is from the lesser prevalence of zymotic diseases, which no one to-day pretends to cure; while the organic diseases show a constant tendency to increase.

Part of this may be due to more accurate diagnosis and clearer definition of mortality causes ; but this will not explain a phenomenon which is too evident to be overlooked."

Before bidding adieu to Dr. Keen's address, I wish to notice the last sentence but one which occurs in it, commencing as follows: "The sentiments of our own profession, so constantly and conspicuously humane, are always against inflicting pain, etc." As a set-off to this gratifying tribute paid by Dr. Keen to himself and his fellow practitioners, I feel called upon to cite some of the replies made by Dr. Emmanuel Klein, one of the noted European Vivisectors, before the English Royal Commission, appointed to investigate the subject of Vivisection :—

Question.	Answer.
3538.—What is your own practice with regard to the use of anæsthetics in experiments that are otherwise painful?	Except for teaching purposes, for demonstration, I never use anæsthetics, where it is not necessary for convenience. If I demonstrate, I use anæsthetics. If I do experiments for my inquiries in pathological research, except for convenience sake, as for instance, on dogs and cats, I do not use them. On frogs and the lower animals I never use them.
3539.—When you say that you only use them for convenience sake, do you mean that you have no regard at all to the sufferings of the animals?	No regard at all.
3540.—You are prepared to establish that as a principle you approve?	I think that, with regard to an experimenter, a man who conducts special research and performs an experiment has no time, so to speak, to think what will the animal feel or suffer. His only purpose is to perform the experiment, to learn from it as much as possible, and to do it as quickly as possible.
3541.—Then for your own purpose, you disregard entirely the question of the suffering of the animal, in performing a painful experiment?	I do.
3546.—Do you believe that it is a general practice on the Continent to disregard altogether the feelings of the animals?	I believe so.

From this evidence of Dr. Klein's, it is obvious what sort of a man he is; that, as far as any feeling for animals is concerned, he might as well be a stone; and he says that the physiologists of the continent, among whom are many doctors, are like him. If these are what Dr. Keen calls humane men, he must use a different lexicon from ours.

I have considered these claims which Dr. Keen has made, so far, solely in the light of whether the discoveries to which he alludes, have been of any real use to mankind or not, and I hope that I have succeeded in proving, to your satisfaction, that, as a general thing, they have not been of use. But I have said nothing of the objections that can be made to Vivisection in a moral point of view, nor have I alluded, except incidentally, to the almost indescribable barbarities which are the natural result of unchecked experimentation upon animals, and which always will be the result, to a greater or less extent, where men are left at full liberty to carry out their investigations in any manner they please, free to gratify every caprice of a sometimes morbid fancy, every experiment suggested by an unnatural and diseased curiosity, as in the case of Dr. Mantegazza, of Florence, who made a series of experiments upon rabbits and guinea pigs, to find how long they would live when kept without food, and subjected every day, while in a starving condition, to the most severe torture that he could devise. He constructed an instrument for the purpose of giving the most atrocious pain it was possible to inflict, and to this the poor little animals were subjected at intervals of a few hours, until they died from the combined influence of torture and starvation, some living six days, some seven, and others still longer. Dr. Mantegazza, does not appear even to have pretended that these experiments were for the benefit of mankind or for any good purpose whatsoever.

You may say, though, that he was a foreigner, and not one of our English-speaking nation, from whom you would look for better things. I am afraid it must be admitted, however, that things almost as barbarous as this have been done, over and over again, in Great Britain, particularly in Edinburgh.

The *Medical Journal*, of that city, for 1868 and 1869 gives an account of thirty dogs that were set on fire after being rubbed with oil of turpentine, or scalded by pouring boiling water a number of times in quick succession over some of the vital organs. The result was that all the dogs died; some in a few hours, and some not for five days. They were narcotized to some extent at the time of the burnings and scaldings, but what must have been the sufferings of those which lived for several days after the influence of the narcotic had passed off?

Dr. Wickham Legg, in the ninth volume of St. Bartholomew's Hospital Reports, gives the history of his experiments upon sixteen cats, opening each one by making an incision in the abdomen, and tying the bile-ducts, for the purpose, apparently, of seeing what would be the effect upon the system of depriving it of bile. The result was the same in all the cases, viz., that the cats died.

Another dreadful set of experiments was performed by Professor Rutherford, of Edinburgh, upon a number of dogs, for the purpose of ascertaining the effect of calomel upon the liver. In these experiments, also, the animals were cut open, the common bile-duct dissected out, divided and a glass tube inserted therein, and the wound in the abdomen then closed up. Dr. Wall, in speaking of these experiments, says, "If any one has witnessed the symptoms of the passage of a gall-stone down the bile-duct in the human being, let him picture, then, a dog suffering more than this awful agony, often for eight hours, and he will have some faint conception of what Vivisection is."

In this country, also, not only is there a great deal of vivisection, but cruel experiments are frequently made. Within a few years, some of Magendie's most inhuman and objectionable experiments have been performed in New York, by way of class illustration. Some years ago, a physician here in Philadelphia, who was assisting a well-known physiologist of this city in carrying on a series of investigations on dogs, told me himself that it was dreadful to witness the sufferings of the animals upon which they experimented.

An experiment sometimes performed by physiologists is, to starve animals to death, and watch them in their dying agonies for certain symptoms, such as they hope or desire to find.

But the list of horrors perpetrated "for the advancement of science and in the interest of the human race" is so long that we do not care to make any further quotations from the gruesome annals of Vivisection. Of some of the worst experiments, such as those upon the nerves, in recurrent sensibility, etc., I have said nothing, for the task is too painful.

Mr. Mark Thornhill, in his book entitled "The Clergy and Vivisection," when speaking of the Report of the Royal Commission (which I have previously mentioned), says, "The Report contains the record of hundreds of experiments and allusions to thousands. The details of several are elaborately discussed. They are such that when their meaning is realized, it is sufficient to make the blood almost curdle with horror. They suggest agonies so fearful, so protracted, as to cause one to wonder how man could inflict them, how

nature could endure them—if it be not irreverent to say so—how Providence could permit them."

What, then, do you suppose, must be the effect of such cruelties upon the moral nature of those who practice them? Such a thing is, of course, possible, as for a man to be a vivisector and continue humane, a notable example being that of Sir Charles Bell, who, in the latter part of his life regretted extremely the experiments that he had thought it might be right to perform, in the beginning of his career, although he had always many doubts upon the subject; but such instances are, I believe, very rare, particularly upon the continent of Europe.

Miss Frances Power Cobbe, when speaking of Dr. Mantegazza, says, "How much benefit to mankind would be necessary to counterbalance the leprosy of that man's soul?" and this remark might, we fear, be found applicable to many others.

Dr. Houghton, in his evidence before the Royal Commission, said that he "would shrink with horror from introducing students into laboratories to witness painful experiments which would demoralize them. Science would gain nothing," he said, "and the world would have let loose a set of young devils."

In view of this almost inevitable demoralization consequent upon cruel and unrestricted vivisection—vivisection as it now exists in every civilized country in the world but England—as well as in view of all the other facts that I have placed before you, will not all of you who read this appeal, resolve that you will no longer support such an iniquitous and atrocious system, or even remain passive or indifferent in the matter; but that, putting on the armor of Righteousness, you will go forth boldly to do battle against the enemies of Mercy and Humanity, and that you will not falter or grow weary in the conflict while life and strength are spared to you. I do not ask you to urge the total abolition of experiments upon animals, if you are satisfied that they are sometimes of benefit to mankind, but that, wherever you may be settled, you will agitate the subject unceasingly until you have obtained a law that will render impossible such scenes as may to-day be witnessed in the laboratories of nearly every part of the civilized world—scenes so atrociously cruel that they would disgrace a Sodom or Gomorrah—a law that will, in short, put a stop to the abuses of Vivisection. The English law has done a great amount of good, but even under that Act many cruel experiments may be performed. Dr. Henry J. Bigelow, of Boston, and Dr. Leffingwell, both of whom I have quoted, unite in believing that vivisectional experiments should be restricted to those that can be performed entirely without pain, by the thorough administration of anæsthetics and the killing of the animal operated upon,

before the return of consciousness. To accomplish this may be a difficult matter, but I beg you, at any rate, to remember that, where in doubt between two courses, it is always better to choose that which inclines the most to the side of Humanity, and remember, also, that there is a sentence in Holy Writ, spoken by lips that we all love and revere, which makes light of any physical advantage gained at the cost of a moral deterioration, and says, "If thy right hand offend thee, cut it off and cast it from thee; for it is better that one of thy members should perish than that thy whole body should be cast into hell." This is not the doctrine of Vivisection.

CAROLINE EARLE WHITE.