The healing art and the claims of vivisection : a lecture delivered at Cambridge, March 10th, 1890 / by Edward Berdoe.

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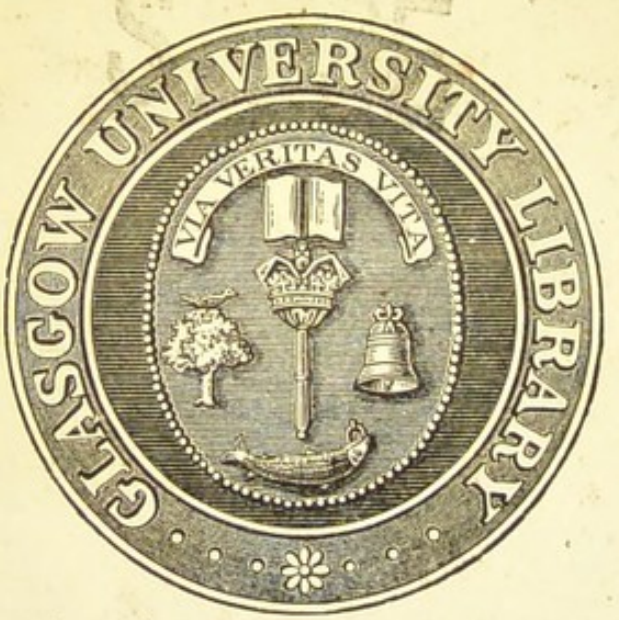

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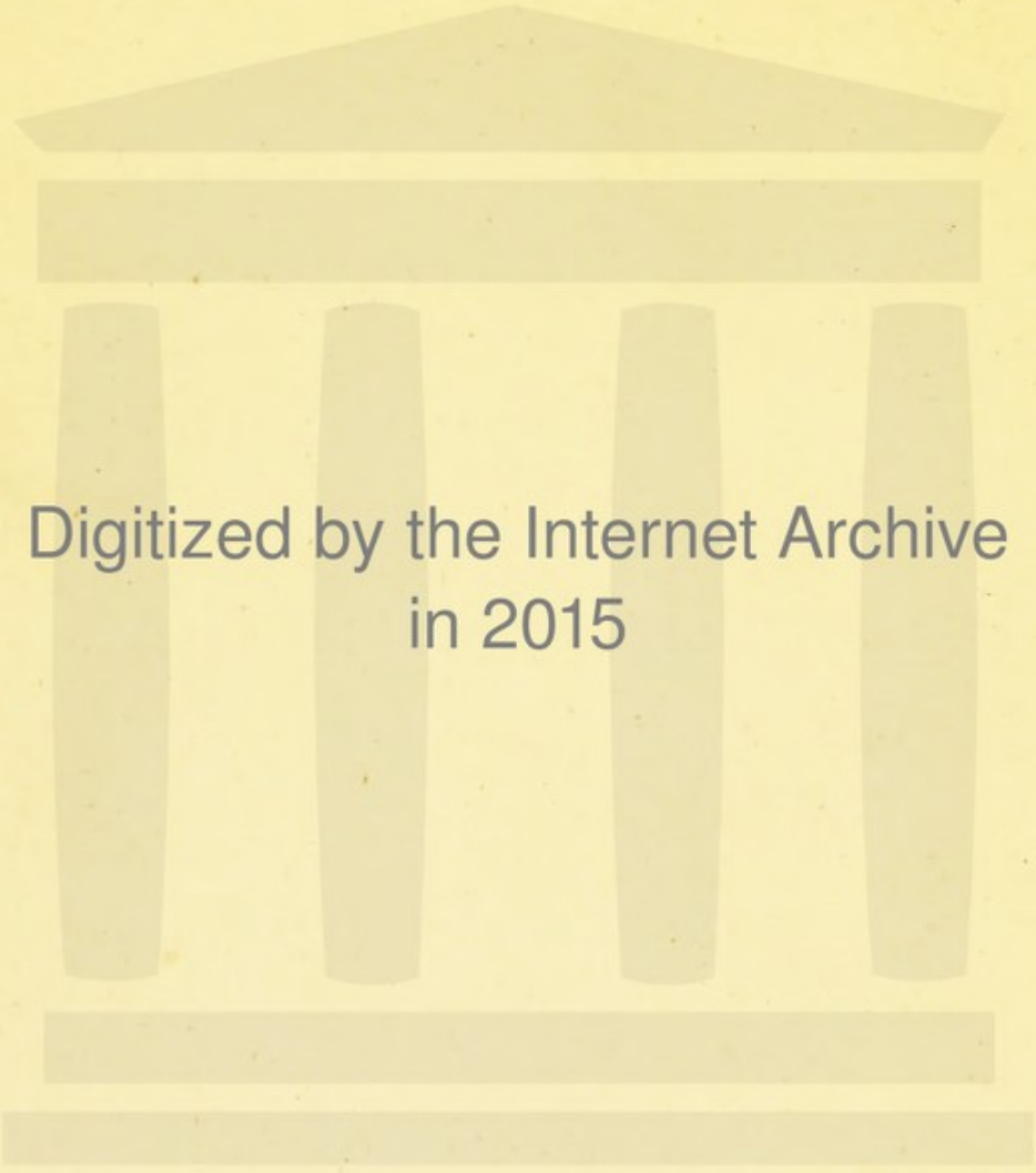

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The Claims of Vivisection.

A Lecture Delivered at Cambridge,
March 10th, 1890,

BY

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THE HEALING ART

And the Claims of Vivisection.

A large and important section of the community is opposed to painful experiments upon living animals for scientific purposes. The public opinion which has been awakened upon this question is neither incompetent nor ill-informed. A number of the Bishops and Clergy, of Ministers of various religious denominations, of men and women of letters, members of Parliament, and thinking persons of all sorts, have studied the matter and have banded themselves into Societies for the total suppression of what is known as Vivisection. It is not necessary that all these friends of the cause I am here to advocate, should have studied medicine, surgery, and physiology, so as to be competent to discuss the matter on its scientific side, because the chief point on which we base our objection to scientific animal torture is the ethical one. As Christians, we say that the animals share in the love and care of the Father of all living creatures; they are as ourselves the creatures of His hand, their bodies are framed in many ways like our own, and they claim our protection when they are not the enemies of our kind. In the

words of Robert Browning, himself an uncompromising opponent of vivisection,

"God made all the creatures and gave them our love and our fear,
To give sign, we and they are His children, one family here."

But the opponents of vivisection are quite competent to judge, even without any special medical training, that a practice about which so many adverse things have been said, even by those who are its advocates, cannot be founded on a very firm scientific basis. Whilst, therefore, we must admit that what is termed the scientific world is largely against us, we have no lack of unassailable scientific authority for our contention that in relation to medical science—for which only, in this country at least it is maintained that vivisection is indispensable—the claims advanced on behalf of vivisection are usually greatly exaggerated and unsupported by the facts.

In considering the question of the value of vivisection, we must not omit to explain at the outset, that even if we grant that it occasionally leads to discoveries, we consider it unfair to attribute them to vivisection if they could have been arrived at by observations on human beings. To put a plaster-of-Paris jacket on a guinea-pig or rabbit, as was done the other day, and so constrict its breathing and other organs as to kill it, is not to discover the evils of tight-lacing by experiments on living animals. These evils have all been recognised before, and the *post mortem* table will unhappily continue to exhibit them abundantly. To grow a living rat together with a cat by bringing and fastening together por-

tions of the bodies of each, is to make no new discovery by means of vivisection, though perhaps the experiment had never previously been tried in the same manner. We already knew that portions of flesh transplanted to the surface of a wound will grow there.

Within the past few years a small, but very influential school has arisen within the ranks of the medical profession, which aims at dominating it, and which claims that it can only be raised from empiricism by the efforts of the experimental physiologist and pathologist. But the highest authorities in medicine incline to the opinion that therapeutics will never be liberated from a certain cultivated empiricism, because it is almost impossible to find precisely the same conditions of temperament, idiosyncracy, and other qualifying circumstances in different cases. Professor Wood therefore says in the preface to his great work on Therapeutics: "The old and tried method in therapeutics is that of empiricism, or if the term sounds harsh, of clinical experience."

When we say that these experiments upon animals are useless for the advancement of medicine and surgery, we do not mean to imply that they have no abstract scientific value. What we do mean is that not only have they failed hitherto to lead to any important discoveries in either of these sciences, but that they have actually proved to be misleading. You may dissect out the great arteries from living frogs, and in that condition keep them alive for four or five days, as Mr. John G. Adami did recently at the New Museum. You may insert glass chambers into the bowels of living rabbits

and guinea-pigs, keeping them in that position for periods varying from four hours to eighteen days, as was done by Dr. Charles A. Ballance last year. You may open the chests of living cats and apply your stethoscope to the exposed hearts of living dogs, as was done by Dr. James W. Barrett in 1885. You may open the abdomens of living cats, guinea-pigs, and rabbits, and apply irritating chemicals to their exposed intestines, causing what you are pleased to term "peculiar rhythmic movements" and "circus movements," but what the unlearned would call violent spasms and convulsions, as was done by Dr. Batten and Mr. Bokenham at St. Bartholomew's Hospital last year. You may dissect out the kidneys of living dogs and cats which you have first paralysed by curare — the "hellish oorali" of Lord Tennyson's poem, so called because the animal's sufferings are intensified by its use, and it is unable to move a limb, or to bite, scratch, howl, or otherwise interfere with the operator's comfort. You may do this, as was done by Dr. John Rose Bradford, at University College, London. You may dissect out the secretory and trophic nerve fibres from the glands under the jaws of living dogs and cats, as did Dr. Bradford in 1887. You may infect 90 cats with cholera poison, and bake numbers of them alive, as did Dr. Lauder Brunton. You may expose the hearts of frogs and stimulate them with electricity, and cut out the intestines of living dogs to demonstrate what was known long before, after the manner of Dr. Theodore Cash, at Aberdeen University. You may

inoculate the eyes of rabbits and guinea-pigs with the material of tubercle, fix glass balls filled with croton oil—a horribly irritating drug—and stitch them into the muscles of the backs of rabbits, then crush them amongst their tissues, as did Dr. Watson Cheyne, at King's College, London. You may perform operations with the view to cause hideous diseases in rabbits, as did Mr. Frederick Eve, at the Brown Institution, in 1888. You may slice, plough, burn and pick away the brains of monkeys and dogs, as did Dr. Ferrier. You may slowly starve to death animals whose vagi nerves have been cut and stimulated by electricity, as was done by Dr. Gaskell, of this University, in 1878. You may extirpate the thyroid gland from the throats of living monkeys, then cut away portions of their brains, open their spinal columns, and in a score of other ways mutilate and injure their frames, after the method of Mr. Victor Horsley, between 1886 and 1888; or cut out the spleens and livers from living rabbits, pigeons and ducks, as was done by Dr. William Hunter, of St. John's College, Cambridge, in 1888, or do a thousand other acts which in a costermonger or a farm labourer would be termed and dealt with as acts of atrocious cruelty punishable by imprisonment. But, though you will have been enabled to write numberless papers for scientific societies, and perhaps have won medals, scholarships, prizes, or even a Fellowship of the Royal Society—the plain unvarnished fact is that you have not advanced the practice of medicine or surgery by a single step! You have not learned the cure for a single malady

which afflicts the human body. You have not reduced the length of time which a patient languishes in, say, typhoid fever, scarlatina, or small-pox, by a single day. You have not learned how to cure gout, jaundice, cancer, or sciatica. We can do no more for these ailments than we could before your experiments were begun. With all our new drugs, with our countless volumes of medicine issued annually from the press; with the unbounded activity which animates the medical world, it is actually the fact that the homeopathic practitioner claims—not without some show of reason it must be admitted—to have been just as successful with his billionths of a grain of chalk, or his trillionths of a grain of burnt sponge, as the Hierarchy, the arch confraternities of Medicine, with their costly laboratories, their innumerable researches, their countless victims, living, suffering brutes tortured day after day for weeks and months. But you have learned certain facts in physical science. You have discovered at precisely what rate the blood travels in the veins of a guinea-pig. You know precisely what force is required to send the blood of a horse up a tube inserted in one of its arteries. Your knowledge of hæmadynamics is increased; but though Stephen] Hales, in 1708, probably as the result of his experiments, came very near the truth, the question is not even yet perfectly mastered in all its bearings. The hæmadynamics of the horse, dog, or rabbit have only the remotest interest for the practising physician; for, as Dr. Lauder Brunton says, the central factor, the heart, varies in different animals. In the rabbit the heart normally beats

rapidly, so that section of the vagi does not cause any great increase in the number of beats, nor is the blood-pressure much raised thereby. In the dog, on the other hand, the beats are considerably increased by section of the vagi, while the blood-pressure rises considerably. How is all this to be made available for the guidance of the bedside practitioner? It is a matter of interest perhaps to the man of science to know that the suffering of great pain causes an animal to give out an increased amount of carbon dioxide, but there is no practical advantage for the physician to know that this is so. To know that when a rabbit is at rest it gives off less $C O_2$ than when its hind limbs are tetanised may interest the chemist, but it is not necessary for the physician. If the rabbit were a test tube there would be no possible objection to the experiment, but a highly organised animal with a claim on man's protection, is a very different thing from a test tube. When, therefore, Professor Mantegazza crucified a number of animals by driving nails into their feet with a view to test the amount of carbonic acid they gave off under varying degrees of agony, we say that no scientific advantage could justify such uses of a sentient being.

We are frequently told that by opposing experimental physiology we are standing in the way of science, that though perhaps it may be granted that not much, if any, benefit is likely to accrue to practical medicine by such a method of research, still, pure science is benefited by the practice which we are opposing. Foreign experimenters are much in the habit of arguing in this manner, and their honesty, at any rate, is to be

commended. They at least do not pretend that they do these things with any idea of advancing the Healing Art; they work for Science alone without prospect of definite gain. Now, of course, there would be nothing to say against this if we were dealing with chemistry, astronomy, or mathematics. When astronomers go to immense pains to send out expeditions to the ends of the earth to observe certain phenomena, as the recent eclipse of the sun, carrying telescopes, spectroscopes, and photographic appliances merely to aid in the observation of a phenomenon which hardly lasts five minutes, and with a probability that cloudy weather will hinder even that short view, nobody objects to all that trouble and expense, though no thought of any practical benefit enters into the calculation. Nobody suggests that the art of navigation will be improved by the observation of a total eclipse of the sun. As the Astronomer Royal for Ireland recently said, "There may be some infinitesimal influence on commerce in view, but it is too small to be appreciable. The true objects which astronomers seek by such observations are entirely of a scientific description." You may be inclined to ask, Why seek to interfere with the physiologist any more than the astronomer? We reply that humanity forbids.

Some of the most cruel and prolonged experiments on dogs, guinea-pigs, rabbits, and the smaller animals, are performed for the ostensible purpose of assisting our efforts to improve our knowledge of the action of drugs upon the human body. In Mr. Erichsen's Report on Vivisection for 1887, he said, that "The therapeutical experiments were 280 in number. These

have been made, in the case of new drugs, either with the view of justifying the further extension of such remedies to man, or of enlarging their present sphere of usefulness; in the case of some of the old drugs, with the view of inquiring whether their action is such as to justify their continued administration for the purposes for which they have hitherto been used." For many years past the *Lancet* and *British Medical Journal* have contained almost weekly long and elaborate reports of experiments with new and old drugs, but at the present moment there are very few of them concerning which our knowledge of their physiological action is not in the most bewildering state of chaos. The difficulties seem to increase with our investigations. Not only do drugs act differently on man and the lower animals, but they act in the most diverse manner on different animals, and even upon different species and varieties of these, as for instance, a field mouse and a house mouse. Not only this, but the action is often modified by climate and the time of year at which the experiment is performed. When using frogs, for instance, it is not enough to remember that drugs act differently upon *Rana temporaria* and *Rana esculenta*, but that spring and autumn will show different results. Even supposing we could get over the difficulties accruing from these causes, we should still have to take into account sex-indiosyncracies, the influence of excitement, fright, pain, and other modifying circumstances. Cases have been recorded where poisoning of whole families has taken place from eating the flesh of animals caught in traps in which they had died in great

pain. Animals, when brought into the physiological laboratories, often exhibit marked signs of excitement, terror, and alarm. Dogs have been known to beg before their tormentors, hoping to escape injury by appealing to them in a way they have found to be effectual under other circumstances. Jaundice has been frequently caused in the human subject by severe fright, and though animals are sometimes said not to suffer from the anticipation of coming injuries, it is possible if they could read papers they might have something to say in opposition to that assumption.

Hares killed by coursing are said often to undergo very rapid changes after death, severe symptoms having been produced when they have been eaten. When dogs are kept in confinement in a dark vault for several days, half starved, howling dismally all the time, then taken into a laboratory, struggling violently for liberty, fixed into an apparatus which restrains their movements, muzzled, and strapped down, it is considered a scientific proceeding by our physiologists to test the effects of drugs upon the system of these animals and attempt to argue from these as to their probable effect on a human patient lying calmly in a luxurious bed. But our physiologists proceed to give chloroform, curare, or other anæsthetics, and narcotics to the animal, then to dissect out, say the kidneys, partly remove these organs from the living body, inject such drugs as digitalis into the animal's veins, and watching its influence on the organs in question, pretend to guide the practising physician with regard to the influence of that drug on the same organ in his human patient. It

requires no technical training to see that for clinical purposes such a method of laboratory experiment must necessarily be valueless because you can never get your equation. I do not say that the experiments are destitute of scientific interest. I do say that the physician who should attempt to convey the teaching of such experiments to the practice he adopted at the bedside of his patients would be a fool, if not a criminal. Professor Huxley very emphatically laid this down in the course of his controversy with Mr. Herbert Spencer in *The Times* last autumn.

"Mr. Spencer assumes that in the present state of physiological and medical science, the practitioner would be well advised who should treat his patients by deduction from physiological principles ('absolute physiological therapeutics,' let us say) rather than by careful induction from the observed phenomena of disease and of the effects of medicines.

"WELL, ALL I CAN REPLY IS, HEAVEN FORBID THAT I SHOULD EVER FALL INTO THAT PRACTITIONER'S HANDS; AND IF I THOUGHT ANY WRITINGS OF MINE COULD AFFORD THE SMALLEST PRETEXT FOR THE AMOUNT OF MANSLAUGHTER OF WHICH THAT MAN WOULD BE GUILTY, I SHOULD BE GRIEVED INDEED."—Professor Huxley (Letter in *The Times*, Nov. 18th, 1889).

We can have no greater authority on this subject than M. Claude Bernard, himself a great vivisector. He says: "Not only do the various species of animals differ in this respect, but even individuals of the same species are so far from resembling each other, that they cannot be submitted to the same experiments. So exquisite is the nervous sensibility of

dogs of the higher breeds, that the slightest operations bring on fever, and are attended with alarming symptoms; they cannot, therefore, be employed in researches connected with the gastric juice, the pancreatic secretion, &c., &c. In fact, all operations performed within the abdominal cavity are liable to superinduce inflammation in these highly sensitive animals, and generally prove fatal. In dogs of a more vulgar class how different are the results of similar experiments! During the operation the animal hardly attempts to move, and scarcely seems to suffer; the appetite remains unimpaired. In the horse these differences are, if possible, still more marked. The characteristics of certain breeds are in colloquial language attributed to *blood*. It would be more correct to attribute them to *nerves*. An irritable, sensitive, and highly organised nervous system is, in fact, the essential difference which separates a race-horse from one of those diminutive, half-wild ponies which hilly countries so abundautly produce. Would not the results of the same experiments be entirely at variance in these different animals? And what comparison could we possibly establish between them? It is, therefore, indispensable wherever great powers of endurance are required for the purpose of scientific research to select an animal of the lower breed; if, on the contrary, sensitiveness and nervous irritability appear desirable, none but the nobler kind will afford the requisite qualities. Experiments on recurrent sensibility, for instance, which in the greyhound and pointer are generally successful, if tried on a shepherd's dog would fail in almost every case.

It will, therefore, easily be conceived that a state which in certain animals would constitute actual disease may be perfectly natural in others." In Claude Bernard's twentieth lecture, 1861, he says: "In our opinion, all the experiments hitherto made on secretion require a complete revision; and the opinions expressed in a former part of this course will, perhaps, be contradicted by future experiments." A large and elaborate work on the Pathological and Physiological Researches of the late Mr. T. R. Lewis, M.B., F.R.S., Assistant Professor of Pathology in the Army Medical School, published in London, 1888, has a statement to the same effect. The experimenter laments the fact that chloroform is so very fatal to rats and rabbits, as also to puppies and young dogs, and he says: "Even in large healthy dogs we calculate on losing one in five through this cause alone." Then he goes on to say: "Moreover, the effects produced were of so contrary a character, even under precisely similar conditions, that we feel convinced that any data of this kind obtained by experiments on small and delicate animals are extremely liable to mislead. This is a very unfortunate circumstance, not only because as a rule small animals are more easily obtained and more manageable, but also because the observations on cholera-material hitherto recorded, and which have exercised great influence on the opinion of medical writers and of the scientific world generally, have for the most part been derived from experiments on even more delicate animals than the above referred to." How is it that scarcely any two experimenters ever do arrive at the same conclusions?

Nothing can better illustrate the difficulties with which the physiologist has to contend than a recapitulation of some of the prominent facts concerning the action of the various poisons on men and animals. It will be seen that these difficulties are insuperable to those who maintain that animals and men are constituted so much alike that they can be made to illustrate one another.

Aconite is one of our deadliest vegetable poisons, yet Linnæus says that the plant is eaten by horses with impunity. Experimenters have arrived at quite diverse conclusions as to the action of the drug on the nervous system of the various animals which they have poisoned with it. Rabbits were caused by it to jump vertically in a very peculiar manner, and often to squeal piteously, then to fall into severe convulsions, yet dogs remained without a quiver. The literature of the subject teems with the opposing statements of physiologists on the action of aconite upon the different animals and men experimented upon with this poison. Ringer and Murrell deny the accuracy of the delicate experiments of Liégeois and Hottot. Mackenzie's experiments yielded contrary results to those of Böhm and Wartmann. Dr. Ringer in his *Handbook of Therapeutics*, says that "very diverse statements are made concerning its action on the nervous system."

Even on a question so familiar as alcohol the physiologists are not more in harmony with one another. Zimmerburg experimenting upon cats said that it *lowered the pulse rate*. Dr. Wood replies that he thinks there must be some fallacy

underlying these experiments, and I expect most persons would agree with that opinion.

Dr. Ringer says:—"Observations on the influence of alcohol on the blood and organs have yielded contradictory results, the most recent and elaborate investigations of Parkes and Wollowicz clashing in most particulars with those of previous experimenters."—(*Therapeutics*, p. 274, 5th Ed.)

Dr. Ringer (himself a well-known experimenter) admits that "as physiology fails to guide our steps amid these conflicting statements 'we must rely solely on experience.'" —(*Therapeutics*, p. 277, 5th Ed.)

Belladonna acts much more mildly upon the lower animals than upon man. Its well-known action in dilating the pupil of the human eye may instructively be compared with its powerlessness to cause any such effect on the pupils of the eyes of pigeons, or, as Stillé says, of those of other birds.

Birds and herbivorous animals eat Belladonna with impunity. "This is one of the many examples," say those great authorities, Drs. Stillé and Maisch, "which show the danger of concluding from the lower animals to man in regard to the uses of medicines, unless the mode of action in the two cases is first proved to be identical. In no animal is there any degree of that delirious excitement which Belladonna produces in man."—(*Therapeutics*, p. 276.)

Dr. Ringer (*Materia Medica*, p. 454, 5th Ed.) says:—"Certain animals, like pigeons and rabbits, appear to be almost insusceptible to the influence of Belladonna," and

"Belladonna, it is asserted, has very little effect on horses and donkeys." So powerful is the action of atropine on the human organism, that it is usually medicinally administered in the very minute dose of from $\frac{1}{120}$th to $\frac{1}{40}$th of a grain. Yet Calmus found that no less than fifteen grains are required to kill a rabbit, and Ringer says that two grains administered hypodermically are necessary to kill a pigeon.—(*Materia Medica*, p. 454.)

Camphor acts differently on different animals. In the articulata it acts as a virulent poison; in birds it causes epileptiform seizures; in mammals it is an intoxicant, causing ultimately convulsions and death. In man, Stillé says (p. 334), "in no instance does camphor seem to have caused the death of a healthy person."

Citric acid, the well-known acid of the lemon, is a powerful poison to cats, rabbits and other animals, causing violent convulsive spasms, but no such results ever arise in human beings from its use.

Common glycerine injected subcutaneously into dogs produces death with symptoms of alcoholic poisoning. I am not aware that it has any such effect upon man.

Goats, sheep and horses are said by Dr. Ringer to eat hemlock with impunity, yet it poisoned Socrates. Some experimenters say it is a powerful depressant on the spinal cord, others that it acts as an excitant. Some declare that it *lowers* the temperature, others that under identical conditions it decidedly raises it.

Henbane is poisonous to us and to fowls, yet it has little or

no effect on sheep, cows and pigs. Fish are poisoned by it, but it has very little effect on rabbits.

Ipecacuanha does not cause vomiting in rabbits. Dr. Lauder Brunton explains this by saying that the rabbit's stomach is so placed that it cannot vomit, but this is part of our contention, that animals being so differently constituted to ourselves, experiments upon them are untrustworthy guides in medical practice.

Let us imagine that a quantity of a new drug, called opium, is being examined for the first time by a special committee appointed for the purpose by the College of Physicians; let us assume that the drug has been brought from a far country, and that nobody knows anything about its properties, except some vague travellers' tales about its medicinal effects. The physiologists proceed to investigate its action by a long series of experiments upon animals; they give it to frogs, and they find that small doses throw them into tetanic spasms. Next they try it on a pigeon; they give him twenty grains, and he is none the worse for it. Emboldened by their success, they give thirty grains to a rabbit, and no effect is produced. They are beginning to believe that the travellers' tales are stupid exaggerations, especially as they discover that ducks and chickens, like the pigeons and rabbits, are never the worse for its administration. They resolve now to try it on a hospital patient, and proceeding with extreme caution, as they think, they decide not to venture at first beyond the dose they gave to the pigeon, namely, twenty grains. The patient is a powerful navvy, yet to their con-

sternation and distress he is promptly killed by the dose! If physiological medicine were of any value, surely the method followed by these investigators was right and cautious. Yet how fatal their method when reduced to practice! When opium is administered to human beings in large doses it contracts the pupils to a pin point; in birds the pupils are not affected; in horses they are widely dilated; in dogs under its influence the pupils first dilate and then contract. Opium seems as if it were created to confound the physiologists! Dr. Mitchell says it is impossible to kill a pigeon by opium given by the mouth; but Flourens affirms that a single grain will throw a sparrow into profound stupor. None of the opium preparations cause sleep in pigeons, ducks, or chickens. With dogs, cats, and rabbits large doses of opium produce sleep, usually with convulsions. In frogs opium only causes tetanus. Race greatly modifies its effects on man. It drives Javanese and Malays into temporary madness.—(Ringer, *Materia Medica*, 5th Ed., p. 478.)

Prussic acid has little or no effect upon horses and hyænas. The elephant, however, succumbs to a relatively small dose.

It is generally believed that the frog is peculiarly sensitive to strychnine, but Falck maintains that in proportion to its weight it is really not so susceptible to its influence as various mammals, and that "it requires four times the dose needed by dogs, cats, rabbits, &c., to produce an equal effect upon frogs."—(*Stillé, loc. cit.*) Birds appear to be comparatively insusceptible to its action. Stillé says that a hen, in pro-

gressive doses, at last took two and a-half drachms of nux vomica daily. Yet half a drachm of this poison has proved fatal to human beings. It requires ten times as much strychnine to kill a chicken as would suffice for a pheasant.—(*Guy and Ferrier's Forensic Medicine*, 4th Ed., p. 572.) The ruminating animals are not so readily affected as other quadrupeds when the poison is taken by the mouth. Ten grains may fail to kill a sheep when thus administered, though half of a grain may kill a man. The same would be fatal to the sheep if administered hypodermically or into the veins. The action of the poison on the goat is similar to that on the sheep.

In whatever way it is given to cats, whether by the stomach, injected into the veins, or under the skin, they "resist it singularly," says Stillé. Yet dogs are easily killed by it. It has been enclosed in fulminating bullets to kill whales, and it has been observed that when so poisoned they perish in the spasms which are so characteristic of its action on many other animals, yet "guinea-pigs and monkeys are said to be comparatively insusceptible to it."—(*Stillé, loc. cit.*)

Tobacco is poisonous to most forms of life, yet herbivorous animals are not readily affected by it.

Woody nightshade berries to the number of 180 had been given to a dog without producing any effect, yet death is recorded to have been caused in a child four years old who ate two of them.

I have selected but a small number from a very long list

of drugs which act differently upon the lower animals and on human beings.*

Yet in face of all these discrepancies, Dr. Lauder Brunton says that our objection to the value of such experiments is due to ignorance. "Almost all our *exact knowledge of the action of drugs on the various organs of the body, as well as the physiological functions of these organisms themselves, has been obtained by experiments upon animals.*"

Ignorance cannot be Dr. Brunton's excuse for this astounding statement!

Sir Charles Bell said that "Experiments have never been the means of discovery, and a survey of what has been attempted of late years will prove that the opening of living animals has done more to perpetuate error than to enforce the just views taken from anatomy and the natural sciences."

In Mr. Lawson Tait's address on "Ectopic Gestation," in the *Provincial Medical Journal* for 1st November, 1889, p. 646, we may see how experiments upon living animals have actually retarded the progress of the branch of surgery in which Professor Lawson Tait is so successful. He says that "unfortunately, Lecorte made his experiments upon lower animals—and this has been the source of all the trouble—this led to the mistake that has existed so long."

Dr. Bell Taylor, the eminent ophthalmic surgeon, confirms this, and adds that "Mr. Lawson Tait, who is certainly the

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* See for a complete list my book on *Futility of Experiments with Drugs on Animals*. London: Sonnenschein. 1889. Price 6d.

most accomplished abdominal surgeon the world has yet seen, has over and over again warned his students against the folly of vivisecting animals. He says that if, instead of cruel experiments on animals, 'we had read the teachings of the masters, and had looked at the experiments in Nature's own laboratory—disease—and taken the conclusions of these experiments as they stared us in the face, *we should have been at least a century in advance of our present position*.' I may add," says Dr. Bell Taylor, "that although mine is a special practice—very few ophthalmic surgeons have operated as frequently—I have never found it necessary to experiment on animals, and I heartily concur in Mr. Lawson Tait's remarks."

Dr. Percy Wilde, writing in the *Medical Annual* for 1888, says:—"We know that certain drugs increase the evacuation of bile; clinical experience leaves us in no doubt on this question; but when we endeavour to study their action further, by experiments on animals, the results hardly accord with our previous knowledge."

Judging by the elaborate experiments of Professor Prevost and Dr. Paul Binet on dogs, we should arrive at the conclusion that a doctor wishing to increase the secretion of bile in his patient should administer oil of turpentine and accept it as a fact that he should not give calomel when the secretion of bile is deficient. To this Dr. Wilde replies (although it must be remembered he is advocating experiments on animals), "But the results do not justify this conclusion, they only prove that calomel given to dogs, in a certain dose, and under certain conditions diminishes the secretion of bile; but as to

its action upon the human body, either in health or disease, they prove nothing." Professor Rutherford, at Edinburgh University, carried out a long series of experiments on dogs to test the action of different drugs which have long had a reputation for stimulating the secretion of bile. He published the account of these experiments in 1875. The dogs were prepared by being made to fast for eighteen hours. Then they were paralysed by injecting curare into the jugular vein; the windpipe was then cut open and a tube inserted, so as to keep up artificial respiration as the curare would have otherwise prevented the animal from breathing; then the abdomen was opened, the stomach and duodenum pushed aside, the edge of the liver was raised, the common bile duct dissected out, divided, and a glass tube inserted into it. The gall bladder was then squeezed so as to fill the tube with bile, and to prevent the bile returning to the gall bladder the cystic duct was clamped. When all this was finished, the wound in the abdomen was closed, and the animal wrapped up in cotton-wool to restore the normal temperature. The operation takes about half-an-hour to perform, no chloroform or anæsthetics could be given, or were given, as their action would have interfered with the success of the experiment. Curare was administered for the purpose of keeping the animal perfectly motionless, and we know from Claude Bernard that the action of curare is to render the animal more than ordinarily sensitive to the pain it is undergoing. Dr. Wall, writing on this, says: "If any one has witnessed the symptoms of the passage of a gallstone down the bile duct in the human being,

he has seen some terrible agony; 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. But the poor animal is not allowed to remain quiescent in this condition, for the wound in the abdomen is opened two, three, or even four times during the course of the experiment, to inject the drug through an opening made into the duodenum. Each time the abdominal wound is closed again, and the dog wrapped up in cotton wool as before."

Now I appeal to anybody of ordinary common sense whose judgment is not blinded by the fanaticism of pseudo-science worship to say if he thinks a dog in this condition is to be taken as a fair example of a human patient whose liver is out of order? Does he think that such a dog is in that healthy, normal state which physiologists insist upon as necessary for obtaining accurate results? Starved, poisoned, paralyzed, cut open, its internal organs mauled, mangled, and in a variety of ways injured, its temperature lowered by shock several degrees, made to breathe by machinery, its sensation heightened so that it suffers intolerable agony, and you have the impudence to come to me, whose duty it is to alleviate by all the means in my power the sufferings of my human patients, and tell me with unblushing effrontery that I may not administer a drug scientifically till you have finished your investigations as to its action on an animal in a condition like that! Dr. Rutherford cautions those who may repeat his experiments against many things which may invalidate them.

For instance, if there be the least dragging at the bile duct, the secretion of bile becomes so irregular as to render the experiment useless. Should the artificial respiration be allowed to fail, or even become somewhat deficient, the secretion of bile will be diminished. Even if the operator's bungling attempts to imitate the rhythm of natural processes were the least likely to be successful, what are we to say about the altered functional activity and its abnormal nervous condition as factors in the calculation you are attempting to make? Then, again, if you give too much curare you make the heart's action irregular, and diminish the secretion of bile; in fact the sources of error are numberless, and it is not difficult to make each experiment tell a different tale. Here we have a key to the contradictory results arrived at by every different experimenter.

Again, putting a drug into the part of the bowel known as the duodenum is a very different thing to giving it by the mouth. When we take a drug by the mouth it is mixed with the saliva and other secretions of the digestive tract before it reaches the duodenum, and these may exert definite chemical changes. The fright, apart from the pain, may also seriously affect the action of the drug, as we know it will by itself produce jaundice. When Dr. Rutherford, in his first experiment, injected 10 grains of calomel into the duodenum of his dogs, he got a marked increase in the bile secretion. In the remaining three experiments the flow of bile was diminished. What was the conclusion? Why—that calomel diminishes the secretion of bile. It was three to one it did. That

does not sound very scientific, but I am not responsible for that.

The consequence of this decision of Dr. Rutherford was that many medical practitioners who desired to treat their patients on purely scientific principles, discarding empiricism as much as possible, and adopting all the latest opinions of the physiological school, gave up the use of calomel and blue pill as liver medicines, not because these remedies had not done good service in clinical practice, but because experiments had proved that those beneficial results were only imaginary. In 1877, however, Dr. Rutherford set to work to see if he had not admitted some fallacy into his experiments, and so he performed another set to correct the errors of the first. He says that "possibly the calomel's non action on the liver was due to an absence of bile from the intestinal canal." So in this second series he mixed his calomel with bile and then injected it into the abdomen; then he got into a greater dilemma than before, the muddle increased, and the whole business seemed in hopeless confusion. It was then suggested that when we take a dose of calomel into the stomach by natural means and not by a surgical operation into the duodenum, it gets mixed with the gastric juice and saliva which partly convert it into mercuric perchloride, commonly called corrosive sublimate. So Dr. Rutherford set to work with calomel digested in water with hydrochloric acid, which is the acid of the gastric juice. Here, however, another muddle was made, for the unnatural conditions introduced another fallacy. In fasting dogs a certain mucus accumulates in their stomachs

which enveloped the drug and prevented its absorption, so that the calomel never got near the liver after all. But Rutherford patiently worked on, and at last found out—What think you, ladies and gentlemen? Why that the doctors had been right all along, and we were graciously permitted to give our patients calomel in liver troubles with the *Nihil obstat* of the physiologists! The consequence was that we—that is to say, such of us as were fools enough to have troubled our heads about their doings at all, reverted to a practice which had been established by patient physicians working at the bed-side with the knowledge which is the result of clinical experience. I have dwelt at some length on this calomel story, because it is exceedingly instructive, can be followed by intelligent laymen, and seems to me to illustrate admirably the difficulties in the way of determining the action of medicines by laboratory experiments.

We need not, however, go so far back as 1877, to demonstrate the absurdity of attempting to direct medical practice by means of experiments on the lower animals. What is going on at the present moment in the medical world with respect to Dr. Lauder Brunton's chloroform experiments at Hyderabad, is, if possible, even more striking and instructive. Every one knows that the administration of chloroform to human beings is sometimes attended with danger, and even death, and the question why and how chloroform kills, has long agitated the medical world, It has long been held that this anæsthetic should not be given to persons whose hearts are weakened or diseased. Syme taught many years ago, as

the result of his observations on human beings, that when an injurious effect was caused, the breathing was first affected, and afterwards the action of the heart. But according to the *Lancet* the truth of this doctrine had been impugned, on account of the results of physiological experiments. Surgeon-General Laurie, a pupil of Syme's, and a man of great experience in his profession, secured the appointment of a Commission *to prove* by experiments on living animals, what had previously been *disproved* by experiments on living animals—that the observations of doctors on human beings were correctly interpreted, This it will be perceived is the calomel story over again, and unfortunately for experimental physiology it is a common one. A generous Indian Prince, the Nizam of Hyderabad, offered facilities to carry out the experiments, and gave £1,000 towards the expenses. Three hundred and fifty-four dogs and seventy monkeys were used in the investigations, and though the experimenters availed themselves of every opportunity that offered the faintest chance of new light being thrown on the phenomena of chloroform anæsthesia, those persons most competent to judge, viz., the physicians who administer the chloroform at the great London hospitals, reject the attempt to force upon them the conclusions which the Commission have arrived at by the experiments. The details of these are horrifying; but I do not propose to excite your pity so much as to appeal to your reason to-night. I refrain, therefore, from detailing the circumstances under which they were performed, and ask your attention to the reception which the report has met

with at home, not from Anti-vivisectionists and incompetent philanthropists, be it remembered, but from experts in such matters.

Mr. Woodhouse Braine, Lecturer on Anæsthetics, and Chloroformist at Charing Cross Hospital, and who is also Chloroformist to the Dental Hospital, London, and Dr. Roger Williams, of the Middlesex Hospital, protest against many of the conclusions arrived at by Dr. Lauder Brunton's committee, The first-named authority on anæsthetics, in a long letter which appeared in the *Lancet* of February 8th, says :—

"If three cases of death in the human subject are brought forward which have happened in England or in climates resembling ours in which the heart has ceased beating and the patient has gone on breathing afterwards, then I affirm that these cases are of far more importance to us as practical anæsthetists, and far outweigh the 430 experiments which were performed on dogs, monkeys, and other animals in the tropical heat of India."

After pointing out his objections to several of the recommendations of the Hyderabad Commission, and showing how contrary they are to the best clinical practice, Mr. Braine says that he cannot reconcile the observations on Dr. Lauder Brunton's Indian dogs with those which he, as an expert in anæsthesia, has made on hospital patients. He very pertinently asks, "Is it not apparent that animals do not suffer from surgical shock and cardiac (heart) failure, and are in this respect different from human beings ?"

Conclusion IX. of the Hyderabad Commission states that

"the administrator of the chloroform is to be guided as to the effect entirely by the respiration." Mr. Braine replies, "From the large experience I have had of administering anæsthetics in England during the last thirty years, I feel absolutely certain that if this deduction is acted upon, the number of fatal cases will rapidly increase."

Dr. Roger Williams in the same number of the *Lancet*, from observations on the administration of chloroform at St. Bartholomew's Hospital during ten years—1878-1887, in which period it was given no less than 12,368 times—utters his emphatic protest against the dictum of the Hyderabad Commission that deaths must be ascribed entirely to carelessness on the part of the administrators of the chloroform. He says, "Such a statement is opposed to all clinical experience, and it is simply preposterous."

In the *Lancet* of February 15th, p. 373, Mr. Dudley Buxton, Anæsthetist to University College Hospital, severely criticises the Hyderabad Report. He pertinently asks "Now that we have got it, are we happy? Dispassionate candour compels me to reply that I at least have been carried no farther. *In the first place I find no attempt is made to bridge over the great hiatus betwixt experiments upon the lower animals and the daily experiments made on man.* Again, I am disappointed to learn no authoritative statement as to whether dogs, monkeys, &c., are liable to syncope under any conditions; personally, I believe, if they are so the occurrence must be most rare. Comparing the statements concerning the lower animals with one's own experience among human beings, a wide discrepancy

occurs." Mr. Buxton thinks also that the action of such a drug as chloroform in the tropics may not be identical with that in the temperate zones. He thinks that the experiment undertaken by Dr. Brunton to elucidate shock under anæsthesia is "*totally opposed to our clinical experience.*" Dr. Shand, of Edinburgh, also writes in the same number of the *Lancet* (p. 373), "to express dissent from two at least of the conclusions arrived at by the Chloroform Commission."

Again Drs. John G. McKendrick, Joseph Coats, and David Newman say, "Some of the inferences are opposed to ours; but they are also opposed, as we believe, to the facts stated in the report itself" (p. 374). At the Medical Society of London on February 10th, Dr. Brunton gave an account of his experiments at Hyderabad.

Mr. Braine in the discussion which followed Dr. Brunton's explanation, said, "*He thought that one case—observed in the human being—was more valuable than thousands of experiments on animals.*"

Mr. Charles Sheppard, Chloroformist to the Middlesex Hospital, and Anæsthetist to Guy's Hospital Dental School, in a letter to the *Lancet* of Saturday last maintains that "if we followed the indications furnished by laboratory experiments in this instance we should be led into grievous error." Now these are the deliberate opinions of men who every year are responsible for the lives of thousands of human beings, who unhesitatingly place themselves under their care while about to plunge into an unconscious condition and undergo operations often of the extremest gravity. These men are

selected for the work because they are experts; they are entrusted with the lives of the patients, so that the great operators who have to do their delicate and perilous work may accomplish it with an untroubled mind so far as the anæsthesia goes, and they tell us that they cannot, dare not, accept the teaching of these laboratory experiments, for the simple reason that your highly scientific teaching would, if carried out, increase the fatal results of chloroform administration.

But this must suffice for the admitted failures of vivisection. I will now address myself to the examination of some of the discoveries which its advocates attribute to this method of research.

It is declared that Harvey discovered the circulation of the blood by this means. I will not insult the intelligence of a Cambridge audience by replying at any length to this objection, because everybody knows now that the circulation was not demonstrated in Harvey's time, and that until Malpighi used the microscope it had never been seen. Our insular pride has caused us to overlook the work of foreigners in this direction before Harvey's time. What Harvey really did in this connection was due to the fact that he was a pupil of Fabricius, of Acquapendente, and Fabricius discovered the valves in the superficial veins—an anatomical observation. The real seed of Harvey's discovery, as was shown at the Royal Commission, was the fact that he saw that the blood can only move in one direction. Fabricius did not see that—Harvey did.

It is maintained by our opponents that Professor Lister has revolutionised modern surgery by his anti-septic system, and that this was discovered by experiments on living animals. The plain truth about this is that Professor Lister made the important discovery that after surgical operations patients very frequently were poisoned by the discharges of their own wounds, and he invented an elaborate ritual of cleanliness—surgical cleanliness—which has had an immense deal to do with the progress of modern surgery. It is, however, as unfair to claim the results of this cleanliness as due to experiments on living animals as it would be for a dairy maid to claim that she had discovered that good butter and cheese cannot be made in dirty utensils and foul dairies by similar means. Surgeons, if the truth must be told, were very dirty persons, operating in very dirty places, till Lister taught them the value of cleanliness. Let me quote the remarks of Professor Savory, late President of the Royal College of Surgeons, which he made at the International Medical Congress in 1881, and which are recorded in its *Transactions*, Vol. II., p. 347: "Surgical wards, not long ago hotbeds of poison, are now made fairly safe for patients. While, no doubt, some startling novelty of practice was necessary, or at least greatly advantageous to this end, yet I cannot doubt that the same end might have been reached by an adequate improvement in simple sanitary arrangements." Many of our most eminent surgeons, I happen to know hold precisely the same view; and Mr. Lawson Tait goes so far as to say that the so-called antiseptic treatment is answerable for a great deal of mischief,

as it offers facilities for the performance of unnecessary operations often undertaken in a reckless manner. Professor Lawson Tait has entirely discarded all the antiseptic dressings and paraphernalia. He uses common tap water only, and his results in his special branch, that of abdominal surgery, surpass those of any other living operator so far as I am aware. In reply to a question I asked him recently, he wrote me as follows :—

"Birmingham, October 9th, 1889.

"Dear Dr. Berdoe,—You may take it from me that instead of vivisection having in any way advanced abdominal surgery, it has, on the contrary, had a uniform tendency to retard it. This I show to be particularly the case in operations upon the gall-bladder, and refer you to the current number of the *Edinburgh Medical Journal*, where, in an article, I point to the fact. As to the use of the antiseptics of Lister, it increased our mortality, prevented recoveries, and did a vast deal of harm by retarding true progress.

"Yours very truly,

"Lawson Tait."

Again, it is claimed that the uses of chloroform were discovered by experiments on guinea-pigs. Sir Lyon Playfair, in his account of the discovery of the anæsthetic properties of chloroform before the House of Commons, said that Sir James Simpson first tried its effects on two guinea-pigs, which died shortly after its inhalation; he then tried it upon

himself, and in the course of a long series of experiments acquainted himself with its properties. How the guinea-pigs contributed towards this result I confess myself unable to understand. If they were of any use in the investigation at all it would have to be argued that, as chloroform was fatal to guinea-pigs, it would be dangerous, if not fatal, to man. As it happens, Sir Lyon Playfair was altogether wrong in his account of the discovery of chloroform and its uses as an anæsthetic. The true story is as follows:—The drug itself was discovered by Liebig and Soubeiran in 1832. "Nothing came of the discovery. Chloroform was merely looked upon as a chemical curiosity, and no one dreamed of using it to annul pain until ether was introduced as an anæsthetic in surgical operations. Then Professor Simpson, who was preparing a paper on ether for the Edinburgh Royal Society, and who, with a view of collecting materials for his article, was inhaling all the likely hydro-carbons he could lay his hand on, by the merest accident hit upon a bottle of chloroform, and finding that on respiring its contents he speedily became insensible, tried it on his friends, Drs. Keith and Matthew Duncan. All this occurred late one evening (on the 4th of November, 1847), and the next day it was administered instead of ether to a Gaelic boy, who was about to undergo an operation at the Royal Infirmary. All went well. It was immediately tried on other patients, and speedily became generally adopted. No experiments on animals, other than the experiments on the animal man which I have detailed, were ever made in connection with the discovery

of the anæsthetic properties of chloroform."—(Dr. C. Bell-Taylor, of Nottingham, in a letter to the *Bazaar*, Nov. 11th, 1889, &c., &c.)

The use of nitrite of amyl in medicine is declared by the Vivisecting School to be a brilliant example of the benefits conferred upon humanity by experiments on animals. It was discovered in 1844, by the French chemist, Balard. In 1865, Dr. Richardson introduced it to the profession. Guthrie had previously observed its action in causing flushing; indeed, it would be impossible for anybody who had ever had a sniff of the drug to avoid observing this action. Some years later—that is to say, after all its clinical virtues had been well ascertained—Dr. Gamgee, by experimenting upon animals, demonstrated that nitrite of amyl lessened the blood pressure in the vessels; in other words, it dilates the capillaries, which is a pretty scientific way of saying it causes intense flushing. Every doctor who used the drug, on the recommendation of Dr. Richardson, must have known all this. "Animal torture was unnecessary," concludes Dr. McCormick, Deputy-Inspector of Her Majesty's Hospitals and Fleet, after remarking that the use of nitrite of amyl for the relief of spasms of the heart "could have been very readily arrived at by letting a patient inhale its vapour." Dogs, rabbits, and other animals were used upon which to *demonstrate* phenomena which had already been observed by clinical methods. It is not true, therefore, that the discovery of the uses of nitrite of amyl was due to experiments upon animals, though it is the fact that they were demonstrated by such means.

Again, it has lately been maintained that the important operation for the removal of a diseased kidney in the human subject was discovered by experimenting on dogs. But the removal of the kidney was a common operation in the days of Hippocrates. Cardan laments it as one of the lost operations—and that we have lost many operations with which the ancients were familiar, it is impossible to doubt. Mr. J. Grey Smith, surgeon to the Bristol Royal Infirmary, says in a recently published book on Abdominal Surgery, that "Surgery, in some of its departments, has done nothing but retrograde for more than a thousand years, and in the last fifty years we have done little more than pick up the clues that were lost when the Alexandrian Library was burnt" (3rd Edit., p. 517). But to return to the assertion that the operations of Nephrotomy and Nephrectomy, as they are termed, were discovered by experiments on animals. The point is this. As every one knows, we have two kidneys—do we want two? If one is diseased, is it safe to remove it? Our opponents say that was learned and could only be learned by "trying it on a dog."

Well, it was performed unintentionally several times before it was undertaken as an ordinary operation in a deliberate manner. Doctors have removed one of the kidneys in mistake for something else. In 1680 a criminal of Meudon, who had stone of the kidney, and was condemned to death for his crimes was offered the chance of submitting to the operation of Nephrectomy to save his head. It was successfully performed, and the man lived for several years. But a reference

to Erichsen's *Surgery*, Vol. 2, p. 980, will settle the question. The author says:—

"Before proceeding to the removal of the kidney, it was necessary to ascertain that a person could not only live, but that the health might be maintained after the removal of so important an organ. That this is possible has been proved by the result of injury, by pathological research, and by physiological experiment. There are cases on record in which, in consequence of a deep stab or cut in the loin, one kidney has been wounded and forced out of the wound whence it has been removed—the patient making a good recovery. Then, again, it has long been known to pathologists that a person may live with one kidney practically useless, either blocked by calculus or destroyed by suppuration."

It has often been claimed by our opponents of the experimental school that the virtues of digitalis were discovered in consequence of the great number of investigations which have been carried out with it upon the lower animals. But this is a typical case of the confusion so often made between a discovery and its demonstration. "Long before" (we quote from Stillé's great work, p. 511) "its mode of action had been experimentally investigated, it was established as the most efficient remedy for dropsy depending upon disease of the heart."

But of all the triumphs of vivisection, the greatest is said by its advocates to be Pasteur's treatment for hydrophobia. This is the Hougoumont round which the battle has chiefly raged for the past two or three years. It is claimed by

M. Pasteur that the awful and always fatal disease known as hydrophobia could be rendered harmless by injecting the virus, in an artificially weakened state, under the skin. Pasteur has compared the effects of this treatment with what is known as vaccination, and though we may admit, for the sake of argument, that there is a certain analogy between the systems of Jenner and Pasteur, there exists an important difference which we cannot consent to overlook. It is this, when we vaccinate a person, or, in other words, inoculate him with cow-pox, he really does have a disease in a mild form, as we actually see by the symptoms which follow the act of inoculation. But when Pasteur inoculates his animals with his weakened virus, they exhibit no symptoms of any disease whatever. Now Pasteur argues that when such animals, after having been bitten by a rabid dog, do not become rabid themselves, we are to conclude that these animals have been protected from getting rabies through the bite of the rabid dog because they have already had rabies in a mild form in consequence of his inoculation. But, as everybody knows, bites from rabid dogs have ill consequences only in a limited number of cases. Happily the penetration of the virus into the system is prevented by many circumstances. The skin may only just be grazed, and the poison may not reach the blood vessels, or the blood in flowing may wash it out, or the dog's mouth may have just been dried by his biting something else. We are not entitled to say, therefore, that Pasteur's system is identical with vaccination. Test this so-called blessing to the human race, which the great French chemist

is declared to have conferred upon it by his anti-rabic inoculations, howsoever we will, it is impossible for the unprejudiced mind to be convinced that we have anything to be thankful for at all. Putting aside the Hero-worship which goes spontaneously forth towards anyone who seems to have done, or been desirous to do, the human race a great service, and looking at the matter in the cold, calm light of science, we ask this question. What is thought of the discovery in the great Continental Schools ?

It has met with very little acceptance. Indeed, to quote the words of Dr. Billroth, in his paper on Dr. Von Frisch's report on Pasteur's method to the General Polyclinic at Vienna, "the opposition to Pasteur's system is already assuming large proportions, not only in France, but also in Belgium, Spain, Italy, and Russia," because, as Von Frisch emphatically says, not only may the inoculation be useless, but "it may be inferred, with great probability, that this method may be seriously dangerous to man." In other words, "perfectly healthy persons may thereby be rendered rabid, that is *sans phrase*, be killed." Bear in mind these are Professor Billroth's own words. But has not Pasteur treated a great number of persons with success ?

Up to the present he claims to have treated some 6,000 persons, most of whom were never liable to contract hydrophobia at all, and of this number 184 have since their inoculation died from hydrophobia.

Ah ! you say no doubt some of these went to Pasteur when it was too late.

Well, one man was bitten on August 30th, 1885, he was inoculated on September 1st, and on September 7th, was dead. Another was bitten on June 28th, was inoculated June 30th, and died July 17th. Another bitten August 4th, 1886, was inoculated the next day, and died on September 16th of the same year. Even that delay of one or two days perhaps you think was too long. But there are records of persons who were inoculated the same day as they were bitten, but they also died within a few weeks of the dread disease. Even then some one may object they were no worse off than if they had not been inoculated at all. That is a poor way of looking at a great boon, but even this last refuge cannot be permitted to you, for there is unfortunately abundant evidence that the inoculations have themselves produced a fatal disease which the unhappy patients could have contracted by no other means. The disease is called *Rage de Laboratoire*. A peasant from Kromy was inoculated on July 26th, 1887, and died September 20th, 1887. The *Novoë Vremya* states that "this peasant died of paralytic rabies contracted at the laboratory."

There are other similar cases on record.

But we have accounts of persons who have been bitten by rabid dogs who have taken no harm, though they received no treatment at all, while others bitten by the same dogs at the same time have subjected themselves to Pasteur's treatment, and have died of hydrophobia. Dr. Potin, who reports the details of one of these cases in the *Journal de Médecine de Paris*, has laid the whole facts recently

before the Minister of the Interior and the Prefect of the Paris Police.

But, if Pasteur's system has proved itself so valuable, where are we to find the record of its services to mankind? At its home in France? Certainly not there. Not only has the annual mortality from rabies in man not diminished in France, it has increased, and notably, in the Department of the Seine, where it reached in 1888 the enormous figure of 19. Says M. Peter, quoted in the *Provincial Medical Journal*, for March, 1890, "In order to mystify the evidence, they seek to astonish us by a veritable inebriation of figures in considering as rabid every animal that bites, and as destined to become a victim of hydrophobia, every person bitten." Dr. John Billings recently said at a Medical Congress that "You can tell as many lies with figures as with words, and bigger ones!" In this very connection we have recently had an illustration of that fact in England. An eminent physiologist—Mr. Victor Horsley, Secretary of the Pasteur Commission—told the Lords Commission on Rabies of 1887, that in Scandinavia the death-rate from rabies reached the enormous total of 181. Mr. Chaplin, acting on this statement, told the Kentish Deputation against the muzzling order that "in Scandinavia the death-rate from hydrophobia had become positively enormous." If Mr. Horsley was not comparing a death-rate of dogs in Scandinavia with a human death-rate here, there must have been "a wonderful inebriation of figures," for after exhaustive enquiries in Stockholm, it appears doubtful whether as many as 18 persons ever died

of hydrophobia in Sweden, not in one year, but in all years of which we have record. Dr. Thomas Dolan in the *Provincial Medical Journal* for March, 1890, says, "The English death-rate has been unaffected by the laboratory experiments at Paris, save in a few instances, as by the deaths of Wild and Goffi" (patients of Pasteur who died from the effects of the inoculations).

Search where we will for the practical benefits of M. Pasteur's discovery the answer is uniform, "Not there! not there! my child." That is doubtless the reason why the scheme of a Pasteur Institute for London hangs fire.

An article on "Recent Advances in Surgery" appeared last autumn in *Harper's Magazine* from the pen of Dr. W. W. Keen, in which the most extravagant claims were made for the share which experiments upon animals had had in those advances. The article is written in a manner calculated to captivate the public mind which has neither the technical knowledge required to check the various statements nor the special acquaintance with controverted points which is necessary even for medical and scientific inquirers. The average medical man cannot find time for, nor does he feel sufficient interest in, a controversy that does not intimately associate itself with his professional work. "One need not be an architect to live in a house," as Kingsley said about theological knowledge; neither is it necessary for a medical man to know the *pros* and *cons* of the vivisection controversy. If, however, he would devote but a little attention to the literature of the subject, he would soon arrive at the conclusion

that we have something to say for the attitude we assume on the question.

Nothing is easier, therefore, than for a writer to claim the gratitude of the world for all sorts of blessings bestowed upon medicine and surgery by practices which we, as Anti-vivisectors, strenuously oppose. It is claimed, for instance, in the article to which I refer, that the success attending the operation known as ovariotomy, is due to experiments carried out on living animals by Sir Spencer Wells. With reference to Sir Spencer Wells's operation for the removal of ovarian tumour, I believe the facts are as follow :—Sir S. Wells claimed to have saved 500 lives at the cost of experiments on 14 rabbits; but Mr. Lawson Tait has reduced the mortality after the operation of ovariotomy far below that of Sir S. Wells—to *nil*, in fact—without reference to any vivisection at all; and Dr. Charles Clay, who successfully performed the operation before Sir S. Wells attempted it, testified that vivisection had "no more to do with ovariotomy than the Pope of Rome."

The head and the abdomen are the two regions in which the greatest achievements have been attained in surgery by means of vivisection, according to Dr. Keen. By numberless slicings, borings, ploughings, and burnings of the brains of monkeys by Dr. Ferrier and others, it is asserted that it is possible to localise brain tumours and remove them with safety to the patient, a performance that we could not have ventured upon without the light which we have received from these experiments.

In the *Medical Press*, of 26th January, 1887, we find some remarks of Dr. Goodart's on brain tumours and localisation. He says: "For the last thirteen years I have been making *post mortem* examinations at Guy's Hospital, and during that time, although I have come across many cases of cerebral tumour, I do not remember to have seen a single case in which the tumour was at the same time *accessible* and *so localised* as to be capable of successful surgical attack. It is very doubtful whether in the region of cerebral tumours other than inflammatory, surgery has any future worth mentioning before it."

Whether the operations for the removal of brain tumours ought not in many cases rather to be termed surgical audacities, a consideration of some of the fatal cases attending them, may throw some light.

BRAIN SURGERY NECROLOGY.

	Patient.	When operated on.	Where.	By Whom.	Physician.	Result.
1	Man, age 23	Nov. 25, 1884	Hospital for Paralysis	Mr. Godlee	Dr. Hughes Bennett	Died Dec. 23 of inflammation of the brain.
2	Man	...	King's Coll. Hospital	Sir J. Lister	Pro. Ferrier	Died a week after the operation.
3	C. H., Boy 7	Oct., 1886	Queen's Hospital, Birmingham	Mr. Bennett May	Dr. Suckling	Died from shock a few hours after the operation.
4	J. H., Man 38	Sept. 23, 1886	Hospital for Epilepsy and Paralysis Queen's-sq.	Mr. Horsley	Pro. Ferrier	Death in six months from recurrence of tumour.
5	J. B., Youth, 18	Dec. 17, 1886	...	Mr. Horsley	Dr. Bastian	Death nineteen hours after operation.

On the debate in the House of Commons, which was the result of the motion to reduce the estimate by the amount

of the Inspector of Vivisection's salary, Sir Henry Roscoe said that anthrax, the Russian cattle plague, could now be actually overcome by inoculation, in consequence of Pasteur's experiments in that connection. If this be so, it is strange that the system is strongly opposed at Berlin and Vienna. From the report in the *Practitioner*, for March, 1882 (edited by Dr. Lauder Brunton), of the Government experiments on Pasteur's protective inoculation against anthrax in Hungary, we find Dr. Aladár von Rószahegyi saying that—

"We cannot overlook the fact that after the protective inoculation the deaths from other diseases, or, more correctly, those in which the *post mortem* appearances were those of other diseases—catarrh, pneumonia, distoma, strongylus, and pericarditis—and not those of anthrax, occurred exclusively *amongst the inoculated animals*. It follows from this that the fatal issue of other severe, but latent diseases, is accelerated by a protective inoculation."

We know that the practice of inoculation for charbon has given charbon to the animals it was intended to protect, and the death of thousands of sheep inoculated at Odessa with the virus of Pasteur proves this.

Here then are as many of the reasons as can be crowded into a paper of this sort, why we Anti-vivisectionists think that medical science would not lose anything by the total abolition of a practice that, on any other ground, is absolutely forbidden by the laws of England. For medical science alone is it that a dispensation is granted to enable physiologists to break the law of 1849, 11 & 12 Vict., c. 92, which

makes it penal to torture animals or cause them to be tortured. Sheltered behind the protecting influence of medicine, scientists do with impunity deeds of awful cruelty which would not otherwise be tolerated for an instant. But the consideration of the reasons I have adduced in this lecture for the belief that vivisection has not advanced the Healing Art a single step will, I venture to think, lead the unprejudiced mind to consider that *all* the reason, the logic and the science is not on the side of our opponents.