What has experimental physiology done for the advancement of the practice of surgery? : an address delivered before the Surgical Society of Ireland, at the opening meeting, 23rd November, 1877 / by Robert McDonnell.

What has Experimental Physiology done for the Advancement of the Practice of Surgery?

AN ADDRESS

DELIVERED BEFORE THE SURGICAL SOCIETY OF IRELAND,

At the Opening Meeting, 23rd November, 1877.

BY

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WHAT HAS EXPERIMENTAL PHYSIOLOGY DONE FOR THE ADVANCEMENT OF THE PRACTICE OF SURGERY?

Gentlemen:

The topic on which I intend to address you this evening is, I conceive, not unsuitable for me to bring before you, now that I, as President of this College, am placed in the honourable position, calling on me to deliver the address which inaugurates the 47th Session of the Surgical Society of Ireland.

In considering this question I do not mean to weary you by a dissertation on vivisection. I wish, as clearly and as briefly as I can, to indicate what share experimental physiology has had in bringing the practice of surgery to its present position.

When we contemplate a stately pile of building—when, for instance, we look with satisfied wonder at Westminster Abbey, or Cologne Cathedral, or even with amazement at the exquisite detail of the Duomo at Milan, or the immense grandeur of St. Peter's—we know that these edifices have been not merely the work of many hands, but of many varieties of skilled artificers; each—from the humble workman who has carried mortar on his shoulder, to the refined artist who has toiled conscientiously with his chisel or his brush—has had his share in the completion of the work. So the healing art, as it at present exists, has been the slow growth of time, and the work of many sorts and conditions of men—the anatomist, the chemist, the pathologist, the histologist, the physiologist, the electrician, the physicist, etc., has each played his part; the practitioner seeks to bring to a focus light coming from every source.

The successful performance of a single great operation

requires the application of much knowledge, and the carrying out of many details—each detail, although to the ordinary eye seemingly unimportant, is an essential part of the whole. Like a fine mosaic, if a few stones are wanting, the work is incomplete, and falls to pieces. Now, the question is: Has experimental physiology contributed much or little to this fine surgical mosaic work, as I may call it? Has it, or has it not, been the means of putting in some key-stones into the arches of the grand edifice of modern surgery?

If this cannot be made clear, then experimental physiology may be pronounced "to have been weighed in the balance and found wanting."

I am anxious to state this issue definitely, because I believe this is the chief ground on which experimental physiology can be justified. Let me say, that in my judgment, some of the advocates of experiment have injured their cause by using arguments readily answered by any thoughtful person, because they are fallacious. Thus, it has been argued that the lower animals do not feel very acutely. It is probable, nay, almost certain, that they do not feel so acutely as men; but it is also certain that they feel quite acutely enough to make it utterly unjustifiable to perform any painful experiment upon them, unless it can be shown that mankind has derived advantages, vastly great from such experimentation in proportion to the amount of suffering inflicted. It would be utterly unjustifiable to lash our cab-horses through the streets of our large cities, unless the advantage to mankind were enormously great in proportion to the labour and suffering thus inflicted on the brute creation.

It is also argued—mark the words—that the experimental physiologist *is not so bad as the sportsman*, or as those who mutilate animals for the table, and for other reasons. To such an argument the so-called humanitarian justly replies: "True, the physiologist is not so bad, but the work of the physiologist, the sportsman, and the muti-

lator are all objectionable; and we," he adds, "commence our attack where we have the best prospect of success; we shall by degrees advance against the others as time goes on."

You see, therefore, that I repudiate the argument that the physiologist is *not so bad as* the others. I assert that he stands on wholly different ground. There is no more just comparison between the sportsman and the experimental physiologist, than there is between the man who goes to take a plunge into the sea on a warm summer day, and one who springs in, clothes and all, in mid-winter to endeavour to save the life of a drowning fellow-creature.

The experimental physiologist derives no pleasure from the infliction of pain; he shrinks from it with that repugnance which is natural to most men; he overcomes these feelings from a sense of duty; with firmness and fortitude, he subdues his natural selfish instinct; he, as it were, dashes aside his repugnance to the ice-cold water, and makes at least an effort to do good.

That he is scorned and scoffed at by a certain class of bystanders—that he is assailed by a certain tribe of childless women (whose sympathies have been little developed), or sensational writers, who have little scruple in dissecting live men with their pens; that a timid, cowardly crowd, too ignorant to see through the mist of prejudice which surrounds them, to the future good, sneer at him, and attribute to him base and unworthy motives, does not make his conduct the less admirable.

It is as puerile as it is false to assert that it is mere love of scientific notoriety which urges on the physiologist. The motives of mankind are mixed. Love of fame—of such honourable fame as forms a halo round the name of Haller, or Hunter, or Harvey—may well actuate the physiologist; but to say that desire for notoriety is the true mainspring of his conduct is as ridiculously false as though one were to assert that the heroic actions performed yearly by many of our countrymen, chiefly arose from the selfish desire of

obtaining the Victoria Cross, or the medal of the Royal Humane Society.

"I must make experiments," writes Sir Charles Bell,* "and that is what I hate to do." But he did not yield to this feeling—he subdued it; he set self aside, and made the necessary experiments, and in doing so he did what has been accounted noble in human conduct since the earliest ages of mankind.

I am aware that it is not an easy matter to point out, even to an audience such as I have the honour to address, the immense influence which the discoveries due to experimental physiology have had on *the general aspect of practice.* To a lay audience, or perhaps, I should say, to a *scientifically uneducated* audience, it would, I conceive, be wholly impossible. If you tell a child, on a cloudy, foggy day, that the diffused daylight by which we see our way about comes from the sun, it has a difficulty in understanding it. The bulk of mankind are in the same position with regard to the diffused light of science. They have difficulty in understanding how one discovery, like that of the circulation of the blood, for instance, or that regarding the compound nerve-roots, or the influence of the sympathetic system over the blood-vessels, can have been the means of letting light into the obscure chamber in which the practitioner of medicine and surgery has to work. The practitioner himself very often does not know where the light comes from. He uses it, he profits by it, but often knows not whence it comes. Like Molière's "Bourgeois Gentilhomme," he is talking prose without knowing it. I

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* It has been so often asserted that Bell did not make experiments on animals, that many who ought to be better informed on the subject have come to believe it. I must refer such persons to his letters and works. Writing in March, 1810, he says :—

"Exp.—I opened the spine and pricked and injured the posterior filaments of the nerves; no motion of the muscles followed. I then touched the anterior division; immediately the parts were convulsed."

"Exp.—I now destroyed the posterior part of the spinal marrow, by the point of a needle; no convulsive movement followed. I injured the anterior part; and the animal was convulsed."

say this without any disrespect to the esteemed practitioners around me; but I must be allowed to state this truth explicitly, for there is nothing which has so much injured the cause of the physiologist as the assertion by persons whose names are familiar to the public, that practice has gained little, if at all, from experiment on animals.

If proof be demanded of how little the light of physiological science has penetrated the minds of some of our best known practitioners, I should say I must appeal to their published works. I conceive that no one really conversant with physiological physics could attempt to justify the application of a ligature to the *carotid artery* for the cure of subclavian aneurism, much less defend such a proceeding in these words: "I had an idea myself that the ligature on the carotid so low down would have the effect of checking by retrograde current the upcoming flow from the heart through the innominate, and thus the back stroke break the force of the stream, and so passing thus disturbed into the subclavian and aneurismal sac, so favour, by retardation of the current, the deposition of fibrine, and the formation of a clot."

No one at all skilled in physiological histology could suppose, as has been recently suggested, that the laminated fibrine deposited within an aneurism was a coating of organised lymph secreted from the walls of the sac; nor could any surgeon even moderately well acquainted with the teachings of modern physiology (or, indeed, who had made any physiological experiments with it) advocate the use of such an instrument as that described and figured in vol. xlvii. of the *Dublin Journal of Medical Science*, "as an artery compressor suggested for the treatment of some surgical aneurisms." It would, I fancy, hardly be fair to attach much value to evidence given by such writers as to what physiology has done for practice. But, unfortunately, physiology and practice in these islands are two things apart.

We do not find every day among even our leading physicians and surgeons, men like Brodie, Robert Todd, Marshall-Hall ; and, at present, Paget, Brown-Séquard, or Lister—individuals who are at once accomplished physiologists and skilled practitioners. Many physicians who can, with the greatest precision, make a correct diagnosis in heart disease, would stand a bad examination if cross-questioned as to the steps by which experimental physiologists gained a knowledge of the heart sounds. Many a surgeon in this hall could dexterously tie or twist a bleeding vessel, who could give a poor account of the subject from Paré's time down to the experiments of Bryant, Humphry, Lister, and others. The man who works an electric telegraph may do it admirably well; yet know nothing of Volta, Galvani, Nobili, de la Rive, Wheatstone, or Faraday.

I do not, therefore, gentlemen, mean to insult you, when I say that you may be excellent surgeons, but many of you do not know whence has come the light by the aid of which you work. I dwell, however, especially on this for two reasons : first, because in this we find the explanation of the strange fact, that some of our most esteemed brethren have been found to assert that experiment on animals has done but little for practice ; and, secondly, because it is in this very way that practice has been most profoundly modified by experimental physiology.

If there is any one among you who may be disposed to support the assertion that experiment on animals has done little for practice (and possibly our worthy Secretary, Mr. Tufnell, who at one time took a strong and somewhat sensational part against the practice may wish to do so), I would merely ask him, after a little reflection, to reply candidly to the case which I have put.

The practitioner admittedly treads an obscure and uncertain path. He daily deals with cases in which the difficulty of diagnosis is extreme, and the line of treatment doubtful. So much so, that no honest physician or surgeon,

who is sufficiently well informed to be alive to the dangers which surround him, will deny that there is some truth in the apologue assigned by d'Alembert to a physician, a man of wit and of philosophy. "Nature," says he, "is fighting with the disease; a blind man, armed with a club —that is the physician—comes to settle the difference. He first tries to make peace; when he cannot accomplish this, he lifts his club and strikes at random; if he strikes the disease, he kills the disease; if he strikes Nature, he kills Nature." Perhaps it is too much in the present day to speak of the physician as a "blind man." He may be more justly compared to one who toils over some delicate handiwork, in a mine but dimly lighted by a few lamps. Now suppose that some of his brightest lamps are extinguished, how would his work proceed? Suppose even that one single great discovery, like that of the circulation of the blood, or the movements of the chyle in the lacteals, or the functions of the spinal nerve-roots, were struck out, how immeasurably would the difficulties of the practice of medicine and surgery be increased! Are these not verily and indeed the key stones of the main portals? What would the edifice of modern surgery and medicine be without them? Yet these, with many lesser discoveries arising from them, are the direct offspring of experimental physiology.

It is, in truth, this diffused light of experimental science which has of late years altered the whole aspect of practice. You ask some aged practitioner how it is that so great a change has taken place since he was young. Then patients were largely bled in all manner of ailments, more especially inflammatory; now, many of our students pass through their entire period of study without ever witnessing the operation of venesection. He replies: "The type of disease is changed since I was young."—*Credat Judæus Apella.* I confess I cannot accept any such explanation. This is but the answer of those who are very naturally unwilling to admit that they themselves have changed;

but I take it that small-pox, typhus, measles, pneumonia, are very much what they were a hundred years ago, and that human flesh and blood, beef and beer, are also the same. The old gentleman, "*laudator temporis acti*," thinks the girls are not so pretty, nor the gooseberries so good, as when he was a boy, and he thinks the type of disease has changed like them. No—practice has changed, because the light of science has slowly and gradually enabled practitioners to see more clearly into the workings of nature. The careful microscopic study of inflammatory processes has shown the part really played by the blood, the vessels themselves, the nerves and surrounding tissues, and the physiological pioneer has by degrees established a more enlightened practice.

Who can say what individual, or individual discovery, put a stop to the burning or drowning of old women as witches? This horrible practice died out as mankind became enlightened.

Who ever demonstrated the absurdity, or preached against the folly of pretending to cure "king's evil" by the royal touch? Such superstitions are the result of benighted ignorance. The diffused light of knowledge dispels them; and in the same way the diffused light of science—wherein experimental physiologists have been the foremost workers—has improved practice. Yet, as I have already said, some of our very best-known practitioners can give no more satisfactory reason for the change, than can the country bumpkin say why his father did, yet he does not try, if an old woman would float on a horse-pond. He might say that the type of old women had changed!

Although there can be little doubt that it is in this way, by shedding a general light on practice, that experimental physiology has most contributed to progress, yet there are many minds which take in with difficulty abstract truths of this kind. They are more readily convinced when the proposition is put before them in a more concrete form. In order to do so, let us compare the operative surgery of

some centuries ago, with that of to-day. There was a time, in not remote antiquity, when surgeons had no other means of stemming the flow of blood after an operation, than by scorching over the raw and bleeding wound with a red-hot iron, or by plunging it into boiling pitch, or applying strong caustics to its surface. Hildanus, the patriarch of German surgery, was regarded as having made a great advance, when he devised the method of amputation with red-hot knives, so as to divide the soft parts and sear up the vessels at the same time. "Without," writes John Bell, "reading the books of these old surgeons, it is not possible to imagine the horror of the cautery."

"Verily, I confess," writes Ambrose Paré, "I formerly have used to stanch the bleeding of members after amputation after another manner than that I have a little before mentioned (by ligature), whereof I am ashamed and aggrieved. But what should I do? I have observed my masters, whose method I intended to follow, alwaies to do the like, who thought themselves singularly well-appointed to stanch a flux of blood, when they were furnished with various store of hot irons and caustick medicines, which they would use to the dismembered part, now one, then another, as they themselves thought meet. Which thing cannot be spoken, or but thought upon without great horror, much lesse acted. For this kind of remedy could not but bring great and tormenting pain to the patient, seeing such fresh wounds made in the quick and sound flesh are endured with exquisite sense. Neither can any caustick be applied to nervous bodies, but that this horrid impression of the fire will be presently communicated to the inward parts, whence horrid symptomes ensue, and oft-times death itself. And verily of such as were burnt, the third part scarse ever recovered, and that with much adoe, for that combust wounds difficulty come to cicatrization; for by this burning are caused cruell pains, whence a feaver, convultion, and oft-times other accidents worse than these. Adde hereunto, that when the eschar fell away, oft-times a

new hæmorrhage ensued, for stanching whereof they were forced to use other caustick and burning instruments. Neither did these good men know any other course; so by this repetition there was great losse and wast made of the fleshy and nervous substance of the part. Through which occasion the bones were laid bare, whence many were out of hope of cicatrization, being forced for the remainder of their wretched life to carry about an ulcer upon that part which was dismembered; which also took away the opportunity of fitting or putting to of an artificiall leg or arm, instead of that which was taken off. Wherefore I must earnestly entreat all chirurgeons, that leaving this old, and too too cruell way of healing, they would embrace this new, which I think was taught me by the speciall favour of the sacred Deity."

In another chapter of his work Ambrose Paré, giving an account of a case in which he amputated the arm, says: "Then presently I stanched the blood with an hot iron, for as yet I knew no other course."

Let us set side by side with this state of things an operation of our time.

Let me give a case of an operation which occurred recently in Steevens' Hospital, witnessed and watched afterwards by some persons present. Although occurring in my own practice, I merely cite the case, as it illustrates what happens in the hands of every hospital surgeon *au courant* with the improvements of the day. A patient has a tumour about the size and somewhat the form of a large lemon, in front of the right wrist joint. An anæsthetic is administered until she is profoundly insensible. Esmarch's bandage is applied. During the operation, the ulnar and median nerves, which are closely adherent to the tumour on each side, are dissected off. There is not one drop of blood to obscure the steps of the proceeding. After the Esmarch's bandage is removed, some vessels are secured by torsion; the wound, having been sponged with a solution of chloride of zinc, is closed by carbolised

catgut sutures and dressed antiseptically; after a time a hypodermic injection of morphia is given; she rests well that night; each dressing is done under carbolised spray; neither the pulse nor temperature ever rise above the natural standard; the wound did not heal by the first intention; but I can honestly say that during cicatrisation there was not a drachm of pus formed.

Could Ambrose Paré—but I need not go so far back—could Sir Astley Cooper rise from his grave and stand by on such an occasion, what would be his surprise! An operation—painless, bloodless, feverless, and almost without suppuration!

Mr. Fleming, the father of Irish surgery, was present at the operation, and watched the later progress of the case; and I think I may say he watched it with gratification and surprise, when he looked back to the recollections of his early days.

Now the question is, how far are these great results due to experiment on animals.

First, as regards anæsthetics, I may observe that chloroform was in fact discovered through experimentation on a low form of animal—the ant (*formica rufa*), which furnished formic acid.

But the action of anæsthetics has been investigated, both as to their local and general action, through experimentation on higher animals. The oft-repeated assertion, that we can gain no knowledge as to the action of drugs on man by their effect on animals is here refuted; indeed the refutation is to be found in the very mouths of those who make the assertion. By Act of Parliament, physiologists are obliged to use anæsthetics. Why? Precisely because we see that they have very much the same effects upon dogs, cats, and rabbits, that they have on man; effects so nearly similar, that much of our knowledge of them has been derived from direct experiment; in proof of which I would refer to the report of the Committee appointed by the Royal Medical and Chirurgical Society of London, "to

inquire into the uses and the physiological, therapeutical, and toxical effects of chloroform, as well as into the best mode of administering it, and of obviating any ill consequences resulting from its administration." *

Even the illustrious Simpson did not fail to seek by experiment on animals to elucidate the subject. I hold in my hand a paper presented to me by Sir James Simpson, and written by himself, on Anæsthesia: "Notes on its Artificial Production by Chloroform, etc., in the Lower Animals and Man."

Do not let me be misunderstood. I do not pretend to say that chloroform or any other anæsthetic is a discovery altogether due to experiment on animals. What I assert is, that this great improvement in surgery would be incomplete but for experiments. They have played their part, and an important one, in the completion of this portion of the edifice.

The subcutaneous injection of morphia and other drugs, now so very generally in use, will, as regards the saving of pain, be admitted to be an important adjunct to the anæsthetics employed during the actual operation. Dr. Alexander Wood† has the merit of having been the first to use it. The late Mr. Rynd, as you know, was one of the first who took it up. Mr. Rynd told me that his first observations were made on sporting dogs. "I feared," said he, "that the injection might excite inflammation of a phlegmonous kind in the subcutaneous cellular tissue. When I found from experiment that this was not the case, I gained confidence and made trials which proved satisfactory on men."

We have also a Report ‡ on this subject made by a Committee of the Medical and Chirurgical Society, in which some important practical points are elucidated by

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* "Transactions of the Royal Medical and Chirurgical Society of London," vol. lxvii. p. 323.

† "New Method of treating Neuralgia by Subcutaneous Injection." 1855.

‡ "Medico-Chirurgical Transactions," vol. 1, p. 561.

experiment on animals. Hundreds of persons are now daily using this admirable method of allaying pain, who know little of the cautious experimental steps which led up to it. But here again experiment on animals has effectively played its part.

We next come to that part of our operation in which we had the truly marvellous spectacle of an incision four inches long, without the effusion of one drop of blood, and a careful, and I might say, minute dissection of the ulnar and median nerves, with no more to obscure the view of the operator than there is on the page from which I read. I do not know whether Professor Esmarch made any experimental trials upon animals or on himself before he actually put in practice his admirable idea. But this is certain: experimental physiology had paved the way for him. To ascertain how long the tissues can survive when deprived of the circulating fluid without risk of mortification was a prime step. The well-known experiments of Brown-Séquard and others had in a great degree solved this problem.

But I will confess I should not have had the hardihood to bind the arm of a patient after the manner of Esmarch, and keep it for twenty minutes or half an hour as completely bloodless as though it were the arm of a decapitated corpse, unless I had satisfied myself by experiments made first on animals, and later upon myself, that it could be done with safety. (To protect myself from prosecution, however, I beg to say that those experiments, which from personal experience I can state are very painful, were made previous to the passing of the penal law against physiologists.)

With reference to the control of the bleeding after the tumour was removed, it is hardly necessary for me to speak. Every one, save those who are so prejudiced as to shut their eyes to the most obvious facts, knows that Harvey's great fundamental discovery (which underlies everything connected with the circulatory system and its

diseases and accidents) was made by the aid of experiment on living animals. He himself writes :—

"When I first gave my mind to vivisections as a means of discovering the motions and uses of the heart, and sought to discover these from actual inspection and not from the writings of others, I found the task so truly arduous, so full of difficulty, that I was almost tempted to think with Fracastorius, that the motion of the heart was only to be comprehended by God."

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"At length, and by using greater and daily diligence, having frequent recourse to vivisections, employing a variety of animals for the purpose, and collating numerous observations, I thought that I had attained to the truth, that I should extricate myself and escape from this labyrinth, and that I had discovered what I had so much desired, both the motion and the use of the heart and arteries ; since which time I have not hesitated to expose my views upon these subjects, not only in private to my friends, but also in public, in my anatomical lectures, after the manner of the Academy of old."

A few years after Harvey's death (1657), Malpighi (1661) demonstrated for the first time the circulation of the blood in the lung of a living frog ; thus by direct experimental demonstration silencing for ever Harvey's opponents.

It does not appear from Ambrose Paré's writings whether he made experiments on the lower animals before using the ligature on man. We cannot, therefore, claim for this great surgical discovery that it took its origin in experiment on animals. Being a truly humane man, it is probable that he did. But this much may be said with certainty, when Paré conceived the idea of giving up the hot iron and using the ligature, his first trials had of necessity to be made either on his fellow men or on some lower animal.

Living in the sixteenth century, he was at least permitted

to do what true humanity would induce any reasonable person to do. But had he lived in this island in the nineteenth century, he would find a law absolutely prohibiting him from making his first trials on any lower animal, unless indeed he so far humiliated himself as to sue at Dublin Castle for such a document as I hold in my hand—a licence granted by the Right Hon. Michael Edward Hicks Beach, Bart., Chief Secretary to the Lord-Lieutenant for the time being, to me, the President of your College.

But Paré's discovery of the ligature left much still to be done. Dr. Jones, by his experiments, greatly improved the practice, and is admitted by some of the best known surgeons to have done much for surgery. Sir William Lawrence says of him :—

"He has banished the use of thick and broad threads, of tapes, of reserve ligatures, of cylinders of cork and wood, linen compresses, and all the contrivances which, employed as a security against bleeding, only served to multiply the chances of its occurrence." The late Mr. Samuel Cooper, who was also an eminent surgeon, and the author of an important work on surgery, says : "The principles which should guide the surgeon in the use of the ligature were not known until the late Dr. Jones published his valuable treatise on hæmorrhage." Professor Spence, of the University of Edinburgh, who is entitled to speak authoritatively as to the value of Dr. Jones's experiments, because he himself has performed many experiments in the same direction, says : "When we look at the principles laid down by Dr. Jones as the results of his investigations, and for which he has received so much credit, it will be found that the great service he performed consisted not so much in pointing out any new fact observed in the process of obliteration of an artery, as in scientifically investigating the subject as a whole ; ascertaining in a great measure the relative value of the different parts of the process, and drawing from his experiments sound practical deductions

as to the causes of secondary hæmorrhage, and as to the best means of procuring obliteration." *

I feel that it is not necessary for me to point out how far the practice of torsion, and the use of the carbolised catgut, are due to experiments on animals. The experimental researches of Bryant,† Humphry, Lister, and others, are too fresh in your minds. Both these methods of arresting hæmorrhage are to my mind great advances in surgery, due almost entirely to experiment. It is a pleasing sight to see a large wound closed up with catgut sutures, and with not even one silken ligature acting as a foreign body to impede the healing process. As yet both torsion and the use of catgut ligatures are, I may say, on their trial. Many surgeons still adhere to the silk ligature. Were we allowed to demonstrate on animals the safety and real usefulness of the other methods, they would sooner come to be adopted by others. Unless there is such an opportunity of demonstration, such improvements come to be adopted slowly. Dionis was the first French surgeon who adopted and recommended Paré's method. This happened towards the end of the seventeenth century, whilst Paré lived in the middle of the sixteenth. Cooper tells us that about a hundred years after Paré, buttons of vitriol were used in the Hôtel Dieu in Paris, for the stoppage of hæmorrhage after amputations.

Demonstrations on animals would, in this instance, have saved mankind for one hundred years the torture of the cautery.

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* Dr. Jones himself, in a preface to his book, says that: "He regrets the necessity of obtaining even this important knowledge by the sacrifice of brutes. But when we remember the incessant scourge of war which has followed man through all the ages of his history, not to mention the consequences of accident and disease, it is not too much to assert that thousands might have been, and may still be, saved by a perfect knowledge of these subjects; which can only be obtained by experiments on brutes; indirectly, and very slowly, by observations on the injured arteries of man; and even these cannot be made until he has fallen a sacrifice to the want of assistance, or to the imperfect knowledge of the surgeon."

† "Medico-Chirurgical Transactions," vol. li. p. 199.

The absorption of carbolised catgut ligatures and sutures may be truly regarded as one of the marvels of modern science. It forms a part of what is due to the beautiful experimental researches of Professor Lister. Its immense value as an addition to practical surgery must be viewed, however, not merely as a means of arresting hæmorrhage, but in connection with the next part of our subject, viz: that which rendered the progress of our case feverless and free from suppuration.

Great as are the triumphs which have rendered operations painless and bloodless, the practical surgeon will value even more highly those which render convalescence after the operation free from fever and free from suppuration.

It would not be possible, within the limits of my already too lengthy address, even briefly to pass in review the observations—clinical, chemical, experimental—which have led to the happy results now frequent, if not constant, after operation. I will allude, as shortly as I can, to the share which experiment on animals has had in the achievement.

As you know, Musitanus, so long since as in 1702, explained surgical, or traumatic fever, as being a commotion excited in the system by the absorption of the putrid matter of wounds. Roser more recently adopted the same view, and many surgeons adapted their practice to this hypothesis of infection by putrescent substances. In December, 1866, Maisonneuve presented to the Academy of Sciences an exposition of the theory of surgical intoxications, in which he came to regard wound-fever as "the result of poisoning, due to the introduction into the torrent of the circulation of toxic substances produced in the organism itself." According to this view, simple wound-fever, secondary fever, and the still more terrible forms of putrid fever, purulent fever (pyæmia), are but varieties of the same sort of infection—an infection or poison resulting from something generated within the organism itself. To investigate the causes of these decompositions; to discover with some approach to precision what particular matters

resulting from them called into being these dreaded fevers; and then, finally, to arrest or prevent their formation, and so avert the terrible train of symptoms but too often ushered in by the ominous shivering fit which made the heart of the surgeon sink within him, and marred the success of the best planned and most skilfully executed operation: this was a worthy aim for the scientific surgeon. It is not going too far to say that much has been achieved in this direction by the united efforts of clinical observation and experimental research. I do not think it possible for any honest man who approaches the subject dispassionately, to say that the latter has not played an important, an essential, part in what has been done.

From Hunter's time up to the present many experimental observations have been made on the temperature of inflamed parts. Many experiments have been made in the hope of solving the problems connected with purulent infection. The researches of Lee, Bennett, Ducrest, Castelnau, Sedillot, and others, are well known. But the experimental researches of the illustrious Professor of Surgery at Vienna —Billroth—and those of Weber, have a greater value than any which preceded them. The experiments also of Breuer and Chroback (Zur Lehre von Wundfieber), leading to the conclusion that the fever accompanying traumatic inflammation is independent of the action of the nervous system, have a value which no thoughtful man will deny. I cannot help alluding to the observations of Billroth and Weber, as they seem to be of strikingly practical import. Billroth, in his second series of experiments, tries to determine which of the constituents of pus may be that to which we should really attribute the fever-exciting property. In order to decide this question, he selected various substances found in pus undergoing decomposition; with these he made injections, both subcutaneously and into the veins. In this way he investigated the properties of sulphuretted hydrogen, bi-sulphuret of carbon, sulphuret of ammonium, carbonate of ammonia, leucine. As regards local effects,

the following results were obtained:—Sulphuretted hydrogen and bi-sulphuret of carbon do not give rise to any local inflammation; sulphuret of ammonium causes suppuration with fever; carbonate of ammonia produces very intense local effects, followed by fever.

As regards temperature, sulphuretted hydrogen does not induce any change; bi-sulphuret of carbon caused a considerable augmentation of heat; sulphuret of ammonium produced no immediate effect on the temperature. But leucine in three experiments excited an increase of heat so considerable that the thermometer stood at 104° Fahr. Carbonate of ammonia gave very remarkable results, entirely different from all the other substances; injections of it were always followed by a lowering of temperature proportionate to the quantity injected.

This phenomenon is unquestionably one of great interest, especially when we consider its bearings on the symptoms of uremia.

As regards the constitutional disturbance which it excites, Billroth has found that perfectly fresh pus acts even more energetically than desiccated or putrid pus.

Weber has verified many of the foregoing results, and even confirmed many points still doubtful. Thus he has found that pus, the filtered serum of pus, sulphuretted hydrogen, and various products of exudation, are of a phlogogenic nature, that is, excite inflammation. Besides, every one of these substances is likewise pyrogenic, that is, they excite a fever and cause an augmentation of temperature not dependent on the local inflammation. He has found also that not only carbonate of ammonia but likewise butyric acid, causes a remarkable lowering of temperature. Weber has also discovered the important fact, that not only does the pus of one animal, when injected into the veins of a second, excite fever, but the pus of an animal when injected into its own veins, is followed by an intense febrile excitement in this same animal. He has likewise shown that the blood itself, under certain con-

ditions, may excite fever ; in fact, when the blood of a dog, attacked with fever, is injected into the veins of a healthy dog, this animal is attacked with fever. The blood bears with it the ingredients which excite the fever, and each fresh injection causes an exacerbation of the febrile condition.

Gentlemen, I pity, but I confess I cannot reason with, the man who is unable to see the bearing of these facts on practice. I should as soon think of descanting on the exquisite colouring in one of the stained-glass windows of Cologne Cathedral, to a man suffering from Daltonism.

It would be foreign to my purpose to enter even briefly on the consideration of the researches, which more or less directly led up to the practice now-a-days so ably advocated by Professor Lister. I may, however, be permitted to say, that I heartily concur with Dr. Roberts in the views expressed by him in his admirable address delivered at the Manchester meeting of the British Medical Association, when he says—"It was the distinguished merit of Lister to perceive that these considerations pointed to a means of preventing septicæmia. He argued that if you could prevent the access of septic organisms to the wound, or destroy them there, you would prevent decomposition, prevent the production of the septic poison, and thus obviate the danger of septicæmia. It is not within the scope of this address to describe the means by which Lister attained this object, still less to pass judgment on his practice ; but I may be permitted to express my belief that the principle on which the treatment is founded is unassailable."

But, gentlemen, I have already taxed your patience too long. I have compared a single operation, such as is of frequent occurrence in the present day, with the horrors of bygone times. I have, I hope, shown you that experimental physiology has had some share in bringing about the grand result which makes such an operation painless and bloodless, and disarms the convalescence which follows

it of the dangers of wound-fever and suppuration. I have striven to state my case fairly and without exaggeration.

Were I to attempt to go further into the subject, to show the vast number of ways to which the practitioner avails himself in practice of the light derived from experimental physiology, I should write a book, not deliver an opening address.

I should, indeed, far exceed the limits of what is suitable for an evening entertainment, were I even to enumerate the researches which have taught us nearly all that we know with any accuracy about respiration, digestion, the functions of the cerebral and spinal nerves, the circulation, and the heart sounds, the growth and development and regeneration of bone, nerve tissue, etc., the phenomena of inflammation, and much more the knowledge of which is as indispensable to him who would practise medicine as it is to the surgeon.

Although it is no portion of the task which I at first laid down for myself, yet I would gladly, gentlemen, if time permitted, enter into the moral and ethical aspect of this question, in its effects on those who engage in, or witness, experiments on animals. I could show, upon good authority, that it is an error to suppose that contact with suffering tends to brutalise and harden us as a profession. I could show that it is the passive witnessing of what is painful or horrible which dulls men's better nature.

Gazing at an execution, or a bull-fight, reading sensational novels, and writing sensational articles, may have this effect. Does the Sister of Mercy, I ask, or the lady-nurse become less benevolent because of her daily intercourse with pain and sorrow? The surgeon who performs, or the student who witnesses, a painful operation is not rendered less kindly or benevolent—the one by his effort to mitigate the suffering or prolong the life of his patient, or the other by his effort to learn how to do so? The physiologist or the student who, with a truly noble object in view, performs or witnesses experiments on animals, is

not rendered "devilish" by this any more than a man would be who went from prison to prison to witness execution after execution, with the very laudable object of making the process of hanging so complete, that even wretched criminals may be saved from a prolonged and painful agony in their exit from this world.

We have the authority of the great moral philosopher, Bishop Butler, for this simple distinction between the passive and active phases of what is awakened within us by witnessing distress in others. "Perception of distress in others," says this learned author, "is a natural excitement, passively to pity, and actively to relieve it. But let a man set himself to attend to, inquire out, and relieve distressed persons, and he cannot but grow less and less sensibly affected with the various miseries of life with which he must become acquainted; when yet, at the same time, benevolence, considered not as a passion, but as a practical principle of action, will strengthen; and whilst he passively compassionates the distressed less, he will acquire a greater aptitude actively to assist and befriend them."

Like the muscles, the human affections and sympathies gain strength and vigour by exercise. The use of the sledge-hammer makes the palm less liable to be galled or blistered, but it is what gives strength and power to the thews and sinews of the smith's right arm. The active effort to relieve distress, the firm determination to subdue the selfish part of the emotion of pity, and the struggle to face scenes, whether in the dissecting-room or the dead-house, the operation-theatre or the physiological laboratory, from which most men shrink back at first with horror and disgust—this is what strengthens within the surgeon some of the noblest qualities that human beings are endowed with. Thus there is developed within him the truest, the most active benevolence. Not that I assert that we are made of a superior clay to other mortals; but merely that the circumstances which surround our lives tend to produce these natural results. "But," continues

Bishop Butler, "going over the theory of virtue in one's thoughts, talking well, and drawing fine pictures of it; this is so far from necessarily or certainly conducing to form an habit of it, in him who thus employs himself, that it may harden the mind in a contrary course, and render it gradually more insensible, *i.e.*, form an habit of insensibility to all moral considerations."

In conclusion, gentlemen, I will ask you to pardon my egotism, when I say that I feel I have done something to awaken among my younger brethren in Dublin the spirit of physiological and histological research. There is no part of my professional career that I regard with so much pride and satisfaction. This gives me a kind of right to address a few words to the younger and more earnest among my hearers *apropos* of the persecution (for I can call it nothing else) now levelled against physiologists.

There is not one among you who feels more keenly than I do, the indignity offered to physiologists by the passing of the Act of 39 & 40 Vic., chap. 77—I will not wound you by reading its offensive title.

Like most efforts wherein benevolence fails to be directed by reason and good sense, it will, I fear, produce ill results.

For two reasons I profoundly regret that it ever became law.

1st. It will inevitably lead, in my judgment—indeed I am convinced it has already led—to clandestine experimentation, causing an increase of animal suffering as well as a certain familiarity with law breaking.

2nd. Because the protective clause of the Act was purchased by a sacrifice of our dignity. For this, we bartered away the honour of our noble profession.

I am fully aware that the indefiniteness of the Act makes it in a great degree unworkable, and, I might almost say, offers temptations to evade its provisions, as no doubt you could easily do if you pleased. But I earnestly beg of you to remember that it is now the law of this land. Fall in loyally with it while it is so. Many an imperfect enact-

ment, if well administered, has done good. It is by no means impossible that this one may have the effect of giving aim and directness to physiological research. It will certainly tend to make the best of our students seek on the continent of Europe that sort of education which is denied to them at home. It will thus enable them to gain a knowledge of those languages, little cultivated in our Universities, yet essential to those who desire to maintain the reputation of our schools.

Do not despair, then, of the progress of physiology in Ireland or Great Britain. Acts of Parliament cannot stop the tide of progress, any more than they can stem the tide of the ocean. To use the words of a song familiar to Irish ears,

> "When laws can make the blades of grass
> Cease growing as they grow,"

then laws may arrest the progress of science. If you, as earnest, honest men, with faith in your cause and a humane spirit such as actuated Haller, Harvey, Bell, Jones, in your hearts advance, you will be met in a similar spirit by the individual who in the main has entrusted to him the carrying out of this Act. I allude to Dr. Burke, a gentleman who unites humanity and good sense, with an honest intention to work the Act fairly, and a just appreciation of what experimental physiology has done, and may do for mankind.

As to fanatics who placarded our walls with sensational diagrams ; advertised in our newspapers, offering bribes for secret information ;* asserted what every one in this assembly knows to be false, that our pupils are engaged in cutting up living animals, when we know they never do anything of the kind ; and who in their wicked foolishness

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* "To Laboratory Assistants, Hospital Porters, and others.—Wanted, the names and addresses of persons, licensed and unlicensed, performing painful experiments on, or dissecting living animals. Liberal remuneration given for expenses and loss of time. Communications considered private if desired.—Apply by letter to George R. Jesse, Esq., Hon. Sec. and Treasurer, Society Total Abolition, &c., Vivisection, Henbury, Macclesfield, Cheshire."—*The Standard.*

endeavoured to injure the sick poor in our hospitals, by turning aside the subscriptions given on Hospital Sunday—as for these people, I would treat them as Harvey treated those in his time. In a letter to Riolanus, he says: "There are some, too, who say that I have shown a vain-glorious love of vivisections, and who scoff at and deride the introduction of frogs and serpents, flies, and others of the lower animals upon the scene, as a piece of puerile levity, not even refraining from opprobrious epithets. To return evil speaking with evil speaking, however, I hold to be unworthy in a philosopher and a searcher after truth; I believe that I shall do better and more advisedly, if I meet so many indications of ill-breeding with the light of faithful and conclusive observations."

But let it not be supposed that those who do not refrain from opprobrious epithets form the bulk of the public. The great mass of mankind have too much good sense *au fond.*

They will ever continue to repose confidence in our great profession; they will feel the practical absurdity of regarding men as wantonly cruel, whom they see and know to be kindly and humane; they will see the folly of looking with distrust and suspicion, as regards their relationship with the lower animals, on men whom they would trust in reference to their soldiers, their sailors, the sick poor in their hospitals, and their wives and children in their homes.

As for the well-intentioned but thoughtless fanatics who have got up this persecution, be not scared at their outcry. If you cannot feel confidence in their good sense, you may have unbounded confidence in their selfishness. How many of them are there, do you fancy, who if, for instance, they had reason to think their bodies were pervaded by thousands of trichinæ worms creeping about in their muscles, would not come supplicating assistance from those who have learned from experiment, and experiment only, the life history of this animal; and if not yet the

method of cure of this loathsome disease, at least its mode of prevention.

How many would allow their children to continue victims of epilepsy, if they thought they had a bare chance of getting them cured, by consulting a physician whose experimental researches had made him specially learned on this subject.

Fear them not, for they are as shabby as they are selfish. They may rail against experimental physiologists, but they will never dream, when the day for operation arrives, of having their limbs amputated with hot knives, because experiment on animals has taught surgeons other and better methods.

In their hour of agony they will not refuse the relief of the subcutaneous use of morphia, because Mr. Rynd's first trials were made on sporting dogs.

I will confess that I do not like introducing into the discussion of this subject any allusion to that "inscrutable Power at once terrible and beneficent, in whom we live, move, and have our being." I feel confident, however, that there are not many who in their actual conduct would be found to act up to the sentiments bravely expressed by one who has shown herself perhaps the ablest and certainly one of the most earnest opponents of experiment on animals. "If that terrible Power," writes Miss Cobbe, "will in truth scourge us with a hundred diseases unless we thus propitiate him, then would I, for one, deliberately pray, let these dread diseases overtake me, and let me die, sooner than share any benefit from such foul rites, or ever say to this new Moloch of science, 'Thou art my God.'" *

Let it not be supposed that I quote these words to mock them. The fearless, truth-loving, vigorous-minded lady who wrote them would never have penned them had she not felt them, as regards herself, to be true. I would, however, remind her that there is something which touches

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* Miss F. Power Cobbe.—*Contemporary Review*, 1877, p. 335.

generous natures like her's even more deeply than their own suffering. If it was her lot, as it is ours day after day, to feel the feebleness, the impotence of our art to grapple with disease; if it was her duty again and again to have to pronounce the fatal word, when asked "Can nothing save my child?" if she had to witness the powerful frame of a stout-hearted man convulsed and bowed down with sorrow when the dread sentence is passed upon his wife, "Yes, it is cancer;" or to look upon the agonised face of a mother who watches her baby (which she will entrust to no other) as it lies upon her lap, and with a power of observation, by experience rendered painfully acute, perceives the movement of the lips, the twitching of the eye-ball, the pallor and subsequent lividity of the face, the restless motion of the head, and helpless cry that usher in the general convulsion;—then I believe she would at least have some sympathy with those who have proved themselves willing not only to suffer pain themselves, but to inflict it on the lower animals, in order to strive to make even one single step towards unravelling the vast entanglement in which the mysterious phenomena of disease are involved. She would admit that the workings of the terrible Power she alludes to are indeed inscrutable; for she would see myriads of animals destroyed by painful deaths for the maintenance of their fellows; she would see few great things accomplished in this world without pain, and travail, and sorrow; the greatest truths making progress not unaccompanied by torture and persecution; the liberties of mankind won not without bloodshed. She would see few, if any, of the great achievements which accompany human progress, gained on the whole with less pain and bloodshed than those which have rewarded the efforts of the experimental physiologist, and sanctified his work.