enable those who use it to furnish statistics such as I have indicated, and thus to enable us to judge more dispassionately of its real merits. In comparing of late the results of my own hospital practice, I have been struck with the success which attended very simple treatment, and this leads me to question our progress in departing from such treatment for more complicated methods. Thus I find that, during a period of three years, out of sixty-three major amputations for disease, there were only three deaths; and of twenty-three cases of excision of joints, only two deaths, at a time when the treatment consisted in thoroughly cleansing the cut surface by pouring tepid water over it, and occasionally applying tincture of iodine alone, or diluted, on the flaps; whilst the dressing consisted merely in laying a veil of lint or thin muslin over the stump. Again, when preparing statistics of my amputations for my published lectures, I found evidence that certain conditions, such as the nature of the disease or injury necessitating the operation, had most important influence on the result; such influence, indeed, as I could not have supposed until the statistics brought it distinctly before me, and my later statistics corroborate my former; so that I cannot accept the statement that any method of dressing, however good, will ever enable us to guarantee success. Whilst I speak of the antiseptic system, meaning the special method, I need hardly say that all surgeons have for their object the avoidance of putrescence, though their views may differ as to the best way of attaining their object.

I cannot close without recommending to notice the advantages obtained in treating lacerated wounds and burns of the extremities by continuous immersion in the tepid bath. The avoidance of all meddling with the injured part, and of the agony of the patient caused by changing dressings, is thereby so completely obtained, that the method requires only to be fairly tried to shew its advantages. In the case of burns these advantages are most conspicuous, for the constant moisture keeps the cicatricial tissue pliable, and motion of the parts can be effected gradually, whilst the limb is immersed in the bath. The water may be rendered antiseptic by carbolic or boric acid should that be considered desirable.

In conclusion, I think the retrospect I have taken shows that when true principles have been disregarded or lost sight of, they have ultimately had their value recognised, and, modified and improved, have become the advances of the present day; and so for the future I feel confident, that all that is true and good in any system will emerge with greater beauty and usefulness for having been purged of its dross by the tests of observation and candid criticism.

NORTHAMPTON.—In Mr. Haviland's report on the sanitary condition of the borough of Northampton, it is stated that during 1873 the death-rate was 20.3 per 1,000 living, whereas in 1872 it was 29.7, one of the highest death-rates in any of the large towns and cities of England and Wales. Last year, the death-rate was 21.5 per 1,000. The reduction in the mortality for this town from 25.1 during the decennial period 1861-70, to an average of 20.9 during the past two years, is a matter of considerable satisfaction, for it indicates a saving of as many as 172 lives *per annum* out of the total population; and it is to be hoped that the town council will do all in their power, by carrying on their sanitary work, to facilitate and render permanent this downward movement of the death-rate. In this direction, there is undoubtedly yet much to be done. Thus, whereas throughout England and Wales the infantile death-rate is 184 per 1,000 infants living, it is 223 in Northampton; and this mortality has, to a large extent, been brought about by diarrhoea and other diseases which are so far preventable that their prevalence may, to a considerable extent, be controlled by efficient sanitary administration. Although there is also room for improvement as regards the "fever" death-rate, it is satisfactory to note that, whereas it was formerly unduly high, it is now less than the average for the country generally; and this result Mr. Haviland in a large measure attributes to the general use of the Northampton Waterworks Company's water, which has taken the place of the contents of shallow wells, which for ages had become polluted by their close proximity, in a porous ironstone, to filthy receptacles of sewage of every description. The epidemic of scarlet fever which occurred in so many parts of our country last year caused a considerable mortality in Northampton; and, in urging upon the town council the provision of proper hospital accommodation for the isolation of those sick of this and other allied diseases, Mr. Haviland calls attention to the beneficial results which attended the construction of the small-pox hospital a few years ago. The lessons to be learnt from the death-rates prevailing in this town were rendered the more plain to the sanitary authority to whom this report was addressed by the use of Mr. Haviland's now well-known maps, in which the geographical distribution of the various diseases referred to was rendered easy of comprehension.

ADDRESS IN PHYSIOLOGY,

BY

WILLIAM RUTHERFORD, M.D., F.R.S.E.,

Professor of the Institutes of Medicine in the University of Edinburgh.

MR. PRESIDENT AND GENTLEMEN.—In the medical schools of Scotland physiology is still designated the Institutes of Medicine. This somewhat ancient title has the advantage of continually keeping before the minds of our students the fact, that physiology lies at the very foundation of scientific medicine. Much, indeed, has been done in Germany and elsewhere to detach physiology from medicine, and to develop it as a branch of purely natural science. Such a development, however, gives to physiology an aspect somewhat different from that which it wears when we regard it as the Institutes of Medicine; and, therefore, this title is useful to the teacher, inasmuch as it constantly reminds him that he is more especially to dwell on those facts of physiology which are of value to the practitioner of medicine.

Physiology is—I need scarcely remind you—an experimental science. Apart from those which are the result of artifice, experiments of great physiological significance are being continually wrought for us by accident and by disease. Physiology owes much in the past to those who, like you, are engaged in the daily study of these pathological experiments; for, was not the function of the spleen an enigma until the effect of one of its diseased conditions was observed? Has not psychology been revolutionised by a study of brain-disease? Did not Hughlings Jackson, with singular sagacity, apprehend from a study of disease, that convulsive movements may spring from the cortex of the brain; a fact which, as you know, has since been demonstrated by laboratory experiment? Was not the crossed action of the cerebro-spinal axis revealed by the study of disease? and, indeed, was not the very foundation of nerve-physiology to a large extent based upon a study of nervous disease by Charles Bell? Assuredly we cannot afford to detach physiology from medicine; for, without a doubt, in the future as in the past, the study of those bodily conditions that are abnormal will throw much light on those that are normal.

But, on the other hand, need I remind you of the difficulties you encounter in the analysis of these experiments of disease, as you endeavour, for example, to determine the nature and the seat of some malady—say of the brain; the cause of a dropsy; the indication of some change in temperature, in the state of the pulse, the respiration, or some secretion; and, need I aver that, in this difficult task of pathological analysis, your labours grow less as physiology advances; and, if you look around you, is it not evident that those whose knowledge of physiology is the most profound, are precisely those who are most able to conduct this analysis with skill and with success.

At the present time, there is reason for some feeling of anxiety with regard to the manner in which this much-needed knowledge of the Institutes of Medicine is to be attained by our students. In recent years, the teaching of physiology has made a great stride in this country. Laboratories duly appointed have been, and are being, organised. The method of physiological instruction has, in most instances, passed from the mere prelection, illustrated by diagrams, to an experimental exposition of the subject. Very many of those whom I now address, had doubtless an experience of physiological tuition similar to my own. I remember well how much we were told about the action of the bodily organs, but how little we were shown of such actions. We were lectured about the action of the heart and its valves, but the motions of the heart we never saw, the operation of its valves was never shown to us. We heard about the manner in which the blood-pressure might be measured, and we were duly apprised of the number of pounds and ounces of pressure on the square inch, but this fundamental experiment we never saw, nor did we even see the instrument that was used for the purpose. We heard about the chemical transformations which the food undergoes when affected by the saliva and the gastric juice; but of the manner in which it might be demonstrated that these changes really do occur we knew nothing. The bile was said to contain certain acids and pigments; the urine had urea, uric acid, etc., but all demonstration of these great facts was neglected. In alluding to these defects in the tuition of physiology from which some of us have suffered, I would not have it supposed that I desire to cast reflections upon the teachers; such is far from my intention, for I know the difficulties against which they had to contend, and I know the success with which they finally overcame them; but I merely desire to remind some of you how much

you, like myself, were crippled in the study of practical medicine, by the sadly imperfect manner in which we learned the cardinal facts of physiology.

At this moment there is a distinct danger of a return to something like this miserable mode of instruction, in consequence of the fanatical clamour of a number of persons excited, it must be admitted, by one or two members of our own profession.

I cannot suppose that any member of this Association entertains the idea that experiments on the lower animals are not justifiable for the discovery of new truth; but I am aware that there are some who entertain the idea that vivisection is not necessary when it has for its object the mere demonstration, for educational purposes, of facts already known. Those who hold this doctrine, appear to me to forget that physiology is an experimental science, and that no right conception of the subject can be obtained unless the student be shown the experiments that are necessary for the demonstration of certain facts. No language, however clear, can depict the movements of the heart or the circulation of the blood, so as to call forth a picture so vivid and true, as that which without an effort springs forth the moment these things produce an impression through the organ of sight. The manner in which the blood-pressure is affected by retarding the heart's action, by dilating the arterioles, or by muscular contraction; the changes which follow division of the sympathetic nerve in the neck; the action of the chorda tympani on the submaxillary gland; the result of stimulating the cortex of the cerebrum; the reflex power of the spinal cord; the tetanus from strychnia; the paralysis from curara; these and other equally great fundamental facts in physiology give rise to nothing more than vague and feeble conceptions unless they are witnessed. Has not every teacher repeatedly observed the altogether different mental attitude which the student assumes the moment you pass from a mere description to a demonstration of phenomena? You far more forcibly arrest his attention, and far more deeply imprint upon his mind the facts which you would bring home to him. He is apt to forget that which he has merely been told, but the impression of that which he has seen is with difficulty effaced.

There is, however, a great deal more than this. It is not enough that the student of medicine should be crammed with so many facts about the bodily machinery—it is of infinitely greater importance that he should be really *educated* in a knowledge thereof. He cannot be educated unless you assume that he has a right to question your statements, and to demand from you a demonstration of the truth of what you say. It is not enough that you merely state to him, in an *ex cathedra* manner, that this or that is so; you ought on all possible occasions to *prove* to him that it is so. You ought to appeal to him as an individual, who can and who ought to think for himself, and, therefore, as an individual who is not merely to be crammed, but to be convinced.

Every examiner in physiology must have repeatedly observed how glibly the student may repeat to you a statement of fact, but how sorely puzzled he often appears when you ask him for the evidence upon which his statements are founded. Moreover, every one must have noticed how often the student lacks knowledge that is definite: he mingles together things that are certain and things that are uncertain, because he has not been sufficiently trained to examine experimental evidence. Of how little avail is this unreasoning and indefinite knowledge when the student comes to thread his way through the difficult analysis of disease! He either believes or he doubts too much. He oscillates from one extreme to the other, because he is wanting in definite knowledge, and because his critical power has not been sufficiently developed.

Now, I maintain that this definite and critical knowledge regarding the bodily mechanism cannot be attained unless our students be shown experiments on living animals; and I hold that those authorities who seem to be of opinion that this method of tuition may be dispensed with, are entirely overlooking the vast importance—not only to the student himself, but to our whole race—of an experimental manner of laying the foundation of a knowledge of the institutes of medicine.

It is, I believe, too much to assume that those who advocate a flimsy and superficial method of teaching physiology, are altogether actuated by feelings that are termed humane. I fear that there is lurking behind this disposition a feeling of indifference to what are the true principles of medical education, or, what is more likely, there is an incorrect appreciation of these principles. It must, however, be admitted that, notwithstanding the vast importance of physiological experimentation in the teaching of medicine, the circumstances under which it is possible demand serious consideration.

No one can deny that animals daily suffer pain in order that they may be eaten by man, and also in order that man may not be eaten by them. If it be thought necessary to put countless animals to a painful death for the purpose of keeping the human race alive, may not the

physiologist inflict pain, not only for the discovery of new truth, but also for the right education of those who have to spend their lives in preventing and in healing disease? Is the one object less important than the other? Indeed, is not the object in both cases precisely the same? Why, then, this clamour against physiologists? The pain produced in all the physiological experiments performed in this country in the course of a year is, I believe, as nothing when compared with that inflicted upon animals by the members of her Majesty's Lords and Commons on an autumn holiday.

I would not, however, for a single moment speak lightly of the production of pain, for it is the function of those who pursue medicine to alleviate pain and suffering. Our students would, indeed, be badly trained for the pursuit of a calling so humane as that of medicine, if they did not find us on all occasions carefully avoiding the giving of pain that is not absolutely necessary. Therefore it is our duty, for this as well as for other and obvious reasons, to use narcotics in our experiments upon animals whenever it is possible. It is not necessary for a sound physiological education that our students be shown *all* the experiments that are needed to demonstrate physiological truths. We probably do enough if we show them experiments on the cardinal points of physiology; and I aver that all the experiments on the higher animals that are really required for the purpose of education can be performed with the aid of narcotics.

Seeing that this is so, why should it be that some have become convinced that, in consequence of the present inflamed state of the popular mind on the subject of vivisection, the right education of medical students must be abandoned? The popular mind has been abused by inaccurate and misleading statements regarding both our motives and our actions. I maintain that a great and deplorable error was committed when the unreasonable clamour of the antivivisectionists was met in the spirit of compromise, instead of the spirit of stern resistance. I believe that the unfortunate vivisection Bill that was laid on the table of the House of Commons conferred a dignity upon the policy of the antivivisectionists which, but for that Bill, it would probably never have possessed. It is true that there has been a withdrawal of that singular Bill, according to which we were to have been fined fifty pounds, or to have been sent to prison for two months, if we had dared to show our students any experiment even upon a narcotised animal; but the effect of the Bill is not effaced. The increased boldness which it has given to the pretensions of the antivivisectionists is only too evident. All that we can now hope for is, that the good sense of our legislators will in the end prevail, and that they will do nothing to hamper the education of our medical men.

I have felt, sir, that in an address on Physiology I could not pass over in silence this, the leading topic of the day. I have alluded to it but briefly; for, although it is of the deepest importance, there is another subject to which I would ask your attention.

Nine months ago, in an introductory address given in this University, I had occasion to review the present aspects of physiology (*Lancet*, Nov. 1874). Having nothing further of a general character to say thereon, I have selected a special subject in physiological pharmacology which will, I trust, prove interesting to the practitioner of medicine.

The remainder of the address was devoted to a report of a large number of experiments on the biliary secretion of the dog, by Professor Rutherford and Monsieur Vignal. As this part of the address was illustrated by a large number of graphic representations of the results of the experiments, and as the expenses of the research have been defrayed by the Scientific Grants Committee of the Association, a full account of the experiments, with woodcuts of the curves shown in the lecture, will be published in the *JOURNAL*. The following is a brief *résumé* of this part of the address.

It has been shown by Professor Hughes Bennett's Committee appointed by the British Medical Association, that in dogs with permanent biliary fistulæ, and living upon a fixed diet, that "spontaneous diarrhoea, dysentery, and purgation produced by blue pill, calomel, corrosive sublimate, and podophylline, always diminished the solid constituents of the bile, and, with one exception, the fluid portion of the bile also".

More recently, Röhrig performed experiments on the action of cholagogues in fasting curarised animals with temporary fistulæ, and found that large doses of croton-oil greatly increased the secretion of bile, and that a similar effect, though to a less extent, was produced by colocynth, jalap, aloes, rhubarb, senna, and sulphate of magnesia, the potency of these agents as hepatic stimulants being in the order mentioned. He found also that castor-oil had little effect, and that calomel, while it seldom recalled the biliary secretion, nevertheless somewhat augmented it when it was taking place slowly.

In Dr. Rutherford's and M. Vignal's experiments a modification of Röhrig's method was adopted. Dogs which had fasted for eighteen hours were curarised, and artificial respiration maintained. A cannula was tied in the common bile-duct; the cystic duct was clamped. The bile flowed from the cannula into a finely graduated cubic centimeter measure, and the quantity secreted was recorded every fifteen minutes. It was shown that this method of continuous observation yielded results far more reliable and instructive than that adopted by Röhrig.

Two experiments on the secretion of bile in dogs that had fasted for eighteen hours, and which received nothing more than the doses of curara used in all the experiments for the purpose of keeping the animals at rest, showed that the biliary secretion was not affected by the doses of curara given; that the biliary secretion, on the whole, somewhat diminishes in the course of an experiment lasting from six to eight hours, but that the chemical composition of the bile remains almost exactly the same.

The curara was always injected into a vein; the various substances hereafter mentioned were injected directly into the duodenum; for this purpose, the wound in the abdominal wall was opened, and the substances injected through the wall of the viscus.

Three experiments with croton-oil showed that, although it produced violent irritation in the alimentary mucous membrane in all cases, it increased the biliary secretion in only one instance. A high place is, therefore, not assigned to this substance as a stimulant of the liver.

Six experiments with podophylline proved that this substance greatly increases biliary secretion. A definite statement regarding the composition of the bile before and after podophylline will be given in the report.

Röhrig's statement that aloes deserve a high place as a hepatic stimulant was confirmed by three experiments, in which the extract of Socrotine aloes was employed. The analysis of the bile (not hitherto given), however, showed that after aloes the bile is more watery; nevertheless, the velocity of secretion is so much increased, that it certainly causes the liver to excrete more biliary matter.

Three experiments with rhubarb proved that it is a far more important hepatic stimulant than Röhrig has stated it to be. Doses of rhubarb were given nine times in the course of the experiments, and they never failed to excite the liver within half an hour after they were given. Analysis of the bile before and after rhubarb in all the three experiments proved the remarkable fact that, notwithstanding the greatly increased velocity of secretion after rhubarb, the bile-solids secreted by the hepatic cells are not diminished. The rhubarb apparently calls forth an increased secretion of normal bile.

Three experiments with senna proved that its power as a cholagogue is far below that of rhubarb. The bile is rendered more watery.

Four experiments with the aqueous extract of colchicum proved that it is a very decided cholagogue. The bile was rendered more watery, but the increase in the velocity of secretion was such that the amount of biliary matter excreted by the liver was certainly increased.

Two experiments with the solid extract of taraxacum proved it to be a cholagogue, though not a powerful one.

Two experiments with scammony proved that it has a slight cholagogue action.

Of four experiments with calomel, the secretion of bile was slightly increased in one, but there was nothing but diminution of the secretion in the other three. Purgative action was produced in all. The bile was rendered more watery.

Two experiments with gamboge gave no evidence that this substance is a cholagogue.

One experiment with castor-oil confirmed Röhrig's statement that this substance has scarcely any cholagogue power.

Two experiments with dilute alcohol injected into the stomach showed that, after the alcohol was given, the secretion of bile slightly diminished.

In the report, a full account will be given of the *post mortem* examination of the state of the alimentary canal (hitherto entirely omitted in such experiments), so that the effect upon the biliary secretion and that upon the intestinal mucous membrane can be compared.

It was shown that the increased biliary flow from podophylline, rhubarb, etc., in these experiments could not be ascribed to reflex contraction of the gall-bladder; for this had been previously well nigh emptied by digital compression, and the cystic duct had been clamped; nor could it be ascribed to reflex spasm of the larger bile-ducts, for the exaggeration of the biliary flow was far too great and far too prolonged to be explained in this way. Reasons were adduced for regarding it as probable that the agents are absorbed, and act on the liver directly. It was not professed, however, that their *mode of action* was definitely settled, the experiments having had for their primary object a determination of the facts of the case.

The opinion was expressed that powerful purgative action tends to diminish the biliary secretion.

The diminished secretion of bile in *non-fasting* animals after podophylline, observed in the experiments of Dr. Bennett's Committee, probably resulted from a diminished absorption of food from the alimentary canal, in consequence of the purgative action.

When a hepatic and intestinal stimulant, such as podophylline, is administered to an animal that is not fasting, it is probable that (1) the liver is excited to secrete more bile; (2) the absorption of bile and food from the small intestine is diminished on account of the purgative effect.

In conclusion, it was pointed out that this research proposed to be simply a contribution to comparative physiological pharmacology; and that it was left to the clinical investigator to compare these results with those observed in human pathological conditions.

AN ADDRESS

DELIVERED AT THE OPENING OF

THE SECTION OF MEDICINE,

At the Annual Meeting of the British Medical Association,
in Edinburgh, August 1875.

By W. T. GAIRDNER, M.D., F.R.C.P.Ed.,

Professor of Medicine in the University of Glasgow; Vice-President of the Section.

IN taking the chair of the Section of Medicine, I have, in the first place, to submit to the Section the letter I have received from Dr. Quain, expressing his extreme regret at being unable to be present at this meeting, owing to unavoidable circumstances which have led to his devolving his duties on the Vice-Presidents. (After reading the letter, Dr. Gairdner continued.) You will not, under these circumstances, expect me to give you a very elaborate or systematic address, the more so as, in occupying the same position at Leeds some years ago, I to some extent anticipated what I might have said on the present occasion. I will rather ask you to receive a few thoughts that have occurred to me almost on the way to this meeting, and in listening to the admirable address which we have just heard from Dr. Begbie; I refer especially to that part of the address in which he spoke of Cullen. It is impossible for anyone, being in Edinburgh, and inheriting the traditions of the Edinburgh Medical School, not to associate Cullen with the *genius loci*, of which, indeed, he was eminently a representative man. The fame of Cullen was, moreover, world-wide, and extended beyond medical circles; he was a distinct and noteworthy personage in that literary and scientific society which included Adam Smith, David Hume, Robertson the historian, and Black the distinguished chemist. Cullen had, by reason of his accomplishments, something in common with all of these and all the other eminent men of that time, and was highly esteemed by them all. In point of medical experience, he was at the very head of his profession; a most distinguished clinical teacher, constantly associated with his students in the observation of hospital cases, and, besides this, engaged in private consultations which brought him into extensive correspondence (as his manuscripts in the College of Physicians show) with all parts of the country. Now, one of the most remarkable facts in the history of the epoch of Cullen is one which was not alluded to by Dr. Begbie, but which, nevertheless, might be made available in discussing, or in reflecting over, the signs of progress in the medical art to which he alluded. About a hundred years ago, when Cullen was at the height of his fame, the Chair of the Royal Medical Society was filled by a man who had been the pupil, the friend, almost the dependent, of Cullen, and who then and afterwards became his most bitter antagonist. He was not a man of large personal experience in the art of medicine; his learning and accomplishments were of a very limited kind; his private life and character were the reverse of laudable; his personal and social standing was in every point so much inferior to that of Cullen as to admit of no comparison. Yet this man, first in the Royal Medical Society among the younger students, and afterwards by a literary and doctrinal influence which extended in a wave of controversy to the very extremities of European civilisation, became the recognised author of a system which all but subverted that of Cullen for a time in Edinburgh, and, up to the end of the century, occupied the minds of men in Italy and Germany to an extent that we can hardly even comprehend now-a-days from anything we know of the system itself. I will almost take it upon me to affirm that there are not two men in this room—if, indeed, there are so many in the whole British Medical