AN ADDRESS

ON

SOME RELATIONS OF PRACTICE TO
THEORY IN MEDICINE.*Delivered to the Fellows of the Medical Society of London on the opening of the One Hundred and Fifth Session.*By GEORGE BUCHANAN, M.D., F.R.C.P.,
President of the Society.

GENTLEMEN,—We meet to-day at the commencement of the one hundred and fifth session of this Society. During our century and more of life, we have seen one after another body established in London, with the object of promoting by conjoint study the advance of our science and practice. But it has remained the speciality of our body that, with younger societies growing and flourishing round us, giving themselves to one and another branch of medicine, and concerning themselves with one or another method, we have consistently included in the sphere of our work the whole round of medical science, medical art, and medical method. We have been eminently the Medical Society of London, and with us there has been natural place alike for medicine, surgery, and obstetrics; alike for physiology, pathology, and therapeutics; alike for preventive and curative, for public and private medicine. It is our boast that we are, before all things, a practical Society; and I venture to say that the reason of our being so is to be found in the universality of our work, in the mutual assistance that inductive and deductive methods afford to each other, and in the light that one department of medical knowledge is for ever shedding upon other departments. And, what is notably the speciality of our Society, we gather our observations and experiences from the widest area of practice, from the operating theatre and *post mortem* room, from hospital wards and out-patients, and also very remarkably from the vast stores of general medical practice. In this last respect we are, I believe, unrivalled; and it may be allowed that I should advert to-night, as a matter of primary interest to our Society, to the way in which the science of medicine is concerned with the daily doings of medical practice, and point out how the practitioner repays the obligations under which he is to the special cultivators of physiology and pathology.

I take it that the truest progress in medical science is to be made by those who are on the watch for empirical laws; who call to their aid methods of experiment and differential observation to confirm those laws; and who then seek to explain deductively the results at which they have arrived. The persons who labour at the generalisation and interpretation of phenomena are not always the same as those who have the function of establishing from experience the actual existence of law; and so far as the two classes of workers have been severed, a tendency has arisen to attach the notion of science too exclusively to the former. In truth, however, the empiricist, in the high sense of the term, the physician or surgeon, who in his daily work arranges and methodises his observations, is doing much more than exercise his art; for he is advancing, by a way itself pre-eminently scientific, to the discovery of ever-new truths in medical science. The facts, and their obedience to law, have to be learnt before the interpreter appears; and we may be sure that the high qualities of mind that are wanted in him whom I am calling the empiricist, will by no one be more keenly recognised than by him who has to follow with processes of explanation.

Let me illustrate my point of the indebtedness of the so-called science to the so-called art of medicine by the familiar truths about vaccination. Here is an example of the highest pathology, proceeding from absolutely empirical data. It is noticed that persons who milk cows with vesicles on their udders escape small-pox more than their neighbours. The facts are put together in such numbers and in such a way, and are so verified by experimental inoculations, as to demonstrate an empirical law of the utmost moment to the human race. At every step of the induction, rigorously scientific methods were applied, and a great scientific truth was arrived at. Here, medical art was in effect identical with medical science; there was no question here of what people call "practical deduction from scientific premisses". But now,

when this truth has been established, and not before, the pathologist appears on the scene, and asks why these things are so. He works at the question, and finds that human small-pox, transplanted into the cow and thence back to the human subject, has lost its intensity and its power of propagating itself by contagion, while retaining the power of protecting the human from further small-pox. And now it has become the turn of the pathologist to help the practising physician. The physician says to him: "Tell me what you infer from these facts as to the properties of infective material. The knowledge will certainly be useful to me for my own purposes." "What have you to say about contagiousness, as distinguished from inoculability?" "Find out for me, I pray, whether the employment of some similar transplantation and retransplantation may not yield a safeguard of a like nature against my abominable enemy scarlatina." At this moment, the pathologist is at work on the first of these questions, and will, we may hope, concern himself with the examination of the others; with what results on practical medicine, time only can show. But that which set him to work was the observation and induction, in the highest degree scientific, of a country doctor, whose business led him to attend to the ailments of milk-maids. When I point to his portrait among those of the founders of this Society, do I not give the strongest incentive to scientific use by all of us of the everyday materials that lie to our hands, and am I not right in declaring that there can be no thought of dissociating the art from the science, the science from the art, of medicine?

If I wished for an illustration of to-day, where practical medicine was, on a question of pathology, distinctly ahead of the mere pathological anatomist, and was helping to throw light even on physiological processes; of practical medicine in effect being "scientifically" in the front; I would take the instance of chest-tapping in pleuritic effusion. I see how this operation was originally resorted to in a mere hope of prolonging existence; as a last resource, when all other means had failed, for getting rid of water that was killing a patient. I note how, in the next place, some adventurous surgeons tried the effect of earlier tapping; and how there have now come to be grounds for assurance that, the sooner tapping is performed on a pleural cavity full of fluid, the better is the chance of recovery to the compressed lung. To the result, thus far, the pathological anatomist has contributed nothing; rather his fears have tended to delay the operation, and to hinder the practitioner in his use of the experimental inductive method. Not until the advantage of early tapping is established, has the question arisen: "What are the anatomical conditions impeding the rehabilitation of the lung after removal of the compressing fluid?" The problem thus presented, along with experience of the operation, to the anatomist, has led to more minute examination of the lung anatomy in pleuritic effusions, with the result of showing good grounds for belief that the diseased processes have influenced the pulmonary apparatus, no less in its circulatory than in its air-holding textures. Observation at Victoria Park has shown the pulmonary artery and its branches to be liable to plugging, in such a way as to prevent their resuming their function of blood-convection after the compressing fluid has been removed; and has led to wholly new inferences as to the methods by which permanent incapacity of the lung may be brought about. Here, it will be observed, it was the practitioner who directed the anatomist; and it was an induction, from bedside experience, that led to a complete observation of the facts of morbid anatomy, and to a clearer apprehension of pathological sequences.

The interpretations furnished to the empirical physician by the scientific reasoning to which he subjects his observations are, of course, in their turn fruitful of instruction; and ever new applications of his experience, when so examined, are suggested to him for his practical purposes. See how bark and quinine were first of all used, on non-medical observation, in the general treatment of intermittent fevers, simply as matter of cure; how, in the next place, quinine came to be recognised as preventing ague, if it were given in large doses immediately before the occurrence of an attack; and how a demand then arose out of this knowledge for a better understanding of the physical circumstances of the circulation in connection with the observed operation of the drug. It was in answer to this demand that new examination was made of the anatomy of ague, with the result of finding that, whereas the cold stage was characterised by a general contraction of capillaries, quinine had the antagonistic power of controlling this tendency to contraction. The physician here has led the physiologist to some new views about the capillary circulation. But he had also himself learned a lesson that he goes on to apply deductively thus: "I will see", he says (W. H. Power), "whether there are not other diseases that may come of a like contraction in some part of the capillary system, and I will observe whether in them some good may be done with quinine. Particularly, there is nervous asthma, as people call it; about which I have been re-

flecting that due supply of venous blood to a lung would seem to be quite as essential a condition for the physiological action of a lung as due supply of air can be, and that the dyspnoea which people generally connect with insufficient air-supply may equally come from interference with blood-supply. Your contraction of capillaries, if of a spasmodic kind affecting the lung-capillaries essentially, would I think very much account for such phenomena as I observe in this form of asthma. I shall certainly use quinine in the next case of spasmodic asthma that comes under my notice." So he goes, taking his newly acquired insight to the bedside, and finds that with quinine he can greatly control and often can indefinitely postpone the attacks of disease that have before seemed intractable. He has acquired, in his turn, a starting-point for new observations that may be expected to lead to the establishment of a fresh empirical law.

Curiously enough, it has been when these problems of therapeutics have been thought of in the light of physiology and pathology, that the notion of practical medicine being something of a different order from physiological and pathological work has arisen; it is then that the word empirical has been used with a scarcely concealed sneer; and it is then that we have heard most about the art, as if it were something estranged from the science of medicine. Happily, the time has now arrived when all this antique folly may be given up. The action of remedies in diseased states is recognised as among the most potent methods of discovery as to the nature of a diseased state, and even as to the conditions of a healthy state; and that this recognition should have been delayed, may more justly be regarded as a reflection on the pathologist and physiologist than on the therapist. Thus, from early times the physician has observed the action of digitalis as a diuretic, of special benefit, in certain cases of dropsy of cardiac origin. Only recently it has occurred to anyone to inquire how this came to be so, and to discover that the drug produced a distinct action on the muscular tissues of the heart (and probably on that of the great vessels also), regulating and increasing the force of the cardiac contractions. Now, the physiologist observer comes to the physician with an explanation: "You have here an agent that, by increasing the blood-pressure in the vessels, assists the secretion of urine by the kidneys, at the same time that it renders the circulation capable of taking up the effused dropsical fluid. It is thus in effect what you call a tonic to the heart, and this is the way in which it operates usefully in the cases you have observed;" an excellent explanation, particularly as our physiological friend has already perceived it to involve some imperfectly understood points within his own particular branch of study; a scientific explanation, too; but not more scientific than the processes of observation and induction by which the virtues of foxglove were apprehended, and the circumstances of its usefulness ascertained many and many a year before.

Now that the truth has been accepted, that the domain of therapeutics is not distinct from the province of physiology and pathology, but that the latter sciences may learn from therapeutics as much as therapeutics may learn from them, it is to be hoped that the empirical facts of therapeutics will be more efficiently taken in hand. What could be more interesting than to receive answers to questions such as these, that are put loudly enough by the practitioner? How do counter-irritants act in the relief of pain? How do ice-bags applied to the chest operate in checking lung-hæmorrhage? How are we to look at pyrexia when we see the marvellous influence that cold bath treatment has on it? These are specimens of scores of questions that the empirist, scientific in the best sense, puts before his fellow-workers. And in the interpretation of these and like facts of experience lies the most immediate service that physiologists and pathologists can render to practical medicine.

"In such cases" (I quote Sir John Herschell) "the inductive and deductive methods of inquiry may be said to go hand in hand, the one verifying the conclusions deduced by the other; and the combination of experiment and theory which may thus be brought to bear in such cases forms an engine of discovery infinitely more powerful than either taken separately. This state of any department of science is perhaps of all others the most interesting, and that which promises the most to research."

Gentlemen, it is in this state that our science of medicine exists at the present day. Let us look, then, on our body constituted as I have shown it to be, as being charged in a special way with the advancement of medical science, for the very reason that it is, among all other societies, the most comprehensive in its range of work.

THE W. and S. Tuke Prize (one hundred guineas) has been awarded by a Committee of the Medico-Psychological Association of Great Britain to Dr. E. C. Spitzka of New York, for the best essay on the Somatic Etiology of Insanity.

REMARKS ON THE USE OF CARBOLISED CATGUT IN THE LIGATURE OF ARTERIES IN THEIR CONTINUITY.

BY JAMES R. LANE, F.R.C.S.,
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THE merits of the carbolised catgut ligature, when applied to arteries in their continuity, has again been brought under the notice of the profession by Mr. Bryant, in a paper read at the Clinical Society on October 12th, when, from the report of the discussion which followed, it appears that there is still considerable difference of opinion among surgeons as to its efficiency and safety.

Mr. Bryant related his experience of fourteen cases, and, notwithstanding the occurrence of slight secondary hæmorrhage in two of them, concluded by expressing his opinion that "we have in carbolised catgut the best ligature at our disposal." In the discussion, Mr. Barwell mentioned that he had tied five cases with catgut, all of which were successful. Mr. Maunder, on the other hand, although quite satisfied in five cases in his own practice, stated that the records of "serious consequences, such as solution, slipping of the knot, division of the coats of the artery in one instance, immunity from this in another, while death had followed from early hæmorrhage and embolism, had decided him, enthusiast in its favour as he had once been, never to use it again. It was obvious," he added, "that no such accidents attended the use of silk." Mr. Callender said that "in one case the catgut had given way and the pulse had returned in the sac of the aneurism. In three other cases, pulsation had returned in the vessel within a few days after the operation with catgut".

The question is an important one, and it is evident that a further accumulation of facts is necessary for its solution. I venture, therefore, to offer the results of my own experience, amounting to fifteen operations, comprising seven of ligature of the femoral artery, two of the external iliac, three of the carotid, one of the brachial, one of the radial, and one of the ulnar. The three latter occurred in the same patient; one of the femoral and one of the external iliac were also in one subject. The result in all of them, so far as it depended upon the ligature, has been eminently satisfactory.

The following case is, I think, worthy of a somewhat detailed account, as it serves to illustrate several of the points at issue. It was one of popliteal aneurism, in which I had occasion to tie the femoral artery, and, twenty-five days afterwards, the external iliac; the death of the patient subsequently affording the opportunity of examining the vessel at both the points ligatured.

The patient, aged 45, a strong and healthy man in outward appearance, and of very active habits, was admitted into St. Mary's Hospital on December 14th, 1876, with a tumour of the size of a large hen's egg, evidently aneurismal, in the left popliteal space. He had first noticed a small throbbing swelling a month previously in this situation, and it had gradually increased to its present size. His pulse indicated increased arterial tension; but there was no other evidence of cardiac disease. His urine was albuminous, and contained casts. I intended, after he had rested for a day or two, to employ compression with the Esmarch bandage; but, on the morning of December 16th, when getting out of bed, he felt something give way in the tumour, which began to increase in size. In the evening, I was sent for to see him, by which time the popliteal space had become greatly distended; the swelling extending downwards under the gastrocnemius, and outwards under the biceps tendon. There was a faint pulsation in the tumour, and the pulse was just distinguishable in the anterior and posterior tibial vessels. It being evident that the aneurism had burst, it was obviously necessary either to tie the femoral artery or to amputate the limb. I decided on the former course, and ligatured the vessel at the apex of Scarpa's triangle with carbolised catgut. The ligature was drawn tight, so as to divide the internal and middle coats, and its ends were cut short. The wound was brought together with silver wire sutures and dressed with lint dipped in carbolised oil. The limb was wrapped in cotton-wool.

On the next day, he appeared to be doing well; but, on the day after, the 18th, there were such evident signs of gangrene in the foot and leg that it was determined, after consultation, to remove the limb. I accordingly amputated at the junction of the middle with the lower third of the thigh, making anterior and posterior flaps. Four vessels were tied with catgut, and, as there was slight oozing from the main artery, notwithstanding its ligature higher up, this was secured in the same way. Examination of the limb after removal showed a rent an