An Address ON THE RELATION OF PHYSIOLOGY TO MEDICINE.*

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THE relations between the science of physiology, which studies the normal functions of the animal body, and medicine, which studies and treats the human body in disease, are so intimate and manifold that the subject before us may be approached from many different points of view. When you, Mr. President, asked me if I would open this discussion, I felt some hesitation in accepting, knowing that I should speak from the standpoint of the physician. Although I have from time to time strayed into the proper realm of physiology, nevertheless I would ask you to remember while I address you that I am primarily a physician. It is for that reason that this paper emphasizes, not the standpoint of physiology, but the standpoint of medicine, for it is the welfare of medicine which I have firstly at heart.

Because the dependence, now, in the past, and in the future, of medicine upon physiology is not dwelt upon; because I do not speak, as I could speak, of the great and lasting benefits which physiology has conferred, and continues to confer, upon medicine as a science, and upon medicine as a healing art, it is to be hoped you will not think on this account that I am insufficiently alive to this relation of the two subjects. It could serve no useful purpose to come to physiologists and tell you what you all know, what is to every thinking man apparent—namely, that any knowledge which has reference to the normal functions of the animal body, however remote from practice that knowledge may seem at the time of its discovery, is sooner or later reflected or incorporated in medical thought and medical practice. Physiology, originally the offspring of medicine, has become to medicine in its old age a chief pillar of support. But, while this filial and noble aim of physiology has fulfilled, and continues to fulfil, its great purpose in nourishing and supporting its sire, it is fully recognized that physiology has other and equally noble aspirations. It seeks knowledge for its own sake, for the enlightenment of thought in other spheres. To take the view that physiology exists as the servant of medicine were as narrow as to countenance and encourage a complete servitude of human offspring to their parents. Each of these offspring may rightly claim a large measure of liberty. But as my own life interest is in medicine, so I may be forgiven if I rank myself with those who place the filial function first; I may be forgiven if, in recognizing that the two sciences, physiology and medicine, have mutual obligations to each other, I place, by reason of the same prejudice, the obligations of physiology well to the fore.

The obligations of physiology to medicine are twofold: to teach the student bent on the practice of medicine the normal functions of the human body, and in doing so to train him as an accurate observer and thinker; and to lay the foundation upon which the house of medical science shall erect itself. If you will, you may weld the two together and say that in its relation to medicine the function of physiology is to gather, classify, and spread knowledge and methods of studying the normal functions of the animal body. The first statement suits my purpose best, for it allows me to divide my thoughts into two chief channels, the teaching of medical students and the direction of physiological research—two channels, however, which should not diverge too far from one another. From both points of view the very closest association of physiology and medicine is necessary. The underlying thought has been to try and find means by which these sciences might be brought to closer and closer union.

Let us come at once to the pith of what I wish to say in regard to the teaching of physiology to medical students. In many respects it is essentially a controversial subject,

and all the more suited therefore for discussion. The views expressed are personal views. It is my personal conviction that the pivot around which physiological teaching should play is human physiology; that human as opposed to animal physiology should form the central point of physiological teaching to medical men. That the physiological lessons are to confine themselves to the human body is not by any means what is advocated; such a course is clearly impossible, while by far the largest part of our knowledge of the functions of the human body is still derived analogically from observations upon the lower animals. The standpoint is that knowledge which has been obtained or is now obtainable by observation upon man himself should be constantly stressed, and that animal physiology should be taught where it supplements knowledge which is so obtained. It is of considerably greater consequence to the student of medicine that he should witness a given and normal phenomenon, or gather a given principle, by observation upon the human rather than upon the animal body; when, therefore, a phenomenon can be displayed to him both in the man and in the beast with equal accuracy, it should be displayed to him as it occurs in the former. When it cannot be displayed in man, or when it cannot be displayed with the same fine degree of certainty, then animal physiology should be called in. I would go further, and say that when a phenomenon can be displayed in man, even though it can be displayed less accurately than it can on the animal body, it should be demonstrated and stressed, not to the exclusion of the more precise demonstration on the lower vertebrate, but alongside of it, so that the two demonstrations may be evaluated and the two compared. There are several vital reasons why this should be the trend of modern physiological teaching to medical students. First and foremost is this reason: *that students in their most impressionable period would thus learn how to investigate the human body.* Not infrequently it is discovered in talking with physiologists that they but dimly realize the difficulties of this task, fail fully to appreciate how enormous is that barrier between physicians and the working organs—namely, the unbroken skin. Yet that barrier must and can be passed; the passage is vital to the present and future success of medical science and practice. The problem of what is happening in the human body is infinitely harder to solve than that which asks what is happening in the animal body, for in the former case our methods of examining and of observing are usually far more indirect. The student is taught to stimulate the vagus nerve in the frog and to note the effects; at the same time he should be taught to press upon the vagus in the carotid sheath of his fellow student, and to observe the consequent slowing of the heart beat. Physiologists have to face the fact that the large majority of the students who pass through their hands will encounter in their daily work problems far harder to solve than those with which they themselves deal in their laboratories. I speak from experience in saying that medical students, when they come to us in the teaching hospitals from the physiological laboratories, come to us insufficiently equipped in this respect. They have not yet learnt to handle a man; their senses of hearing, and especially those of touch, are not sufficiently developed; yet they are to handle men for the rest of their days; and my plea is that, with very few exceptions, they never will learn efficiently if they do not begin to learn while they work under your direction. They come to us in the hospitals after practising for two or more years relatively direct and conclusive methods; they come, many of them, with a rich knowledge of the functions of the body, with clear ideas of how problems of the animal body are approached and solved; they soon find by experience—often bitter experience—that most of these relatively exact methods which you employ are inapplicable to the study of living men; that many which are applicable are by their specialization soon beyond their reach; that far less direct, far cruder tests are of necessity used; that inferences are based upon premisses of a totally different quality. At the same time they are brought face to face with a mass of detailed knowledge of a new kind, they are asked to accept a new series of conclusions; upon many of these conclusions, it is true, their past teaching will bear; with many more their physiological teaching will not directly connect. The arduous work of the medical student in his final years is known to you; no one can pass through this last course, sifting and weighing a fraction

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of what is taught him. The great bulk of what he hears in his hospital years must be taken on trust. It is by the force of these circumstances that the medical student invariably becomes empirical; and the measure of his empiricism is the measure in which he forgets his physiological teaching; this teaching becomes buried more or less in a mass of new experiences and methods. My contention is that his physiological training would not be lost to him in the large measure in which it indubitably is lost were the shock of change less, were his old experiences more in harmony with the new. It has been an intense interest of my own to watch for not a few years this stage of the medical student's career: it is his most critical time. I have closely watched a goodly number of men unusually well trained in physiology—I refer to men who have spent more time in physiology than the curriculum dictates—and have yet to see an example in which the deadening influence of this transition has not been conspicuously displayed upon the faculties of criticism and inference. It is an influence that I am conscious, from personal experience, that I have not escaped; it takes years to outlive it. You train a man to weigh with a nicely adjusted chemical balance; when you start him on his life's work a crude scale is alone available to him, and from the moment it comes into his hands he is set to weigh hard. He weighs and he forgets the old balance; in general he never fully appreciates the defects and limitations of the new; in time he acquires a mistaken confidence. These limitations, these defects, he should surely know beforehand; familiarity with those tools which are to be his as a workman, exact knowledge of their capacity to help him or to lead him astray, should be his from the very start. To train a man fully in the use of delicate instruments, to bring him then to his first real workshop and to set him to his bench of coarse and unknown tools, is to inflict upon him a thing which to many brings apathy, to some revulsion. Let him see and use his delicate instruments by all means, but let him clearly understand that these are in his hands, not only for the knowledge they bring, but also for comparison with those more simple devices which are also there for him to see and use, and which are to be eventually the weapons with which he will fight disease. In that way, and in that way only, so it seems to me, will he lastingly appreciate the limitations which circumstance and method impose upon him, and in that way will he assess the true values of his method. Such is, largely in metaphorical language, what I have in mind in pleading for human physiology. It will be said, perhaps, that the fault lies in medical teaching; in great part that is unquestionably true, but it is not to be forgotten that medical science labours before unusual obstacles; neither is it to be forgotten that teachers of medicine have themselves all passed through this same ordeal—an ordeal which leaves none unscathed; medical practice, driven by necessity, seizes those weapons which are alone at hand; the old trapper has a too easy-going confidence in the gun which has long been his only available companion. The confidence displayed by medical men in their methods is a chief fault in medical practice to-day; it is a personal opinion that that confidence can be modified, most materially and most quickly, in the physiological laboratory, by training students how to examine the human body and teaching them what physiological conclusions they may legitimately draw from such investigations; to show them side by side with such methods of investigation what can be done when, as with the lower animals, greater facilities are presented to them; to supplement conclusions drawn from experiment on other animals. An arrangement seems desirable whereby clinical material, illustrating clean experiments of disease which throw light on normal functions, should be brought into the hands of physiologists and their students. Such co-operation would form a valuable connecting link between physiology and clinical medicine, stimulating research and urging teaching in the direction which it should take—physiological teaching becoming more human, and medical teaching more in consonance with physiological thought. Such cases would illustrate instances of simple lesions of the nervous or cardio-vascular system or special senses. I have in mind clean transections or ablations and their effects which, when shown side by side with corresponding evidences from animal physiology, would form most impressive demonstrations. Demonstrations upon

the human subjects are always impressive. Few physiological students who have heard the story, forget Alexis St. Martin. None would forget if they saw him in life. Salivary, gastric, or biliary fistulae are not so uncommon that they might not from time to time be shown to physiological students. Many of the patients who visit our hospitals because they present anomalies of the ductless glands might also enter your laboratories. I look forward to a time when all physiological laboratories which train medical students may be equipped with these instruments of examination which are to be found in teaching hospitals, for there are very few of these instruments which will not display in some measure the normal functions of organs of the human body. As an example I may cite the movements of the alimentary canal as these are shown by the x-ray tube.

But in regard to such instruments and devices I have some particular remarks to make.

There is, if I mistake not, a prevalent opinion that the medical man of the future is to be equipped as a physicist, as a chemist; that he is to have behind him an array of delicate apparatus and reagents with which exact measurement is to be made possible to him; and that in these directions the salvation of medicine is to be sought. If I am correct in my belief that such is a current view, then I would state emphatically that I believe it to be a profoundly erroneous view. It confuses two issues—the daily routine of the practitioner and the work of the special investigator. These stand apart: they always will stand apart. You cannot deal with masses of patients by using refined methods; repeatedly it has been tried; it always breaks down. The chief weapons of the general practitioner are to-day, and will remain, his own unaided senses; these are supplemented by a few simple devices. The healthiest trend of modern medicine—and let it be said at once that it is a trend which is anything but universal—is to dispense as far as possible with special methods of investigation in dealing with the everyday patient. The special method is for the investigator into pathological processes; it is not for the pure clinician. The use of special devices, of special tests, almost always leads one way: the devices and tests are employed by men who cannot have the training, cannot afford the time fully to understand, and a method imperfectly understood is, generally speaking, worse than useless. Take the method of electrocardiography; you will agree with me that it has added a very great deal to our knowledge of both the normal and perverted functions of the heart during recent years. I take this example because it is one with which no one in this country is more familiar than myself; and I say without hesitation in regard to it that so far as its employment as a direct aid to diagnosis, prognosis and treatment in individual patients is concerned, it has done far more harm than good. That is so because many of those who employ this instrument in clinical routine rely upon it to an extent far above its merits, and because it uses up time which would be spent more profitably in other ways: it fails especially because it attracts attention from other and far more important issues. The greater the number of such devices employed, the more the outstanding question recedes into the background. Is this man, as a whole, healthy or unhealthy? Be careful not to misinterpret me; I do not say that electrocardiography as a branch of medical science has harmed medicine more than it has served it; the reverse is decidedly true; my meaning is that in its application to the individual its effect is in general harmful, and will remain so. I have taken the galvanometer as my example; I have done so because it frees me from any possible suspicion of prejudice. The polygraph might have been used as an equally apposite illustration, and the same statements might truthfully have been made in regard to it. In my own clinical routine I gave up the use of both these instruments when, by their use, sufficient experience had been accumulated to identify the disorders of the heart which they display, by means of sight and touch. For that we ought constantly to strive. Let the knowledge of disease be built up by means of such instruments; let us sift from this knowledge what is of immediate and unquestionable service to the practice of medicine; let us then find how the special test may be supplanted, by methods perhaps not quite so accurate, but methods which can be at once and universally employed. Believe me, you will never see the day when the medical

practitioner will rightly interpret delicate records of the heart beat; but you will see the day when he will know the chief things which those records have taught us, and how he may utilize that knowledge in his daily work. A medical practitioner cannot hold a working knowledge of these refined methods; each has its growing complexity, each formulates innumerable rules and reservations; each is to him an abstruse method, applicable to but a small fraction of his patients. The plea for him is simplicity, not bewildering complexity. Treat polygraphy, treat electrocardiography as sources of knowledge and of generalization, and you may say that these methods have potentially fulfilled their great purpose; they have greatly simplified the diagnosis and treatment of heart disease; but treat them as parts of the medical man's equipment and you will load further what is already overloaded. It is eight years since, in the preface to my small book on *Clinical Disorders of the Heart*—a book describing how varieties of disordered heart action may be recognized by simple tests—I first expressed these ideas; they have since grown towards convictions, and, as you are aware, have found a powerful advocate in Sir James Mackenzie. Medical practice will grow more scientific as it becomes more and more simple—as the practitioner grasps great and fundamental truths in their relation to common diseases; it will not grow scientific in proportion to the mass of detail—impressive enough, maybe—which he is able to pack into the compass of his brains. The place of these refined methods is the laboratory and the teaching hospital. In the teaching hospital they are there so that the student may learn how knowledge is acquired. I have never, as a teacher of medicine, taught these methods to undergraduate students; neither will I do so. It is for them to use their fingers and their sight, but the instruments are there to check such observations, to show what can, and equally what cannot be done; they are there so that the student may see how the knowledge which he is to use is won. We have to teach our students not only how to handle disease, but also upon what the knowledge we impart is founded.

The second reason which is urged for a greater concentration upon human physiology in teaching is of a different, though hardly less important, character. Medical men are insufficiently conversant with what constitutes normality, or the range of normality, in the human being. They regard as pathological many phenomena which are in reality not only compatible with health, but are actually physiological. It is of much consequence that this argument should be brought home, and it may be done by means of a few notable examples. I take these from that sphere with which I am most fully acquainted by personal experience—the sphere which studies the circulation. They could be multiplied, and many others of the same kind could be cited from other branches of study, but these will suffice. Take a very simple case: Stroke the back of a normal healthy young subject and watch the vascular reaction in the skin; the line pressed and the surrounding skin displays after a while a red line; maybe it soon becomes surrounded by an area of blanching, maybe a line of blanching alone appears. These reactions, perfectly healthy reactions as they are, have been used, are still used, and will be used for many years to come, as important signs of particular and serious disease. Take the effect of respiration upon the human pulse. If the pulse falls away in strength in inspiration—a *fortiori*, if it disappears—it is held to be a sign of serious mischief in the pericardium, or of some other grave disorder; actually both signs are compatible with—nay, are common in—perfect health; the normal waxing and waning of the pulse rate in natural breathing has been responsible, and is still responsible, for the enforced invalidism of hundreds of young children. Normal variations in the amplitude of heart sounds, the presence of those murmurs which almost always accompany an accelerated action of the healthy young man's heart, send hundreds to their beds or couches annually; the same normal phenomena kept very many hundreds of strong, able bodied men from their proper stations in the time of the great war. These and similar variations in normality are insufficiently taught in our physiological schools, where they can be most convincingly taught. Any departures from a single physiological standard come inevitably to be regarded as abnormal; busy medical men have no time to seek for and to study controls; for the most part they leave the physiological study of man when they leave the physiological

laboratory and lecture theatre. The result is that the knowledge of normality in man is often far more elementary than it has business to be.

Take the physiology of exercise, the measured amount of work which healthy men of different ages and of different training can do in a given time, or the reactions of pulse and respiration which a simple test exercise should call forth in his body. That simple physiological knowledge is vital to proper medical practice, yet I have no hesitation in saying that only a few medical men have this requisite knowledge to-day. That surely is a reflection on the method, if not of physiological then of our combined teaching. It is sad to think that medical men should fail to recognize a sick man, or should call a healthy man sick, because they lack full knowledge of healthy reactions; yet these are common daily blunders. When I look back over the work upon which I have been engaged in the last eighteen years since I left the physiological laboratory I discover that a large fraction of my time and energy has gone in discovering phenomena of health which hitherto I had learned to regard as manifestations of disease. To teach the principles of their science, to teach men to observe accurately and to think critically are wise aspirations of physiologists. A man may be given a scientific training in chemistry, but if practical chemistry is to occupy little or no part of his active life, much of the virtue of the training goes out of it. The argument—it is no new argument—is that physiology as it is now taught to medical students is not sufficiently correlated to medical practice: it is not human enough. If, in illustrating a principle or in impressing accuracy of observation, there is a choice of subject—and selection has become imperative with the rapid expansion of your science—it is better to choose an illustration which may be followed by a corresponding demonstration upon man. Do medical men show unusual discrimination when they come to weigh the pros and cons of a new problem? Are their explanations weighed in the critical spirit which their physiological teachers have tried to instil into them? Are they unusually acute in assessing the value of evidence? I do not answer these questions negatively, but lack the assurance to answer them affirmatively. If they are to be answered negatively, does it not mean that there is some weakness in physiological teaching, for physiology is the chief philosophical subject which they study? And may not this defect, if it exists, in part at least result because they learn to philosophize upon questions which are soon forgotten in the daily routine of practice? It matters little to a practitioner whether Ludwig or Heidenhain was in the right; he does not base his treatment on one or other view. An account of many such controversies, and especially those which have led as yet to no final conclusion, might well be replaced by a historical account of those problems which are more vital to the daily needs of medicine, and these problems might equally illustrate processes of argument and thought. With purely physiological hypotheses, we, as pathological investigators, have little or no business. I use the term hypothesis in its true meaning—an assumption intended as a temporary explanation of observations, and put forward in the hope that with extended investigation such hypothesis may harden into conclusion. A published hypothesis is usually in essence a redoubt built by its conceiver; he and his party labour to strengthen it; but its very nature invites, and sooner or later provokes, an attack, and, as with redoubts, it is but rarely impregnable. The attack and counter-attack may be and often are prolific of discovery, whether the redoubt holds or falls. It is at once conceded that physiological hypothesis is essential to the physiologist; it has not the same value to medicine, it is often dangerous when introduced to medicine. The observations and conclusions of physiology are indispensable to us; but too often, from the inability to weigh evidence, hypothesis and conclusions are confused, and the former becomes the basis of pathological speculation. This is a widespread and regrettable habit amongst pathologists and clinicians. Who builds a house upon an exposed and threatened redoubt builds unwisely. The pathologist or clinician who founds his own assumptions on the assumptions of physiology, builds to see the complete collapse of his edifice. He who founds his assumptions on the observations and conclusions of physiology may still see his house totter, but the foundation will remain secure, and, happily, some parts of the superstructure may remain intact if solidly laid.

In turning more particularly to questions of original research, I again stress the need of more closely uniting physiological and pathological thought; in that way only will the student of medical science remain conversant with physiological conclusion, and be in a position to divide it sharply from hypothesis. While it would be good to see our anatomists in our *post-mortem* rooms, it would equally be good to see our physiologists more often in our wards. It is scarcely to be doubted that such visits would lead to very fruitful investigations, would correct misconceptions on both sides. That is but one way in which constant touch might be attained. In the "Physiological Society" we have possessed in the past a connecting link, though it be but an occasional link; to this society the leaders of pathological research, many leaders of clinical research, have belonged; in times past they have provoked some of your most interesting and fruitful discussions, stimulated or produced many important physiological researches. But as physiology becomes ever purer (more physical and more chemical) it becomes more and more difficult for men engaged in clinical investigation to feel at home at these meetings. It is no answer to say that medicine should follow the same lines, go back to the simple physical and chemical basis; it cannot do so at the same rate; medicine is inevitably preoccupied with end problems, and cannot escape from the expedients and opportunisms which its constant association with very sick people forces upon it. Here arises a tendency for the pure physiologist and purely clinical investigator to drift apart. They must be kept together, not only for the sake of medicine, but also for that of physiology. The breach has been partly closed by contact at the meetings of the admirable society to which I have referred; it has also been closed in part by a group of men who have devoted much of their energy to this task. I refer to those investigators who have devoted themselves to what is now often termed "experimental medicine." Taking for their field of study diseases of particular organs or systems of organs, they have sought to bring the physiological and pathological side of their subject into harmony. Horsley, Ferrier, Head, Garrod, Mackenzie, and many others have been recently or are still amongst this group of active workers. The group of men has been formed from those practising medicine or surgery, notably from the ranks of neurologists; students of applied physiology have been drawn too exclusively from the ranks of those who are primarily clinicians, and insufficiently, as I think, from the ranks of trained physiologists. A chief reason why this has been so in the past is that "experimental medicine," as it is often called, has offered no immediate chance of livelihood. My view is that the number of physiologists who by contact with clinical problems are inspired to work upon problems of applied physiology is still insufficient; the war period, during which many physiologists were so engaged, showed the fruitfulness of such contact. Medical research is now nourished on a new basis in this country, and it looks as if the link, "experimental medicine," may, when it is fully forged under the Medical Research Council, go far to fill what is still an obvious want—namely, the closer co-operation of all research work in the clinical and pathological fields with contemporary physiological thought.

In the discussion that followed, Professor H. E. ROAF thought some alteration in the teaching of physiology was needed. He was impressed by the plea for simplification, so as to give the student less detail and more fundamental principle. While agreeing that as much as possible should be illustrated on the human subject, he feared there was a danger in too much "clinical" physiology. The clinical applications were largely a subject for the clinical units and experimental medicine; the physiologist should inculcate a sound knowledge of principles rather than illustrate a multiplicity of technical instruments. Professor SWALE VINCENT found himself very generally in agreement with the views expressed. Dr. Lewis had pointed out certain defects in the current methods of teaching physiology to medical students. Most of the physiologists present could point to a hundred other defects; they were nearly all due to lack of funds. Extension of the teaching of human physiology by observations on the students themselves was the more desirable, and indeed necessary, since students had little opportunity for any other kind of mammalian work, their observations usually being confined to the frog.

EIGHTY-EIGHTH ANNUAL MEETING

OF THE

British Medical Association.*Held at Cambridge, June 30th to July 2nd.*

PROCEEDINGS OF SECTIONS.

SECTION OF SURGERY.

Sir GEORGE H. MAKINS, G.C.M.G., C.B., P.R.C.S.,
President.DISCUSSION ON
THE END-RESULTS OF INJURIES TO THE
PERIPHERAL NERVES TREATED BY
OPERATION.

OPENING PAPERS.

Sir WILLIAM THORBURN, K.B.E., C.B., C.M.G., F.R.C.S.,
Professor of Clinical Surgery, Manchester University.

I PROPOSE to place before you some general considerations, and to endeavour, as far as I can, to give you a summary of recent work upon the subject with which we are concerned. I do not intend to trouble you with personal experiences, first, because I do not regard this as the proper function of the introducer of such a discussion; and, secondly, because I have been abroad throughout the greater part of the war, and thus I have not had so large a number of personal operation cases as those who have been working in base hospitals in this country.

In civil life injuries to the peripheral nerves, although by no means rare, do not occur in sufficient numbers to allow of the collective investigation which they receive in war. Hence much of our earlier clinical knowledge was founded upon the work of Weir Mitchell during the American Civil War, and since that time research has been conducted mainly either on experimental or pathological lines or on the limited clinical opportunities of a few observers. The European war has again supplied us with a large amount of material, which has been thoroughly studied, and which has been treated under the most modern surgical conditions. Great numbers of these cases have been submitted to the operation of secondary suture, and not a few to that of grafting, so that we may hope that it will be possible ultimately to express almost in a statistical form the end-results which have thus been obtained. This day has, however, not yet arrived, and a long period of time and a careful series of observations by neurologists and anatomists will be required for full investigation. We are thus not yet able to give a perfect picture of end-results; all that can be done is to collate some of the more complete records and to formulate what may perhaps be regarded as an interim report.

Before doing so, however, I would like to refer briefly to a few preliminary considerations which are of importance in directing our treatment, and which indicate the type of end-result which we may legitimately hope to obtain. It may now be agreed that the regeneration of a peripheral nerve is in all cases accomplished by the downgrowth of axons from the proximal end into channels or sheaths awaiting them at the peripheral end. The proximal axon is connected with a sensory or motor neurone, while the peripheral sheath leads to or is connected with a terminal organ, either motor or sensory. It must therefore be recognized that, apart altogether from anatomical continuity, repair which may lead to a good functional result requires that a sufficient number of axons shall enter into a sufficient number of peripheral sheaths which are capable of leading them in the right direction. Thus, for example, motor axons entering channels connected with sensory terminals are probably lost for ever, and so with afferent axons which become connected with muscle endings. So far as I know, the union of a purely motor with a purely sensory nerve is not followed by any physiological connexion whatever.

It is clear, therefore, that motor axons must enter motor channels, and afferent axons sensory channels. Not only