AN ADDRESS ON THE PROGRESS OF KNOWLEDGE IN THE PHYSIOLOGY AND PATHOLOGY OF THE NERVOUS SYSTEM.

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MR. PRESIDENT AND FELLOWS OF THE ROYAL MEDICAL AND CHIRURGICAL SOCIETY,—I have, in the first place, to express to you my very grateful appreciation of the great honour you have done me in awarding me the Marshall Hall Prize this year, and in the further high privilege you have accorded me in asking me to address you on this occasion. To be the recipient of a prize associated with the name of Marshall Hall, and for work done in that department of medical science which he himself so adorned and enriched, is a distinction of which I am indeed proud. I know no name in the ranks of scientific medical inquiry, in this or any other country, which stands higher than that of Marshall Hall, and no work which has done more to advance physiology and pathology of the nervous system, and not this only, but medicine and surgery in general, than his investigations into the nature and conditions of reflex action. This he was undoubtedly the first to formulate and expound, and clear from the vagueness and confusion which prevailed before him. The importance of his researches can scarcely be overestimated. If it were possible to eliminate from modern medicine all that relates to reflex action, its bearings and applications, we should practically derationalise the better half of pathology and therapeutics. Few scientific inquirers have been more keen to perceive the practical bearing of their own researches than Marshall Hall himself; and most of the principles that guide us in the recognition and treatment of diseases and symptoms dependent on reflex irritation were first clearly laid down by him. Not long prior to the time when Marshall Hall was pursuing his investigations on the spinal cord and medulla oblongata, Flourens was engaged on his ever memorable researches on the physiology of the cerebral hemispheres; and Flourens, also, to whom he dedicated several of his memoirs, was almost the first to perceive and recognise the value of the work done by Marshall Hall, which many of his own countrymen endeavoured vainly to detract from and depreciate. If, at the distance of fifty odd years, we compare the relative stability of the work done by these great men, that of Marshall Hall on the spinal cord, and that of Flourens on the cerebral hemispheres, we find that the doctrines enunciated by Marshall Hall, modified, perhaps, as to detail, and further extended by the numerous researches of recent years, are in all essential points those which still prevail, and show no signs of failing; while those of Flourens, which have also exercised an enormous influence on clinical medicine and pathology—what shall we say of them? In the brief sketch which I purpose giving you on the present position and probable future of this question, I fear that, however much we may try to avoid it, I shall appear more in the character of an advocate, than of an impartial judge. But I am fully conscious that I am before a highly critical audience, who will not allow any bias of mine to warp their own just judgment.

The views of Flourens are familiar to you all. They seem to the scientific world at once in accordance with the facts and experiments on animals, at least of the lower orders, and in harmony with the prevalent metaphysical conception as to the unity and the indivisibility of mind. Like the mind itself, said Flourens, the organ of the mind is also one and indivisible, there being no differentiation of function, but each and every part possessed of the potentialities and capability of exercising every function pertaining to the whole. These doctrines speedily met with general acceptance among physiologists, but there were a few keen clinical observers—De Boyer, Andral, and others—who saw in the phenomena of cerebral disease facts which appeared wholly inexplicable, except on the hypothesis of a differentiation of function, such for in-

stance as the occurrence of limited paralyses in connection with limited lesions. But unable, on the other hand, to account for the facts of experiment on animals on the principle of localisation, which the clinical data seemed to demand, they wisely suspended their judgment, neither denying the facts of experiment, nor doubting their clinical observations, believing that the apparent discrepancies between human pathology and experimental physiology would one day be cleared up and dispelled. This was eminently the attitude of my distinguished predecessor in the honourable position I now hold. Dr. Hughlings Jackson, neither contesting nor trammelled by the doctrines of the school of physiology, saw, in the occurrence of a limited unilateral muscular spasm, in connection with certain cortical lesions, phenomena which, to him, signified irritation or discharge of grey matter directly related to muscular movement; and with a keen prophetic insight, which far outran the slow march of verified fact, he evolved many brilliant and fruitful conceptions, as the anatomical substrata of the organ of the mind. There were, however, many other clinical facts, particularly those relating to the loss of speech in connection with lesions of the left hemisphere, which formed the subject of much lively debate and discussion twenty years ago, which remain equally obscure and mysterious, whether on the hypothesis of localisation or the reverse. It is not going too far to affirm that, up to the time when the researches of Fritsch and Hitzig inaugurated a new era in cerebral physiology, general doubt and confusion prevailed. It would even now be premature to claim that all this has been dispelled; but we may truly say that the questions in dispute have assumed a definite and manageable shape, and that every day the dark atmosphere is being cleared. I will not weary you with the keen controversies which arose as the true significance and interpretation of the results which followed application of electrical irritation to different regions of the cerebral hemispheres. On these, as well as on the consequences of destructive lesions of the cortex, the most divergent views continued to be entertained and expressed. The fundamental question of localisation or no localisation was brought to a crisis at the meeting of the International Congress here in 1881. You are all aware, many of you doubtless heard and saw, that Professor Goltz, the chief of the few remaining champions of the Flourensian system, brought with him from Strasburg a dog in which he had long previously, at intervals, destroyed a large extent of the cortex of both hemispheres, and which he exhibited before the physiological world as a practical refutation of the theory of cerebral localisation. He enunciated the following thesis (I translate literally). "1. The cerebral cortex is the seat of the higher intellectual functions; the removal of large portions of both hemispheres degrades the intelligence. 2. It is impossible, by any localised cerebral lesion, to cause paralysis of any muscle; the animal operated on retains volitional control over all its muscles. 3. It is also impossible, by any localised cerebral lesion, to cause permanent loss of any sense. The animal retains all its sensory faculties. After removal, however, of large portions of the cortex, defective perception is caused. 4. Animals in which the parietal regions have been destroyed are permanently awkward in their movements, and defective as regards tactile sensibility. Animals in which the occipital lobes have been destroyed are, as a rule, more demented than those in which only the parietal regions have been destroyed." (*International Medical Congress Report*.) He pointed to his dog as an illustration of the truth of these propositions: awkward in all its movements, especially those of the hind legs, but paralysed as to none; defective in all its faculties of sense and sensory perception, but neither blind nor deaf, nor otherwise totally deficient. Some exceptions were taken by Professor Yeo and others as to the exact accuracy of Professor Goltz's description of the extent to which the motor and sensory faculties were impaired; but I pass over these as not essential to the matter in hand. When the brain of the dog was examined by a committee appointed by the Section, it was found that the lesions, though extensive in both hemispheres, were less than Professor Goltz had imagined, and did not implicate the whole either of the so-called motor region or of the region of special sense as defined by his opponents. Over against Professor Goltz's dog were set two monkeys, in one of which the greater portion of the region called motor had been destroyed many months previously in the left hemisphere. The animal had remained hemiplegic on the right side from the date of the operation, and at the date of the exhibition presented the typical features of hemiplegia, with late rigidity, as exhibited in man. The hemiplegia was seen and admitted by all, and the permanency of the result was sufficiently plain to all conversant with the incurable nature of hemiplegia with late rigidity in human patients. It was afterwards proved by

the committee of investigation, that the lesion in this case affected only the cortical matter and subjacent medullary fibres in the region where it was said to be; and the fact of descending degeneration of the pyramidal tracts of the right side of the spinal cord was also established at a later date. With the exception of the motor paralysis of the right side, the animal was in all other respects perfectly well, full of intelligence, acute in its sensory faculties and powers of perception. The other animal, in which the superior temporo-sphenoidal convolutions had some time previously been destroyed on both sides, presented a marked contrast to the first. There was no question as to this animal's motor powers; it bounded about and climbed hand over hand with the greatest agility and vivacity. Nor was there any question as to its sensory faculties, except one, namely, hearing; and in that respect it was admitted to be totally deficient; for it was remarked by one present that it was the only animal in the room that was not startled by the explosion which I made. In this condition of total deafness, with retention of all its other faculties, both sensory and motor, it had been since the operation, and continued so till its death, a long period subsequently. The brain of this animal was exhibited at a meeting of the Physiological Society, and will be described in another place. Suffice it to say that the lesions were confined to the cortical and subjacent medullary fibres in the regions I have indicated. It is at once obvious that the second and third of the propositions enunciated by Professor Goltz (and it was admitted by himself) are demonstrably erroneous as applied to monkeys, and that, in their case, the localisation of function was incontestably proved.

To some, however, it appeared as though these demonstrations, instead of definitely settling the question of cerebral localisation, had made confusion worse confounded: a word or two, therefore, on the lessons taught on this occasion, and on the conclusions to which they logically lead. Localisation of function being admittedly established in the case of one vertebrate animal, then, given facts and experiments on another animal of the same type seemingly at variance with this law, there is no alternative between either denying the universality of the law, or holding that the facts, though seemingly at variance, are, in reality, in perfect harmony with it. I cannot think it possible that, in the present state of biological science, any one will be found to maintain that animals constructed on the same anatomical type can differ so fundamentally in their physiological constitution as would be implied in the first alternative. We must assume, therefore, that the whole question turns on how, on the principle of localisation, we are to account for such striking differences as are observable between the effects of destruction of the cerebral hemispheres in the different orders of animals. It would be difficult, on a cursory examination, to say whether a frog or a pigeon had had its cerebral hemispheres removed or not, if they each maintained their normal attitude: the one will hop, and the other walk or fly, and otherwise respond to the sensory stimuli, in a manner little, if at all, different from the normal. The universal powerlessness, however, of even dogs and much more so of the higher vertebrates—after a similar operation, will be very apparent. This fact, admitted by all, clearly demonstrates the existence of great differences in degree among the different orders of animals in respect to the part which the cerebral hemispheres play in the ordinary modes of activity and response to the various forms of external stimulation. If it is difficult with some animals to estimate the results of the removal of the whole of the cerebral hemispheres, how much more must it necessarily be to determine the effect of removing only a portion. Neglect of the most obvious lessons of comparative physiology has been the cause of much useless barren controversy; facts being urged in opposition to other facts, between which no antagonism really exists. If to these considerations, founded on comparative physiology, we add that, unless the whole of a given cortical centre be destroyed, the function remaining may suffice for the continuation, in some degree at least, of the function allotted to it, we may arrive at a completely satisfactory explanation of the conduct of Professor Goltz's dog in thorough harmony with the fundamental principles of cerebral localisation.

I have purposely excluded the elements of clinical medicine in reference to this quest, for the exact solution of problems in physiology by fortuitous experiment on disease in man is always difficult, and nowhere more so than in the domain of cerebral function. The phenomena of cerebral disease are so complex, the factors so indeterminate, that they seldom admit of exact analysis or computation; but, though all these clinical observations may be unable to carry rigid conclusions as to cause and effect, all physiological generalisations otherwise founded must square with

the facts of clinical observation before they can be admitted as integral parts of the institutes of medicine; and on many points, particularly when subjectivity is concerned, the observations on man constitute the only reliable means of interpretation of vital manifestation. Apart from some differences on matters of detail, which do not affect the main principle, localisation of cerebral function is accepted by every recent writer on cerebral disease as the only possible explanation of the phenomena. Mere numbers in authority, however, will not stand for evidence, and I do not quote them as such; but, as it would be impossible for me to set forth the evidence here, I can only refer inquirers to what I have elsewhere written on the subject, and to the facts contained in the more recent publications of Charcot and Pitres, De Boyer, Grasset, Nothnagel, Exner, Ross, and others, as well as to the cases, such as those recently recorded by Dr. Sharpey, which are constantly appearing in every medical journal.

Assuming, therefore, that the principle of cerebral localisation has become established in physiology and clinical medicine, the ground is cleared for the consideration of further questions of great scientific and practical importance, on which opinion is not unanimous, and on which, therefore, further research is desirable. The first is, is the localisation of centres a matter of indifference or accident; or is it dependent on structural peculiarities and actions, which render each centre as distinct from others, as, for instance, one limb from another, or the organs of vision from the organs of hearing? Or, to put it in the form of a simile, is the brain-crust divided into fields, each of which, though yielding some kind of produce, usually may, if need be, yield any other? Or is it divided into fields which yield only one kind of produce, and can yield no other? Or, according to still a third supposition, is it divided into fields each of which yields most of one kind of produce, but more or less of every sort? Each of these suppositions has its advocates and defenders. If the localisation be merely accidental or indifferent, perhaps the function of a part lost may be taken on by some other of those remaining; if it be structural, this would be as impossible as that the organ of hearing should perform the functions of the organ of vision; or that the nervous apparatus which moves the leg should also move the tongue, or act as the centre of taste. The question is one which ought to admit of definitive settlement, by properly directed research. The indifferent theory has been adopted by many under the name of "functional compensation," as affording the most satisfactory explanation of the apparent recovery, after destruction of the cortical centres, of motion in certain of the lower animals. If it should appear, however, that in monkeys and still higher animals it is possible, by total extirpation of a given centre, to cause complete and permanent annihilation of a given faculty of sense or motion, all others remaining intact, it is obvious that the hypothesis of functional compensation is uncalled for. Those who consider this established, explain the apparent recovery in the lower animals on the principles already laid down respecting the differences in degree in which the motor faculties and powers of response to the sensory stimuli are affected by destruction of the cerebral hemispheres in different orders of animals. The so-called "functional compensation" of the hemispheres is, in reality, no compensation at all, but only a manifestation of the activity of the lower centres. In addition to the facts of experimental physiology demonstrating the possibility of entirely and permanently annihilating given functions or faculty, others are adduced in favour of a rigid structural localisation. Though the grey matter of the cortex appears similar throughout, and composed of elements common to every region more or less, yet recent histological researches, particularly those of Bevan Lewis, have succeeded in determining the existence of numerous peculiarities in structural arrangement and collocation in different regions of the cerebral cortex. The significance of these structural peculiarities is by no means apparent; but that that they exist should have a meaning which may one day become clear, there is no room to doubt. Perhaps a more intelligible signification than these histological inquiries lies in the facts now indubitably established both by physiological experiment and by human pathology, that when certain regions of the cortex, namely, those termed motor, are destroyed, and no other secondary degeneration ensues in the pyramidal tracts, down the whole length of the spinal cord, the inference drawn from these data is that, just as the motor nerves are connected with the anterior horns of the spinal cord, so the pyramidal tracts of the spinal cord are connected with certain regions of the cortex cerebri, showing therefore a fundamental difference as regards anatomical connection between one region of the cortex and another. It is argued, also, that if the pyramidal tracts had other connections, destruction of other regions than the motor

would induce secondary degeneration also, which is not the case, and that those connections, if they exist, should prevent the atrophy and degeneration which otherwise occur on destruction of the true motor area. As regards the region related to the organs of sense, it is stated as an established fact that the destruction of a certain cortical region on both sides causes complete and permanent loss of vision, followed in due course by atrophy of the optic discs. This is relied on as proving that this region is anatomically the cerebral expansion of the optic tracts, and that the complete abolition of vision and subsequent atrophy of the optic nerve show that this region is above the centre of vision, and that if the optic nerve have connection with any other region or regions, these are unable to form the substrata of the visual sense, or prevent the wasting of the optic nerve. No similar observations have as yet been made on the atrophic relation, if any, between particular cortical areas and other organs of special sense; some cases have, however, been published in favour of the occurrence of atrophy in certain cortical regions as the result of long-standing deprivation or congenital absence of some limb or organ of sense. Many of these, however, have an extremely slender foundation, and much is required, in the way of future research, before conclusions can be safely founded on them; but if succeeding research should establish such facts, free from doubt and uncertainty, and if all the statements should be substantiated in reference to the permanency of the effect of cortical lesions and the consecutive degeneration of certain tracts and organs, the question of indifferent or rigid structural localisation will have been definitely settled.

After the fundamental principle of localisation itself, whatever, all the other points involved in it, whatever may be their scientific interest, sink in importance, as regards practical ends, in comparison with those relating to the exact delimitation of the respective cortical centres. Here we meet with a considerable diversity of opinion among those who have experimentally and clinically investigated the subject. This has been made use of by some, very illogically, as an argument against localisation altogether. It might be alleged, with as much reason, that any difference of opinion among the Fellows of this Society on questions brought before them, are a sign of instability on the part of the Society itself. I should require a treatise rather than the limits of a brief sketch to place before you the various facts and arguments on each head, and I should have to import many things of my own as yet unpublished, for which this is neither the time nor the place. I content myself, therefore, with merely indicating points on which there is more or less harmony, or the reverse. The convolutions bounding the fissure of Rolando are, without exception, regarded as having a special relation to the motor powers. Whether motor disorders are invariably caused by lesions in this region, is a point of capital importance. If clinical research should establish the existence of an unexceptionable case of total destruction of this so-called motor region without motor paralysis, such a case will do more towards demolishing the whole theory of the existence of motor centres in the human brain than any hundreds and thousands of cases adduced in support of it. But, naturally, considering the harmony which otherwise exists between the positive clinical instances and the established data of experimental physiology, we have a right to demand something more than mere vague assertion or crude investigation before a negative instance can be admitted as proved. None such has as yet been put on record; all the doubtful cases, extremely few in number, bear only on the precise limits of the respective centres, and not on the general question of the existence or not of a special motor area. The centres of hearing have been shown before the physiological world to be in the superior temporo-sphenoidal convolution; and numerous clinical observations have been put on record favouring the existence of a causal relationship between the lesions of this region in the left hemisphere and the occurrence of the defect termed word-deafness—a defect in the representative functions of the auditory centre. Nothing, however, comparable to the absolute deafness, presentative as well as representative, seen in the monkey from bilateral destruction of this region, has as yet been observed in man.

The centres of vision are still the subject of some differences of opinion; but these differences have reference not so much to the general position as to the exact delimitation of the anterior boundary, and to the relations of each centre to the central and peripheral portions of the retina respectively. Clinical observations are being published in considerable numbers demonstrating the occurrence of isolated affection of vision in connection with cortical and sub-cortical lesions in the occipito-angular regions, and the differences

between the published views on the relation of the centres of the organs of vision are in process of being settled.

The centres of smell and taste are less definitely determined, owing to the difficulties of experimentation; but the greatest differences of all obtain in reference to the localisation of the centres of common and muscular sensibility. A considerable number of physiologists place them in the so-called motor region, while I have all along held, and hold, both on experimental and clinical grounds, that the centres of common, including muscular, sensibility, are anatomically entirely distinct from those of motion, and situated in the subcortical region. It is of the utmost importance that this question should be settled, for it involves vital issues in the region of diagnosis and differentiation of cortical from other cerebral lesions. In this respect, clinical observation, careful and accurate work, has a transcendent value, and there is every reason to believe that this, in combination with a more precise experimental method, will at no distant date succeed in solving outstanding difficulties, securing a general harmony of opinion, and placing cerebral localisation and the regional diagnosis of cerebral disease on a firm and unsailable foundation. In no department of medical science is more activity being manifested, and there is no field more inviting, or more likely to repay cultivation. The following words of Marshall Hall are perhaps more pertinent now than they were in his own time: "In every point of view there is much to be done. Every encouragement should be given to the diligent, devoted investigator; every obstacle, every kind of injustice, every source of disgust and indignation ought, for the sake of science and for the honour of our institutions, to be removed. The physician who devotes himself to investigation makes a thousand sacrifices; his faith requires cheering, and should not, need not, be unjustly obstructed or beset with thorns. Though the value of scientific investigation is by no means to be measured by its practical utility, yet the value of a scientific fact or principle is enhanced in the eyes of all mankind if it be useful as well as true." Tested by the standard of practical utility, what, it is asked, has cerebral localisation done, or is it likely to do towards a more successful treatment of cerebral disease than we can yet boast of? Up to the present, I think that, with a few though insignificant exceptions, the benefits of the scientific doctrine of cerebral localisation have been absorbed, like so much latent heat, by medical science itself, as distinct from medical or surgical practice. It has been as a lamp to lighten the path of the clinician through a darkness almost chaotic; it has sharpened clinical vision, so that now many things are clearly seen which were formerly supposed not to exist; it has given a decision to clinical and pathological descriptions which will be searched for in vain in the older records; it has cleared our conception as to the significance of numerous symptoms, and rationalised many purely empirical generalisations, and is every day bringing us nearer that which Virchow has termed the goal of modern medicine, namely, localisation of disease. But when this has been reached as regards cerebral disease, when we are able to determine the nature and position of the *materies morbi*, is it likely that we shall stop here? This would be very improbable, even though we should as yet have none or only the most vague notions in what direction practice may be influenced. But there are already signs that we are within measurable distance of the successful treatment by surgery of some of the most distressing and otherwise hopeless forms of intracranial disease, which will vie with the achievements of abdominal surgery.

It is natural that physicians should hesitate to advise surgical operations on the brain, and that surgeons should stay their hands until the principles and diagnosis of regional disease had been established with as near an approach to certainty as is possible where all is hidden from the eye and hand. But, granting this, is there any reason why a surgeon should shrink from opening the cranial cavity, who fearlessly exposes abdominal viscera? The peritoneum was, until a recent date, held sacred and inviolable. The dura mater and the brain are much in the same position now. But, after what I have seen of the unflinching safety, the freedom from all untoward results as regards health and life, with which the most formidable and repeated operations can be performed on the brain and its covering, under stringent antiseptic precautions, and these on animals of the most delicate, almost human organisation, I cannot but believe that similar results are capable of being achieved on man himself.

Apart from secondary inflammation and its consequences, which can be absolutely prevented, there is no risk to life from even extensive destruction of the cerebral hemispheres. It is true that, in attempts to remove tumours, or locally treat other forms of

disease, we may injure or destroy healthy portions of the brain-tissue. But what will follow? Not necessarily mental disorder or appreciable mental defect.

With the exception of certain functions arrogated by the left hemisphere, we have, for mental purposes, practically two brains; and diseases are not always on the left side. The records of our daily experience of disease and injury of the brain show that considerable portions of the brain-substance may be destroyed without great danger to mental stability. But paralysis may ensue, more or less extensive, or defects in special sense, according to the position and amount of lesion. Such risk undoubtedly exists, as well as others incident to the operation itself, as to all surgical operations. But the choice is not between this and any other mode of treatment, but between running these risks and certain death; and I have no doubt that there are many who, if they had the choice, would rather go through, if not enjoy, life, halt or hemiplegic, than perish miserably after years, it may be, of incessant suffering. "Ne sutor ultra crepidam," I think I hear some one say. I applaud the sentiment; but as in this matter I have had the experience of the surgeon, as well as the physiologist and the physician, I have thought that I might venture on these suggestions without appearing to go very far beyond my last.

OLEATE OF QUINIA.—Dr. Squibb, in his *Ephemeris*, considers that oleate of quinia, although it has not attracted general attention, is probably one of the most important of the oleates. From the larger quantity of quinia needed in medicine, this oleate is made as strong as possible. While a normal oleate of this alkaloid would by calculation contain about 53 per cent. of the alkaloid, it has been found impracticable to dissolve that quantity in the acid. In a series of experiments made some years ago, when this oleate was introduced, it was concluded that 25 per cent. was the best proportion. But as the quinia of the market holds some hygrometric moisture, it is better to take 26 grains of the alkaloid and 74 grains of oleic acid as the formula. The alkaloid is simply rubbed to powder, and added to the acid in a bottle. Like the other alkaloids, this dissolves so readily that, being in much larger proportion, it is liable to clog together and be slow in dissolving. But this clogging together is easily broken up by means of a glass rod, or the solution may be effected in a capsule, and the clogging be prevented by a pestle. In this way it is easily made in a few minutes. A fluid ounce of this oleate weighs about 410 grains, and, therefore, contains about 102 grains of quinia, which is equivalent to about 140 grains of the ordinary sulphate of quinia; therefore, a fluid drachm contains the equivalent of about 17 grains of the sulphate, and a minim is equivalent to a little more than a quarter of a grain. An hypodermic injection of a fluid drachm will, therefore, carry the equivalent of 17 to 18 grains of sulphate of quinia. It happens, perhaps, oftener with the administration of quinia than most other medicines, that the physician wants to save the stomach; and many conditions need quinia when the stomach will not accept it, or will not utilise it if given by the mouth. These circumstances have long indicated the hypodermic use of quinia, but up to this time no solution has been proposed that is well adapted to hypodermic use, first, because of the large dose required, and again, because of sparing solubility of available quinia salts. Hence this oleate has been sometimes used hypodermically, but with what success is unknown to the writer. The epidermic use, however, is of late not uncommon, and, since it was first proposed, occasional trustworthy testimony from private sources has led the writer to consider it an important adjunct to the more common methods of using quinia. The quantity of oleate needed here is considerable, often amounting to one or two fluid drachms. Hence it should always be applied under oiled silk or gutta-percha tissue. When put directly on the skin, a minim will require about four square inches of surface or it will run, and a fluid drachm would require about two square feet of surface, an area hardly accessible under ordinary circumstances. But two pieces of very thin fine old muslin or linen, six by nine inches, will easily hold half a fluid drachm each, and may be applied to the inside of the thighs, covered by oiled silk. This leaves the abdomen available for another similar application if desirable, and the oleate can be renewed on these places as rapidly as it is absorbed. Another good way of applying it, especially in walking cases, to get a moderate continuous effect, is to anoint the spinal tract for an inch or more on each side of the spinous processes, morning and evening, with a half fluid drachm, and cover it with a strip of oiled silk under the clothing. The writer has heard of several instances in which a ringing in the ears was speedily produced by such applications of the oleate.

INTRODUCTORY LECTURE ON THE PROVINCE OF ANATOMY.

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THIRTY years ago it would have been unnecessary, even for a new professor in a new place, to commence his lecture on anatomy by defining, in a somewhat apologetic strain, the position, in the medical curriculum, of the science which it is his province to teach. No one then questioned that upon a carefully taught anatomy as a foundation all sound medical teaching should be based.

Within the last few years, many changes have taken place; the range of medical study has widened, new subjects have been imported, and new branches of older subjects have largely developed; while the student is expected to master this increased curriculum in the same period as that which was formerly thought necessary for a much more limited course.

The consequences of these changes are obvious. Many of the new subjects, from their very novelty, from the attractiveness of their method, the skill of their teachers, as well as their intrinsic importance, now claim a large share of the student's attention; and to provide the time necessary for their study, there is a growing tendency to curtail that spent in the pursuit of other subjects, each teacher setting forth his own science as that of primary importance.

In this conflict of opinion, the relative positions of the sciences fundamental to the medical course is liable to be misunderstood; the older and less attractive falling into a position inferior to that which they really deserve, while at the same time the diversity of the student's occupation tends to encourage superficiality rather than thorough knowledge, so that the man who passes through the extended course in the prescribed time can rarely do more than absorb a smattering of each science.

The real solution of this difficulty, and that to which experience is fast bringing us, is the lengthening of the curriculum, so that in the additional year the balance of subjects of study can be more carefully adjusted with a due regard to their relative importance.

But we have not met together here and now to legislate as to the future arrangements of medical study, but to consider what position anatomy should occupy in your own system of studies, and how much of your time and attention you ought to give to it.

By your presence here, I assume that most of you intend to devote your lives to the study and practice of medicine or surgery. A few of you may be pursuing the subject for its own sake; some, I hope, with the prospect of being the scientific teachers of the future; but, judging from the history of Cambridge graduates in the past, I conclude that, even in this most favoured of schools, most of you wish for a kind of knowledge which you can turn to practical account; useful not merely for the intermediate ordeal of your examination, but also for your after-life of practice. If this be so, it is surely of foremost importance that you should have as perfect a knowledge as possible of the human body in health, and it is this knowledge which constitutes anatomy.

I wish you to bear in mind that anatomy is an exceedingly wide subject, one which requires to be viewed in two aspects, as an art and as a science. The art of the anatomist deals with the examination of the structure of the human body, not only of its coarser larger arrangements, recognisable by the naked eye, and separable by the knife and forceps, but also of the finer building up of the textures, the minute organisation revealed by the microscope, seen in sections, developed by reagents—in short, with the unfolding of the physical characters and relations of the component parts of the human body, including all that we now call histology, which is but one small subdivision of anatomy.

But the science of anatomy is much wider than the art. Every tissue and every organ in the body has had a history, which it is the business of the anatomist to search out and record. At some period in the life of the individual, it has begun to be, and, through many vicissitudes of time and circumstance, has attained to its adult condition. This branch of our science we call embryology; but it is simply the historical side of anatomy, essential to and inseparable from the rest of the science, to which we shall have frequent occa-