The limitations of surgery : past and present / [Archibald Young].

THE LIMITATIONS OF SURGERY, PAST AND PRESENT.

BY

ARCHIBALD YOUNG, B.Sc., M.B., C.M.,

F.R.F.P.S.G.

Dr. Morrison —

Larkhall.

With Compliments &
Kind Wyards.

Oct. 1928.

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Presidential Address

DELIVERED TO THE ROYAL MEDICO-CHIRURGICAL SOCIETY OF GLASGOW ON 3rd OCTOBER, 1924.

BY

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THE LIMITATIONS OF SURGERY, PAST AND PRESENT.*

LADIES AND GENTLEMEN,—Already I have told you, in sufficiently grateful terms, I hope, how much I have appreciated the honour you have conferred on me in electing me your President for the ensuing two years. It shall be my privilege as well as my duty to endeavour to justify your choice. I promise you that if I fail it shall not be from lack of desire to meet the obligations of office.

One of these obligations—indeed, almost the first—I have to meet to-night, when, according to usage, I present to you something in the nature of a formal address. It will be your duty—it may not be your privilege—to receive it with what forbearance it may require. I crave your most tolerant goodwill.

I have not chosen to attempt any startling pronouncement, any epoch-making disquisition on a surgical subject, and for the very good reason that I have none such to make. Nor have I chosen, I hope, a subject that might be regarded as too restricted in its interest, too specialised in its application, to appeal to all, or most, of my fellow-members. It seemed to me

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that to present to a Society like ours, whose membership includes all classes of workers in medicine, an address dealing with a special branch or department of the latter, or having only a restricted application, would not be in correspondence with the fundamental purpose and function of a presidential address. I have chosen, therefore, to put before you a few, perhaps rather fragmentary, thoughts of a general kind, which, though they are presented by a surgeon, and are suggested by the surmises, the outlook, and the musings of a surgeon, are not without a bearing on the many similar, and often allied, problems of general medicine. I have chosen to give the subject of my remarks under the title "The Limitations of Surgery." I might have chosen the title of "Some Problems of Surgery," and, indeed, that was my first idea, but a moment's reflection, I think, will suggest that the former title allows of a wider outlook, and permits one to include matters which, not themselves strictly surgical, perhaps not surgical at all, yet, influence, qualify, limit or completely check surgical advance. It is the wider view I am anxious to take; hence my choice of title.

It is well, perhaps, that I should make it clear, from the very outset, that I take no gloomy view as to the prospects of surgical advance. The title of my address might seem to suggest that I do. Nothing is further from my thought or intention.

When thinking over what I should present to you, my mind carried me back to undergraduate days, when, as an enthusiastic member of the Student Society at the University, which bears a similar name to ours, and which can boast an antiquity slightly longer than ours, I took part, as one of the leaders, in a discussion as to the relative greatness and possibility of medicine and surgery. The subject of debate was entitled "Medicine *v.* Surgery: which is the greater, as regards possibility and results?" The leaders for medicine were James

F. Gemmell (now Professor of Zoology at Dundee) and J. C. Robertson (afterwards a distinguished member of the Indian Medical Service, and quite recently dead). I led for surgery, being ably supported by my old friend, William Burns, whose early death so many of us mourn.

It is interesting to me to find that I took then a particularly sanguine view of the prospects of surgery, though I may say, in parenthesis, that I tried to advance and support, at the same time, a much more gloomy prognostication respecting medicine. I argued then that there were certain more or less fixed limitations which rendered the possibility of advance in pure medicine unpromising; and I proved—as I thought then—that for surgery the limitations were not so definite; that, in fact, we were still far from approaching the limit of great surgical advance; that there was still abundant work to be done; that there were still many fields to explore, many conquests still awaiting achievement.

I was then in the hey-day of youthful arrogance, especially in respect of my outlook on medicine and on the province of the pure physician. Much that I contended for then I could not now approve, but at least I have not altered the view I then expressed as to the future of surgery, though I recognise now —what, for the sake of argument, at anyrate, I set myself on that occasion to try to disprove—that medicine has a future probably equally great and promising. Both have their necessary limitations, but we should see in these very limitations only the greater reason for effort in the search after knowledge and truth.

I hope that I have made it clear, then, that what I desire to direct attention to is not so much the limitations of surgery, regarded as more or less fixed—the limited or circumscribed condition itself—but rather the restrictions, restraints, limiting conditions, qualifying factors which check achievement, which restrain advance.

It seems well to define, at once, what I have specially in view, in speaking of surgery in this connection.

Surgery is both a *science* and an *art*. Regarding it as a *science*, it includes the study of injuries and certain diseases affecting the body as a whole, or limited to special regions, organs, parts, or tissues of the body, and it deals with morbid growths and pathological processes of particular regions and of different types. Surgery as an *art* may be taken as dealing particularly with the treatment of any or all of these by operative means.

In this paper it is my intention to deal chiefly with surgery as a science, but it is obvious that the two aspects of surgery can never be completely dissociated, and that advance in the one is bound to be affected by, and to affect, the other.

Considering surgery as an art, it is not difficult to show that it is conditioned by two main factors, the one of a general character, and dependent to a preponderant degree upon the state of surgery the science, its advance, its enterprise, its freedom from the inertia of self-satisfied conservatism; the other in which the personal element is of chief importance.

The personal factor is, of course, bound by limitations peculiar to itself. Personal proficiency, dexterity, practice, anatomical knowledge, facility for taking quick decisions, for storing mental pictures from the experience of the passing years, and in allowing free play for wise experiment in the application of knowledge and experience to each fresh situation and emergency—these and other qualities in the individual surgeon count for much in advancing surgery the art, in the case of every surgeon who would rank as an artist in his work. Qualities such as these are given to men in varying degree; what is even more important, they are cultivated in very different degrees. It is matter of common knowledge that there are great differences in dexterity, greater differences in diagnostic powers, and that similar emergencies are met with very different

resource and very different success by different surgeons. Take even the matter of healing of wounds, as seen in the practice of different surgeons. Consider the attitude of different surgeons to the occurrence of sepsis in the wounds they make. In one surgical clinic the occurrence of occasional sepsis in an operation wound is regarded, even to-day, as a thing that is almost inevitable. "All possible precautions," you will be told, "have been taken to prevent any sepsis, but, of course, you cannot help an occasional 'stitch abscess' happening." In another clinic, such a happening is not so lightly regarded. Not long ago, a number of questions were put before a large selection of surgeons in this country, dealing particularly with this question of occasional sepsis. I do not remember the actual questions, or the wording even of the one I should have liked to quote here, but it was something like this—"Is it still regarded as a more or less natural thing that an occasional case will 'go wrong'"? My answer to the question was sufficiently emphatic, I can assure you; but the point I wish to make is this, that the mere fact that such a question could be gravely put, in this year of grace, suggests that sepsis as a possible limitation must still be a real thing, and its persistence is only to be explained—it cannot be excused—as dependent on the personal factor, some defect for which the person, the individual, must be held responsible.*

Surgery as an art, then, viewed from the personal side, is hung

* The actual wording of the question was as follows :—

"Would one be told in an ordinary hospital ward that such and such a case had 'gone wrong,' or had a 'stitch abscess,' or is suppuration after operations on unbroken skin really now much rarer than it used to be"?

My answer was as follows :—

"We do not know, in my practice, what you speak of as cases 'going wrong.' Stitch abscesses do not occur. I never see such a thing in operations on unbroken skin, whether operations of emergency or otherwise. If such a thing were to happen, I think my staff would commit suicide in a body rather than face me. These things simply do not happen."

about with many limitations, but these limitations can be overcome if the individual surgeon pursues his calling with assiduity, with conscientious earnestness, and with a moral and spiritual motive.

Speaking of quite another subject, Conrad says, in *The Mirror of the Sea*—"This is why the attainment of proficiency, the pushing of your skill with attention to the most delicate shades of excellence, is a matter of vital concern. Efficiency of a practically flawless kind may be reached naturally in the struggle for bread. But there is something beyond—a higher point, a subtle and unmistakable touch of love and pride beyond mere skill; almost an inspiration, which gives to all work that finish which is almost art—which *is* art."

It seems to me that it is just this something beyond mere dexterity, mere superficial proficiency, this something which is an inspiration, which imparts the higher finish to surgery and to surgical work, that marks the artist, who alone is fully qualified to make the best of, to produce the best from, surgery as an art.

Viewing surgery the art, from the standpoint of its general aspect, it is not difficult to demonstrate its dependence on the limitations imposed on it by the state of surgery the science. Nor is it difficult to demonstrate that, in so far as it connotes mere manipulation, mere mechanical proficiency, mere technique, there is a certain amount of justification for regarding its outlook as unpromising; for regarding its further achievement as checked by substantial limitations and restraints.

As far back as 1886, Erichsen, in an address on "The Tendency of Modern Surgery," delivered at the opening of the Section of Surgery, Annual Meeting of the British Medical Association,[1] made the following pronouncement:—

"That the final limits of surgery have been reached, in the direction of all that is manipulative and mechanical, there can be, I venture to think, little doubt. Within these limits there

may be much of movement, of change, of modification in the 'technique'—if I may borrow that word from art—but movement is not all advance, it is often the mere restlessness engendered by dissatisfaction with established methods; change may be the outcome of mere personal vanity, or of more unworthy motive." . . . "Every artery has been tied, by every kind of ligature which the ingenuity of man could possibly invent." . . . "Every limb has long since been amputated, up to its highest point." "The operations have been modified by every kind of incision that could be invented, —circular, flap, oval, quadrilateral—and by every conceivable combination of these." . . . "Every large joint has been excised. These operations, also, have been modified in every possible way." . . . "The extraction of the cataractous lens has long been done successfully, but the ingenuity of ophthalmic surgeons has devised no fewer than 14 different methods by which this single operation can be effected." "Vesical calculi have been extracted from the bladder by every channel through which that organ can possibly be reached." "We have thus reached, in many of our most important operations, the final limit to which Surgery can be carried."

Now, while such a pronouncement, made almost forty years ago, and by one of the foremost surgeons of that day, cannot, in view of the notable advances in operative method and technique since that time, be accepted as even approximately correct, it contains, nevertheless, more than a grain of truth, and it emphasises the separate *rôles* and the very different outlook of surgery, the purely technical art, and surgery the science.

We are much more accustomed, I am afraid, to statements of quite another kind. Many addresses and lectures have been written in recent years, and many papers contributed to medical journals, whose chief purpose has been to dilate upon the great advances of modern surgery. The lay Press has contributed its

quota to the same process of what one may almost call self-deception. We are too apt, in consequence, to get into the habit of thinking that most of the achievements of present-day surgery are peculiar to this century, half century, or perhaps even this last decade; that there was no such thing as surgery, in the real sense, until almost within the memory of, say, our grandfathers.

Historical Retrospect.

It is a very salutary corrective to this impression or assumption to glance back for a little upon the history of surgery in the past. When one does so, it is not its earlier primitiveness, its extremely limited scope and achievement in former times, that surprise us, but rather its extraordinary state of development, especially on the technical side, even centuries ago.

Hippocrates (360 B.C.) carried out many surgical procedures. He made use of counter-irritation; used the actual cautery; set fractures; trepanned the skull. Of the last, more anon.

Celsus (A.D. 17) performed lithotomy, even if in a primitive way; did amputations; described an operation for cataract; first recommended ligature of arteries to arrest hæmorrhage. He even attempted radical cure of hernia, and, like Hippocrates, he trepanned or trephined the skull. He was a skilful surgeon for his time.

Galen (150 A.D.) developed the art of bandaging; described and used complicated machinery for the treatment of fractures and dislocations. It is of interest to compare this with the complicated pulley traction employed not so long ago, and discarded almost within our own time. Like Celsus, Galen employed ligatures for the control of hæmorrhage, and he tells how he got his silk and catgut ligatures in a shop in the Via Sacra in Rome (quoted by Ballance in his Vicary Lecture on "the History of the Surgery of the Brain," 1922).[2]

Paulus Aeginata (700 A.D.) performed many operations, such

as lithotomy, laryngotomy, tracheotomy, amputation of the breast, &c.

And so on down the years. One might multiply evidence, but this is not possible in the short time at our disposal.

Trephining or Trepanning.

Let it suffice to take, further, as a fairly striking example, the operative procedure of trephining or trepanning the skull. One is very apt to think of operations on the skull as being

Fig. 1.

Skull from Neolithic period, showing primitive trepanning. (Lucas-Championnère—reproduced by permission of Sir Charles Ballance.)

amongst the very latest of the achievements of surgery. And yet, operations on the skull have been performed almost as far back as one can go in history.

I have already mentioned that trepanning was carried out in the time of Hippocates, *i.e.*, at least 300 years before the Christian era. But one need not stop there. Lucas-Championnière depicts, in his "*Les Origines de la trépanation décompressive*," a skull from the Neolithic period, which shows a large smoothly rounded orifice in the left parieto-occipital region, which is almost certainly the relic of a therapeutic trephining (Fig. 1). Had it been done at the present day it

could not have been bettered; and, remember, it must have been done with flint. * He examined many skulls collected from the burying grounds of the Incas of Peru, and from caves and tumuli in France—some certainly of the Stone Age, and others somewhat later—and among them he found evidences of the operation having been performed with some frequency. He was convinced, not only of the operation having been carried

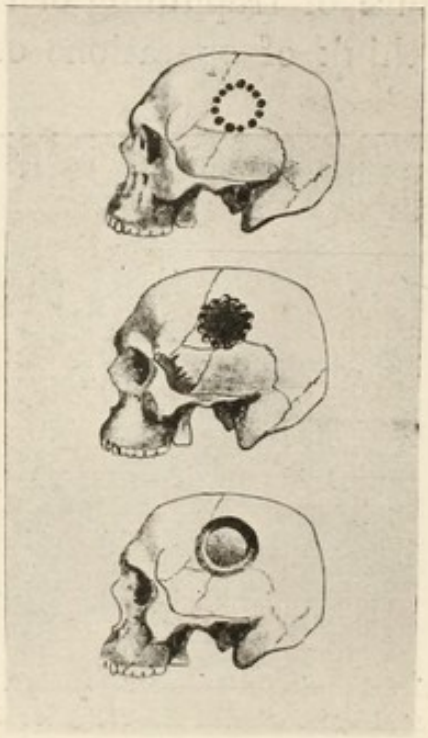

FIG. 2.

Schematic representation of method in which such trepanning was probably carried out. (Diagrams combined and reproduced by permission, from Ballance's book.)

out, but also that the appearances went to prove that the patients had survived operation for long periods. It has long been recognised that prehistoric craniectomies were not uncommon, and some authorities, such as Broca, have

* Lucas-Championnière shows, by means of the three diagrams which I have combined on one plate, how the neolithic trepanning was probably carried out, so as to produce the rounded opening illustrated in the previous plate (Fig. 2).

suggested that they had some religious or ritual significance. Victor Horsley, however, after making a careful investigation of the skulls in the Broca Museum in Paris, in 1887, concluded—(1) "That the operative openings were nearly always over the site of representation of movement on the cerebral cortex; (2) that the patients had probably been suffering from Jacksonian epilepsy; (3) that the operation would gain a certain reputation for the cure of convulsions

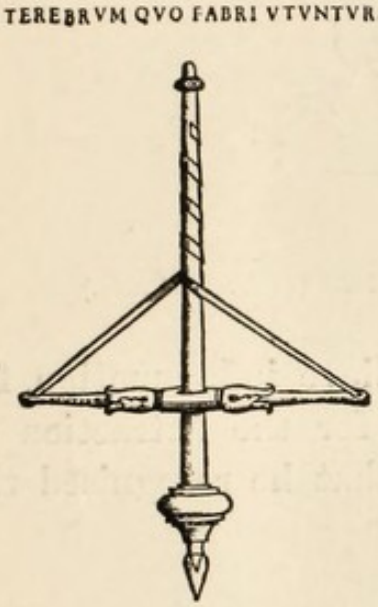

FIG. 3.

Primitive trepan. (Andreas a Cruce.)

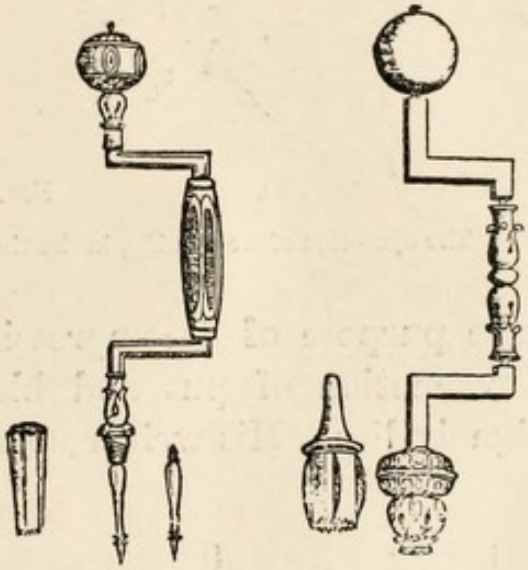

FIG. 4.

Primitive trepan (left), and trephine (right) of Hippocrates' time. (Andreas a Cruce.)

generally; and (4) that at least some of the operations, *e.g.*, for depressed fracture, would result in cure" (Ballance [2]).

As Ballance [2] says—"It is indeed of surprising interest to note that thousands of years before trephining was deliberately employed for the treatment of organic disease of the brain a *decompressive craniectomy* was performed in many parts of the world as a therapeutic measure for pain in the head, for fits, for insanity (the Biblical disease 'possessed of the devil'), and for fracture of the skull."

Hippocrates' writings and teaching show that his application of trephining was based on a knowledge much more advanced than one would have looked for. It is clear that he trephined

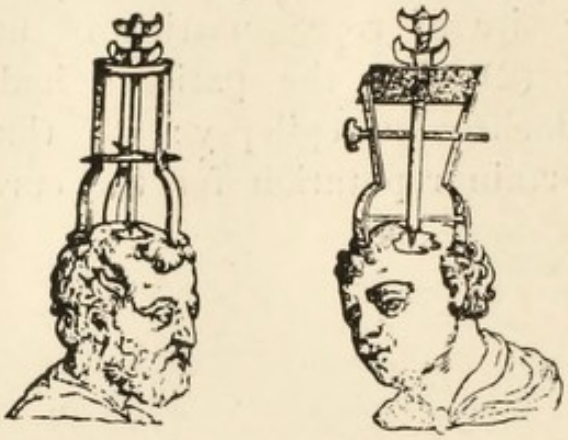

FIG. 5.

The operation of trepanning, in the time of Hippocrates. (Andreas a Cruce.)

for the purpose of decompression, to relieve inflammation, for the evacuation of pus and blood, and for the extraction of foreign bodies. His writings show also that he recognised the

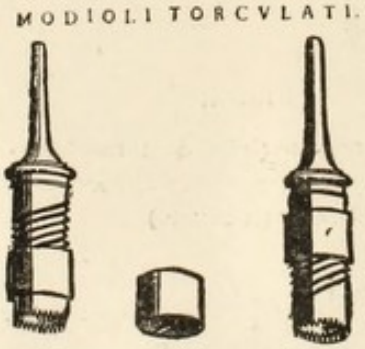

FIG. 6.

Modioli, or trephines (Hippocratic), with guards. (Andreas a Cruce.)

FIG. 7.

The operation of trephining, in the time of Hippocrates. (Andreas a Cruce.)

contra-lateral distribution of convulsions and paralyses following certain head injuries. It is on record, too, that he recommended the use of the trephine for the treatment of blindness occurring without any visible disease of the eye. This is surely the very

earliest known evidence of the recognition of the value of decompressive trephining for the relief of what we now know as optic neuritis, and its consequent blindness.

Celsus not only described the operation of trephining but urged its performance even in cases (of injury) where there was not any obvious fracture of the skull. He advocated its use for the evacuation of blood poured out between the dura and bone, and forming a clot there, even where the bone was not broken.

FIG. 8.

Hippocratic trepanning. Note the method of working the trepan. (Andreas a Cruce.)

From the times of Hippocrates, Celsus, and Galen, and on through the centuries, operations on the skull continued to be practised, and we find in the writings of Roger of Salerno (1170 A.D.), Walter of Agilon (middle of 13th century), Ambrose Paré (1560), Andreas a Cruce (1573), Fabricius Hildanus (1560-1634), references to trephining or trepanning; for fracture of the skull, for the relief of pressure by blood or pus, and for treatment of "dolor capitis." Descriptions are given as to how a sufficiently large opening may be made by joining

up adjacent trephine holes so as to allow of the removal of the intervening bone. Numerous illustrations are given in some of the works, *e.g.*, those of Andreas a Cruce and Peter Paaw, of the

FIG. 9.

Hippocratic trephining. Note the use of the carpenter's brace and bit. (Andreas a Cruce.)

actual operations, and of the instruments used. It is interesting to compare the latter with some of the present-day instruments. The resemblances are more remarkable than are the differences.

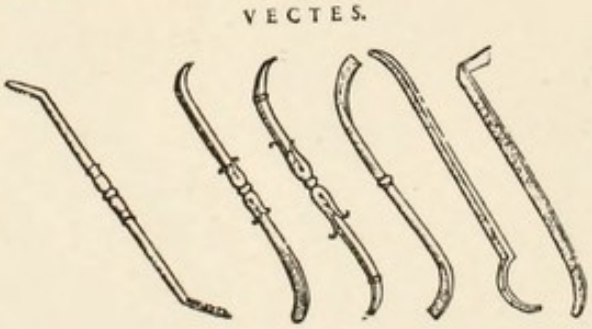

FIG. 10.

Vectes—levers or elevators—Hippocrates. (Andreas a Cruce.)

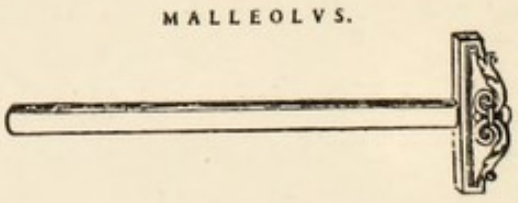

FIG. 11.

Malleolus—hammer—Hippocrates. (Andreas a Cruce.)

It is noteworthy that some of the instruments credited to Hippocrates—both trepans and trephines—are furnished with devices for preventing them penetrating too deeply. These are,

in some, of the nature of bars, or rings; in some they are bevelled from below upward; in some—the trephines—there are metal collars much like our present trephine collar guards.

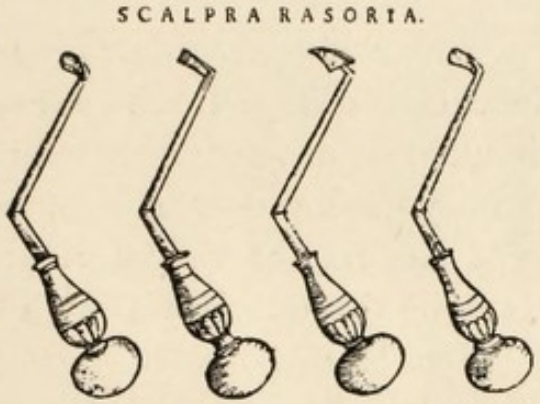

FIG. 12.

Scalpra rasoria—Raspatories—Hippocrates. (Andreas a Cruce.)

Some of these, illustrated here, you may see for yourselves, if you are sufficiently interested, in the original works of

FIG. 13.

Serrulæ—Saws—Hippocrates. (Andreas a Cruce.)

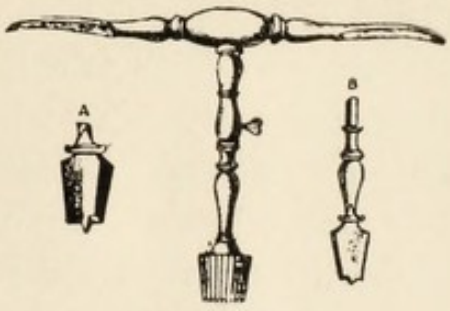

FIG. 14.

Petit's trephine and his *trépan exfoliatif*. Note resemblance of the trepan to the carpenter's bit; also the conical shape and outside teeth of the trephine. (Reproduced, by permission, from Ballance's book.)

Andreas a Cruce, Peter Paaw, and Percival Pott, which I have had laid out for your inspection. They are precious possessions of the Library of the Royal Faculty of Physicians and Surgeons

here. It is of interest, too, to note the different methods of applying power to the trephine or trepan—from modifications in various ways of the Archimedean screw, to the carpenter's brace and bit arrangement that we know to-day (Figs. 3 to 9).

There are many other simpler instruments figured in the same works, which, I think, you will admit have very close resemblances with such as are in use at the present time, and some of these I show you now:—vectes, or levers; a malleolus, or hammer; scalpra rasoria, or raspatories; serrulæ, or saws (you cannot fail to recognise our Hey's saw of the present day). Let these suffice * (Figs. 10 to 13).

The words of the Preacher must surely come back to our minds, when we survey the Hippocratic armamentarium, and compare it with our own to-day:—"The thing that hath been, it is that which shall be; and that which is done is that which shall be done; and there is no new thing under the sun. Is there anything whereof it may be said—See, this is new? It hath been already of old time, which was before us."

And so on to the days of Percival Pott (1713-1788), Petit (1674-1750), John Hunter (1728-1793), Larrey (1766-1842), and others, who, with varying courage, and basing their procedures on a gradually advancing degree of clinical exactitude, carried out operations on the skull both for injuries occurring in civil and in military practice, and began to correlate clinical observation with operation and *post-mortem* findings. Larrey, *e.g.*, observed the effects of injury to various regions of the brain, and noted several facts of importance in cerebral localisation, such as loss of memory, aphasia, contra-lateral

* Ballance points out that Peter Paaw, Vidus Vidius, and Andreas a Cruce probably obtained the illustrations of the Hippocratic instruments either "from Greek manuscripts preserved by the Arabs, or from Arabic translations of Greek manuscripts, or from Greek manuscripts treasured in the monasteries during the dark ages: the 1,000 years of disorder and chaos which followed the Galenic period."

palsy, the homo-lateral paresis of cerebellar lesions, the respiratory failure of cerebellar lesions, the connection of cerebellar abscess with otitic disease, &c.

Petit's instruments, illustrations of which I have taken from Ballance's book, are very similar to some that have been in common use in our own time. His trephine was cone-shaped, and had outside serrations. His trepan—or, as he called it, his "*trépan exfoliatif*"—was, as you see, very like a carpenter's bit of the present day (Fig. 14).

In Hunter's writings are accounts of operations by him on the skull, brain, and meninges, and his accounts are good examples of his well-known carefulness in observation and in recording.

It was not to be expected that operations on the skull and brain, though, as I have shown, carried out by surgeons through all these years, would escape the varying fate that has characterised other surgical procedures. Indeed, trephining, or trepanning passed through many vicissitudes, in respect of the degree of its acceptance. One could record very different testimony as to its repute from time to time. For example, Pirrie, writing in 1840, speaks as follows:—"These fractures, and the numerous inventions of instruments for cutting the skull, are sad monuments of the surgery of past ages." Or, take the testimony and judgment of Desault, of Paris (1813), who pointed out the numerous unfortunate results of the operation of trephining, or trepanning, and gave it as his opinion that its practice was reduced down to only trephining in compound depressed fractures of the skull, and to depressed fractures with symptoms. That the lay mind, however, even quite early, was not averse to seeing good in the operation, is attested by many examples, and there is the historic instance of the Count of Nassau, Philip, who was thrown from his horse when riding, and sustained a fracture of his skull. He was trepanned by one Mr. Henry Chadborn, no less than 27 times,

and having made, ultimately, a good recovery he gave his surgeon the following testimonial:— "I, the underwritten Philip, Count of Nassau, hereby declare and testify that Mr. Henry Chadborn did trepan me in the skull 27 times, and after that did cure me well and soundly."[3]

Astley Cooper (1768-1841), though practising trephining, in carefully selected cases, did not recommend its free use—rather otherwise, indeed. The general tendency of his teaching seems to have been to discourage its use almost entirely, except in compound fractures of the skull, though he mentions one case of cure of Jacksonian epilepsy by operation.

It is interesting to note how much opinion varied in comparatively modern times. Francis Adams, in his treatise on *The Genuine Works of Hippocrates* (Vol. i. 1849) refers to this. He shows how Pott (1713-1788) had established the rule of practice "that in every case of fracture with depression, the skull should be perforated, and the depressed portion of bone either raised to its level, or entirely removed." After Pott's time, however, opinion so changed that this rule of Pott came to be very generally rejected, Abernethy being the chief exponent of the view that the operation might often be dispensed with, even in fractures complicated with depression. Abernethy, to support his view, related five cases of fracture with depression, which, in the space of twelve months, "under his own eyes, in St. Bartholomew's Hospital, terminated favourably, although no operation was performed." Adams points out how these cases, supported by the great name of Abernethy, so profoundly affected professional opinion in this country "that it became the established rule of practice in British surgery never to interfere in cases of fracture, unless with the view of removing urgent symptoms." In leading to the overthrow of this teaching, Adams gives a notable place to the "elaborate and trustworthy statistics, published in 1844, by Dr. Lawrie, of Glasgow." This Dr. Lawrie was, I believe,

the Dr. Lawrie, who was the second occupant of the Regius Chair of Surgery in the University of Glasgow, and his statistics and opinions led to the dislodgment of the rule of practice established by Abernethy forty years before. Lawrie's statistics, compiled from the records of the Glasgow Infirmary, led him to conclude as follows:—"From what we have said, it will appear that we coincide with those who, in using the trephine, in cases of compound fracture of the skull, look more to the state of the bone than to the general symptoms, and who employ it more as a preventive of inflammation and its consequences, than as a cure for urgent symptoms, the immediate result of the accident."[4]

It required a large development of the *science* of surgery, before it became possible for the *art* of surgery to burst the bonds, to widen the scope, to overcome the limitations, the restraints, within which it was bound, and, as I have taken cranial surgery as a type, it is well to illustrate how some of the factors which checked its advance came to be overcome, and their limiting influences either set aside altogether, or greatly qualified.

Brain Localisation.

Even before the time of Cooper, much empirical information had been accumulated regarding localisation of brain function, and to some of these ascertained facts I have already alluded. Even Hippocrates recognised that function was controlled from the opposite side of the brain, and that blindness was due, in certain cases, to a cerebral cause. And down the years there had been a gradual collection, at random, of important observations relative to cerebral localisation. But it was not until the great advance in exact anatomical knowledge in the sixteenth and seventeenth centuries, associated with the names of Vesalius, Fallopius, Eustachius, Willis, Sylvius, Valsalva, Morgagni, and others, that anything like an exact knowledge of brain

localisation was rendered possible, or that the foundation of modern surgery could be said to be firmly and truly laid. The last of those I have named (Morgagni, 1682-1771) was much more than an anatomist. He may be said to have been the first great morbid anatomist, the founder of pathology as a science. From 1715 to 1771, the year of his death, he was Professor of Anatomy at Padua, and his classical work, "*De sedibus et causis morborum per anatomen indagatis, libri quinque,*" was published in two volumes, in Venice in 1761, when he was 79 years of age. His was the "anatomical idea," as it was called by Virchow, his great successor of nearly a century later. Virchow, in his address before the Eleventh International Medical Congress at Rome, 1894, spoke of "Morgagni und der anatomische Gedanke," and though Virchow concluded that, great as it was, "die anatomische Untersuchung insufficient ist," he had no hesitation in saying also, "Ihm sei die Ehre"—which, being interpreted, means, "To him be the honour." My old friend and master, the late Lindsay Steven, took as the subject of his presidential address before this Society, in November, 1905, "Morgagni to Virchow: An Epoch in the History of Medicine," and he might equally well have put it, "in the History of Surgery," for it can hardly be denied that it was Morgagni's work that rendered possible the great achievements of Auenbrugger[5] (1722-1809) and Laennec[6] (1781-1826), who developed the science of physical diagnosis in affections of the lungs to a degree of perfection that could hardly have been expected. This led in turn to further and further advances in exact clinical investigation and diagnosis. Gairdner claimed, in 1874, "not only the professed and exclusive morbid anatomists, but also, and still more, almost all the greatest physicians and surgeons of our own and the last century, as the legitimate successors of Morgagni, and the inheritors of his method of working." In Great Britain, the two Hunters, working on similar lines to Morgagni, and

influenced, no doubt, greatly by his example, at a period corresponding with his later years, did more perhaps than any others of their time, or any time—and particularly John Hunter, the younger of the two—to transform surgery, a mere mechanical art, into a science.

The whole field of surgery was, of course, affected, but there is not time to do more than follow out the progress of advance along one particular line, viz., that of cranial surgery.

The development of exact anatomy, the equally important development of the science of pathology, the progressive growth of physiology from Harvey (1578-1657) to Haller (1708-1777), the modern science of histology, which may be said to have been founded by Bichât, who died in 1802 at the early age of 31, the work of Schleiden and Schwann on cellular physiology (1837 and 1838); the correlation of all these, and of many other factors, led directly or indirectly to the epoch-making advance of the science of cranial surgery in the latter part of the nineteenth century.

The introduction of the ophthalmoscope, by von Helmholtz, in 1849; the first accurate proof of cerebral localisation, by Broca, in 1861, when he demonstrated *post-mortem* a lesion of the third left frontal convolution in a man who had not been able to speak for twenty-one years; the pioneer lectures of Hughlings Jackson (1835-1911) on the "Diagnosis of Tumours of the Brain"[7]; Jackson's classical papers on "Epilepsy," and on "Affections of Speech"; von Graefe's demonstration of the association of optic neuritis with cerebral tumour[8]; the marking out, by Fritsch and Hitzig, in 1870, of certain centres in the brain by electrical stimulation[9]; the classical experimental work of Ferrier on the brains of monkeys, in which he proved beyond question the localisation of function in the cerebral cortex[10]; his demonstration at the International Medical Congress, 1881, of two monkeys on whose cerebral cortex experimental injuries had been inflicted, one monkey having a characteristic cerebral

hemiplegia, while the other had lost its hearing; these finally proved beyond all doubt that cerebral function, and, indeed, the function of the whole cerebro-spinal system, were localised and localisable. Further experimental research, and the microscopical investigations and staining methods of Clarke, Marchi, Golgi, and others, but completed and amplified the evidence.

These events, and these signal discoveries—and only some of the most important have been recounted—serve as milestones in the path of the great advance in cranial surgery in the last century; or, rather, they mark the great steps—to change the metaphor—in the ladder of growing knowledge which made possible the triumphs of cranial surgery of the period immediately following. They led up to the famous work of Horsley, Macewen, Bergmann, Keen, and Cushing. Horsley, who, like Ferrier, had done much experimental work himself, in a paper read at Brighton in 1886[11] correlated the diagnosis in each case described with his experimental work on the monkey's brain. A year later, he described ten cases—examples of cerebral surgery—and elaborated the importance of experimental physiology.[12] Macewen's address on "The Surgery of the Brain and Spinal Cord," at Glasgow, in 1888, may almost be said of itself to mark an epoch. It recounted a series of twenty-one cases of cerebral lesion, operated on with only three deaths. Apart from the success of the operative record, the outstanding feature of the address and demonstration was the account of the accurate correspondence of functional disturbance and clinical signs and symptoms, with the operation findings. Macewen's pioneer work on "Pyogenic Diseases of the Brain and Spinal Cord" appeared in 1893, and may be said to have established itself as a classic for all time.

It is unnecessary to further develop the theme. The later history of the surgery of the brain and spinal cord—indeed, of the whole nervous system—stands out as a striking demonstration

of the steady widening of the bounds of surgical possibility, of the removal of checks to progress, of the expanding of the limitations hindering advance. And what I have said regarding more particularly cerebral surgery, both in its earlier and in its later stages, has been true in equal degree of all branches of surgery the art, and surgery the science; advance in the latter largely conditioning and rendering possible advance in the former. It may quite well be followed out and demonstrated in any of the great departments of surgery.

The Discovery of Anæsthesia and of Antiseptic Practice.

But I have left to the last the reference that must be made to two further great victories over the restraints and limitations which, had they not been overcome, would have rendered progress almost impossible. I have not left them to the last as being the least—indeed, they overtop immeasurably all the rest.

They were the discovery and elaboration of the use of Anæsthetics, and the conquest of microbic infection, by the introduction of Antiseptic practice and technique.

One need not attempt to assess the comparative value of these two great factors in the promotion of surgical advance, but their combined importance, as preliminaries and as adjuvants to progress, can hardly be over-estimated. The discovery of the practicability of employing anæsthesia during the performance of surgical operations antedated by about twenty years the discovery of the truth of the microbic theory, and the practical application of this in the development of antiseptic technique. Together they opened the door to surgical achievement and advance that without their aid could not have been more than dreamed of.

We owe the discovery of anæsthesia to four people—Sir

Humphry Davy, Horace Wells, Morton, and Simpson. Davy experimented with nitrous oxide gas, in the hope that it might be used to relieve pain during surgical operations, in the beginning of the last century, but his experiments did not prove satisfactory, and the project was abandoned for the time being. Wells, in 1844, tried the gas successfully for producing transient insensibility during the extraction of teeth. Morton, in 1846, introduced the use of ether, the first public demonstration of its use being at the General Hospital, Massachusetts, 16th October, 1846, when a vascular tumour was removed from the neck by John C. Warren. James Young Simpson, in 1847, in Edinburgh, introduced the use of chloroform. Other general anæsthetics have been discovered since, but to these three we owe the practical realisation of the use of general anæsthesia as an adjunct to surgical advance. And the application of the same principle in a different form gave us the inestimable boon of local anæsthesia also, in the discovery of cocaine and the many allied or derivative preparations that have since been employed.

Historically, the recognition of the microbic origins of disease came inevitably in the natural sequel of events following upon the development of pathological research, and our great master, Lister—perhaps foremost benefactor of the human race—simply applied to surgical practice, with unerring deduction and judgment, the principles that were suggested by the laborious and brilliant observations of his co-worker and friend, that other great master, Pasteur. And even although others before them had had at least inklings of the truth, like Semmelweis, who, in 1847, traced puerperal fever to infection, and showed how it could be prevented; or like Lemaire, who, in 1863, in a treatise on carbolic acid, advocated its use for the destruction of germs in wounds; or like Bottini, who, in 1866, advocated the employment of carbolic acid in suppurating wounds, because he was of opinion that germs were the active

agents of suppuration; it remains none the less true that Lister's practical application of the scientific results of Pasteur, by which he deduced the general laws of antisepsis, has been the very foundation of modern surgery. If one were to question for a moment to whom should be ascribed the first glimmer of light on the problem of wound infection, it would not, indeed, be sufficient to go back the few years between Lister and Semmelweis. One must go back farther still. Even as far back as the twelfth century, more than one writer could be found to deny the necessary connection of suppuration with the healing of a wound. Clifford Allbutt, in his "*Historical Relations of Medicine and Surgery*" (1905), quotes these words from Theodoric (1206-1298)—"Such a practice" [the encouragement of suppuration] "is indeed to hinder Nature, to prolong the disease;" and these from de Mondeville (1260-1320)—"Every simple wound will heal without any notable quantity of pus. Many more surgeons know how to cause suppuration than how to heal a wound."

It required the master-mind of Lister to solve the riddle, to work out the problem of prevention of wound infection which had baffled surgeons up to his time; to show how Nature could be entrusted with repair, so long as infective germs were excluded from a wound.

The introduction of Anæsthesia and of Antiseptic Practice meant an extraordinary impetus to surgical advance, an enormous victory over the restraints and limitations hampering it; and the results were very quickly seen in the achievements of such masters of the art of surgery, as Spencer Wells, Keith, Lawson Tait, Ferguson, Erichsen, Paget, and many others of the English-speaking race, as well as numerous others of Continental race, and, it is unquestionable that, without them, the great work of Horsley, Macewen, Bergmann, Cushing, Ballance, and others, in the domain of brain surgery, would have been rendered impossible.

We may say, of each of the great discoverers of the principles of anæsthesia and antisepsis, what Virchow said of Morgagni—"Ihm sei die Ehre."

The Limitations of Surgery in the Present and in the Future.

In what has gone before I have endeavoured to show how surgery has progressed, with varying force and speed through many centuries; how, step by step its limitations have been widened, its bonds loosened, its field of operation expanded. The question follows naturally—Has surgery reached its utmost limit of achievement? Has it still more worlds to conquer? Are there no more problems to be solved? Or are the limits finally set to surgical advance? I think it can hardly be questioned that the field for discovery is still enormous; that the future still holds in store many chances for the industrious and for the intrepid worker; that in the domain of surgery, as in every other branch of science, advance is inevitable, if we are not to retrogress. Anything like stable equilibrium is impossible. Not to advance must be to fall back.

Many problems have already been solved, or at least partially so; many secrets have been wrested from the previously closed book of Nature. Many more still remain. And not a few seem contingent on others already solved.

Professor J. Arthur Thomson says, in the preface to his *Secrets of Animal Life*—"Nature so often tells us one secret in terms of another." I think this is very true. It may be illustrated in respect of many of the great discoveries of science, and it can certainly be demonstrated in respect of many of the already partially solved problems of surgery.

In many cases, too, the solution of one problem merely leaves exposed the apparent insolubility of another. Were it otherwise, the correlation of organic nature would be less complete.

Just as Nature may tell us one secret in terms of another, so she may refuse to disclose one secret till another has been laid bare. The problems that remain will demand probably even more earnest investigation than those that have gone before, perhaps for the reason that we are approaching—though still very far off—the very heart of Nature herself; and certain it is that the innermost secrets of Nature's heart will not be lightly—if indeed ever completely—surrendered. We shall need all our resources, all our energy, all our perseverance, all our faith. We shall need, too, to call to our aid all the help, and all the support, that we can obtain from all the allied sciences, from all schools of thought, and from the patient investigators of all the world.

Before we can hope to set aside materially the restraints and limitations that still remain, we shall have to adopt all sorts of lines of research, and, particularly, to very largely develop the line of experimental research. We must adopt, in the most unselfish spirit, a generous and all-embracing system of collaborative research, by which the experience, enthusiasm, original thought, and varying points of view of many workers, not only in pure surgery but also in all the allied sciences, may have cumulative weight.

It is interesting, in this connection, to revert to the address, to which I have earlier alluded, delivered by Erichsen in 1886. I quoted his general conclusion to the effect that surgery as an art might be said almost to have reached its final limit. He goes on, however, to say—"Art is final, but science is illimitable. We have reached finality in the mere mechanical *Art* of surgery, both as to the expansion of its limits, and the precision of its practice. But we are yet on the threshold of the *Science* of surgery. We have yet to learn much from that compound science, biology, in its application to the elucidation of surgical problems; that compound science of modern creation, into which physiology, and histology, pathology, chemistry, and physics

enter in equal proportions; which works by experimental research. And I venture to think that this is the line of investigation which promises the greatest and most useful results in the evolution of surgery in the immediate future."

He proceeds further—"The old lines of clinical observation and of dead-house pathology have long since been followed to to their final termination. We can but multiply the facts already so carefully observed in every civilised country." "It is not by following these old lines that modern surgery will owe its advance, but it is in the application to it of those means of experimental research which are now being worked out in the biological and pathological laboratory, that we may hope to find the solution of many of those problems that have hitherto baffled the surgeon."

If one might quarrel with these dicta at all, it would be on the score that, important though experimental research may be —indeed, is—clinical observation and deduction would seem to be given, by Erichsen, too little weight. Accurate clinical observation can still teach us very much that even the most elaborate experimental research can never teach, and more especially along the pathway of diagnosis and prognosis—originally mutually inclusive terms. And in many departments of surgery it is particularly because of defective diagnosis and prognosis that advance is limited, that development is held in check.

Erichsen's main contention, however, is true enough, that it is to advance in surgery, the science, that we must chiefly look for such developments, for such widening of the limitations imposed by ignorance, as we may reasonably foreshadow for the future.

To suggest merely a few of these limitations is to suggest a number of problems that may well take up the energy of many workers for a long time to come. I may but mention one or two to-night, but they will suggest to you some of the lines

along which, it seems to me, surgery the science, and surgical workers everywhere, must direct their energies.

The Problem of Repair.

Take the simple question of Repair, whether in skin, bone, nerve, brain tissue, or any other tissue. Do we know even yet what are the conditions that determine it, that respectively encourage or discourage it? We have, as we think, surmounted the limitations imposed on repair by the presence and effects of microbic agents. But the end is not there. Growth *in vitro* may throw light—indeed has done so—on not a few aspects of the problem, but much remains to be done, not only to bring the results of growth *in vitro* into line with actual tissue conditions, but to explain why certain tissue elements admit of more easy culture than others; in what respects growth *in vitro* differs from true growth, and why it can be carried on in a given case just so long as certain factors carried from the parent body into the culture medium remain unexhausted; to demonstrate what are these specific growth factors, what are the growth stimulating substances which are present in the tissues and the body juices, which have the faculty of determining and controlling growth of the tissues. Harrison, Carrel, and others, have made important additions to our knowledge on this subject, but, as Carrel said in his paper before the Section of Pathology and Bacteriology, at the Annual Meeting of the British Medical Association, 23rd July, 1924, "What is wanted is such improvement in the technique of cultivation as will enable tissue cultivation to be carried on not only with the ease with which bacterial cultivation is carried on, but upon a large instead of a microscopic scale."

So far, tissue cultivation has not carried us far, but its importance in showing what are the factors which may influence the healing of wounds of any tissue, and how it may be possible,

ultimately, to provide these very factors in such simple processes as skin-grafting, bone-grafting, nerve suture, and the like, can hardly be over-estimated. Given such an advance in knowledge, it may be possible to do something more than merely allow Nature free play. We may be able to do something to aid, actually to supplement our old friend, *Vis Medicatrix Naturæ.*

Here, surely, is a large field for investigation, successful results in which must mean a further relaxation of the bonds which limit surgical achievement. And it is evident that it calls not only for the collaboration of the surgeon and the physiologist, but also for the help of the histologist and the chemist. Each of these, too, will profit from the data that can be furnished by the bacteriologist, skilled in culture phenomena, as applied to microbic research.

Other problems present themselves, some of which it would seem to be necessary to solve finally before we can hope to arrive at a solution of even the most elementary growth problems. Take the question of repair of nerve, after suture, or after injury. How can we rationally explain nerve repair, and therefore profitably seek the correct or most appropriate line of treatment, until we have settled definitely what element in nerve is the active agent in the process? In spite of all the work that has been done on nerve regeneration, it cannot be said that we have reached any definitely established finding yet. One of the two main schools of teaching on the subject regards regeneration in nerve fibres as a type of unicellular regeneration, in which each nerve fibre represents a thread-like prolongation of the primary nerve cell (neurone). It is, in fact, an essential part of the cell, just as are the protoplasm and nucleus. This school teaches that the part of the divided fibre separated from the central end dies, and can have no further effective part in regeneration, while the proximal end, still in continuity with the central cell, continues

alive, becomes elongated, grows out and ultimately re-establishes union with the periphery, and restores function. In fact, the original cell is itself responsible for the regeneration of the part lost or divided. The other school ascribes some part—more than merely serving as a scaffolding on which the proximal segment may build—in the regenerative process, to cells within the sheath of Schwann, both in the proximal and in the distal segment of the divided nerve. The former school has, I think, at present, more support, and the process, as thus understood, is in line with most that we know otherwise about repair of different tissues. In muscle, for example, repair is almost certainly due—where it is real repair—to the activity of muscular tissue itself; though the fibrillar portion, *i.e.*, the real muscular substance, may almost entirely disappear in the process of repair, regeneration being due, mainly if not completely, to the undifferentiated, or very imperfectly differentiated protoplasmic material, which can be demonstrated apart from the fibrillar portion of the muscles. This undifferentiated portion is the active regenerative part of a muscle, and immediately regeneration begins in it, differentiation begins, ending in the formation of the mature muscle substance—the contractile elements—the striated fibres. And so it is in other tissue, *e.g.*, in bone, where the bone cell must be regarded as the essential cell in repair processes, and the active element around which all the changes determining, and contingent upon, regeneration must be grouped. The "diffuse or interstitial growth" of the embryonic stage of development, as contrasted with the "focal or appositional growth" of the more mature stage—to quote the terminology of Minot and others [13]—does not solve the difficulty of deciding how far we can accept the old idea of the "indifferent cell," or reject the concept of the "essential or special cell."

It seems desirable to try to settle such fundamental questions if we are to arrive soon at a settlement of those larger problems

on which future developments in the science of surgery very greatly depend.

The Necessity for Determination of the Manner in which the Bodily Organs Function.

Take another of those problems which seem to me of great importance in their bearing on surgical development. What, after all, do we know of the manner in which some of the bodily organs function? Take the kidney, for example. It is, obviously, of great importance, in the case of certain projected operations on the kidney—or, for the matter of that, any part of the urinary passages—to know beforehand what is the state of efficiency of the urinary function, or, as we usually express it, what is the renal efficiency. Many tests have been worked out, and put forward, for the estimation of renal efficiency—the cryoscopic test, the electrical resistance test, the methylene blue test, the toxicity test, the phloridzin glycosuria test, the urea concentration test, and so on. Now while, in certain cases, one or other of these tests may afford some light on the outlook as regards renal function, I am not overstating the case when I say that none is yet worthy of much reliance. Indeed, many physiologists, physicians, and surgeons are prepared to reject all, as being none of them of any substantial value—certainly not of such value as to justify reliance where life is at stake. Is it not necessary to get right back to the root problem of urinary secretion itself? Do we have yet any clear idea, any accepted theory, any established proof, as to how exactly the kidney acts, how urine comes to be given off? Is it a secretion or an excretion? What is the present position of physiological teaching regarding the process or processes of elimination of urine? Are we still to accept the old views of Ludwig, Heidenhain, and others—the blood-pressure theory, the pure osmosis theory, and the like—or are we to adopt the

more recent ideas regarding filtration and re-absorption? How far does any one of the theories harmonise with ascertained facts in respect to both physiology and pathology? It seems to me that we shall have to get back to the fundamental facts of kidney physiology before we shall be able to hope for any advance in our knowledge of renal efficiency and of renal efficiency tests.

Regarding this question, too, there is another very important point that is suggested by recent work on "Periodicity as a Fundamental Mode of Action," and Professor Gamble, in his recent presidential address at the Zoological Section of the British Association, dealt with this interesting theme, illustrating it with reference to kidney function. Gamble defined the principle of periodicity as a development of an old idea widely recognised by physiologists, which might be expressed in this form—"That an animal works its organs in shifts." The shift unit for each organ we do not know, but in the higher animals more tissue exists than is required, so that there is a reserve for times of stress. Thus it happens that one kidney may be removed, and the animal still has the capacity of living. The remaining kidney can do the work of two. It is not quite the same thing at all ages, however, for the adult who has lost one kidney is dependent merely on the reserve furnished by the excess which normally permits of the periodic rest phases, and if he loses one kidney the remaining one must lose part, if not all, of its partial activity rest period, and it is hence more susceptible to disease or breakdown; whereas, in the child, the remaining kidney has the power of developing its reserve by forming additional tubules and glomeruli, and it is able ultimately to reach a volume equal to that of the two kidneys. "It is thereby enabled," as Gamble puts it, "to continue its action on the line of partial activity, and to afford each of its functional units their periodic phases of activity and repose."

Apart altogether from the importance of such views on the question of removal of the kidney, it seems to me that such a theory applied to the kidney renders futile much of the work on renal efficiency tests, if for no other reason than that none of these makes allowance for possible periods of functional inactivity, or partial activity; for definite cycles of rest and of activity, which may altogether vitiate any test.

Problems of Abdominal Surgery.

Problems almost innumerable arise constantly in connection with the surgery of the abdomen and the abdominal organs. Take merely the subject of the surgery of the bowel. What do we know, after all, of the factors which determine success or failure in any of the ordinary procedures which concern what one might call the surgical emergencies of the intestine? What, for example, are the relations subsisting between general powers of resistance and local recuperative possibilities in any case of bowel damage; in any case of local infection of the peritoneum; in any case of obstruction, and so on? What is, indeed, the present position as to peritonitis? Can we find some sure way of preventing its occurrence, or of dealing with it effectively when it has developed? Are the findings of von Mikulicz as to the pre-operative administration of nucleic acid, or similar substances, to be the last word in the matter of the therapeutics of peritonitis as a possible complication of abdominal operations? Surely there is room here for much useful work for the pathologist and for the surgeon.

Or take that fortunately rare, but sufficiently terrifying complication of abdominal surgery, acute dilatation of the stomach. The text-books and the records give to-day about as gloomy a story regarding this dread condition as could well be imagined. We do not even know why it occurs, or how

it is produced. We certainly do not know of any line of treatment that affords almost the slightest hope of accomplishing a cure. It occurs, of course, in a variety of conditions, some other than abdominal, but the surgeon meets it mostly, I think, after operations on the appendix, and operations on the stomach itself. Pressure on the duodenum by an abnormal superior mesenteric artery may explain a few cases, or partially explain them; but other cases would seem to have no such explanation, and it is not clear at all whether there is some nervous defect in the muscular arrangements of the stomach itself, or whether the cause is toxic, or is of central origin. I have seen it only twice, and I do not wish to see it again in a hurry. I know of few conditions in which the surgeon feels so helpless as regards treatment. None of the expedients advised seems to be of any value whatever. Until we know something more of its pathogenesis, it must remain, I fear, a nightmare to any surgeon who does much abdominal work, and a substantial limitation in his outlook.

Or, again, what are the limits of excision of bowel, consistent with continued life of the patient? Most of us are confronted, from time to time, with the apparent necessity of excising a large extent of bowel. How much is it safe to excise, not only in respect of immediate rallying power of the patient, but also in respect of the possible continuance of function of the intestinal tract, and the maintenance of the nutritive processes on which life itself is contingent? Six months ago I had to excise just over 90 inches ($7\frac{1}{2}$ feet) of the small bowel, in a woman who had multiple strictures due to active tuberculous ulceration. The patient, who had also gall-stones, and a tumour of the right kidney, survived the operation; indeed, she never looked at all like succumbing to it. She lived to leave hospital, and died about two and a half months later from a tuberculous affection of the lungs. This is by no means the

largest extent of small bowel that has been removed, without producing death. As much as 12½ feet have been excised,[14] and the patient made a good recovery, his health not apparently deteriorating, although analysis of his urine and fæces, after an ordinary diet, showed that his metabolism was distinctly abnormal; there were deficient digestion of meat, fat, and starch, marked indicanuria, and a high urinary ammonia nitrogen, as compared with urea nitrogen. We ought to be able to find out how much small intestine can be removed without disturbance of metabolism. At present there are such widely divergent views as that of Pirie Watson, who considers 6 ft. 7 in. as the maximum[15] and Moynihan's view that 14 ft. 8 in. may be safely removed. It ought to be possible to find out, with the aid of the physiologists, at what point there comes in a degree of metabolic disturbance such as might be prejudicial to life.

These are only a few of the many problems that may be cited in respect of abdominal surgery. There are very many more. The accumulation of facts, the advancement of knowledge, the correlation of observations, clinical and experimental, on these and many kindred subjects, should in time set wider the limits that presently exist.

Fractures.

May I take another subject, viz., that of fractures? Here, again, are numerous problems the settling of which should afford a prospect of advance. Few subjects could be more important to the economic future of the worker and the community. And yet how great are the limitations by which the present-day treatment of fractures is beset. Even with the most modern procedures, even with the great enlargement of the field of operative treatment of fractures, we are still far from having reached what can be regarded as a satisfactory position in respect of early and efficient restoration of function

in these very common, everyday accidents. We do not even yet know exactly how bone grows, how it is reproduced, how union of a fractured bone takes place, what is the active agent, how growth of bone may be encouraged, how retarded. And if we lack in our knowledge of these fundamental facts, is it to be wondered at if we are still unable to regard even functional restoration after fracture of certain of the larger long bones as a matter that can be predicated with certainty or even with considerable hope? Take, for example, those common injuries, fractures of the leg. Clinton Dent,[16] in 1908, discussing the after-results of fractures of the leg, and basing his opinion on his observations among the London police force, made the following somewhat depressing statement * :—

"Recovery, from the point of view of the surgeon and that of the patient, does not always coincide in date. . . . The standard of efficiency in the police is high—the class of men principally under consideration. The work demands fair activity. Thus, for either two periods of four hours, or for one tour of eight hours, a man must be continually on his legs, day after day, though with frequent short spells of leave. Judged by this standard, and desirous of keeping well within the mark, my estimate is that at least 30 per cent fail to attain this standard after simple fracture of both bones of the leg; very few regain it under six months. . . . Fractures of the leg, involving the knee or ankle joints, or fractures of the femur, uniformly lead to permanent unfitness for the work that devolves on these men; fractures occurring in the neighbourhood of these joints nearly always have the same result. *I can hardly recall a case of Pott's fracture, or a Dupuytren's fracture, where recovery has been complete enough to enable a man to resume police work.* (The italics here are

* Arbuthnot Lane quoted Dent's views in his opening statement at the discussion on "The Operative Treatment of Simple Fractures" in the Section of Surgery, Annual Meeting of British Medical Association, 1910.

mine.—A. Y.) . . . My experience with regard to fractures of the leg has led to the expression of rather gloomy, perhaps pessimistic, views." As Arbuthnot Lane said—"If these statements of Mr. Dent are true in the case of men whose duty is merely to patrol the streets, how much more must they be true of men whose lives depend on the perfect accuracy of their ability to climb ladders, stand on heights, &c.? These latter form a very large proportion of our labouring class."

Dent's statement was made sixteen years ago, Lane's comment on it fourteen years ago, and since then much has, no doubt, been done to improve the outlook in such injuries, but mark the following:—

In December of last year (1923), I received a letter from Dr. Charles L. Scudder, of Boston, Mass., one of the first authorities of the day on the subject of fractures and their treatment, in which, referring to my paper, delivered before the Surgical Section, Annual Meeting of the British Medical Association, Glasgow, 1922, dealing with our increasing employment of the open operative treatment of fractures, he said—"I wish you might record, with the painstaking care employed in the above article, the functional results obtained in these cases, and the exact time away from employment necessitated by the injury thus treated." "Have you"—(*and here is the striking question that at first surprised me a good deal*)—"Have you *any* fractures of the femur back at the *same* job as before the accident?—treated either by operation or not?"

The suggestion in this question is quite in keeping with the gloomy outlook of Dent made sixteen years before.

I may say that, following on the reception of this enquiry, I made an investigation into our results, and I am happy to find that we have had better results than would seem to be looked for by Dr. Scudder. As a matter of fact, in our recorded cases of fracture of the femur, over half have been

able to resume their former, or similar, work; in some cases involving such exercises as climbing ladders.

But that such a question should have been seriously asked, such a suggestion seriously made, surely implies that much remains to be achieved in the treatment of these common emergencies; that there are still considerable limitations to advance, which have yet to be overcome.

Tumours and Tumour Growth.

There is much still to be accomplished in the domain of the surgical treatment of tumours, and time will not permit me to do more than mention a few of the problems, the settlement of which would seem to be an almost necessary preliminary to advance. It is quite impossible to discuss the etiology of tumours at any length, or even to outline the main line of theory on this subject, but a few thoughts may be suggested, in respect as they seem to have a bearing on the possibilities of surgical treatment.

First of all, there is the problem of control itself. This may well come under review in connection with the investigation of growth problems generally, to which I have already alluded in an earlier part of this paper, but it has a most important bearing on the outlook of the surgeon who has to deal with tumours. Whatever we think of the causation of tumour growth, it must obviously be of value if we can discover something which may explain, even if only in a measure, what is the influence which disturbs the orderly processes of growth and nutrition of the different tissues, what factor or factors determine the disturbance of the usual orderly regulation of tissue maintenance or increase. We know that there is this growth regulation—*e.g.*, in respect of the normal size of any animal, at least of the higher orders—though it may be lacking in the case of some of the lower animal forms, such as certain

fishes or reptiles, which may continue growing so long as they live. We know, also, that gigantism and dwarfing are dependent, in certain cases, at anyrate, on disturbance of growth regulation that may be traced to increase or deficiency of certain products of the endocrine glands. We know that a tissue like the skin has a sort of reserve power of growth, or at least certain elements of it have, and that, without something to hold it in, these elements may, and do, overgrow. Some influence of an inhibitory character exists in the skin, or is exerted on the elements of the skin, which regulates its growth. We know that, in the lower animals, the power of reproduction of lost parts is considerable. The tail of a salamander may be cut off, yet it will be reproduced; just enough being produced, so that the new tail is of the length that it ought to be. And if the same cutting off process is repeated, the same repair process will follow. It is obvious that the growth power is present, but it is under control, under appropriate regulation. The nature of the control we do not know, but it seems not at all unlikely that, could we discover it, we should be, at least, on the threshold of discovering the nature of tumour growth. Minot, in his lectures on "*The Problem of Age, Growth, and Death*," 1908, puts it this way—"The phenomenon of things escaping from inhibitory control, and overgrowing, is familiar. Such escapes we encounter in tumours, cancers, sarcoma, and various other abnormal forms of growth that occur in the body. They are due to the inherent growth power of cells kept more or less in the young type, which, for some reason, have got beyond the control of the inhibitory force, the regulatory power which ordinarily keeps them in." "No picture of the growth or development of the living animal would be complete if it confined its attention only to the power of growth in relation to cytomorphosis. It must also include the contemplation and study of the regulatory power of the organs." "Nothing, perhaps

is more to be desired at the present time than that we should solve the mystery of the regulatory power which presides over growth." "Could we understand it, and could we, from our understanding, derive some practical application of our scientific discoveries in this field—in other words, could we learn to regulate the formation and growth of tumours—we should say of it justly that it was as noteworthy a contribution to medical knowledge as the discovery of the germs of disease, and it would doubtless prove equally beneficial to mankind."

This seems to me a fundamental matter in the great question of the nature of tumour growth, and it must needs be fundamental also in respect of the prospects of surgical treatment. For, think of some of the problems with which the surgeon is confronted. Not only is there the primary growth to consider, there is also the matter of secondary growth, the explanation still to be sought of the dissemination of the growth. What do we know at present as to the factors which determine the spread of the tumour locally? What can we predicate as to its extension to other sites, whether by contiguity of tissue, or by lymphatics, or by the blood-stream? Even in those tumours which readily form metastases, what do we know yet as to the period in their growth when the first metastasis takes place? Take the case of sarcoma of the long bones. Look at the depressing statistics as to the probabilities of life in these, whether operated on early or late. We know now that almost no patient who has had a sarcoma of a long bone has a prospect of life of more than one or two years. How is this to be explained? What is the surgeon to do in the face of such a depressing probability?

Or take the even more fundamental question as to the therapeutics of cancer, and as to the method in which cancer leads to death. What, after all, do we know of this?

About a year ago I had a conversation with Sir James Mackenzie on this subject, and he floored me with a whole

string of questions such as these—"How does cancer kill a man?" "Why do our cancer patients die?" "What is it that kills them?" "Is it the cancer?" "If not, what is it?" These questions I found unanswerable then; I find them so still. But when one seriously considers questions such as these, they do not appear so strange as they at first seemed to me. They go to the very root of things, and they serve to show how ignorant we are of some of the most fundamental, the very basic, problems of life itself.

X-RAYS, RADIUM, IRRADIATION.

May I make a very brief reference to another problem that perplexes me not a little. It concerns the matter of the effects of the *x*-rays, radium and radio-active substances—indeed, of irradiation effects generally. Much has been written regarding the selective effects of irradiation, and there are the two apparently contradictory phenomena of the same or similar rays having the capacity both of exerting a remedial action and of themselves producing, or encouraging the production of, malignant growth. Can these two effects be reconciled, or are they capable of being explained? If they can be explained, what bearing has the explanation on their use as therapeutic agents in the treatment of malignant growths? Has our knowledge, fragmentary and unsatisfactory as it is, any bearing on our conception of the etiology of malignancy? Professor Lazarus-Barlow a good many years ago suggested, and he has quite recently put the suggestion forward again with some force, that radio-action may be a factor of some importance in the development of cancer. Sir George T. Beatson, in an interesting letter in the *British Medical Journal*, 26th April of this year (1924), drew attention to the fact that the suggestion was made even as far back as 1909 by Watkins-Pitchford, in a paper read at the South African Medical

Congress, Durban, August, 1909, that the initial cause of cancer is "usually some irradiation." And he (W.-P.), went on to say that "cancer is in essence a failure on the part of the more independent cells of the body to inherit the normal tendencies which control their multiplication"; he held that "this 'psychic' defect is usually the result of an actinic 'illumination' in excess of that which can be dealt with by the protective agencies of habit, posture, coverings, and external pigmentation." Sir George Beatson pointed out that Lazarus-Barlow did not attempt to explain how radio-action works in the production or encouragement of cancer growth, and himself put forward the view that, if there is anything in the theory at all, the influence may be found in the nuclear pigment; that cancer may have a pigmentary origin traceable back to the potential energy of the nuclear chromatin. It is an interesting speculation, and its further exploration may yet help in the conquest of cancer. It is, in any case, all the more interesting in view of the suggestion I have already quoted from Minot as regards the mystery of "the regulatory power which presides over growth." Just as the physicists foreshadow to us to-day the release, and perhaps the beneficent control, of the components of the atom, may we not even look forward to a future in which it may be possible for the "potential energy of the nuclear chromatin" to be brought under control, by actinic, or radio-active influences, and directed towards the therapeusis of growth?

The Limits of Relief in Brain Surgery.

In the first part of my address I referred at some length to the development and advance of the surgery of the brain and central nervous system. I did so chiefly as it constituted a useful type. May I now make a more brief reference to the same subject, from the point of view of the present and future of surgical science?

Great advances have certainly been made in the matter of cerebral localisation, and great advance, also, in operative technique. Many of the results of such advance have been brilliant almost beyond the dreams of the earlier surgeons. But it must at once be admitted that, even with all that can be said that is commendatory and that is hopeful, achievement falls still far short of perfection.

One need not elaborate here the great amount of work that must be done even yet on the subject of localisation. In spite of the great advance in our knowledge of cerebral localisation and function, made by the labours of many workers in many lands, how defective still is our diagnosis in many cases of brain tumour. Not all of us, I fancy—indeed, probably few of us—are prepared to take the roseate view of Gordon Holmes, as to the proportion of correctly, or approximately correctly, diagnosed cerebral tumours. There is still a big hiatus between what one might regard as the present day possible and what one may hope for as the ideal achievement of the future. Till we are able to arrive at a much more certain and reliable diagnosis and prognosis than we are capable of at present in cases of cerebral tumour we cannot look for the improvement in our results that our improved operative technique might lead us to hope for. The limitations of our still very imperfect diagnosis and prognosis in brain lesions are only too evident to the mind of any surgeon who is sincere, and whose interests lie in this line of work.

But, even granting an accurate or fairly accurate localisation of a cerebral tumour, and granting an operative technique of moderate competence, what, after all, are we able to accomplish to-day, in at least the majority of cases? Let me take, for illustration, three fairly recent cases that have fallen to me to deal with.

Case I.—A. H., a woman, aged 47 years, who, in the course

of five months, gradually developed a paralysis of the left upper extremity, and, later, of the left lower extremity, with, in the later weeks, gradually increasing headache and vomiting, and some giddiness. There were gradual blurring of vision, at times diplopia, progressive dulling of cerebration, inco-ordination, and, latterly, facial paresis. Optic neuritis was pronounced when the patient came under my care, and there was also nystagmus on movements of the eyes to the right. The other features of the clinical picture I need not trouble you with.

Operation (on 10th June last).—Novocain local anæsthesia. The Rolandic area on the right side was exposed freely by a large osteoplastic flap, which was turned down and afterwards replaced. In the neighbourhood of the upper third of the motor area, in a similar portion of the sensory area, and just above the posterior end of the Sylvian fissure, the cerebral cortex was much firmer in consistency, and presented a puckered appearance. On the surface of these areas were scattered small yellowish-brown nodules of the size of a pin's head. Portions of the second and third of these areas mentioned were removed for pathological examination, the blood-vessels ramifying over them being secured by catgut ligatures, and bleeding from the exposed tissue beneath being checked by passing deep sutures of catgut below it. After loose suture of the dural flap, the osteoplastic flap was replaced. and the wound was closed without drainage. It will be observed that no bone was removed, and that the osteoplastic flap was not secured except by skin suture.

Immediately following upon operation a great improvement in the patient's condition took place. Power had returned in the face, hand, fore-arm, and leg by next day, and this improvement developed for a few days, during which it was noticed that the scalp and bone flap bulged considerably. The condition of the fundus oculi also improved, so that, on the sixth day after operation, Dr. Ballantyne reported as follows:—

"The papillœdema of both eyes is not so pronounced as previously. . . . There is no disturbance of the visual fields." The patient began to take more interest in things around her, and, in particular, was much interested in the returned power of her limbs. We knew, however, that the improvement could be only short-lived, and so it was. Death took place about two months later, power having gone again entirely in the affected limbs for some weeks before death.

Sections of the portions of tissue removed at operation showed that they were merely outcrops of a certainly extensive, deep-seated tumour of malignant type, and the temporary relief was due only to the decompressive effect of the operation—a decompressive operation, however, in which the bone of the osteoplastic flap was left, and acted as a protective shield.

The pathological report by Dr. Blacklock showed that the tumour was "a papillomatous type of carcinoma, probably originating from the choroid plexus" (Fig. 15).

No autopsy was obtained.

Case II.—J. G., a man, aged 33 years, was admitted to a medical ward on 17th March of this year, and was under observation there for a month. His symptoms of a cerebral nature began about nine months before, with a gradual loss of the power of concentration on his work, and in the later weeks before admission he had twitching of the left side of the face, alteration in his speech, headache, giddiness, and vomiting, diplopia for about nine days before admission, and a tendency to fall to the left. There developed later, marked ptosis of the right eye, palsy of the right external rectus, and partial palsy of the third nerve. There was a left facial palsy, and a well-marked optic neuritis, worse on the right side. While in the medical ward, patient gradually became more and more apathetic and drowsy, lost flesh rapidly, and became quite

incontinent of urine and fæces. He was practically comatose when transferred to my ward for operation.

Operation (on 18th April, 1924, *i.e.*, a month after admission to hospital).—Chloroform anæsthesia. A large flap of scalp, including everything down to temporal fascia, was turned down on the right side of the skull. The bone was denuded of temporal fascia, muscle, and pericranium, and a trephine was used to remove a large disc of bone, after which the opening

FIG. 15.

Papillomatous type of carcinoma. Section from Case I (× 60 diam.)

in the bone was enlarged rapidly, nearly to the size of the scalp flap, with stout bone-cutting forceps. The bone was found to be remarkably thin. There was at once evidence of greatly increased intracranial pressure, the dura being tense, and brain pulsation being almost absent. On opening the dura, the brain bulged at once very considerably, and pulsation returned. The convolutions were flattened. No tumour was visible externally, but the existence of a probably large central tumour

was suggested by reason of the great bulging, and by reason of the absence of any increase in the fluid in the lateral ventricle, which was tapped in several places. The under aspect of the brain was explored by raising the temporo-sphenoidal lobe—with a negative result. The operation was completed, therefore, as a simple decompression operation, the upper dural flap being sutured to the temporal muscle over the bulging brain. The scalp wound was closed without drainage.

Following upon operation, though patient developed a bi-temporal hemianopsia, and a transient left hemiplegia (partial)—the result evidently of the pressure of the brain on the edges of the skull opening—a very great improvement in his general condition took place. He became much brighter, recovered the power of moving his right eye, lost his ptosis entirely, recovered control of urine and fæces—though this control was not constant—and in a few weeks was allowed up on a couch now and then. Dr. Ballantyne examined his fundi for the second time, about five weeks after the operation (he had reported on them prior to operation, when optic neuritis was very marked), and he reported as follows:—"Swelling of discs practically disappeared, except in the nasal part of the disc on the right side. There is no nerve atrophy." Here, again, however, the improvement was only transient, though it lasted longer than in the other case. The bone having been removed over the trephined area, and not replaced, there took place a steady and progressive increase in the size of the hernia cerebri, so that, ultimately, there had developed a very large tumour forming a striking deformity which it was not easy to camouflage. You see it in the slide I am able to throw on the screen, and in the colour drawing of which the slide is a reproduction (Fig. 16). It may be said that the man had his life prolonged, probably, by about three and a half months, and he was freed from the severe headaches, the vomiting, and the

giddiness. His ptosis and his ophthalmoplegia were cured, and he recovered in great measure his control over bladder and bowels. But these were the limits of his relief. Some might argue that they were hardly worth while. One can only suggest that, unless we build even on such partial and not very effective results, we cannot hope to rise to higher degrees of remedial achievement. It is impossible to avoid the admission

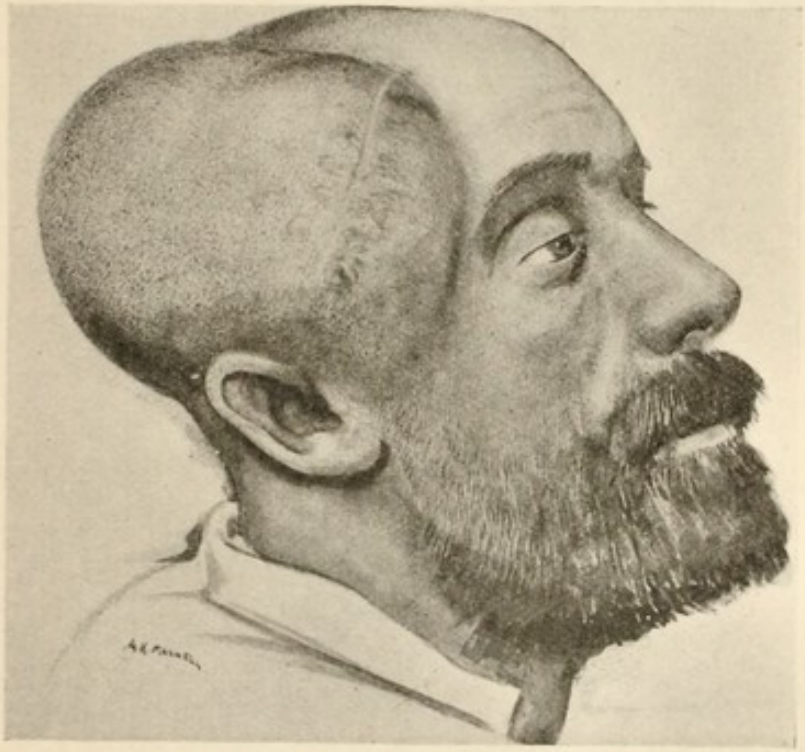

FIG. 16.

Decompressive trephining—drawing two and a half months after operation. (Case II.)

that no very great merit can be claimed for such a result, if, indeed, it is to be the utmost limit of accomplishment that may be reasonably foreshadowed for the future. It should be stated that the nature of the tumour could not be ascertained in this case. A *post-mortem* was refused. It is certain, however, that it was a malignant tumour of central site, and probably of indefinite, uncontrolled spread.

Case III.—J. E., a married woman, aged 43 years, was transferred to my ward, from the ward of Professor Stockman, on 5th August, 1922, with the following history (in brief):— From as far back as December, 1917, she had had, at intervals, some twitching of the left angle of the mouth and face. In February, 1918, Professor Stockman had her under observation, for two weeks, in his ward, but during that period no

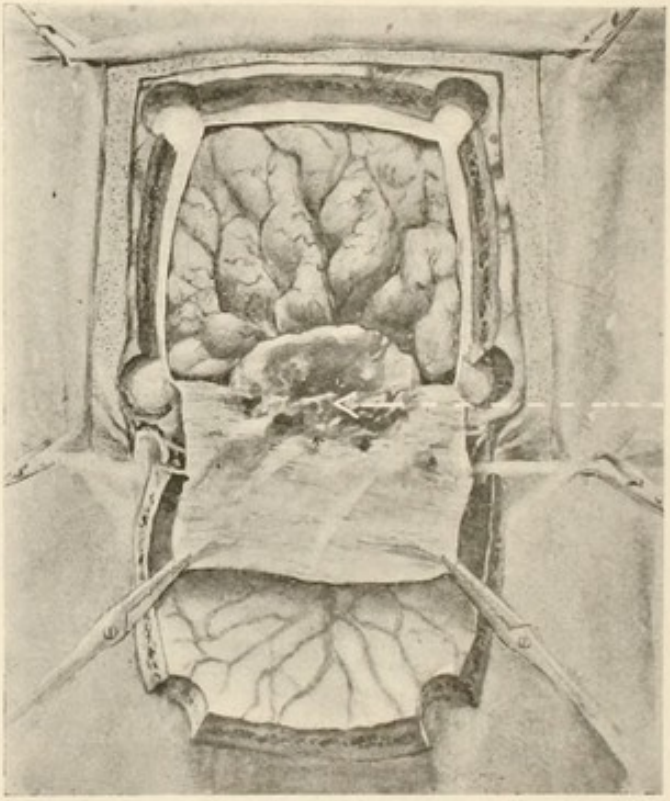

Fig. 17.

Osteoplastic trephining—Case III. Note the position of tumour.

twitchings occurred, and she was dismissed accordingly. Minor attacks of a Jacksonian order, however, continued to occur at intervals, up to the early summer of 1922, when the attacks began, for the first time, to take on occasionally, the character of *Grand Mal*, sometimes ending in a temporary loss of consciousness. Until about this period there was never any proper *Aura*, but now she began to notice that, just before a fit

came on, she had some sort of curious sensation in her stomach, or a curious feeling in the left forefinger and thumb. There was noticed now a slight, but definite, weakness of the left side

FIG. 18.

The tumour removed—Case III. Appearance from three aspects.

of the tongue. The right pupil was a little smaller than the left. Both pupils reacted to light. Dr. Manson, acting on

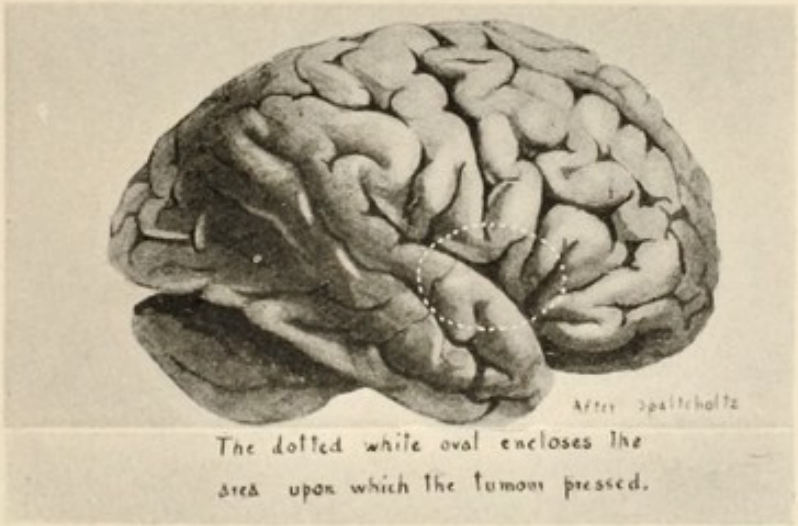

FIG. 19.

To indicate position of tumour in relation to the cerebral cortex (Case III).

behalf of Dr. Maitland Ramsay, found that there was some congestion of both optic discs—but only slight; no actual "choked disc."

During the ten days that patient remained in my ward, prior to operation, she continued to have frequent attacks of twitching of left side of face, left side of tongue, and left hand, especially of thumb and forefinger, preceded by the *Aura* described. Some of the attacks ended in general convulsions of moderate degree, with short periods of coma.

Operation (on 16th August, 1922, *i.e.*, eleven days after transfer to my ward, or over four and a half years from the first onset

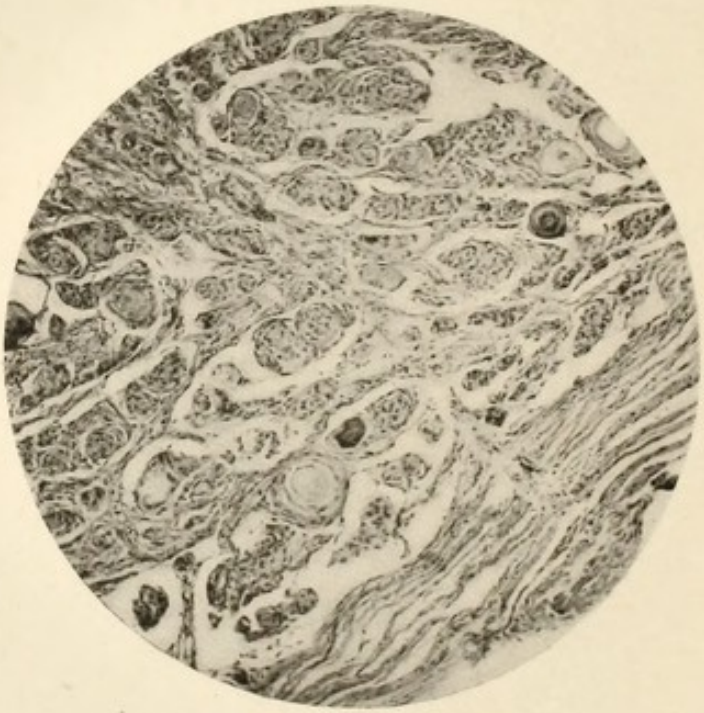

FIG. 20.

Microscopic appearance of tumour—a psammoma—Case III (low power).

of symptoms—though, as has been stated, these early symptoms were very slight).—Osteoplastic trephining. Right Rolandic area exposed. Large tumour exposed below, adherent to the dura, and apparently arising from it, and pressing upon the upper and outer aspect of the right tempero-sphenoidal lobe, and so to facial area. The middle meningeal artery had to be crushed in its canal to check bleeding, and one pial vessel was ligated. The tumour was readily removed, with part of the

dura adherent to it. The dural flap was partly closed, a drain being passed down into the middle fossa and brought out through the lower posterior trephine opening. The osteoplastic flap was replaced, and the wound of the scalp closed, except at the place of exit of the drain.

The drain was removed in a few days, and normal healing took place. The patient made an uninterrupted recovery, so far as the immediate effects of the operation were concerned.

FIG. 21.

Microscopic appearance of tumour—a psammoma—Case III (high power).

You will see on the slide, and on the original colour drawing, the method of procedure followed in turning down the osteoplastic flap (Fig. 17), and, in succeeding slides and drawings, the character and shape of the tumour removed (Fig. 18); also, on separate plate, I have indicated as nearly as possible the position of the tumour with reference to the cerebral cortex (Fig. 19). The tumour weighed several ounces, and measured, roughly, 5 cm. by 4·5 cm. by 2·5 cm. Examined histologically,

it proved to be, as expected, a psammoma (Figs. 20 and 21). I show you here slides prepared from a section of the tumour. The appearances are fairly characteristic. It probably took origin from arachnoid fringes along the spheno-parietal sinus.

I have said that recovery was uninterrupted, and so, in one sense, it was. I have showed the patient previously at a Medical Society meeting. I believe she is still quite well, so far as her general health goes, and has had no return of fits or other cerebral manifestation; but—and here is the point of interest—operation has not prevented the development and advance of her optic neuritis. Her sight has failed considerably, and when last examined, she was reported as having a definite degree of optic neuritis, going on to atrophy.

I have chosen these three cases, not as representing even a fair average in the surgery of brain tumours, but really because they indicate, in my opinion, how far, even now, we fall short of satisfaction of the great hopes that must have arisen in the minds of those who have lived almost in the very midst of the great advance in knowledge of cerebral localisation, and who hoped to see it taken advantage of, and brought to full and beneficent fruition, in virtue of the great freeing of the bonds which shackled opportunity before the principles of antiseptic surgery made us free. I might have painted a far more depressing picture, had I chosen instead to recount a succession of undiagnosed, or wrongly diagnosed cases of cerebral nature, such as—if we are to be honest—we could all disclose.

Even the best of these three, you see, is left with optic atrophy and failing sight. The others were relieved only for a short time, and were not, even for that time, made useful members of society.

Ladies and gentlemen, I have finished, and it is time, and more than time. I have not, perhaps, given you a very bright

picture of the future of surgery. I have not attempted, at anyrate, to paint a brighter picture than I feel is warranted by the facts. I hope I have, at least, done something to justify my choice of title—"The Limitations of Surgery, Past and Present."

It may seem strange—passing strange, perhaps—that one occupying the position which—very humbly, I hope—I have the honour to occupy, should dwell as I have done, not on the brilliant achievements of his science, but rather on the checks, the hindrances, the limitations with which it is set about.

My excuse is not that I take a sombre view of the future, but rather that I believe it is only by realising, and admitting—a far more important thing—our ignorance in many directions, that we shall be able to take even the first step towards the fuller knowledge with which we must be armed ere we can overcome the many difficulties ahead; ere we can solve the many problems that are pressing for solution.

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