ABSTRACT OF LECTURES ON THE STRUCTURE AND FUNCTIONS OF THE VERTEBRATE SKELETON.

Delivered at the Royal College of Surgeons of England.

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LECTURE I.

In studying the vertebrata, the first thing that suggests itself is the low origin from which they have come, and the great metamorphic changes they have undergone, till at last man is reached. To ascertain their connection with the invertebrata, from which they undoubtedly spring, we must not compare the higher invertebrates with the lowest vertebrates, as the former are specialised forms; but we must go down in the series, as far as the vermes, before we can find any archæological type from which the latter can be derived. At this point, it is probable, although the distinct chain has not yet been made out, that the vertebrates branch off from the lower or invertebrate kingdom. The differences between the vermes and the lowest vertebrata depend apparently upon a certain degree of specialisation having taken place in the present existing forms with which we are acquainted. Before we can define what a vertebrate is, it is necessary to ascertain the lowest type-form from which the others have sprung. This we find to be an animal possessing a head and body, a continuous nervous system on its dorsal aspect, and a hæmal system and mouth on its ventral—hemastomatous. In the vermes, the nervous system is broken up into segments, the mouth is situated on the neural aspect of the body—neurostomatous; and the head is segmented. The animals, of all others now existing, which approach nearest this type-form are the amphioxus and lamprey; but they are further apart than even the latter is from man. In the lamprey, there is a solid cartilaginous skull. Tracing the earliest development of the vertebrata and invertebrata, we find that the embryo of the one is undistinguishable from that of the other till the two primitive layers—the epiblast and the hypoblast—are formed; but immediately after that the two groups diverge in different directions—the one becoming developed into a vertebrate, the other into an invertebrate. The vertebrate develops two tubes—the one the neural, the other the ventral, tube; while the invertebrate has only one. The first great specialisations in the embryo of existing vertebrates are in the development of the three brain vesicles, and in that the *continuous* skull is a structure prior in time to the *segmented* spinal column. The latter is therefore a later formation, and is modified so as to suit the spinal part of the neural axis. The skull can no more be said to be a modified spinal column than the spinal column is a modified skull. The skull in the invertebrates is only a modified form of the body-segments; as, for example, in the arthropoda we find that the eyes are placed upon two limbs, and all the appendages of their head are rudimentary limbs. In the primordial vertebrate, the head occupied a much greater proportion of the creature than it does in the existing forms, and passed into the body. The neck is comparatively a recently acquired part. That the head was of great length in primordial form is evident from the length and size of the respiratory pharynx, which, in the lamprey, has seven pairs of openings or clefts, two directly behind the mouth being suppressed, which would have made nine. The seven are supplied by branches of the vagus, which fork again and again over the clefts. After the three primary divisions of the brain are formed, the skull begins to form under the vesicles, at first in membrane, which soon becomes transformed into cartilage. The spinal cord and brain are developed from the outer layer of the blastoderm by a groove forming along its dorsal aspect; this gradually closes over till a complete tube is formed. From the inner layer or hypoblast, the ventral cavity is formed. This is covered in by soft parts, which are developed from a layer of cells which form between the two layers. Under the epiblast, even before it has become formed into a tube, there appears a condensed structure, extending the whole length of the body, called the notochord; and, on each side of this cord, the mesoblast or middle layer breaks up into the primitive segments or muscle-plates. These are not the rudiments of the vertebræ, but of the axial muscles; the vertebræ appear afterwards between these segments, and are due to a secondary segmentation.

MR SPENCE ON SURGICAL STATISTICS.

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[Concluded from page 239 of last number.]

My former communication on this subject broke off in the course of a reply to Mr. Spence's inquiry, "What are Mr. Lister's results in excisions of the mamma?" In order to complete the answer, I have to add that my other fatal case was one in which strict antiseptic treatment had not a fair chance. The patient, sixty-two years of age, had suffered from sinuses of the mamma for forty years; and at length scirrhus had affected the gland and invaded the axilla. I made an attempt to get rid of the putrid element while removing the cancer, but in this I was foiled. Putrefaction took place in the wound, attended with sloughing; and she died exhausted three days after the operation.

Mr. Spence refers with something of a taunt to the frequency with which the presence of putrid sinuses in articular disease must render strict antiseptic treatment inapplicable. Such cases are, indeed, too common; but I would venture to say in all seriousness to Mr. Spence, that for this unfortunate fact teachers like himself are, to a great extent, responsible. How often have I had occasion to lament the irreparable mischief done by some well-meaning practitioner, who, acting in accordance with the teaching of his school, has plunged a knife, without any special precautions, into an abscess connected with a joint, perhaps within a day of sending the patient into hospital!

But when it shall come to be universally recognised in the profession that so to deal with such a case, though in accordance with the time-honoured maxims of our art, is grievously to compromise the best interests of our patients, and when, through the profession, it shall have become impressed upon the public mind, that to allow such an abscess to break of itself is a thing by all means to be avoided, as likely to sacrifice articular usefulness, limb, or perhaps even life, then will putrid sinuses about joints become rare, and the benefits of strict antiseptic surgery will be correspondingly extended.

In some of these cases, however, we have the satisfaction of undoing the mischief done by the septic surgeon—for such may be justly termed the man who needlessly introduces septic ferments into the sacred fabric of the human body. By carefully purifying the epidermis with carbolic acid, and applying chloride of zinc to the wound and sinuses after scraping away all granulations with the sharp spoon, as advised by Volkmann, we sometimes succeed in eradicating the pre-existing putrefaction, and see the wound pursue a typically aseptic course. It so happens that I have two examples of this success among my patients in King's College Hospital at present, one an excision of the wrist for caries in a young man, the other necrosis of the femur in the vicinity of the knee-joint in a youth. But even though we fail in this attempt—as is certainly my experience in a very large proportion of such cases—we gain from the use of the chloride of zinc the incalculable advantage of avoiding putrefaction during the first two or three days, and thus tiding the patient over the critical period of greatest irritability of the wound and greatest proneness to septic absorption.

But, in spite of the too great frequency of putrid sinuses, the great majority of our operations happily admit of the application of the strict antiseptic principle; that is to say, in the great majority we find present the two indispensable conditions, an unbroken skin, and the seat of operation sufficiently removed from one of the natural outlets of the body to afford space for an effectual antiseptic dressing. And thus, out of my 725 major operations during the period referred to, 553, or ten-thirteenths, were conducted strictly antiseptically. And it is important to bear in mind that the strictly antiseptic operations, besides being far the most numerous, included those which without antiseptic measures would have been the most liable to disaster, such as those for ununited fracture treated by cutting down upon the fragments, removing their extremities, and in most instances drilling and tying them together with wire. Without antiseptic treatment, the risk of pyæmia was so great after operations of this kind in the lower limb, that prudent surgeons regarded them as inadmissible; but, relying on antiseptic measures, I did not hesitate to operate upon all such cases that presented themselves; and it speaks strongly in favour of our principle that, out of twenty-six such operations, including eight in the thigh and nine in the leg, not one was followed by a fatal result. Some of them were very difficult, from the situation of the fracture and the position of the fragments; and Mr. Spence has been correctly informed to the effect that in some instances the operation was repeated in the same patient.

In answer to his inquiry as to the results in the thigh, I may say that solid osseous union was ultimately attained in all the cases except two. One, a fracture of the neck of the femur in a woman beyond the middle period of life, where fibrous union only was obtained. The patient, however, recovered the power of walking without crutches, and entirely lost the pain in sitting or turning in bed, which was the circumstance that induced me to operate. The other failure occurred in a fracture close to the knee-joint in a middle-aged man. Putrefaction took place in consequence, as it appeared, of inadequacy of the window left for antiseptic dressings; and, as soon as this was ascertained, I thought it best to amputate. With reference to the occurrence of putrefaction in this last case, it would certainly be a very serious thing if we could not reckon on being secure against such an event. I may, therefore, add, as affording some explanation, that this case occurred at an early period, when we were comparatively inexperienced in the use of our present means.

I have felt it my duty to reply to Mr. Spence's observations, so far as they can be conceived to have any bearing whatsoever upon the value of the strict antiseptic principle; but, while I have been thus led to refer again to my results after amputation, I am well aware that they *prove* nothing in favour of the system which I advocate. It would be fairly open to any one to contend that the success which I have described may have been as purely a piece of good luck as Mr. Cheyne, in the last number of the JOURNAL, has shown to have been the case with Mr. Spence's selected specimen. And even with regard to the operation for false joint, involving the conversion of simple fractures into compound ones, and universally allowed to be attended with considerable risk of pyæmia, especially in the lower limb, although I have said that the occurrence of such a considerable number without any fatal consequences speaks strongly on behalf of the principle which guided the operations, yet I know well that even a much longer unbroken series of successes with this or any other recognised surgical procedure would only afford probability, not proof, of the value of the principle.

It is this lack of demonstrative force in surgical statistics which has always made me regard them with little favour, and kept me from producing them until I found it repeatedly hinted, in published statements, that my silence implied some general failure of antiseptic treatment in my hands;* while I unexpectedly found the necessary materials in Mr. Cheyne's laborious analysis of my hospital case-books.

With regard to the general question of blood-poisoning, indeed, I have published on more than one occasion, because the facts seemed of such a character as hardly to admit of explanation on the hypothesis of mere good fortune. I described (*Lancet*, January 1st and 8th, 1870) how my wards in the Glasgow Royal Infirmary, which had for years been peculiarly liable to the three hospital pests—pyæmia, erysipelas, and hospital gangrene—entirely lost their unhealthy character coincidently with the adoption of strict antiseptic treatment, and remained "models of healthiness" during the three years in which I continued to conduct it there. This striking contrast between my own experience in the same institution, in the antiseptic and non-antiseptic periods, seemed to afford something very closely approaching demonstration of the efficacy of strict antiseptic treatment against hospital insalubrity. And further, as referred to in the former part of this communication, I described a similar almost entire immunity from hospital disease during five years in my Edinburgh wards, though under very unfavourable general hygienic conditions. I have now to thank Mr. Spence for the good service he has done by inviting special attention to his own results. For, from a careful scrutiny of the statistical tables to which Mr. Spence appeals, as well as from Dr. Braidwood's work on pyæmia, Mr. Cheyne has obtained evidence that, in the very same institution, though with much more favourable surroundings, modes of dressing which were not guided by the strict antiseptic principle, though in other respects employed under the best auspices, were powerless to prevent frequent pyæmia, not to speak of other forms of blood-poisoning.

But that which I have always felt to be especially deserving of publication is evidence of the occurrence of new pathological phenomena, implying the operation of a fundamentally new principle of treatment. Such is the organisation of the blood-clot in an exposed wound. I need hardly remark that there is nothing new in the fact of the organisation of blood-clot. It occurs not only in the thrombus within an artery, to which Mr. Spence refers, but in every coagulum that forms in a simple fracture or other subcutaneous injury, and in every case of healing by scabbing under a dry dressing placed over a bleeding open wound. The novelty consists in its occurrence under new circumstances, viz., under a moist dressing in an open wound; so that it lies open to our

inspection and study, while it forms an incomparably superior substitute for the old process of putrefaction of the clot, with its attendant inflammation and fever, and subsequent healing by granulation. If Mr. Spence had ever witnessed a typical example of this; had he seen a blood-clot, occupying an open wound to the level of the skin, remain from day to day unaltered in appearance till in due time it showed evidence of its vascularisation, in bleeding when scratched with a knife; or had he, at a still later period, on raising a superficial layer of the coagulum, found beneath it a cicatrix, formed without any suppuration or granulation, he would, I doubt not, have recognised a new pathological phenomenon, implying the operation of a new principle, viz., that of the total exclusion of septic ferments, together with the avoidance, as much as may be, of the irritating influence of the agent by which their access is prevented.

Another instance of new phenomena is presented by the behaviour of a joint, healthy in its textures, treated by antiseptic incision and drainage. A wound of a healthy joint used to be a very grave occurrence, though, under careful management, including complete rest and apposition of the injured surfaces, recovery might take place without suppuration. But, supposing that a wound in a previously healthy articulation, instead of being closed, were kept systematically open by means of an India-rubber drainage-tube, having, when a large joint like the knee is concerned, a thickness about equal to that of the little finger, this drainage-tube being taken out every day, washed and replaced, while the wound was kept covered with a moist dressing, there would, if I mistake not, infallibly occur suppuration of the articular cavity, unless the process were conducted in some thoroughly efficient antiseptic fashion. Whether this is done by means of carbolic spray and gauze, or by other antiseptic agents employed under other forms, but in such a way as really to prevent putrefaction, is of course indifferent; only I would remark, that a man ought to be very sure of the trustworthiness of his means, whatever they be, before he ventures upon such procedures. When we see a wound in a healthy joint treated antiseptically in the manner I have described remain not only without suppuration, but free from uneasiness, tenderness, puffiness, or inflammatory blush, while the general health is unaffected, here again we recognise a new train of pathological phenomena, resulting from the application of the principle of the total exclusion of septic ferments—combined, in this case, with such effectual drainage as prevents the chance of irritation of the joint through distension with blood or plasma.

And when Mr. Cheyne published, some weeks ago (see *BRITISH MEDICAL JOURNAL*, November 29th, 1879), a statistical table giving the details of all the cases, twenty in number, in which I had opened healthy joints in operations during the period to which the case-books at his disposal referred, the treatment being by antiseptic drainage, and was able to state that in no single instance did suppuration of the articulation occur, he produced what I think will be generally regarded as a valuable piece of evidence of the trustworthiness of the means now at our disposal for carrying out the strict antiseptic principle.

How is it, I may fairly ask, that the record of facts like these produces no impression whatsoever on Mr. Spence? It must surely be either because he regards statements which proceed from me as undeserving of his notice, or else that he distrusts my word. Probably, it is from a combination of these two causes. For, in the communication to which I have felt it my painful duty to reply, he distinctly charges me with exaggeration, and hints that I give "false facts". This charge is a very serious one. I am not aware that I ever wrote anything that I did not believe to be strictly true. But if my character for truthfulness is "filched from me", there must be at once an end to any hopes I might otherwise have of usefulness in my day and generation. On what, then, is this grave charge founded? It is absolutely and utterly baseless; and I confess it fills me with astonishment that Mr. Spence has not by this time seen it to be so, but that he reiterates it in the last number of the JOURNAL. I am accused of exaggeration in stating that "I have published numerous cases to show that a great abscess connected with disease of the vertebræ may be opened by free incision, a drainage-tube introduced, strict antiseptic treatment being used, and that from that hour there is not another drop of pus"; and the proof of my exaggeration is supposed to be afforded by the fact that some cases of vertebral abscess remained in my wards in the Royal Infirmary, still uncured and discharging, when I ceased to be connected with that institution. But can Mr. Spence really imagine that, when I stated that in cases of this kind not another drop of pus might be formed after the evacuation of the original contents of the abscess, I intended to convey the impression that there was no further discharge of any kind? Even if my statement at the meeting at St. Thomas's Hospital had been my first utterance on the subject, I could hardly have been so misinterpreted, seeing that the mention of the use of a drainage-tube makes it surely abundantly plain that

* Thus Mr. Savory, in his address at Cork last August, quoted with approbation the following passage from Mr. Bryant. "The publication of isolated cases, however good, proves nothing, whereas the withholding of the whole suggests much."

discharge of some kind was to be provided for. But it is now long since I first brought this matter before the profession, and from the first I have stated that a serous oozing always occurs, profuse and tinged with blood in the first instance, and afterwards diminishing, but almost invariably continuing in small amount for a protracted period. Mr. Spence's charge would have had exactly the same basis as it has, if every case of psoas abscess were cured within a week under antiseptic treatment, provided that during that week, or even during two hours, a serous discharge occurred. My statement had reference to the great new pathological truth of the cessation of *suppuration* from the pyogenic membrane and carious bone when a vertebral abscess is opened and treated antiseptically in combination with free drainage, while rest is given to the weak part of the spine by the recumbent posture. Under these circumstances, the chronically inflamed osseous tissue has opportunity afforded it to recruit. How long this process of repair may require for its completion varies greatly in different instances, and here and there we meet with a case in which, in consequence of the presence of pieces of dead bone too large to make their way to the surface, complete cure appears impossible. Happily, however, as experience has now amply proved, the great majority of these formerly hopeless cases, highly interesting to the poor creatures affected and to their relatives, though "not" yet "very interesting" to Mr. Spence, admit of complete cure, if patiently treated to the end with strict antiseptic care and rest.

And here again I have to thank Mr. Spence for having, by his reference to my cases left in the Infirmary, brought out incidentally the fact—I had almost said the glorious fact—recorded by Dr. Bishop in the number of the JOURNAL for the 31st ult., that under his good care almost all these types of hopelessness, as they might naturally be supposed to have been from the way in which they are alluded to by Mr. Spence, have been restored to health and usefulness.

EXTROPHY OF THE BLADDER.

By JOHN WOOD, F.R.C.S., F.R.S.,

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THE proverbial troubles of inventors and improvers, like those of parentage, are by no means confined to the throes which attend the birth and delivery. The result must be protected and looked after, sometimes for an indefinite period, and even when it might reasonably be thought to be strong enough to assert its own rights.

The occasion on which I venture to inflict upon the readers of the JOURNAL this somewhat trite moral is in reference to a paper published in the JOURNAL of February 7th by Mr. J. Greig Smith, of the Bristol Royal Infirmary, on "Two Cases of Successful Operation for Extrophy of the Bladder by a New Method". This paper has afforded me the satisfaction of finding that my former efforts to relieve this deplorable deformity have not been without the result of directing others in what I believe to be the right way of relieving it. In his remarks upon these two interesting cases, Mr. Smith apologetically states that "he has made free use of several methods of operating, rather than invented an entirely new plan". Almost all improvements in the art of surgery have been arrived at by the slow process of building upon the work of our predecessors. But I have looked carefully and in vain through Mr. Smith's paper for any important departure from methods of operating which I have frequently practised, and which have already become public property. It must have been due to an unfortunate chance that my name is mentioned in the paper, in connection with a misapprehension of an inconsiderable part of my method of operating. I have never in any of my now numerous cases employed hare-lip pins, as he states, so as to include "*both* thicknesses of the transplanted tissue, involving as it does a doubly acute flexure, and consequently a greater or less extensive strain on the tissues", which, he adds, "cannot be otherwise than detrimental". I have, indeed, used hare-lip pins to secure the opposed edges of the groin-flaps over the reversed abdominal flap; and I have frequently made them to take up in the middle line a *small portion of the connective tissue* on the raw surface of the subjacent reversed flap. But I have never applied them so as to penetrate the *whole thickness* of the reversed flap, and never so as to produce a doubly acute flexure. They were put in (as all the steps of the operation were conducted) with every whit of the scrupulous gentleness and care so fully emphasised in Mr. Smith's paper, and no more produced the doubly acute flexure which he deprecates than they do in the operation for hare-lip, properly performed. In my late operations, I have found out (like Mr. Smith in other matters) better modes of keeping the flaps together, and for some years have used silver-wire sutures only for this

purpose, applied so that they can be easily drawn when the proper time comes.

The strain upon the sutures in this operation depends almost solely upon the forcible protrusion of the hinder wall of the bladder by the action of the expiratory muscles, more than upon the way in which the sutures are made; and no operation which does not take this condition into account will succeed. Sloughing of any portion of the groin-flaps has scarcely ever occurred in my cases; but sloughing has taken place at the lower border of the reversed abdominal flap, just where I understand Mr. Smith to place his elongated portion intended to cover the urethra. Such an elongation of the flap, so far from its vascular connections with the abdomen, and subjected to the pressure of the resilient dartos and the tension of the scrotal skin brought over it, I have found to result frequently in sloughing or atrophy. Still I agree with him in thinking that it is always worth trying for. The plan of extending the groin-flaps downwards as far as the scrotum and side of the penis I have also frequently followed; and I have often found (as Mr. Smith will also find, if he watch his cases long enough) that, as the upward contraction of the cicatrix progresses and the child grows, the opening left at the root of the penis will enlarge, and in many cases will require some further procedure at a future period. In such an emergency, Mr. Smith will regret the loss of the prepuce by the sloughing he mentions, which seems to be an instance of the consequences of attempting too much at once. The retention of the urine for half an hour at a time in one of the cases is a very encouraging sign; and it tends to justify the doctrine that I have laid down, that the involuntary tonic contraction of the dartos-fibres connected with the artificial orifice into the bladder goes for something in its production. In several of my own cases, and conspicuously in one which is now under my observation at King's College Hospital, the partial power of retention, much doubted by some surgeons, is also very remarkable.

The plastic method of dealing with this deformity is shown by the cases (now numerous enough) to have been successful in restoring comfort to the patient, keeping him, with the aid of an ordinary railway India-rubber urinal, quite dry, protecting him from the constant irritation of wet clothing, and making him less an object of disgust. It is a far more safe and really a more reasonable method than any of the desperate and theoretical experiments for attaining this end, such as attempts at making a permanent communication between the bladder and rectum, or placing the ureters within the latter cavity by formidable operations, which have invariably produced the most disastrous consequences to the unfortunate patient.

In conclusion, I may add, with reference to Mr. Smith's very successful and interesting cases, that the boracic lotion, with which he carefully washes the surface of the flaps, is a very good and useful application, but that it is not absolutely essential to produce primary union of the flaps in plastic cases.

RECOVERY OF A BROKEN BOUGIE FROM THE BLADDER.

By GEORGE E. WALKER, L.R.C.P.,

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ON the evening of October 6th, I was sent for to attend G. P., aged 47, who, I was informed, had broken the bougie—a No. 7—which he was himself in the habit of passing into his urethra for an old stricture. He informed me that the instrument was an old one that he had had in use for ten months; and that it was cracked across in several places. He also told me that he had succeeded in passing it quite easily into the bladder, and that the accident happened in withdrawing it. The portion which he produced was seven inches in length, which would leave about five inches as the length of the portion retained. As he appeared to be in great pain, with a highly distended bladder, I found it necessary to give him relief at once. I could feel the broken end nowhere along the track of the urethra, and a No. 7 gum-elastic catheter passed freely down to the stricture, which was situated a few inches above the neck of the bladder, where its further progress was arrested by some obstruction. I was equally unsuccessful with a No. 5. I then determined to try to push the broken end into the bladder with a silver catheter, as it was evident that the obstructing fragment was situated too far down to admit of its being seized by any urethral forceps. I used a No. 7 for this purpose, and succeeded, without much difficulty, in passing it into the bladder, drawing off about two pints of urine, which afforded him instantaneous relief. In withdrawing the instrument, there was some little difficulty—a circumstance which I attributed to the spasm, and which I shall have occasion to mention hereafter.