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 VIII.

THE birds are divided into three primary divisions or groups. Of these, the *Saurura* is the oldest, and contains only one species, the archæopteryx. This bird possessed many reptilian characters; its tail was long and pointed, and the quills of the tail were obliquely set. It had two digits, with a claw on each, the metacarpals being free and distinct from each other, showing in this respect a marked difference from the birds now existing. The next group—the *Ratite*—of which several specimens still exist in the ostrich tribe and apteryx, possess flat sterna and rudimentary wings, and in most the number of toes are diminished, the ostrich having only two. The last group is the *Carinata*, which possess well developed wings, and the power of flight is in some, such as the frigate bird, remarkably strong.

The *Ratite* have undoubtedly existed for a long period, as they are of a low and ancient type. Many of the extinct forms were of great size, as, for example, the *dinornis*, which rivalled the giraffe in height, a character often indicative of low type and ancientness. Those birds possess no power of flight; the wings are rudimentary, and there is no glenoid cavity, or, when present, is only slightly indicated. In all *Ratite* the hind limbs are very strongly developed and muscular. The sense of sight is very strong. The size of these animals, especially of the extinct forms, seems to have been the mechanical cause of the wings not being developed so as to enable them to fly. Several of the birds of the carinate type now existing are probably descended from parents which at an earlier period were of much larger size. Corn-crakes were, till a short time ago, as large as turkeys, and even cassowaries, in New Zealand. This is the case, also, with other birds, as we find that doves larger than our gallinaceous poultry existed formerly in the Madagascare Islands; but, in these latter, the keel of the sternum was diminished in size, and also the wings, so that the power of flight was impaired. In the chalk-formations, birds are found with teeth which have had in some instances sheathed sockets. These are relicts of the reptilian form retained in ancient birds. In recent birds, teeth are not found, but their homologue is present in the form of horny projections which are found in some. Claws are still found on the terminal processes of the manus in the ostrich, swan, goose, and many other birds. The metamorphosis which has taken place in the manus of the bird has been gradual; the outer digits have been suppressed in the first instance, leaving only the central ones, which have also become modified, till the manus attained the form best adapted for the requirements of the animal. The number of carpal bones has been also gradually reduced to two, thus enabling the creature to bend its manus rapidly, and giving great range of movement. The mechanism of the legs in birds is remarkable, the bird being the only animal which is able to stand without exertion on one leg. It has the power of fixing the tarso-metatarsal joint by means of the ankylosed tarsal bones, which act as wedges; of these, there are only two fused to the ends of the tibia and metacarpus. The metacarpals are mostly fused together, though they are partly divided into five in penguins; to those are attached three or four digits, each with three phalanges.

LECTURE IX.

THE mammalia in their upward development resemble the lower stages of animal life. The embryo has no supply of yolk on which to support itself; hence it has to receive its sustenance from without. The brain takes on a different range of development from that of other vertebrates. The fore-brain develops till it is so large that it covers the hind-brain and mid-brain. The corpus callosum is always present. The organs of sense are much more fully developed in mammals, but this does not entirely depend upon the height of any particular type; for instance, we find that many of the lower mammals have far stronger senses of hearing and smell than man, the highest, possesses. In frogs, there are no turbinals, and although they occur in birds, they are

not the homologue of mammalian turbinals. The orbito-nasal nerve is the nerve in them which is sensitive to smell, but is only imperfectly so. In mammals, the olfactory bulbs are always well developed and distributed on the surfaces of the turbinals. The frontal, ethmoidal, and other sinuses, which are well developed in man, and well seen in the early development of the pig, are simply the remains of the nasal sacs of the lower animals which have projected backwards. The nose is formed in cartilage in the lower vertebrates, but in the mammals it develops into bone. The eye modifies the appearance of the skull by the way in which it is surrounded and protected. The ear-sac is developed as a separate capsule, which becomes fused with the sides of the skull, which afterwards ossify and become extremely hard. The lower jaw as well as the hyoid arch is included in the mammalian skull. In the modification of the extremities, the order of suppression is different from that in the birds; in mammals, the first digit to be suppressed is the thumb, and secondly, the little finger; suppression may take place till there is only one digit remaining, as in the horse, or it may stop when only the thumb and little finger are suppressed, as in the rhinoceros. The true vertebrate kingdom has been in existence for a long time, and has only by slow degrees attained to the perfection we find in its higher members. Formerly, the horse was a three-toed animal, and has passed through several stages before he attained the form he now has. This has been very beautifully shown by Professor Marsh in one of his recent works. Surrounding circumstances have had much to do with modification of form. Those forms which yielded more elastically to the surroundings have survived, while those which were more inelastic of adaptation have become extinct.

SURGICAL STATISTICS.

By JAMES SPENCE, F.R.S.E.,

Surgeon to the Queen in Scotland; Professor of Surgery to the University of Edinburgh; Senior Surgeon to the Royal Edinburgh Infirmary.

PROFESSOR LISTER, in his reply to my communication of January 24th, begins by representing it as consisting of two elements; the one tending to disparage his character as a surgeon; the other, to diminish the effect of the statistics he had adduced in favour of antiseptic surgery. The first mentioned element I entirely repudiate. I stated, at the very commencement of my paper, that I had been reluctant to make any remarks on the subject, as hitherto any criticisms on Mr. Lister's system had been characterised as attacks. I regret exceedingly the querulous tone of Mr. Lister's reply, and the personal feeling it exhibits. I would dismiss it very briefly, by simple repudiation of the feeling he unjustly attributes to me, were it not that the same accusation of personal attack pervades the whole of Mr. Lister's remarks, and seems to divert his attention from the scientific and practical points at issue. I therefore desire to clear off these accusations of want of charity, etc., so as to deal distinctly with the true objects of my former communication.

It is difficult to deal with a general charge of this kind, and I must, therefore, take one or two examples of the spirit in which Mr. Lister views any remark of mine. If, referring to a statement of his own, I say it seems to be admitted that the antiseptic system is limited in its application, that is "something of a taunt". If, in all good faith, after stating why I had not felt at liberty to try Mr. Lister's dressing in excision of the breast, I ask what was Mr. Lister's success in such cases, the question seems to hint, "not obscurely", that his practice had not been successful! But Mr. Lister commits himself to graver accusation, which I cannot permit to pass so lightly. He says that I hint that he gives "false facts", and goes on to speak of his character for truthfulness being "filched" from him. The best reply to this accusation is to quote the passage in which these words occur. In speaking of Mr. Lister's statement, "I say, if a case show new pathological facts, one individual such case is worth as much as a million (the italics are mine)", I remark: "As to Mr. Lister's very peculiar views of the value of isolated facts, or supposed facts, he ought to remember the dictum attributed to John Hunter, that 'there are more false facts than false theories in medicine'. The difficulty is to decide how far the treatment used is the cause of the result." I then allude to a case of my own to show my meaning. According to Mr. Lister, if I say there is a risk of his forming deductions on insufficient data, I accuse him of untruthfulness. But I think I may safely leave my quotation to meet his accusation. I do not think Mr. Lister justified in such expressions as, "If Mr. Spence succeeds in showing his own superiority" and "my inferiority as a surgeon", "in my lame hands", etc.; as if I had used any expression to warrant such statements. In criticising the cases referred to by Mr. Lister in his speech, I make no remarks

except on the information he himself gives, and such as I consider to be within the bounds of fair criticism. As to hinting at my own superiority, while I do not pretend to undervalue any attainments resulting from long practice and experience, I think those who know me will acquit me of boasting or self-assertion.

In proceeding to consider Mr. Lister's reply to the proper object of my former communication, I beg to recall the circumstance that produced it; viz., a leading article in the *BRITISH MEDICAL JOURNAL* challenging those who practised other methods to produce anything like similar results to those given by Mr. Lister in his speech at the "antiseptic discussion". In my reply to that challenge, I distinctly stated that I had no intention to enter on the general question of Professor Lister's system, as I had been, and was still, testing it in comparison with other methods. I limited myself: first, to show what was obtainable by simple treatment, by showing what had been obtained; second, to ask some explanation of statements made by Mr. Lister, and of some important omissions.

With the view of showing what had been obtained, I compared Mr. Lister's statistics of eighty major amputations during five and three-quarter years after he considered his system perfected, with sixty-three major amputations during three successive years when my method of treating stumps was very simple. Mr. Lister objects to this, that my statistics are not taken from the same period as his. Certainly not. He selects a period when his system was perfected; surely I have the same right to select a period when my treatment was simplest, as a contrast. Mr. Lister asks: "What is the reason for this selection? We can hardly suppose that Mr. Spence took his illustrations from the practice of many years ago, because he thought his method of treating wounds then superior to that which he employs at present." Had Mr. Lister read my communication carefully, he would scarcely have put the question in the form he does; for I state that I have returned to that simple method, slightly modified. Had I not thought it better than other methods I had also used, I would not have done so. The two methods I "at present" use are either Mr. Lister's antiseptic or the simple method, a description of which I append to this communication. As to Mr. Lister's remark about "good fortune", that might be said of any series of successful cases. He next remarks that his cases include primary amputations, whilst mine are for disease. I see that in my statistics there are three successful primary amputations at the shoulder; that is by oversight. I intended to mention only amputations for disease; for I object to amputations for injury, as not giving fair grounds for comparison. The nature and complication of the injury, the previous habits and health of the patient, the distance from which he has been brought, loss of blood, and mode of conveyance, differing in each case in nominally similar injuries, are all important factors which prevent equality of comparison; whereas amputations for diseases of similar character are much more easily comparable, and also serve to bring out important results as to the influence of certain morbid states on the mortality after operations. I take, therefore, Mr. Lister's sixty-four cases with five deaths, against sixty of my cases, with three deaths. Mr. Lister seems to complain that my cases are given in a tabular form, and catch the eye. I most sincerely wish his had been in a tabular form at first, as it would have saved me much trouble.

The next subject was the comparison of Mr. Lister's hip-joint amputations with mine; and here it is, I suppose, that he considers I disparage his surgery by objecting to the withdrawal of two of his cases as deaths from non-preventable causes. Without at all disparaging his surgery, I still object; and I consider that the proper plan is that which I take in regard to my twelve hip-joint cases; viz., to summarise the histories of the seven fatal cases, and let others judge of the result being preventable or otherwise.

Mr. Lister is perfectly at liberty to protest, and to hold his own views as to the place for amputation in malignant tumours of bone; but my experience has shown me that, in osteocephaloma, or in myeloid tumour affecting the shaft of a long bone, there is no safety for the patient in amputating through the continuity; for, even when the shaft above the tumour feels of natural thickness and form, the morbid material generally infiltrates the whole medullary canal. Mr. Lister's case is just another instance showing the risk of trusting to apparent limitation in malignant growths in the shaft of a bone. Of course I do not speak of condyloid growths, which in their earlier stages are more limited, and where I have frequently successfully amputated at the middle of the thigh.

In a foot-note, Mr. Lister, referring to a remark I made about a shoulder-joint amputation of his, which I saw when secondary hæmorrhage had occurred, and which I understood had died, says, "Mr. Spence is mistaken in supposing that there was any other fatal case". It would have been as easy to say, "The case that Mr. Spence alludes

to recovered"; for, as Mr. Lister puts it, some might suppose that no such case as that to which I allude had occurred.

Mr. Lister then sums up the comparison of my statistics with his, showing numerically that his results surpass mine. Nothing is easier, if he subtract some of his fatal cases as "non-preventable", whilst he criticises and hesitates about one of my cases of the thigh which died from albuminuria, and which I do not subtract, but allow to remain as a death following operation.

Mr. Lister refers to my general statistics and clinical reports. These cannot be fairly compared with the limited statistics he gives. My general statistics comprise upwards of 557 major amputations and 264 excisions of joints, together with clinical reports of my hospital practice over a long series of years. The clinical reports were not merely obtained from hospital journals and registers, but were drawn up and published at the dates of the reports by the Resident Surgeons, each of whom had seen and treated the cases included in his report, and was personally conversant with all the circumstances.

Such statistics, if properly studied, teach more than one lesson; they require to be pondered as well as numbered; but I have a right to expect them to be weighed by those who have independent practical experience in surgery—experience of more than one system; and who, from their knowledge of the varying fashions of treatment during the period embraced by the statistics, are capable of comparing my results with those of others during the same period. To such competent judges, I can fearlessly leave the results of my general statistics and clinical reports. So far, however, as regards the present controversy, I think my general statistics teach a very plain lesson; namely, that, when my treatment of wounds was simplest, then it was most successful, and that, just as I gradually return to that simple method, my results improve.

The next subject to which Mr. Lister refers is my statement that his practice was in the same hospital, and under the same hygienic conditions, as mine. Mr. Lister expresses his astonishment that I should not have known how very bad the hygienic conditions of his wards were. I only know that his predecessor, Professor Syme, used to speak of the wards which Mr. Lister afterwards occupied as almost perfect, and preferred to remain in them when the New Surgical Hospital was opened in 1853. From what Mr. Lister says, it would seem that the state of the wards of which he complains depended very much on his own action in permitting overcrowding. If a surgeon have wards allotted to him suitable, say, for fifty-five patients, and choose (contrary to the hospital rule) to cram in ten or twelve more patients, it is his own fault. I know the temptation, and in my case it was greater, because I had fewer beds, whilst for many years I had been accustomed to upwards of sixty, and had many recommended cases, owing to my long connection with the infirmary; but, except in any temporary emergency, I positively forbade crowding of the wards. As to the cleaning of the wards, that was biennial, not annual, and Mr. Lister could have had his wards cleaned as well as the other surgeons.

Mr. Lister next replies to one part of my observations regarding the excisions of the mamma. Mr. Lister explains that he enters into particulars, as my question seemed to hint that his practice had not been successful. I have already stated that he was wrong in this supposition. I asked the question, because some cases operated on in my wards by his method had not progressed favourably, and hence I had never ventured to try it myself. As it now appears, however, the results are not better than mine. Of his cases, in number thirty-eight, three died; in thirty-eight cases of mine, two died. Seven of his cases were removals of simple tumours, leaving the gland. In all my cases, the whole breast was removed, except in the recurrent cancerous mass from the axilla, where it had been removed previously.

Mr. Lister explains that, in one of his fatal cases, failure in the proper application of the spray during dressing led to the result. Mr. Lister seems to ignore constitutional causes in malignant disease leading to unfortunate complication, such as erythema or erysipelas; but the risk of failure from some accidental cause, even in his own practice, with skilled hospital assistance, should teach some of his followers to be more cautious in "guaranteeing" a successful termination to operations.

I would observe, however, that Mr. Lister has failed to offer any explanation of the principal question I put regarding excisions of the mamma, viz., "How are we to account for the escape of these (my successful cases), in which we have a large cut surface exposed to the atmosphere during the operation, and whilst arresting hæmorrhage, if the germs floating in the atmosphere be the causes of all the evils attending operations?"

The cases of operation for ununited fracture are not so very marvellous as I had been led to expect. In operations for ununited fractures of the leg, I have generally been able, by the subcutaneous

method, to effect union without constitutional disturbance; and in those cases where I have cut down upon and pegged the bones, the patients have all done well. Those on the femur are undoubtedly the testing cases. In several of Mr. Lister's eight cases, we find the operation had to be repeated before union was accomplished; in one, it failed; and in one, amputation was resorted to in order to save life. In the only case I have met with and operated on, as stated in my paper, the case did well without special antiseptic precautions; but I do not draw deductions from one case, and, in such cases as resection of the femur for ununited fracture, I should not be inclined to neglect any means likely to preclude risk.

Mr. Lister seems to object to my saying that I considered his statement about the treatment of abscesses connected with diseased bones or joints as exaggerated, and likely to interfere with what I considered one of the best parts of the system. He asks, "Can Mr. Spence suppose," etc. Now the question is not what I suppose, but what Mr. Lister said; and I repeat that anyone who contrasts Mr. Lister's present explanation of the course of such cases with his previous statement, will see that there is a great difference. As to the question of the subsequent discharge not being purulent, that is a matter of opinion and observation. But the question as to the length of time required for the probable cure of such cases is a most important one as regards hospital management, and would almost suggest the necessity for special institutions for their treatment. There is one of Mr. Lister's statements in regard to opening abscesses and sinuses connected with diseased joints, which I cannot pass over. "Such cases," he says, "are indeed too common; and I would venture to say in all seriousness to Mr. Spence, that, for this unfortunate fact, teachers like himself are in a great measure responsible", etc. I would say to Mr. Lister in all seriousness, that, before he makes a charge of this kind, he ought to be sure that it is true. I never taught that to plunge a knife into an abscess connected with a joint, without due precautions, was proper practice. The precautions used might be different from Mr. Lister's, but, long before his system was known, the dangers were fully recognised, and special methods in dealing with such abscesses were recommended. Opening by various forms of exhausting apparatus, or with tubing under water, injecting iodine and water, opening freely to get rid of curdy pus, and then painting the whole pyogenic surface with tincture of iodine or chloride of zinc solution, were amongst these precautions. And in cases where abscesses existed, in knee-joint disease, the rule in my own practice, and that of other surgeons in the Royal Infirmary, was to avoid opening them before amputating. Fortunately, my teaching can be found in my published Lectures; and anyone, by referring to pages 25 to 28, may judge how far Mr. Lister is warranted in his statement regarding my teaching. This want of "a little charity towards a late colleague" reminds me that Mr. Lister has not replied to my remarks on his teaching regarding antiseptic suprapubic lithotomy, and the results of his practice.

NOTE.—The following is the simple method of dressing stumps referred to in the preceding communication.

Suppose an amputation of the thigh. After the operation is completed and the drainage-tube inserted, the stump is laid upon a Gooch's splint, padded and covered with gutta-percha tissue, and fastened to the splint by a turn of carbolised gauze bandage; the end of the stump and the drainage-tube should project a little beyond the end of the splint. The patient is then laid on a mattress, about one-third of which has been cut away on the side corresponding to the amputated limb. The stump is then swung from a cradle with the point dependent, and under it a plate containing a strong solution of carbolic acid or chloride of zinc receives the blood or other discharges as they fall from the wound. The cradle is covered with a veil of gauze.

Thus no textile fabric is in contact with the wound, and, as the stump is slung, all discharges drain away at once into the plate, and are there acted on by the antiseptic fluid. As no manipulation beyond lightly touching the cicatrix with a moist camel-hair pencil occasionally is required, perfect rest is ensured.

PRESENTATIONS.—Mr. Alfred S. Perkins of St. Sidwells, Exeter, upon resigning as Medical Officer to the "Loyal Rougemont" Lodge of Odd Fellows, has been presented with an inkstand of polished oak mounted with silver, inscribed "Presented to A. S. Perkins, Esq., as a mark of esteem from the members of the Loyal Rougemont Lodge, I.O.F., M.U. February 26th, 1880."—Mr. Frederick J. Parsons of Portland has been presented with an elegant clock, as a mark of esteem, by his patients and friends.

THE sum of £107 has already been subscribed towards a painted window to be placed in the Bampton parish church, and a monument in the old church-yard, as a memorial to the late Mr. Alexander Thom.

RESULTS OF SURGICAL TREATMENT, WITHOUT ANTISEPTICS, IN THE KILMARNOCK INFIRMARY.*

By JOHN C. McVAIL, M.D., Kilmarnock.

THE important discussions lately published on antiseptic surgery have induced me to collect the chief statistical results of the simple or non-antiseptic treatment practised by Dr. John Borland in the Kilmarnock Infirmary. During the past three years, much fuller tabular statements than formerly have been issued in the annual reports; I therefore confine myself to that period.

The total surgical cases treated to a termination were 543, and Table I contains a list of the deaths that occurred.

TABLE I.

No.	Nature of case.	Time in Hospital, or after the Operation.	Notes.
1	Railway injury.	4 hours.	All moribund on admission.
2	Extensive burns.	9 hours.	
3	Internal injuries.	5 minutes.	
4	Smashed legs.	2 hours.	
5	Smashed legs.	3 hours.	
6	Fracture of pelvis, etc.	9 hours.	
7	Laceration of lungs from fractured ribs.	13 hours.	
8	Gangrene of feet and legs.	11 days.	Gangrene, caused by exposure to cold, in a tramp, with broken-down constitution.
9	Laceration of legs.	11 days.	Died of tetanus.
10	Disease of knee.	45 days.	A hectic emaciated subject, who lived almost entirely on stimulants. Knee quite disorganised, and discharging from numerous sinuses. A series of abscesses formed along the thigh, a new one appearing every two or three days. Never was fit for operation.
11	Syme's amputation.	41 days.	A strumous subject. Stump healed in about twenty-eight days. Patient was taking out-door exercise, and was about to be dismissed, when a very large abscess formed over the sacrum, in 48 hours, and death resulted from pyæmia.
12	Amputation of both legs.	7 days.	Aged 52. Died from gangrene of back (the result of a blow from an engine-buffer) and internal abdominal injuries.
13	Amputation of thigh.	3 days.	Aged 76. Internal injuries caused death.

This gives a death-rate of 2.33 per cent. But if we eliminate the seven cases under twenty-four hours in the wards, the following is the result.

In 1877		4 deaths in 171 cases, or 2.3 per cent.
In 1878		2 deaths in 167 cases, or 1.1 per cent.
In 1879		0 deaths in 199 cases, or 0.0 per cent.

Total 6 deaths in 537 cases, or 1.1 per cent.

Operations.—In the above 537 cases, 107 operations were performed, of various degrees of importance. The deaths were three—Nos. 11, 12, and 13 in the table. The rate is therefore 2.8 per cent. The following are the fifty principal cases. Those marked * are included, owing to joints having been opened—the elbow in two, and the wrist in one case.

TABLE II.

Operation.	No.	Deaths.
Amputation of thigh	11	1 (No. 13)
" both legs	1	1 (No. 12)
" leg	8	0
" ankle (Syme)	5	1 (No. 11)
" foot (Chopart, 2; Hey, 1)	3	0
" arm	5	0
" forearm	1	0
Excision of elbow	1	0
* " head of radius	1	0
" mamma	2	0
" cancer of back	1	0
" cancer of upper jaw	1	0
" epithelioma of arm	1	0
* " metacarpal bone	1	0
* Reduction of compound dislocation of elbow	1	0
Removal of sarcoma from head of tibia	1	0
Exposure and stretching of sciatic nerve	4	0
Ligature of superior circumflex artery of humerus	1	0
Extirpation of eyeball	1	0

Of these fifty cases, eleven were primary, with two deaths, and thirty-nine for disease, with one death.

* Read before the Glasgow Medico-Chirurgical Society.