Observations

ON

LITHOTOMY, AND ON CERTAIN CASES OF ENLARGED PROSTATE.

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ALTHOUGH probably no subject in surgery has been written about so voluminously as lithotomy, it has often appeared to me that many things associated with this operation have been carelessly passed over, or so slightly noticed that, practically, they may be said to have escaped observation. Supposing this statement to be correct, I am disposed to account for it by the circumstance that the most experienced lithotomists have not always been the best or most comprehensive writers on the subject. The reticence of Rau and of Cheselden is an appropriate illustration; and treatises on the operation have been written by many who have had such limited personal experience that important practical matters have often been overlooked, while things having no existence have often been dwelt upon as if of common occurrence. As an illustration of this last statement, I may refer to the so-called infiltration of urine alleged to be so common after this operation. Such an event, if it ever occurs, is exceedingly rare, and I have never seen an example of the kind. An erysipelatous inflammation, with considerable infiltration of serum, lymph, and pus, has occasionally been mistaken for this state, I believe. I have often seen instances of great accumulation of urine in the bladder after lithotomy, without the smallest infiltration.

As my personal experience in lithotomy extends to nearly two hundred cases, besides numerous operations I have witnessed in the hands of others, I trust that it may not be deemed presumptuous on my part if, claiming some authority on the subject, I venture to record some points which have been little noticed, if at all, by the generality of writers on this absorbing subject. I am the more induced to do so, because I have reason to think that some observations on lithotomy in children, made by me in my lectures at the College of Surgeons in 1864, have had good effect. What I then stated was the result of much thought and considerable individual experience. The distance of the neck of the bladder from the perineum, the ease with which the membranous portion of the urethra may be torn across, and the facility with which the prostate and neck of the bladder may be thereafter pushed upwards and backwards, were particularly dwelt upon; and a seemingly correct explanation, as I hope, was given of the frequency of not finding the stone in this operation, as also regarding the conclusion that stone had not actually been present in such cases,—the real fact being that the operator had failed to reach the bladder. More mistakes of this kind have been made on children than on adults, and experience has convinced me that the difficulties and mechanical dangers are actually greater in children than in the adult. On the occasion referred to, I pointed out the common error of supposing that because lithotomy is comparatively so successful in its results in children, it is easy of performance in similar proportion. Such however is, in my opinion, not the case; and youth, with the strong natural disposition to reparation of wounds in early years, are the principal reasons for the happy results of this operation on patients under the age of puberty.

In my experience I have learned something more about the prostate than is usually stated in books about lithotomy, or about that organ itself. Operative surgery and surgical pathology have been largely taught by the pure anatomist, who has had little experience in either subject; and there

has been on that account, in my estimation, a large amount of questionable doctrine laid down for the guidance of the practical surgeon. He may, indeed, be years in practice before he can, by thinking for himself, get rid of what I am often disposed to call dissecting-room surgery. I look back with amazement at the amount of writing and so-called instruction as to the limit or want of limit of incisions in the prostate in lithotomy. The healthy prostate, such as it may be considered between puberty and fifty years of age, has usually been taken as the standard of size. In accordance with theories of the teacher or author, a limited incision has been the favourite with one, whilst a free wound through the whole substance has been inculcated by the other. Yet lithotomy is more rarely required during the period referred to than at an earlier or later date. In two-thirds of the cases for this operation the prostate is either below or above the size most generally indicated in these speculations. In early years there can be no doubt that it is either cut or torn throughout; but in advanced years it may be doubted if it ever is either torn or divided entirely, excepting in rare instances, even by those who profess to use the knife freely in this part of the operation. Without proposing at present to discuss the question of free or limited incisions, I am desirous of drawing attention to the circumstance that the lithotomist often deals with the prostate in old men under circumstances very different from those under which it is seen by the pathologist or treated in ordinary practice. The pathologist and practitioner usually look upon the enlargement as a whole, or perhaps with reference to the special enlargement of the middle lobe. The one looks to the appearance of the enlarged parts; the other thinks of the effects of these upon micturition, or how, where there is much irritation associated with the condition, he can best and most readily give his patient relief by catheter or otherwise.

Now, although most formidable cases of stone are often met with in old age without any marked enlargement or change in the prostate, it frequently happens that the lithotomist finds the gland enlarged, and in the ordinary lateral operation his knife, finger, and instruments, pass through the part where enlargement is almost invariably greatest—viz., the lateral lobe. In the normal state of the prostate, the firm fibrous structure, as usually seen by the anatomist, can be readily appreciated. Even when there is enlargement, the firm fibrous character may still seem predominant. But in many instances, as the forefinger passes towards the bladder, the sensation is as if its point glided through several rounded bodies in the substance of the gland, which are but slenderly in contact with each other. It is seldom that the pathologist takes this route in examining or displaying the gland, and so perhaps little has been said about this condition. For my own part, although, like others, familiar with the condition, I had never heard any reference to it in association with lithotomy, and it was not until I had some considerable experience in this operation that I perceived the great importance of the fortuitous combination. Repeatedly I had been conscious, during a succession of operations, of feeling, and often, too, of seeing, this condition. The feeling may be easily understood, but seeing can only be appreciated after explanation. When the stone is being extracted, within the grasp of the forceps, there is often considerable resistance, even when the knife and forefinger have been freely used to give ready passage to and from the bladder. In some instances the stone, whilst all but through the wound, seems to be held *in situ*. If at this time the operator looks carefully, particularly on pushing the skin and margin of the wound a little aside, he will see some tissue or substance between the blades in the space between the hinge and stone. With a slight pressure of the point of the finger this object will disappear from between the blades, and extraction will then seem comparatively easy. This is in reality one or more of the lobules referred to. It has not been grasped between the blades in seizing the stone, but has slipped into the space, as the forceps and stone have been drawn outwards. On such occasions I was glad to let the substance remain without further notice, and I confess to the fear that by referring specially to what I had noticed I might be deemed to have used unwarrantable liberties with the prostate. After repeated experience of this sort, it occurred to me that the propriety of leaving such loosened masses was questionable; and at last, on a tempting occasion, I seized,

after the extraction, one or more of these bodies, and partly tore and partly cut them away. To my great delight, no evil followed, and the patient, about seventy-five years old, made a most satisfactory recovery, having no special symptoms afterwards.

In 1848, I exhibited specimens of this kind at the Pathological Society of London, and not long after heard whispers about rough and improper proceedings in lithotomy,—coming chiefly, or rather solely, from those who had rarely, if ever, performed the operation. Criticism of the kind was almost enough to deter from similar pathological information, but I have since followed the practice repeatedly whenever it seemed to me needful, and I have never had occasion to regret doing so. The subject has recently been brought under my notice in a peculiarly interesting manner. During lithotomy on a patient nearly eighty years of age, in whose case my anxiety was special, I recognised this condition of the prostate. There was no peculiarity in the operation, but in extraction I recognised the hitch above referred to. When the stone was extracted I turned my attention to the lower part of the prostate, and found it so loose already that I accomplished its enucleation with the point of the forefinger of my left hand, with as much readiness almost as if it had been a stone lying loose in the left lobe of the prostate. The circumstances were peculiar, perhaps unprecedented in the history of lithotomy, and I drew the special attention of a professional brother, a near relation of the patient, who was present, to the circumstance. I felt convinced at the time that, whatever might be the result, I did the best for the patient, although his condition was critical in the extreme, and the operation was deemed a last resource. His recovery was slow, owing to previous exhaustion, but there never has been any irritation in the prostate. I do not feel at liberty to mention all the features and history of this case, but I pledge my word and name, for what may be their value, that it is worthy of being a precedent case.

The thought of being of service to any future sufferer, or any professional brother, induces me to put such experience on record in a manner more forcible than heretofore. The interests of patients and of surgeons are alike involved in these observations; and there are points or questions connected with them which, in my estimation, deserve attention from all engaged in the department of surgery to which the above remarks relate. In many cases of stone in the bladder the distress of the patient is plainly attributable to this cause; but occasionally the presence of stone produces so little irritation that its discovery is a matter almost of chance. Often, however, enlargement of the prostate gives rise to most, perhaps all, of the symptoms and sufferings of stone. When stone is combined with enlarged prostate, who can say from which cause the distress is greatest? So if it happens that the surgeon is called upon to relieve his patient from stone, and finds an enlarged prostate at the same time, why should he not, in extracting the stone, when it seems in his power, as in the cases above referred to, take away a superfluous portion of the prostate at the same time? The masses alluded to seem like adenoid tumours in the original substance of the prostate; their removal may be deemed a relief, and a possible solution of the distress of enlarged prostate; and, as the entire wound in lithotomy most generally closes by granulation, the very removal of such well-isolated masses will leave less surface for inflammation, suppuration, and granulation. If allowed to remain, it is not likely that they will adhere to their former site by immediate union; if they do not, a larger surface must be exposed to the irritating influence of urine, and a double suppurating and granulating surface must be the result.

But, without speculating on such matters, I hasten to enunciate the conclusion at which this paper aims. There is a certain timidity in the practice of surgery which may in many respects be deemed becoming; and precedents are often as much prized among us as with practitioners in law. There would, however, be no progress unless someone from time to time stepped out of the accredited course; and so, with such authority as I can humbly claim, I have thus ventured to put on record what some of my professional brethren may have hesitated to do, from a fear that they have been guilty, in their operations, of perpetrating some rough mechanism not in accordance with that nicety of manipulation which is thought so essential in the performance of the master handiwork in surgery—lithotomy.

ON THE LOCATION IN THE SPINAL CORD OF A SPECIAL SET OF MOTOR NERVES DIS- TINCT FROM THE VOLUNTARY MOTOR.

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In the appendix to my work on the Central Nervous System,* I have already pointed out the existence, in the base of the brain, of a large number of nerve-fibres having a great share in the production of convulsive movements, either as reflecto-motor or otherwise, and quite distinct from the voluntary motor nerves. I have lately made experiments which show conclusively that in the spinal cord there are motor (reflecto-motor) nerve-fibres occupying a distinct place, so that they may be paralysed while the voluntary motor nerve-fibres are not. The following facts leave no doubt on this point:—

1st. If I divide the posterior column and almost the whole of the lateral column of the spinal cord, with the posterior and central parts of the grey matter, in the dorsal region, in a guinea-pig, I find, when the animal has become epileptic, that the irritation of the part of the face and neck which I have called *epileptogenous*, determines reflex convulsive movements everywhere, except in the posterior limb on the side of the injury. This lack of reflex movements is not due to a paralysis of the nerves serving to voluntary movements, as, if that limb is at first a little paralysed after the operation, it soon recovers power, and has no trace of weakness by the time epileptic fits can be provoked.

2nd. If only the posterior column and a very slight part of the grey matter, with a still slighter part of the lateral column of the spinal cord, is divided in a guinea-pig, in the dorsal region, the four limbs are attacked with reflected convulsive movements when the *epileptogenous* zone is irritated. In this case the encephalon communicates with the posterior limb on the side of the injury for both voluntary and reflected convulsive movements.

3rd. If, in another animal, the lesions mentioned in the preceding experiments have been made, at the same level of the cord, one on the right side, the other on the left side, I find that the two sides of the face and neck acquire the epileptogenous power, and that fits can, therefore, be produced by the irritation of either side; but whether the right or the left side be irritated, there are reflected convulsive movements only in three limbs; the posterior one, on the side where the lateral column of the cord is divided, remaining without the least convulsion, while the three other limbs are violently convulsed. Both lower limbs, however, remain endowed with strong voluntary movements.

4th. In animals having had a complete section of a lateral half of the spinal cord, at the level of the tenth dorsal vertebra, and having become epileptic, I have ascertained that the voluntary movements, after a period of very great diminution in the posterior limb on the side of the lesion, return gradually almost to the normal condition, in a variable number of months. In many guinea-pigs, having recovered voluntary movements even for a year or eighteen months, I have never seen but very slight convulsions in the posterior limb on the side of the injury. Reunion of nerve-fibres serving to voluntary motor fibres can, therefore, take place to a very great extent in the spinal cord, after having been divided; but there is hardly any reunion for the nerve-fibres which, in attacks of epilepsy, give rise to reflected convulsive movements.

These facts, and others which I do not think it necessary to publish, seem to prove conclusively—

1st. That nerve-fibres employed in making muscles contract, in fits of epilepsy, are quite distinct from voluntary motor nerve-fibres.

2nd. That these two sets of motor nerve-fibres do not occupy the same place in the spinal cord, the voluntary motor being more in the anterior (grey and white) parts of that organ, the others being chiefly in the lateral column.†

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* Course of Lectures on the Physiology and Pathology of the Central Nervous System, p. 241. 1860.

† This is in harmony with the fact well proved by my very able friend, Dr. Charcot, that inflammation (sclerosis) of the lateral column of the spinal cord is invariably accompanied by strong muscular spasms.