LECTURES ON THE PRINCIPLES OF SURGICAL DIAGNOSIS: ESPECIALLY IN RELATION TO SHOCK AND VISCERAL LESIONS.

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LECTURE I.—LESIONS OF SPINAL CORD. (Concluded.)

Introductory Remarks.—Tetanus: its Varieties, Causes, Symptoms, and Signs.—Comparison with other Convulsive Affections; Hydrophobia, Epilepsy, Chorea, Hysteria.—Acute and Chronic.—Shock.—Tetanus in Lower Animals.—Morbid Anatomy.—Repair of Cerebral Substance after Breach of Texture.—Affinity between different Convulsive Affections of the Cord.—Effects of Spinal Caries on the Cord.—Surgical Operation in Fractures of the Spine Discussed.

BEFORE quitting this interesting subject I will briefly advert to the morbid anatomy of tetanus, on which considerable light has been recently thrown. The rare occurrence of this malady, as compared with the frequency of such wounds and contusions as are known to be the most rife exciting cause of its existence, would seem to indicate that a variety of concurrent circumstances must be present to determine the development of the disease. No doubt many of these are extraneous and purely accidental; such as season, locality and temperature; or the presence of any depressing external agency. But there must be, also, other and intrinsic predisposing conditions which influence the production of this convulsive affection; and it would appear probable that these reside especially in the nerve-centre itself. I cannot say I have ever attached much importance to the *post mortem* appearances which were supposed, formerly, to be associated with fatal tetanus. As far as I have been able to judge, they are so ill-defined and irregular as to be both unreliable and insufficient—accidental changes which might or might not be associated with the fatal disease. When a nerve of sufficient magnitude has been wounded, and this injury has been succeeded by tetanus, the injured nerve generally betrays signs of inflammation. Flesh-wounds are often angry-looking, especially when the symptoms of convulsion are developed early. But these conditions are by no means necessary or uniform. On the contrary, I have known such a wound, involving the ulnar nerve, entirely healed, before the first indication of tetanus was observed; and flesh wounds will heal kindly during the progress of the disease. We must, therefore, seek for some other explanation of its pathology than can be afforded by the unhealthy or unhealed condition of the wound. The peripheral source of irritation is not necessarily perpetuated, but is transferred to the nerve-centre; and these cases are, perhaps, among the most hopeless, and run their course rapidly. The natural inference under such circumstances is, that some organic change has been established in the nerve-centre, which no longer needs the primary irritation in the circumference to maintain it. On the other hand, such peripheral irritation alone would appear to suffice, in some cases, to excite tetanic convulsion, by reflexion through the cord; and the removal of the offending part will then arrest the disease. Under these circumstances it becomes a matter of vital importance to distinguish such cases as are of a purely eccentric character; for, in such only should an operation be contemplated. Certainly these instances of successful interference are rare, especially where the disease presents itself in the acute form—where there is no remission of symptoms; this, indeed, appears to be the class of cases in which the disease is centric and, probably, as such always fatal. But in subacute and intermittent tetanus, the removal of the injured member, say a finger or a thumb, has been immediately followed by relief. Perhaps a careful analysis of these circumstances, founded on more accurate and extended pathological observation, may lead to more satisfactory results.

To Dr. Lockhart Clarke we are indebted for our acquaintance with those pathological changes in the organic structure of the cord, which are found in fatal cases of tetanus. In several specimens examined by this careful observer, there were noticed extensive areas of disintegration in the gray substance of the cord, especially round the central canal, with softening and exudation of a finely granular fluid, and the debris of blood-vessels and nerves in the same locality. Swelling or falling in of the cord at the parts affected was also noticed, and extravasation of blood into its texture. And these changes, though varying in degree in different parts, were not limited to one segment of the cord, but involved the greater part of its extent, from the upper cervical region downwards.

Fig. 1.—Section of spinal cord in tetanus, showing its distortion and falling-in from the fluid disintegration of the grey matter; a condition which prevailed, in this instance, more or less throughout the whole length of the cord. After a drawing by Dr. Lockhart Clarke.

Several inquiries, and of an eminently practical character in relation to diagnosis as well as treatment, are suggested by these facts, and are referred to by Dr. Lockhart Clarke, at the conclusion of his paper on the subject. The first which naturally presents itself is, whether the structural changes observed are present in cases which recover. The reply to this inquiry must necessarily be purely conjectural; but the probability is in favour of the negative. If the positive be assumed, the alternative is implied of entire restoration to a normal condition, or the persistence of some indication of permanent functional nervous derangement. The former is scarcely consistent with our present knowledge of these organic changes in the structure of the cord; and, as regards the latter, I cannot recall any instance in which evidence of such enduring derangement has existed, after recovery from tetanus. It seems more probable that, in such cases, irritation without organic change—an expression which is admissible in the absence of one to which a more definite meaning may be attached—is the explanation of the consequent phenomena.

As regards the relation of these structural changes, as produced by, or the cause of, the tetanic spasms, Dr. Lockhart Clarke remarks that they most frequently occur where the nerve-cells are scanty, and are not found in the anterior gray substance which gives origin to the motor nerve-roots; and that similar lesions are found in those cases of paralysis which are unaccompanied by tetanic convulsion. Moreover, the blood-vessels are often found dilated, and even in a state of disintegration. His conclusion, therefore, is, that the symptoms are due to a morbid state of the blood-vessels, and not to excessive functional activity of the cord. Yet it appears to me that this exalted activity of the cord must exist, albeit as a consequence of such morbid condition of its blood-vessels; and this supposition is consistent with the conjecture that the vascular disturbance, whether functional or organic, does not necessarily imply any structural lesion of the nerve-tissue itself. May it not be that the rapid evolution of these organic lesions produces tetanic convulsion, and that their slow development causes paralysis, with the accompanying phenomenon so frequently observed, of spasm or twitching of individual muscles? Just as chronic affections of the gray matter of the hemispheres produce dementia, and acute affections produce delirium. Muscular atrophy has also been shown, by the author to whom I was just now referring, to be due to softening and disintegration, chiefly of the gray substance of the cord, and to atrophy of its nerve-cells; accompanied by morbid exudations, and frequently by great dilatation of blood-vessels, the result of inflammatory action; and where paralysis is the consequence of lesions of the gray substance, in any limited part of the cord, reflex action is increased in parts receiving nerves from portions of the cord below the seat of alteration.

Examination of the brain and spinal cord, after death from hydro-

phobia, has failed hitherto in demonstrating any organic change in their structure. The great vascularity and pink hue noticed in their texture may be a consequence rather than a cause of the excited action of these organs.

In epilepsy, Schröder van der Kolk describes dilatation of blood-vessels,* and Dr. Lockhart Clarke has found disintegration of the surface of the fourth ventricle in the same disease.† In chorea, the same author has remarked extensive disintegration of the gray substance, and softening of the white.‡ According to other observers, the sensori-motor ganglia (the corpora striata and optic thalami) at the base of the brain, are the seat of this disease; and embolism of their vessels has been assigned as the actual condition to which it is due. These observations are remarkable, considering the usually curable nature of this affection.

On the other hand, my friend and colleague, Dr. Clapton, suggests that epilepsy is generally a blood-disorder, and that it is dependent on some chemical change, probably the slow abstraction of animal quinine from the system, and its substitution by some morbid matter which exerts its own specific influence on the nervous centres, and gives its peculiar character to the convulsive affection, whether tetanus, hydrophobia, or epilepsy. And he bases this conjecture on the peculiar effect of quinine in large doses—from five to twenty grains—if given in solution during the premonitory symptoms of an epileptic seizure, when they are of sufficiently long duration to administer the draught. In about fifty cases, in which this treatment was tried, it failed in four only; two of these were instances in which the fits followed injury to the head; one was a case of syphilis, and one of tuberculosis.

In connexion with this subject I may allude, incidentally, to an interesting circumstance mentioned to me by Mr. Erasmus Wilson, which seems to suggest the presence of a condition affecting the sensory nerves, analogous to those convulsive affections of the motor nerves which have been under consideration; and identical as regards its causative agency, if we may draw that inference from the efficacy of the remedy. Mr. Wilson has found some most aggravated cases of prurigo and pruritis yield to large doses of quinine, after every other form of treatment which suggested itself had been tried in vain; moreover, the relief proved to be permanent as well as speedy.

The facts which I have mentioned, both pathological and therapeutic, would seem to raise the question whether the morbid changes observed in the spinal neurine, in these convulsive affections, stand in the relation of cause or effect to the phenomena, presented by and characterising this class of diseases. We know that either epileptic or tetanic convulsion may be due entirely to central irritation, as where it is traumatic from the presence of a depressed fragment of bone; and also in some diseases of the brain and spinal cord; and the pathological appearances to which I have referred, as noticed in the tetanic cord, are similar in their nature to those which are observed in the slower disintegration of the cord, in ordinary paralysis and in muscular atrophy; but, in locomotor ataxia, as Dr. L. Clarke has shown, the nature of the lesion is somewhat different, and is always limited to the posterior columns of the cord, and the posterior gray substance.

If we assume that these convulsive affections are usually dependent on blood-poisoning—as they certainly are often influenced by endemic causes operating in conjunction with peripheral irritation—we may the more readily understand how, in certain instances, the malady may continue to be purely functional in all its attendant phenomena; but that organic change in the cord, dependent probably on the intensity and persistent operation of a diseased circulating medium, will react by producing an aggravation of the functional disturbance; and, further, that the disintegration of texture in the spinal cord, noticed in fatal cases of this class, is irrecoverable. On the other hand, in tetanus caused by peripheral lesion, the reflected agency seems to be so clearly established, that it is difficult to evade the conclusion that the central change in the texture of the cord is directly dependent on the peripheral irritation, and is itself the cause of that convulsion. Yet, this does not, in my apprehension, exclude the operation of some morbid condition of the circulation in the production of the disease. In the allied disease of hydrophobia, the agency of a blood-poison seems scarcely to admit of a doubt. In this affection, as I have remarked, the cord does not exhibit any discoverable changes, similar to those which are noticed in tetanus; and it is an interesting fact that strychnine produces no morbid change in the cord: this, I am assured by Dr. Lockhart Clarke is the result of experiments performed by him; and M. Vulpian also has shown that the spinal cord of frogs, kept in repeated convulsions by strychnine for a month, presented no change. (*Archiv. de Physiologie*, 1868). The phenomena of tetanus and those produced by strychnine

are undistinguishable; but the causative influence is different. It seems, therefore, justifiable to infer that the mere occurrence of muscular spasm is insufficient, in the former case, to account for the morbid change in the texture of the cord.

I am disposed, from the foregoing considerations, to draw the following conclusions.

1. That these convulsive affections may all be dependent on blood-poisoning.

2. That the phenomena of rabies are distinctly due to this cause.

3. That even in traumatic tetanus the pathological changes in the cord are probably influenced by a morbid condition of the blood: but this progressive disintegration would appear to be the cause, and not the consequence, of the muscular spasm.

The condition of muscular fibre which had been the subject of tetanic spasm, was first noticed, I believe, by Mr. Busk, and has been commented on by Mr. Bowman in the *Physiological Transactions* (vol. for 1840, p. 490; and vol. for 1841, p. 69). These accurate observers found the fibres ecchymosed and ruptured, and presenting appearances of disintegration, "to such an extent that, in many parts, neither transverse nor longitudinal striae could be discerned, but only a confused mass of primitive component particles, held together by the sarcolemma." This condition I have verified in an acute and rapidly fatal case of traumatic tetanus recently under my care. I may further remark that, in this instance, the extensor muscles of the spine were deeply congested, and presented, in their cellular interstices, numerous broad patches of extravasation of semi-fluid blood and coagulum. The case ran its course in about seventy hours.

We have no very definite information as to the extent to which breach of texture of the cerebral substance may be repaired, and the mode in which such repair is effected. Yet there seems to be no reason why brain texture should be absolutely exceptional in this respect; indeed, instances of restoration of disturbed or interrupted function, after the lapse of time, would seem to indicate that repair is sometimes accomplished. But loss of texture, as in abscess, is not restored by reproduction of nerve-substance, as, in all probability, elementary degeneration, constituting organic change, is not supplanted by new and healthy neurine. In the rare instances in which abscess of the brain does not destroy life, and heals, its walls become thickened and contracted; and with this and inflamed surrounding nerve-substance, a sort of cicatrix is formed; or the space is occupied by a portion of the contents of the abscess becoming mixed with chalky deposit.

In studying the phenomena which accompany and characterise the various diseases of the spinal cord to which I have briefly alluded, and the distinctions on which their diagnosis depends, it is difficult to resist the conviction, as I remarked at an earlier part of this Lecture, that a nearer affinity between them is manifested, as the physiology and morbid anatomy of the nerve-centre become better understood. There can be little doubt, I apprehend, that many of these affections, evincing even aggravated symptoms, and having an eccentric or peripheral origin, are purely functional and therefore remediable; whilst others of the same class, as regards their source, are accompanied by organic change, and therefore resist treatment. But wherever these convulsive affections, whether tetanus or chorea, epilepsy or hysteria, are centric in their origin, and consequent exclusively on central irritation, the cause must be organic change; and recovery, as a rule if not invariably, is hopeless.

Progressive disintegration of any portion of the spinal column, from slowly progressive disease, such as caries, is, in exceptional cases only, accompanied or succeeded by functional disturbance of the cord, and still more rarely by organic disintegration. Permanent loss of texture in the bone and the connecting fibro-cartilage between the vertebrae may entail great distortion, and yet the cord may preserve its functions intact. This destructive process attacks the bodies of the vertebrae and the intervening elastic texture which binds them together; and the superincumbent weight or position acts mechanically in inducing approximation of the adjoining bones, thereby excluding from the canal the disorganised tissues and pus, which find their way to the surface, either in the loins, or, guided by the sheath of the psoas muscle, at the groin. But, in more rapid cases paraplegia may ensue, and then becomes a diagnostic sign, in company with others, of the disease. The continuity of diseased action, in such cases, to the anterior columns, explains the secondary affection of the nerve-centre; and the paraplegia is due rather to softening or fatty degeneration, similar to the changes which I noticed in a former lecture as occurring in the brain. When the destructive process extends more deeply, anaesthesia also supervenes.

Apart from these decisive proofs that the cord is involved, a distressing sense of tightness and pain, extending round the umbilical or epigastric region, is often complained of in caries, and is very characteristic of the condition of which I am speaking. It is almost

* Spinal Cord. Sydenham Society.

† Beale's Archives, No. xiv, 1864.

‡ British and Foreign Medico-Chirurgical Review, January and April, 1868.

constant in some cases, and is the principal source of suffering and complaint of the patient. This pain would appear to be due to pressure or irritation of the posterior roots of the nerves distributed to these parts, and of course manifested in their sentient extremities. But I have noticed the same condition in some instances of fracture of the spine, where the paraplegic condition has not been complete. Thus, it was present in a patient of mine whose cervical spine was fractured, and whose chief complaint was pain and tightness across the epigastric region and round the abdomen, and also in the arms and between the shoulders.

I propose closing this lecture, and the present division of my subject, by some remarks on the propriety of interference by surgical operation in cases of fracture of the spine.

The analogy and close alliance between the functions of the brain and spinal cord has led to the inquiry whether there are any diagnostic indications on which dependence can be placed, as a guide to the surgeon in entertaining the proposal to operate, for the purpose of relieving the spinal cord from irritation or pressure; whether there is any condition which can justify an interference so grave in its nature, that it may be regarded as more momentous, in all its risks and bearings, than the corresponding operation of trephining the skull.

This question must be viewed in different aspects, such as the following inquiries embody. What extent and character of lesion demand an operation: how can the surgeon satisfy himself of the nature of the injury and of its position: is it possible to relieve the condition for which the operation is undertaken: what are the risks of the operation: and what consequences beyond those intended may be entailed by interference?

I take it for granted that no surgeon would advocate the performance of an operation, which is to lay open the spinal canal, unless the symptoms are so pronounced as to leave no doubt that the cord is so far compressed or irritated, that there is no reasonable probability of its unaided recovery. The diagnosis in this respect may, no doubt, be affirmative in many instances; but whenever such is the case, I am unacquainted with any sign or symptom by which we can judge that the nature and extent of the injury is not such as to render an operation altogether abortive. This dilemma may result from the aggravated nature of the injury, amounting, perhaps, to complete disintegration or severing asunder of the cord; or it may be the consequence of the position of the injury, which is inaccessible to the operator, as exemplified in a case I related in my former course of lectures, in which a spiculum of bone projected into the cord from the *front* of the canal; and it must be borne in mind that, although fracture of the *arch* of a vertebra is by no means infrequent; the injury sustained by the cord is rarely due to this cause, but to concurrent fracture with displacement of the body of the vertebra implicated. On the other hand, complete paraplegia is not necessarily an irrecoverable condition, even when accompanied by fracture; the prospect of amendment depending on the nature of the lesion, and the extent to which the cord is disintegrated: and these are circumstances of which it is impossible to predicate positively, at such early time as an operation would be proposed. But, it may be urged, does not the doubt justify the interference? This question must be answered by ascertaining what additional risk the operation entails; and, in a proper appreciation of the attendant peril, we have, in my opinion, a practical answer to the inquiry.

The *end* proposed in an operation of this kind is, to remove displaced bone which is supposed to press upon or irritate the cord. But, as I have already remarked, however justifiable, on theoretical grounds, such a proceeding may be, it is most likely to prove abortive from the inaccessibility of the displaced bone. Of the many cases of fractured spine which I have on record, and which I have examined *post mortem*, I cannot recall an instance in which the depression of the arch *alone* sufficed to account for the symptoms. I am aware that such cases are recorded, but I speak only of my own experience, and therefore I conclude that they are rare. Yet, in such recorded fractures, which have been usually in the upper region of the spine, death has almost always supervened speedily; thereby proving the serious and probably irreparable nature of the injury inflicted. The expanded form of the vertebral arch is not favourable to its simple fracture, for such injury, with depression, could scarcely occur except as the result of direct violence. That force directly applied to the back of vertebra is likely to produce fracture of the spinous process I have already shown; and fracture of the arch, as a sequence of fracture of the body, of a vertebra, and the result of overbending of the spine, is a typical form of injury—I should say the most common. But the direct violence which would suffice to fracture the arch and drive the spinous process into the cord, the body of the vertebra remaining unbroken, must almost inevitably prove hopelessly destructive to the nerve-tissue; as is illustrated in a

rare and interesting case recently recorded by Dr. Maccormac of Belfast; in this instance, the cord, "had been compressed, almost to complete division."

Again, the question of time is one which must necessarily press upon the surgeon, in contemplating such an operation. He must ask himself, is it justifiable to operate without waiting to ascertain the amount of permanent injury, which, in some instances, time alone can develop? Yet, if an operation is to be performed, certainly delay diminishes any chance of success that may exist; and thus the surgeon is compelled to risk the consequences of an operation which, if unnecessary, very seriously prejudices the prospects of a patient's recovery.

The mode of proceeding adopted by the advocates of this operation is, to expose and remove a part of the spinal column, and to open the canal containing the cord, perhaps to lay bare the cord itself. A simple fracture is thus converted into a compound fracture, and a communication is established with a canal having the most delicate and susceptible relations, and which must be liable to the intrusion of the products of inflammation during such period as the patient may survive the effort to repair the lesion. If the cord have been crushed, and the operation have been consequently useless, probably life may, thereby, be only curtailed; but if (as must be presupposed for the operation to have a chance of success) the cord be not crushed, it appears to me that the best chance of the patient's recovery is thereby extinguished. Indeed, my conviction is that the operation has been advocated on the erroneous hypothesis that the spinal cord can be compressed, without serious disintegration of its texture. I believe this is scarcely ever the case, unless such pressure result from extravasation of blood—a condition which, if it could be ascertained, certainly would not justify the risk of an operation. In the majority of instances probably the amount of compression witnessed in a *post-mortem* examination, is but an inadequate measure of the disintegrating force which was applied at the time the column snapped under the violence to which it was subjected—violence sufficient not only to fracture the arch, but to break through the bodies of the vertebrae, or to wrench asunder the tough intervertebral substance which connects them together.

It is true that cases are recorded in which amelioration of symptoms is supposed to have followed the operation. But are such indications to be depended on? Unfortunately this is a class of cases in which it is almost impossible to affirm what is due to operative interference, as we cannot estimate or limit the amount of improvement which might have occurred, if no operation had been practised.

Certainly instances occur from time to time, as I have witnessed, in which injury of a very unequivocal character, especially in the lower region of the spine, has not been followed by the result which the decisive symptoms seemed to foreshadow. Life has been protracted, and even great amelioration in the general condition of the patient has occurred, though without any marked change in the paraplegic condition. The management of a patient, under these circumstances, is all-important, and is very influential in protracting life. Rest in a suitable position and on an appropriate couch; attention to the bladder and bowels, as well as to the diet and cleanliness of the patient, should be the constant care of the surgeon; and will accomplish much which might be attributed to an operation, where the same advantages had not been so scrupulously secured previous to such interference.

As regards the diagnosis of the nature and extent of the lesion, my own experience affords me no certain or reliable diagnostic guide, except the completeness of the paraplegic condition. I cannot say I place dependence on the activity of the excitomotor phenomena, as developed in an inverse proportion to the amount of damage of the compressed cord; frequently in the most serious and irreparable lesions, the excited muscular movements are by no means strikingly marked; indeed, they are often almost in abeyance.

A parallel has been drawn between the consequences of pressure from the presence of purulent deposit external to the theca vertebralis, and that resulting from traumatic lesion; and also between the effects of curvature from caries and the sudden distortion of the spine from violence. I think such parallel in either case is unreal. I do not deny that traumatic pressure on the cord, without contusion of its structure or intrusion within its theca, may be relieved, and suspended function be restored, by removing such pressure. But there is a vast difference between the slow and gradual encroachment on the cord by an abscess, or by the yielding curvature of progressive caries, and the sudden and violent consequences of fracture with depression of bone, or of distortion from the same cause. Indeed, recovery from the consequent paraplegia in the former class, when the cause ceases to be operative, and the generally speedy death which ensues in the latter, prove this contrast. There is no organic change in the one instance; but in traumatic pressure, even where there is no change apparent to the naked eye, the microscope reveals organic lesion as the consequence of violence,

and hence the fatal result. But if the pressure be due to extravasation of blood, this may be absorbed; and in such cases an operation would greatly diminish the patient's chance of recovery.

The only supposable form of spinal injury which, in my opinion, might be benefited by operation, is a fracture of the vertebral arch alone, with limited depression, or the recent intrusion of a spiculum of bone within the theca, whereby the cord is pricked and irritated. But where are these cases to be met with, and how are they to be recognised?

I fear we must abandon this operation. I have seen it performed on three or four occasions many years since, but certainly not with such results as to induce me to repeat the experiment: and the recorded advantages of more recent cases do not impress me more favourably. Even admitting, as I have admitted, that certain special features might justify surgical interference, I believe that they are rare, and very difficult of diagnosis, if not absolutely impossible to identify. I am satisfied that the risks attending an unnecessary operation are great: and, in balancing these risks against the possible good which may be accomplished in isolated instances, my conviction is that the preponderance is decidedly in favour of non-interference. I am aware it may be urged that any risk is justifiable where a patient's condition is hopeless. But, I rejoin, how are we to identify the absolutely hopeless cases? Some linger on a long time, and even partial recovery is occasionally witnessed under favouring circumstances; and it is precisely these cases in which interference is likely to prove mischievous, by extinguishing the prospect of spontaneous repair, and the chances in favour of the patient's recovery or protracted survival: for, it can scarcely be alleged by the advocates of this operation that, if unproductive of good, it is harmless. To weaken still further the remaining connexions of a broken spine; to convert a simple into a compound fracture; to expose the sheath of the cord and possibly the cord itself; and to entail the risks attending the period of repair,—cannot be regarded as circumstances of indifference. Accidentally, here and there, an instance may occur in which benefit does, or seems to, result from surgical interference; and the time may arrive when, perchance, the means of diagnosis at our command may enable us to judge with more precision of the nature and extent of the injury inflicted: but at present, with every disposition to regard this subject impartially, and to give their due weight to the arguments and facts which have been advanced in favour of this operation, I cannot regard trephining the spine as brought within the pale of the justifiable operations in surgery.

CASE OF SPINA BIFIDA OF UNUSUAL CHARACTER.

By RANDLE BUCK, L.R.C.P., M.R.C.S., Inkberrow.

MRS. F. D. was delivered of a female child on July 4th, 1868; the labour was in all respects a natural one. After securing the umbilical cord, my attention was attracted to a peculiar appearance in the back, which, on closer inspection, proved to be spina bifida, but presenting these curious features. The child lying on its face, there was before me in the lower dorsal region, what, at first glance, looked like a blister produced by cantharides, perfectly circular, and measuring three and a half inches across it. Down the centre, the spinal cord was plainly visible; it seemed raised out of its canal and adherent to the sac, which was distended with fluid. I applied a light bandage and placed the child on a soft pillow, on which it might be carried. The spinal cord being so little protected, I thought death would soon take place. There was a small escape of fluid from the sac, so that the bandage in a few hours became saturated.

The child continued in this state till July 30th, when the escape of fluid above mentioned ceased, and in a few days the head (which had hitherto been of natural proportions) began to increase in size, the sutures became widely separated, the frontal bone very prominent, the eyes depressed and squinting. From this time up to death, on November 8th, both head and sac gradually increased in size, the sac discharging some fluid at intervals. When this occurred, the head diminished in proportion. The circumference of the tumour was 11½ inches; of the head, 19 inches. Examination, twenty-six hours after death. The sac consisted of epidermis and membranes of the cord. It contained a very large quantity of cerebro-spinal fluid, and three inches of the spinal cord with nerves adherent to the sac. The spinous processes of the eleventh and twelfth dorsal and first lumbar vertebrae were missing.

I should mention that, for some time before death, the child was completely paralysed, but there were no convulsions.

ON A CASE OF OVARIOTOMY:

ACUTE PERITONITIS AT THE TIME OF OPERATION: REMOVAL OF A VERY LARGE MULTILOCULAR BLOOD-CYST: LIGATURE OF RIGHT CORNUA OF UTERUS: RAPID RECOVERY.*

By ALFRED WILTSHIRE, M.D., M.R.C.P. London, Medical Inspector H.M. Privy Council; late Senior Physician to the Islington Dispensary; and WILLIAM TYNDALE WATSON, M.A., M.D. Dublin, M.D. Oxon.

THE case which we are desirous of bringing before the Association, is one of so exceptional a character and presents such remarkable features, that we earnestly call the attention of members to it, feeling assured that they will find it to be of unusual interest.

Mrs. S., aged 49, an industrious active woman, married at 18, had nine children and four miscarriages. Her first husband died when she was 34. She remained a widow for nine years. After the birth of her second child, the veins of her lower limbs became varicose; with that exception she had been always healthy in every respect, the catamenia being quite regular. About March 1867, she first noticed that the catamenia were irregular, being too frequent, profuse, and offensive. About the same time some abdominal enlargement was noticed; and it is worthy of remark that, as the abdomen increased in size, the varicose condition of the veins of the legs disappeared. For a year, during which time the abdomen steadily increased in size, she suffered from general ill health; the menstrual function continuing deranged as at the onset. She, however, consulted no medical man, believing that her symptoms were mainly due to "the change of life." From time to time she had attacks of pain and tenderness of the abdomen. Her appetite continued good, and she did not lose flesh, though the tint of her skin was observed to be peculiar; and, at the time of her consulting Dr. Watson, was suggestive of malignant disease. She was, at last, obliged to lie by in consequence of general weakness; and, when she consulted Dr. Watson, March 30th, 1868, she complained of debility, pain, and numbness in the right arm and hand, dyspnoea, nausea, irregular menstruation and pain, tenderness and enlargement of the abdomen.

Dr. Watson examined her, and found the lungs, heart, and chief viscera, to be healthy. *Per vaginam*, nothing abnormal was detected, and the nature of the abdominal enlargement was not determined upon, as there was considerable tenderness and distension. The urine was scanty, but not otherwise abnormal. Diuretics were given, and under this treatment the enlargement of the abdomen subsided to some extent, and although she had attacks of pain in and tenderness of the abdomen from time to time, yet she got about pretty well until Thursday, April 30th, 1868, when, after a long walk, she was seized with vomiting, severe darting pains and tenderness in the right iliac region, and the abdomen rapidly increased in size. Dr. Watson saw her on May 2nd, when he found, in addition to the above symptoms, quick and hard pulse, hurried respiration, and hot skin. The bowels were confined. The urine was thick and high coloured. She was ordered an effervescing mixture with hydrocyanic acid, and a pill containing half a grain of calomel and one of opium every two hours. The bowels were opened by an enema of warm water.

May 3rd. When seen by Dr. Watson, at 10 A.M., she was much worse; vomiting was incessant; the abdomen was now very large and extremely tender. Breathing was difficult. The tongue was small, hard, and dry; the pulse quick and feeble; the skin cold. There was marked collapse. Her condition became more and more alarming in spite of attention, and, as death appeared imminent, Dr. Wiltshire was called in late at night.

Dr. Wiltshire's notes are as follows. May 3rd, 1868. I was called down to Tottenham to see Mrs. S., aged 49, in consultation with Dr. Watson. I found the patient lying in bed with her knees drawn up; countenance anxious and haggard; skin cold; pulse 105 and very feeble; indeed, scarcely to be counted. The tongue was moist and only slightly furred. The extremities were quite cold; and the pressure of the finger left a white spot in the midst of purple skin, the capillaries of which did not soon refill. The abdomen was very large and tender. There was a large hard tumour centrally placed, and it was round. There was fluid in the peritoneal cavity. Vaginal examination gave evidence of the existence of a tumour, which filled the pelvis and pressed much upon the rectum. It feels solid, but it was thought that the feeling of solidity was due to extreme tension. The uterus was normal in size, but somewhat drawn up; and the vagina was thus elongated. Ex-

* Read in the Surgical Section before the Annual Meeting of the British Medical Association in Oxford, August 1868.