

## POST-TRAUMATIC SYRINGOMYELIA

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In 1963 we described a series of eight patients, paraplegic from trauma at or below the fourth thoracic vertebral body, in whom a progressive loss of higher spinal cord function developed and gradually progressed so as to convert paraplegia into varying degrees of quadriplegia.

Of 729 spinal cord injuries cared for in Lyndhurst Lodge Hospital in Toronto, 13 have now developed this complication—an incidence of 1·8 per cent. Altogether we have had the opportunity of observing 15 cases with this progressive syringomyelic complication of serious spinal cord injury.

The development of this complication tends to occur after a variable lapse, usually of several years from the time of the trauma. The time of onset of the upper limb symptoms is independent of the level of the injury. Where the trauma rendered the patients totally paraplegic, the average time interval until new symptoms developed was 4·1 years. When the injury initially left the patient incompletely paraplegic, the time was more than double, at 9·7 average.

It will be recalled that in all cases initial symptoms and signs clearly reflected an asymmetrical central gray matter spinal cord syndrome. Frequently the history suggested pain and/or numbness ascending from the level of the original trauma up to the highest spinal segments. Invariably it began unilaterally involving fibres conducting pain and temperature, reflex fibres, and in some instances anterior horn cells. The picture became bilateral in nine of the cases. In five instances, a severe upper limb involvement produced a very serious disability.

Our concern was, of course, with the worsening of the patient's condition and after much thought and consideration we were able to induce our surgical colleagues to investigate the lesion and try surgical measures for relief.

The present paper, then, in addition to reporting more cases, deals with the surgical measures of treatment used and the results thereof. We are happy to be able to state that significant improvement has been obtained in certain cases surgically and in one or more others there has been apparent arrest of progression.

### Discussion

Mr. P. HARRIS (G.B.). I, of course, agree with Dr. Morgan, and he quoted me as being a non-laminectomy neurosurgeon as far as spinal injury goes, and that is well known. I was aghast at these complications following laminectomy—it is very disturbing to all of us, I am sure, and I would like to ask him if he could give us some idea of the numbers of neurosurgeons and their seniority in this series. Was this one group, one surgeon, as this is terrible that this should happen.

With regard to Dr. Bedbrook, he knows my feelings about this business of stabilisation. I think it is overstressed, and I would agree with most of what he says. I understood that he does not see any patients with delayed neurological damage. I am not too sure that this is correct and he may answer this. And, finally, I might, as far as Dr. Bedbrook's paper goes, ask him about stability of atlanto-axial dislocation. It is a tricky problem—I don't think he had any in his series. I just don't remember. Regarding Dr.

Jousse, I agree with Mr. Hancock about terminology. I wonder if any of these so-called syringomyelic cysts communicated with the fourth ventricle, and if so, of course, this would suggest a different type of therapy to the surgeon.

Dr. H. TALBOT. I should like to stretch my prerogative as Chairman at this point to interject a personal opinion. I think the first two papers of this series of three which we are now discussing are extraordinarily important and should receive very wide dissemination. Each in its own way presents an argument which has long been needed against, what I can only describe, as meddling, interfering, unnecessary surgical intervention. I feel very strongly that the case for intervention whether in the direction of early or somewhat delayed exploratory laminectomy with a view to achieving neurological improvement, the case for intervention in matters of so-called stability has yet to be proved or has yet even to be supported by convincing evidence, except in the rare, unusual instance. In my own clinic, I have recently had occasion briefly to look over the records of our last 600 patients. There were four of those in whom I thought some type of fusion or improvement of stability was indicated—four out of 600. Orthopaedic colleagues make rounds with us from time to time and I am perfectly willing to state that they have picked out a dozen or so more upon whom they would have liked to have operated had I permitted. But, that dozen have done perfectly well and have had no difficulty. Some 20 of the 600 had had so-called stabilisation procedures done before they got to me, and more than half of those still presented the same indications for further stabilisation that they had in the beginning, which has not been done and they're no worse off. I think both of these papers are tremendously important and as I say they must be weighed against the vast amount of unnecessary surgery to which these people are being subjected. So much for my own opinion. Now, Sir Ludwig may have something to say.

Sir LUDWIG GUTTMANN (G.B.). Mr. Chairman, I would like to echo your warning, and I agree Morgan's and Bedbrook's papers are indeed of greatest importance, because they confirm, especially that of Dr. Morgan, on a large scale what is going to happen if people, whether orthopaedic surgeons or neurosurgeons, in particular, rush into laminectomies. I think we have now statistics enough to show the great danger of these untimely procedures to these patients. Regarding the statistics given by Dr. Morgan, I would like to add one point. One must not be misled by the statement that there has been some improvement. This improvement, as we know, occurs in the great majority of cervical lesions who appear at first complete and after the shock or the first few days or weeks pass they begin to recover. What is not included in the statistics of Dr. Morgan—he gets the patients who survive—is that we don't know how many patients have died, and there are only few statistics, given in the literature, where we can make some conclusion. One of them has been published lately by a neurosurgeon from Czechoslovakia. He had a very large percentage of deaths one or a few days after immediate laminectomy in cervical cases and he ascribed this, of course, to the type of injury. He doesn't take into account at all that it is his unwarranted interference in the acute stage following injury which had finished off the patient. It would indeed be very important if Dr. Morgan could enquire from his colleagues from whom he got the patients how many cervical patients died immediately after operation. I think his statistics would be even more disturbing and of greatest value.

Dr. Bedbrook's paper was also an excellent one, because it came a very short time after Dr. Cheshire's paper published in *Paraplegia*, about indication of stabilising operations. We have to be very reluctant to rush into immediate and early fusions, in particular in cervical cases. Just last year I published in *Paraplegia* the result of what can happen after early fusion. Even a man like Clovard who, as you know, is a very radical man in this respect, quite frankly admits that in 50 per cent. of his fused cases they had afterwards a deformity of the spine where the operation was done. Fortunately, nature with its

forces of repair is a great blessing and help. In the case I published last year of a young girl of 16 who had a very simple compression fracture of C6 she was immediately operated by a neurosurgeon, and an orthopaedic surgeon was present. At the operation they discussed the question whether the fracture was stable, and they both decided it was stable and closed up. You can read in the paper, and see from the X-rays taken after the operation, what happened, namely an enormous deformity of the spine. Yet in spite of this operation and in spite of the profound vertebral deformity which followed, the patient is still walking. I saw her last year in South Africa and she is still able to walk a little.

That brings me to an important point which I have emphasised again and again, that is the question of discrepancy between the bony damage and the spinal cord damage. I have written some years ago concerning a patient with a profound lateral dislocation of the first lumbar vertebra who recovered from her initially complete paraplegia afterwards and walked. There is no hurry whatsoever to rush into immediate operation. Watch the patient as I said in a previous discussion in our Society on this subject—watch and observe, and the longer you observe the patient the less you will operate.

Lastly, I have a question to Dr. Jousse. I agree with Dr. Hancock who emphasised the difficulty in the diagnosis of post-traumatic syringomyelia. I think in these cases Dr. Jousse wisely spoke about post-traumatic myelopathy, and I think that is the correct expression. There has been great discussion of traumatic syringomyelia in the neurological literature, I doubt whether this exists. What you have shown is really not a syringomyelia, it is a hydromyelia, probably on a vascular basis. In true syringomyelia there is gliosis throughout the spinal cord, sometimes with a little cyst. I was particularly interested that your neurosurgical colleague found some improvement after this operation, and you said it had 'somewhat improved'. That is, of course, not good enough, and I would like to ask you to tell us more exactly what kind of improvement it was. I had some experience in the 1920s of operations on hydromyelia in co-operation with Professor Foerster. There is sometimes, immediately after operation, some improvement of the spasticity which, however, disappears after a very short time.

Dr. G. BEDBROOK (*Australia*). I would like to ask Dr. Jousse a couple of questions. First of all, how many of the patients that he has seen had originally been operated upon: in other words, how many of them had originally had laminectomies at the time of the original trauma? Secondly, has he any ideas as to the aetiology of this cystic change? I have just a couple of ideas about this. I have had the experience of having one boy with a cervical fracture, a crush injury of C3, with very incomplete tetraplegia. He developed an acute kyphosis, become completely quadriplegic and ultimately died. At post-mortem, we found that the cord, of course, because of the original pathology, was completely tethered and, in fact, had been stretched between two fixed points. Now, I am wondering whether the original pathology of perispinal fibrosis which is so obvious in the post-mortem specimens did have some effect on these cases of Dr. Jousse's.

Mr. D. HANCOCK (*G.B.*). I think we have got to be extremely careful in nomenclature and define exactly what we are talking about. The natural syringomyelia related to developmental defect, the developmental cyst formation, I think is probably safest not to be called syringomyelia. Let us reserve this term for congenital malformation. It may perhaps lead to difficult decisions in treatment, and we may make, perhaps, the wrong decision because we are using the wrong terminology. I understand from the literature that some of these cases of post-traumatic cyst formation have cured themselves spontaneously. I wonder what Dr. Jousse thinks about this, has he more examples to quote of this? I wonder to what extent one needs to operate on them and I wonder very much, too, why, on the side of the spinothalamic loss, the reflexes are depressed or absent. What is the anatomical background to this extraordinary finding?

CHAIRMAN. I will now call on our essayists to close the discussion.

Mr. MORGAN. In reply to the specific question, I would say that approximately two-thirds of the laminectomies have been done by private practice neurosurgeons and probably one-third of the rest at Sinai or Hopkins University Hospital. I would have said that these people were in general competent neurosurgeons. I cannot explain and nobody is prepared to explain but they will say if you cut the laminae away in the acute stage the bruised cord bulges out backwards and the level goes up to however many. But this doesn't explain the bad results, the very much worse results of delayed laminectomy, though this is said to be a vascular change. Nobody has put a reasonable explanation. I think part of the problem in America is that there are many neurosurgeons—many, many more than there are in this country, and I worked here 20 years, often it was extremely difficult to get a neurosurgeon when you wanted one because there were so few, relatively, throughout the country; in America your difficulty is to avoid getting neurosurgeons!

Dr. G. BEDBROOK (*Australia*). In answer to Mr. Harris, first of all I haven't seen any cases of delayed neurological damage after spinal column injury—we haven't had one such case in our series where any thought of surgery might have given them a better result. But, we have had three cases of neurological damage occurring after injury—one which falls into the group which Dr. Frankel reported very adequately in Israel, another which probably falls into that group and a third which I have just mentioned as being the young boy with the high cervical vertebral damage, who went on growing posteriorly with an acute kyphus, with a fixed spinal cord, and this is something I think we should remember.

The second question, stability of the atlanto-axial dislocation, I believe that they are perfectly stable also and I purposely excluded those but just recently I have looked through all the cases coming into the orthopaedic unit at the Royal Perth Hospital—only a small series of about 30 cases and all of them treated conservatively and all of them with perfectly stable spines, but the one I am really worried about is the odd patient who has the so-called whiplash injury who comes back to the department later who has got pain and is then found to have a hypermobile atlanto-axial dislocation.

As far as the 70 cases are concerned, I think I should answer that one; our collection of cases is done mainly because of the great help we get from the Coroner's Department and also because of the extraordinary tenacity with which we follow up all pathology in spinal cord injury, whether it be immediate or late, and so a number of those late cases have died many years later, usually from renal tract infection.

Dr. JOUSSE (*Canada*). In reply to Dr. Bedbrook's question as to how many of them had been operated upon primarily, I don't know. I should look into that. I know that they had not all been operated upon and I know of cases not included in this list who after minor trauma, long after the event, developed progressive paraplegia, which on exploration revealed large, confluent cystic cavities in the cord. I don't know what the pathology is, I have not seen it in the spinal cord and the brain in those who are exposed to blast injuries, without any infiltration of the spinal cord or without any manifest misalignment of the vertebral column. Holmes's cases as originally reported were largely, I believe, gunshot wounds and there were some of them, as we know, where the gunshot wound did not strike the cord directly but lodged against part of the vertebral body and caused irreparable damage to the cord and sometimes cavitation. There was one case, a woman who died over here, very carefully studied by Dr. Blackwood at Queen's Square and some of his colleagues. They did multiple sections throughout the whole length of the cyst, which was a long one in the cord, and there was no communication at any place with the central canal and there was no evidence of ependymal lining of the cyst. And in no case either have we been able to establish a communication with

the fourth ventricle. Now, whether subsequent endeavour will reveal there is a communication sometimes or not I do not know. We have taken that into consideration and it would indeed require a different approach, I am sure.

With reference to the discussion about operation versus non-operation, you may recall some years ago we endeavoured to find out the results of operation on patients who were treated in Toronto, and we found out it didn't seem to matter whether you operated or not, there was no statistical difference. Nevertheless, I think that there must be cases when surgical intervention is beneficial, and I think it behoves the medical profession to find out the indications for operation and to operate on that basis. I may say that I have not been impressed by the number of people operated upon in Toronto who were made worse at the time of operation. I have often wondered when they were operated upon, however, if there is not an optimal time for operation and if that had not been missed, perhaps through delay or perhaps it was done too soon. And, I think this matter has to be studied, and it is being studied, starting with experimental animals all over again, repeating the work of Frasier and Allen and others, and endeavouring to start back there now and find out what they can learn from experimental studies.

## **PSYCHO-SOCIAL ASPECTS OF SEXUALITY IN SPINAL CORD INJURY PATIENTS**

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OF all the problems confronting the spinal cord injury patient, there is none from which it has taken longer to dispel the mists of ignorance than the matter of sexual function. The unwillingness of the medical profession to inform itself on this subject has been incomprehensible and inexcusable. I am sure that mine has been no isolated experience in having known scores of patients who had been solemnly advised that their sex lives were over. Many more had been unable to get any answers at all to their questions. Yet sexuality is an important if not a dominant motive in human life and a sense of sexual adequacy is a major factor in the patient's psycho-social rehabilitation. Nor is there any validity in the complaint that, in the world of today, our sexual preoccupations are too prominent. They are perhaps more readily and openly discussed than was the case 50 to 100 years ago. Yet there have been other periods in which sexual customs were more permissive than they are now. For as long as man has left records of his thoughts and feelings he has filled them with references to the same concerns we know today.

It is strange that definitive studies were so late in coming—that the myth of universal impotence persisted so long. The earliest reference to spinal cord injury that has come down to us—the often quoted description from the Smith Papyrus which is dated around the beginning of the fourth millennium B.C.—mentions the presence of erection. Now, at long last, a sufficient number of careful studies have published to render ignorance inexcusable. But it is no longer sufficient to consider only the bare facts of neuromuscular function and the extent of its departure from normal. Sexual function in modern society cannot be simply equated with sexual intercourse. Sexuality may express itself in a variety of forms other than biological activity; the satisfaction, the sense of adequacy which derive from it may be quite independent of voluptuous sensation. In this, as in many other instances, man has transformed a physiological function into a behavioural pattern. We must