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Discussion: Afternoon Session

Chairman: Sir LUDWIG GUTTMANN

Dr. NEUBAUER (*Austria*) showed pictures of a 26-year-old patient who at the age of 13 (1957) developed a complete paraplegia below T8 following smallpox vaccination. Following rehabilitation, the patient was able to walk with the aid of calipers and crutches but in 1965 an ex-articulation of the left leg from the hip was performed because of a perforating decubitus. The left ischial ramus had been removed previously (1963). The patient was provided with a corset, which, however, produced a deep pressure sore below the right anterior iliac crest, which in 1968 was healed by plastic repair (fig 1). As a result of the removal of the left leg, the patient developed a marked scoliosis (fig. 2). In order to counteract the scoliosis and, moreover, at the same time to relieve pressure from the buttocks, a special suspension corset, fixed on both sides of the side-arms of his wheelchair, was made, as shown in Figure 3, which enables the patient to sit upright and to continue his job as a telephone operator (fig. 4).

Dr. MEINECKE (*Germany*). To Professor Adler: you demonstrated a case named Y. F. who made a very good recovery. I would be interested to know what was his initial neurological state at the beginning of his paraplegia. Perhaps I overlooked it on the slides.

Professor ADLER (*Israel*). This was a very odd case. He had a fracture of C5 and C6, but without dislocation and only slight compression with right monoparesis and right hyperalgesia. He recovered spontaneously with a certain kind of physiotherapy, but I am sure that without this physiotherapy he would have recovered too.

Dr. MEINECKE (*Germany*). To Dr. Katznelson: you demonstrated cases operated upon following traumatic lesions. How long did the follow-up study of these patients last? It would be very interesting to know what happened later on.

Dr. KATZNELSON (*Israel*). These patients treated by the Harrington method are recent cases. With the dual plates we have control of five years. One showed dis-



FIG. 1

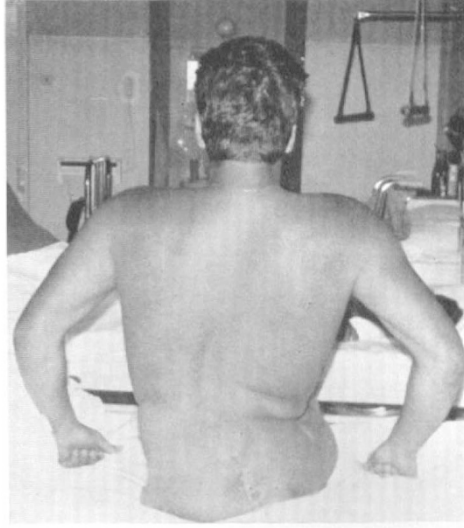


FIG. 2



FIG. 3

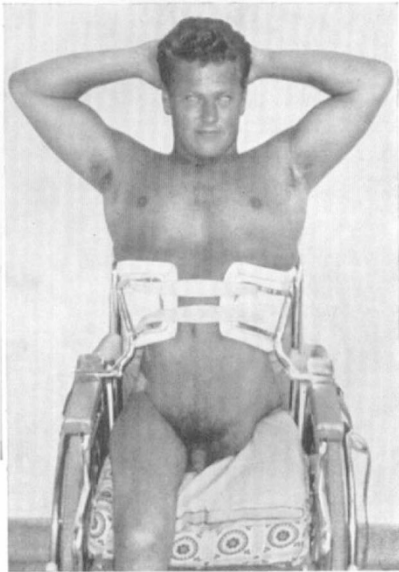


FIG. 4

placement of the plate, and in another there was an angulation, but the plates remained in place. In patients treated by the dural Harrington rods, we have control of only one year. We have used this method only since June 1967. Here again, as in the cases shown by Sir Ludwig, it can evulse if improperly placed, if the bone gives way, or if the hook of the Harrington rods cut into the lamina rather than being placed below it. But these are technical errors. Poor results of poor surgery happen to everybody.

Dr. MEINECKE (*Germany*). Dr. Ikata showed us a system of what is called tidal drainage. We have used that at Bochom, but we have withdrawn it, because it is very difficult to control, and I would like to hear how you control the system, and what are your results.

When do you remove the plaster jacket and shell, what are the results on the spine? We all know that when we use plaster casts for patients with broken spines there is no guarantee that the broken spine will remain in good position in the plaster.

Dr. IKATA (*Japan*). With regard to Dr. Meinecke's first question, the management of our drainage system is done in co-operation with a biologist of our university.

Second question: nothing happens when we remove the plaster. We usually remove the plaster jacket after three or four months. At that time, the consolidation of the injured spine was completed.

Dr. SPIRA (*Israel*). I would like to ask Dr. Katznelson: from my memory and experience, it is a well-known phenomenon that the paraplegic gains two to three segments after the injury after a certain length of time. This is spontaneous, without any surgical action. This is due to the disappearance of the oedema at the site of the lesion. Therefore, if this is the criterion of your enthusiasm, I am inclined to be careful about it.

Secondly, I would like to ask our Japanese colleague. It is not clear to me what was meant by the averaging days of recovery of micturition. Was he referring to the automatic bladder, or was he referring to the first overflow of the catheter, or was he referring to the first voluntary micturition after the catheter was removed? Forty-six days at an average does not convey very much from the point of view of neurophysiological recovery of bladder function.

Dr. HARDY (*Great Britain*). I suppose that I belong to the people who have perhaps the largest experience of the major cutting. Some of you may remember that in Tokyo I did review the operative and non-operative groups of patients. That review has continued, so much so that in Sheffield now, when we have visitors, we do not often have cases of operative interference as illustrations of our major treatment. In other words, we do have a major conservative regime now.

There are two things I would like to say: I have experience of about 800 acute injuries, that is having seen them in a very early stage. I would say that in neither the operative nor the non-operative group have we profoundly altered the course of neurological recovery that might have been anticipated by a careful examination. In other words, the two groups are very similar. We have had cases of conservative treatment which have improved. We have also had operative cases which have improved. So that you cannot necessarily compare one with the other. But, looking at the overall group, I do not think that we have profoundly altered the course of neurology. I would like to think that sometimes we have facilitated recovery by our surgery. In all honesty, I would not do it, if I did not think that we get results. But that is conjecture.

Point number two—Sir Ludwig did show some cases—I hope they were not mine. I looked very carefully at these X-rays. I also looked very carefully at Dr. Katznelson's X-rays, and that was bad technique. There were two plates on the top and bottom of the cases shown by Sir Ludwig, and that again was bad technique. Even if the surgeon is the best surgeon in the world, I would still say that. You cannot condemn a method by bad technique. I think that at Sheffield we have some good techniques.

Third point—we know that deformity, *per se*; does not produce neurological disturbance. I would like to say that it is the forces of displacement which are producing the neurological disturbances. In acute injury you cannot decompress forces which have already been spent. These major forces, which occur at these violent accidents, cannot be compressive forces. The damage is done. I do not think that we are going to do very much in the line of surgery.

Lastly, this question of rolling plasters or rocking stretchers. We are all looking for the same principle—the principle of changing position. I do not mind how we do it, as long as we continue to change position. The best method is the method which is most appropriate to any individual circumstance. You must change the position—I do not think it matters how we do it, as long as we do it well.

Dr. SILVER (*Great Britain*). I would like to ask four questions. Professor Adler and Dr. Katznelson, how were the paraplegics in the recent war transported? I understand that some new treatment was employed on the bones—dressings that were obtained from Viet Nam which gave very good results. Can we be told anything about that?

I would like to congratulate Dr. Ikata and Dr. Hamada on a magnificent paper, which I thought was of great benefit, and I would like to try it out myself. I suspect that it would take more staff to manage it well than our existing methods. Could he tell us a little bit more about the staffing and how the plaster is applied? Is there a special padding of the prominences, or does the plaster go straight on the skin?

I would like to ask Dr. Melzak, were the twins identical?

Professor ADLER (*Israel*). Our medical corps was working excellently, bringing the patients at a most rapid speed by helicopter to the hospital on a stretcher, without doing anything excepting shock treatment. As the battlefield was not so far away, within 15 to 20 minutes most of the people were in hospital.

Dr. IKATA (*Japan*). We used a rocking stretcher—about a staff of 10. We have 40 beds, 20 doctors and 12 nurses. The ratio of nurse to patients is 1 to 4.

Dr. MELZAK (*Great Britain*). All examinations of blood performed including all sub-groups, were identical.

Professor MAGLIO (*Italy*). I have experience of more than 2200 cases, 50 per cent. are acute cases—25 per cent. of them are tetraplegics. At no one time have we carried out laminectomy in cervical lesions, because our experience has shown that if the traumatic lesion has not transected the cord, it is better to wait and immobilise the neck. I think the operation in traumatic lesion of the cervical cord is very dangerous. In lesions of the dorsal and lumbar spine, I had 5 to 10 per cent. laminectomy with very good results.

In my unit we try to move the patients as soon as possible and in the case of cervical fracture we put on a Minerva plaster so that we can move the patient as soon as possible.

As to the rolling plaster referred to by my Japanese friend, we do not use his system. We do not have Stryker beds, we have normal beds. We have a lot of staff and maybe this is the reason. The percentage—beds, nurses, doctors is 1 to 1.8. My people are very well trained and we turn all cases every three hours, night and day. No problems; very good results—and we do not use tidal drainage. In old cases which come to my unit from other hospitals, I am obliged to continue self-retaining catheter, if they have got one, but I try to withdraw the indwelling catheter. In new cases we use intermittent catheterisation and later bladder training is carried out by myself or by my first assistant. I do not know why we must complicate matters with rolling plaster if it is possible to do the turning by men well trained to do this.

Mr. T. MCSWEENEY (*Great Britain*). You must excuse my poor English—(*Laughter*) I was educated in Ireland and I still think in Gaelic—(*Laughter*).

I would like to congratulate Dr. Melzak for a wonderful paper—and very well presented. I was under the impression that these were not identical twins because, as I see it, Gargoylism and Brailsford-Morquio syndromes are examples of an inborn error in muco-polysaccharide metabolism, and I would have expected that if one child had the condition, the other might well be expected to have it too. So I just wondered about this; though he raised the question of genetics and obviously has gone into it, it comes as a surprise to me.

A second point is, I think, the fifth child of the family if he or she has not already been X-rayed, should be X-rayed, because we see a fair amount of this, not with paraplegia but presenting in orthopaedic clinics—and, as you know, the slipper-shaped vertebrae can be identified within the first few months of life.

I would not have the temerity to comment on Sir Ludwig's paper. (*Laughter.*)

Dr. Adler seemed to be concerned about laminectomy. Apart from two, your other cases were gunshot wounds. Now I do not know why you were concerned that they had laminectomy. Surely, in any gunshot wound, it is the first principle that the track of the wound be explored. If your people did not have laminectomies you may find a piece of Israeli uniform wrapped round the cord!

And my last few comments are on Dr. Katznelson's paper and on Mr. Hardy's remarks.

I have a certain amount of experience of open cutting—not in connection with spinal injuries but in the field of scoliosis—and I have the greatest difficulty in persuading myself that Harrington's Rods, or even my colleague's plate, will stay in the place we put them for sufficiently long to allow the soft tissues to heal. Now, I am equally convinced that any cancellous bone can only heal by deformity.

Finally, on Mr. Alan Hardy's comments—a Sheffield graduate is shortly to publish a paper from Oswestry 'unbiased', which I think will make your point. That is, we found a 50:50 ratio. Because we are fairly busy there now, we have given up the question of open surgery.

Chairman. Thank you very much. Ladies and Gentlemen, we are always learning. I have now heard the first Irishman ever to apologise for his poor English. (*Laughter.*)

Dr. LEIDHOLDT (*U.S.A.*) It is fun to sit here and hear an orthopaedic surgeon hold his stand from an 'unbiased' point of view—(*Laughter*)—and a paraplegist speak from his 'unprejudiced' position. This reminds me of the story written after the Civil War in America. It was called: 'An unbiased study of the Civil War from a Southern point of view'—(*Laughter*).

I want to make a comment regarding Dr. Katznelson's paper. I thought that the work done with the Harrington instruments was beautifully done—they were well placed. I have had the chance to do some surgery with Dr. Harrington—I have done some of the Harrington technique for scoliosis and I think the instrument is a good instrument. At a recent meeting in Houston of the Scoliosis Research Society the people felt that this instrument should be used in the treatment of scoliosis. But this means that the fusion of bone must extend beyond the instrument because the instrument fails in a very short time.

This means that if an orthopaedic surgeon is going to use a Harrington instrument he *must* extend the fusion over the *entire* length—otherwise it will break out, loosen and cause difficulty. Now to do this, two segments above the fracture and two segments below is about as close as one can get to the injury, because it is a difficult instrument to put in. That means that too much of the spine is immobilised or fused.

I have seen, in the cervical spine, fusions of the spine two or three segments above an injury, and this is not necessary, it makes the spine too stiff. I have seen, in the low

back, fusions of the lower two segments and then marked osteoarthritic changes occurring above it. So one must save the flexibility of the spine for obvious reasons in the paraplegic.

I would think that if there were a situation—and I am sure there are some—where Harrington instruments were to be used, they should be used temporarily and then removed. Dr. Harrington has done some remarkable corrections of spondylolisthesis—*unbelievable* corrections, correcting third degree displacements of the vertebrae back into almost the normal position, but he plans to remove the instrument. I would think that is the logical way to use that particular instrument.

Mr. P. HARRIS (*Great Britain*). Now English with a Scottish accent. (*Laughter*.) From the neurosurgeon's point of view I would like to make this comment on Professor Adler's paper. I think some of us are in doubt, Professor Adler, how many of your patients had penetrating wounds of the spine and how many had closed wounds of the spine. I would like to be cleared up on this one point please.

Secondly, as one who has written about and spoken about spinal operation in the acute stage, in the very acute stage, in my own experience, is very rarely indicated, but there are indications. I would entirely agree with our President, Sir Ludwig, that bad medicine is *bad*—whether it's bad operation or bad non-operative treatment.

My next point is a comment—that is, that we in Edinburgh in the Spinal Injuries Service are against plating, and the only spinal operation we do at Edenhall Hospital is to take out spinal plates occasionally, if patients should have had plates put in elsewhere.

And the last point I would make is that laminectomy decompression is not a procedure to be recommended at all.

Professor ADLER (*Israel*). Two were non-penetrating wounds, four were fractures; the others were penetrating wounds, without any doubt.

As far as laminectomy goes, I am sure that in our hospital it is done much more than necessary and I am fighting this, but if people go into another department they do it there and they also do other things. I would like to add, for instance, bedsores. In my Department bedsores do not exist. We do nothing else but turning the patient, as has been said here. But sometimes patients come into our Department, either from other hospitals or Departments, who do have bedsores.

Dr. BEDBROOK (*Australia*). I would like to ask Dr. Melzak if he has come across any cases of achondroplasia with paraplegia and, if so, what has been the aetiology? He has talked about the Morquio-Brailsford Syndrome—I wonder if he could tell us exactly why he thinks the paraplegia occurs and what might be the pathology behind it?

I think the study of this pathology is going to be interesting from the point of view of the incidence of paraplegia, not only in the non-traumatic cases but sometimes in the traumatic ones.

Secondly, I would like to ask Dr. Katznelson three questions. First of all, he talked about cases where he had demonstrated pressure on the cord. This has not been my experience. Pressure on the spinal cord in trauma can easily be reduced by conservative means—and, in fact, in my opinion is best relieved by such.

Secondly, he talked about lesions which were irreversible without surgery. Once again, most of the lesions I have seen have not been helped by surgery at all—and this has been a very careful study for us over some period of time.

And, thirdly, I noticed as an orthopaedic surgeon that he was using Harrington's distraction rods—and I wondered why.

The last point I would like to make, is I believe that in increased deformity of the spinal column seen after spinal fractures—that has been so readily demonstrated by a number of speakers today—this is a question of avascular necrosis of bone; and this has been a factor which British colleagues will know a lot better than I in tuberculosis

of the bone but has not been readily recognised in trauma. But a colleague in Melbourne has done quite a lot of work on the blood supply of the vertebral body, and from our own studies we can demonstrate a vascular necrosis in bone which, of course, takes a long time to slowly necrose and to slowly collapse—and this, undoubtedly, is one of the reasons for this long-term collapse after vertebral fractures. Indeed, this is undoubtedly one of the reasons why plates and so on cut out and is another good reason for not using a plate.

In some 400 cervical fractures, which I have very carefully studied, some over 80 per cent. of them fused by bone; another 12 per cent. fused by non-bony methods but were stable; and only about 8 per cent. were unstable—in the term that after three months there was enough vertebral movement to worry us. That was in the cervical area. In the lumbar-dorsal area, where I have been helped by David Cheshire in Melbourne, we have followed up over 250 cases and we have found that only two cases have had non-union after a period of some years. But there have been some with considerable deformity—not one of them over 90°. I would suggest that it is after 90° that we have got to get worried and I really think that that boy of mine who became tetraplegic over a period of time was a problem of anchoring of the cord and slow stretching of the cord over a long period of time.

Dr. KATZNELSON (*Israel*). First, I have to correct a misinterpretation by Dr. Ralph Spira. Not all cases recovered. Two had no improvement. But, on the other hand, two have complete recovery, but for their urinary and bowel functions.

From the point of view of stabilisation by the Harrington rods, I have used the more solid, the more compact distracting rod rather than the mobile, springy hooks of the compression system. I was able by those means to maintain the height of the vertebra. True, spongy bone heals a spongy bone, whether it is a fracture of the Colles type, heel or otherwise. However, by adding bone posteriorly, when doing a lateral gutter fusion, I was able to add stability.

As yet, our rods are in place. I am well acquainted with Dr. Harrington personally. I have been to Houston and have been trained there and I know that he only removes them after at least a year. Our patients are now coming close to a year. As yet, I have not got a fractured rod in the paraplegics; I have had a fractured rod in my scoliotics.

I did find severe pressure on to the cord in two cases—where there was a segment of bone, about 1 cm. by 1 cm. unilaterally compressing the cord; it was not symmetrical on both sides and it is this section that I have taken out.

Dr. TALBOT (*U.S.A.*). Surgery cannot be justified simply by saying that it does no harm, that the morbidity is slight and the mortality is zero. This is no justification for doing an operation—although we hear it very often. A surgical intervention must, to the best of your honest consideration, give some hint or some possibility of doing the patient some good.

Now, confirming what Dr. Hardy said, the publications of Bors and Comarr with a series of some 850 patients, divided roughly equally between those who had had laminectomy and non-intervention patients, were totally inconclusive. It was absolutely impossible to demonstrate by any statistical evidence that the patients who had had intervention had done any better. On the other hand, it was equally impossible to demonstrate that the ones who had not been operated upon had done better. We can always fish up out of our own experience some successful cases and talk about them—either successful because of what we did or because of what we did not do. And I dare say that those of us who are generally conservative can look back and, if we are honest, recognise in our experience patients to whom we did some damage by not intervening; but I think that the number of these is very small compared to the patients who have been damaged by injudicious surgical intervention.

I think it is wrong to look for patients for operations. You have to look for the

operation or the non-operative treatment that will fit the patient—and we are entitled to have a basic philosophy (as I have said mine is conservative), and yet we must be prepared in any individual case to depart from that, or to change it if necessary. I should like to see a little bit more individualisation, a little less tendency to generalisation and over-simplification in all of these discussions.

Dr. W. J. WHATMORE (*Great Britain*). How encouraged I was to hear Dr. Talbot's remarks. As a young neurosurgeon in training, I am a member of this Society and I come to these Meetings to learn the techniques, the treatment of spinal deformities and spinal injuries, but instead of going away with knowledge I feel at times, and I felt the same thing two years ago, that I am going away just with confusion.

I am particularly interested in surgery and in the complexity of this problem of the use or misuse of surgery, as I am a surgeon myself. May I say, Sir Ludwig, I was extremely interested to hear your paper on the problems of spinal surgery causing spinal deformity, particularly because in my five years' experience in neurosurgery I have seen very few of these complications. It would seem to me that the whole key to the solution of this problem and discussion of the misuse and use of surgery lies in the words you used at the beginning of your paper, sir, when you discussed the timing and the type of surgery which should be indicated. Surgery is not just an operative technique: it encompasses a wide field of admission of patient, treatment, diagnosis and assessment, and I feel that all these factors should be taken into consideration and each case should be judged on its own merits.

The operation should not be untimely and indiscriminate. As Sir Ludwig indicated, this should be avoided and surgery should be carefully timed and the proper and adequate surgical technique employed. I feel that if these criteria are observed then many of these complications should be avoided.

Dr. J. MELZAK (*Great Britain*). I do not know of a case of achondroplasia with paraplegia. We had one case of achondroplasia who also had an accident.

As far as the cause of paraplegia in our case is concerned, in the second case of Fiona we did a myelogram and the myelogram showed a complete block. She was operated on in Oxford and it was found that she had a stenosis of the canal. The structure of the vertebrae was changed: instead of having a bony consistency it was fibrotic and soft and it is being examined histologically under the electro-microscope.

As far as the identity of the twins is concerned—I asked this question of our Haematological Department. The first child, Alison, has the blood group A positive; the twins have a blood group A₁ negative—and all the examinations of sub-groups many of which he has performed were all identical.

Chairman, Sir LUDWIG GUTTMANN. If there are no more questions and comments, it is now my pleasant duty to summarise.

First, I just want to enlarge on what Dr. Melzak said regarding Dr. Bedbrook's question. I would refer to a paper of Dr. Hancock's, published in *Paraplegia* last year, who has drawn attention to stenosis of the vertebral canal as part of the mal-development in achondroplasia which gradually may lead to compression of the spinal cord and which may then be relieved by decompressive laminectomy.

I was interested in Mr. Whatmore's remarks, probably one of our youngest colleagues. I sympathise with him that the controversial views expressed today on the deformity of the spine and its management has put him in a state of confusion. But he certainly understood my own philosophy on conservatism and surgery in injuries of the spinal cord. To one who as a neurosurgeon has learnt from personal experience, it is the *timing* of operation and not the question of whether or not operation, which is the decisive problem. I can only repeat this again and again. It is the timing through careful neurological examination and observation of the patient which must be done

first. Unfortunately the last thing surgeons learn in the course of their experience is to know when not to operate but to wait and watch progress, allow time and give Nature a fair chance. If people tell us that after an operation the patient improved in two or three segments that does not cut any ice with those who have experience in this matter, because this, as a rule, takes place anyway whether you operate or not.

In injuries of the lumbar spine with cauda equina lesions one of the main reasons for open reduction without or with plating was to promote earlier recovery of the roots. I am still waiting for the proof, and I am glad that Dr. Hardy himself has made it clear that there is no proof for this; in fact he had made this point clear already in Tokyo. Dr. Hardy mentioned bad technique in plating, a point which also was made in 1954 by our friend Holdsworth. That is quite correct and I agree that there is bad conservative treatment and bad surgical treatment. But there is no doubt that even the best technique of plating cannot prevent later angulation, *i.e.* deformity of the spine when the initially erected broken vertebra collapses later as a result of avascular necrosis. That is probably the main reason why Wilson in U.S.A., the originator of plating, and others have abandoned this method. In this connection Dr. Leidholts comments on difficulties with Dr. Harrington's stabilising technique of the spine using rods are of great interest. At Stoke Mandeville, as I mentioned in my paper, many plates which had been put in elsewhere had to be removed.

I am well aware—and it has also been indicated by Holdsworth long before it was confirmed in the United States—that plates should only be put in temporarily and remain until the spine is stabilised, and then they should be removed. Of course, one can ask again: Are these two operations really necessary, except in very, as also mentioned by McSweeney, rare instances, as the readjustment forces of nature can consolidate the broken spine as a rule, perfectly.

This also applies to the conservative treatment by plaster casts as introduced by Dr. Ikata's new method. He removes the plaster cast only after three or four months when the fracture is consolidated. Of course, this can be achieved even earlier by more simple methods. On listening to Dr. Ikata's paper and illustrations and considering the various other approaches to the management of spinal cord injuries it occurred to me that our society can now be divided into four groups—the cutters, the turners, the rollers and the rockers. (*Laughter.*) However I was very pleased to learn that by Dr. Ikata's turning-plaster-beds method sores did not develop. I asked Dr. Ikata already in Hong Kong: 'How is the condition of the back muscles after you remove the plaster?' From my personal experience, I suspect they will be rather weak. I have broken my spine too—not from an experimental point of view, of course—(*Laughter*)—and I made the experiment to have a plaster cast made as, fortunately, I had no paralysis. I did everything that Professor Böhler from Vienna had advocated with the exception that I did not have a weight on my head, as I had to continue my work in the hospital. But when the plaster was taken off the paralysing weakness of my back muscles as a result of the continuous pressure from the plaster was very marked for three or four weeks afterwards. And that is the point I should like to make to this type of management, and in spite of Dr. Ikata's personal good result, the danger of pressure sores appears to be pretty great.

A very important point in our discussion on spinal deformity was mentioned by Dr. Bedbrook, namely why, after any stabilising operative treatment, the broken vertebra may collapse later. He was absolutely right to stress the often inevitable avascular necrosis as the cause. The broken or compressed vertebra, even if well erected, may collapse later because of the avascular necrosis, as I could show you in those X-rays of cases following open reduction and plating. And in my view it does not make any difference whether one employs Lane's, Wilson's, Meurig-Williams plates or Harrington's distracting rods.

Dr. Melzak's and Dr. Audic's papers on vertebral deformity due to congenital malformations have been of special interest to all of us, and I was glad that Dr. Audic

and Dr. Maury confirmed the development of kyphosis and scoliosis following laminectomy, spinal osteotomy and scoliosis, especially if the nursing after these operations is not correct. A very important point our French colleagues have made by drawing attention to the effect of marked spinal deformity on the renal system which I can confirm from my own pyelographic studies in such cases.

Finally, I feel we should be more precise when we talk about infection of the urine. I would really like to know what is meant by 'slight infection', as mentioned by Dr. Katznelson and other speakers. In our highly specialised society of paraplegia when this most important subject is mentioned in discussion on management of patients the various degrees of infection should be clearly defined as otherwise the whole report is useless for true assessment of the method employed. I also hope that the time might come, when our friend, Professor Adler, when referring to paraplegics transferred to his department from other departments or hospitals following initial treatment, will not say: 'Of course, the bladder was infected'—but 'of course, the urine was sterile'.

Now, Ladies and Gentlemen, I think we have had a most interesting and instructive day, and we have all learnt something. I also feel that we are getting closer in our approach to the all-important problem whether and when surgery is indicated in the *immediate* and *early stages* of traumatic paraplegia and tetraplegia. In this respect it was, in particular, gratifying to hear the prevailing conservative attitude towards laminectomy and stabilising operations in traumatic tetraplegics, whether they are complete or incomplete, by most of the speakers.

I wish to thank all those who have given their interesting papers and also all those colleagues who have taken part in what I would call a very useful discussion.

Thank you very much.