

LETTERS TO THE EDITOR

Dear Sir,

The paper "Unstable trochanteric fractures. A comparative analysis of four methods of internal fixation" by J. Steen Jensen et al. in *Acta Orthop. Scand.* **51**, 949–962, 1980, discusses an extremely topical and interesting problem.

One cannot but agree with the authors that "the internal fixation of unstable trochanteric fractures is primarily a mechanical problem". It should logically follow, then, that a mechanically correct solution to the problem be adopted. However, to judge from the X-rays presented, the failures illustrated by Figures 6A, 6B, 7A and 7B should in my opinion be attributed to faulty technique rather than to any inherent inadequacy of the Ender method as such. In the first of these cases more favourable results might have been expected if a better distribution of the nails in the femoral head had been achieved. The same applies to the second case, in which, moreover,

longer nails should have been used and the point of insertion in the knee region should have been properly located to provide the necessary support for the nail by allowing it to rest against the supracondylar region of the femur. If these examples are representative of the technique used in all cases treated by Ender nailing, which after all has the advantage that integrity of the medial cortex does not constitute an indispensable requirement for stability as it does in nail-plate fixation, I feel that the conclusions regarding the final results should have been stated somewhat less categorically.

It is also surprising that the authors found a higher infection rate in the series treated by Ender nailing, despite the fact that this technique has the additional advantage of avoiding deep tissue dissection and as a rule involves a shorter operation time than the nail-plate type of internal fixation.

Julius Soreff, M.D., Department of Orthopaedic Surgery, Karolinska Hospital, S-104 01 Stockholm, Sweden

Dear Sir,

The above letter calls for a few comments. Concerning the rate of deep infections following surgery in the hip region our percentage of less than 4 per cent is consistent with the figures given in previous reports from departments of orthopaedic surgery in the Copenhagen area (*Acta Orthop. Scand.* **46**, 795–803, 1975; **49**, 392–397, 1978; **51**, 811–816, 1980). As stated in the paper under discussion no significant difference was found between surgery in the hip region

and that in the supracondylar area (Ender nailing).

We certainly do not agree with Dr. Soreff regarding a less categorical conclusion about the use of Ender nails for the internal fixation of unstable trochanteric fractures. This is because even improvement in the operative technique is not liable to solve the problems of unstable fracture reduction without bony support. The magnitude of the hip joint load during walking is approximately three to five times the body weight and

will inevitably lead to secondary fracture dislocation until a reliable weight transmission system is established. It is agreed, however, that the operative technique has not been perfect in all cases, which is due to the fact that these patients are most often operated on by orthopaedic surgeons with limited skill, as is the case in the hard-working, municipal emergency departments. It should be added that this was also the situation as regards the other methods included in the study, which is demonstrated by the equally

low rate of successful reductions of unstable fractures, irrespective of the method used.

We are still of the opinion that the sliding screw plate fixation method is the only one suitable for the internal fixation of unstable trochanteric fractures.

For further information it is recommended that one should read *Acta Orthop. Scand.* **51**, 817–825, 1980 and Suppl. 188, vol. **52**, 1981.

J. Steen Jensen, S. Sonne-Holm & E. Tøndevold, Department of Orthopaedic Surgery T-2, Gentofte Hospital, Hellerup, Denmark

Dear Sir,

Carlsson et al. (1980) recently presented a report in this journal on "Function of fifty-seven septic, revised and healed total hip arthroplasties". They compared their results with ours (Ahlgren et al. 1980) and Clegg's (1977) after simple removal of the prosthesis.

That patients perform better with a hip prosthesis than without should not be unexpected.

The way Dr. Carlsson and coworkers compare the series can not, however, pass without comment. They use as a basis a material of 77 infected and revised total hip arthroplasties (Carlsson et al. 1978). From this group 25 cases (every third patient) were *excluded* mostly because of persistent or suspected infection. To the remaining 52, five new, revised and healed cases were added making a total of 57. After having thus excluded their own failures they proceed to make a comparison with our unselected material and that of Clegg. They especially stress that none of their patients needed crutches or a wheelchair which was the case in 5 patients after removal only. How did their own 23 excluded survivors manage? The condition of these patients should be taken into account before a conclusion can be drawn that one-stage revision with gentamicin-containing bone cement is the best method in most cases.

Furthermore, the authors state that their rate of healing of the infection after revision arthroplasty (78 per cent) was "almost the same" as following simple removal. Out of 56 cases, 51 (91 per cent) healed permanently, 30 primarily and 21 at a later date.

Carlsson et al. (1980) also declare that their small number of patients does not make a statistical analysis of the difference between one-stage and two-stage operations meaningful. All the same they advocate the one-stage operation.

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Dear Sir,

Dr. Ahlgren presents two main objections to our paper "Function of fifty-seven septic, revised and healed total hip arthroplasties" (Carlsson et al. 1980):

- a) Our selection of patients.
- b) Our comparison of the rates of healing in revised cases with those in extracted total hip arthroplasties.

Re a) Neither Ahlgren et al. (1980) nor Clegg (1977) have tried to distinguish between the function of healed cases and cases still infected following extraction of a total hip. In our opinion there is also reason to believe that if there is such a difference it is negligible. On the other hand, it is well known that function is poor in total hip arthroplasties with deep infection.

To compare the function of our healed cases with a material of extracted hips therefore suggests what may have happened with *all* our patients, with respect to function, if the method of exchanging prostheses with the use of gentamicin-containing cement had not been available. The poor mechanical function following the Girdlestone procedure is well established in the literature, most recently by Hamblen (1981).

Re b) The rate of healing in our material was 78 per cent. However, some of the cases classified as doubtful with respect to healing of the infection finally proved not to be infected. Other patients healed after a second revision and altogether between 80 and 90 per cent of our revised hips finally healed.

Clegg (1977) found that 24 out of 30 cases (80 per cent) healed but 8 only after secondary surgery. Hunter & Dandy (1977) described 85 cases with healing in 75 (88 per cent) but 19 only

after further surgery. Hamblen (1981) found recurrence of "deep sepsis" in 11 out of 45 cases (24 per cent).

Referring to the data above we still find it justified to express the healing rate as "almost the same" following exchange operation and extraction. However, we apologize for referring to Ahlgren et al. (1980) in this context. They describe a 100 per cent healing rate without any further surgery. The course of their 8 cases with "persistent or recurrent drainage for longer periods" remains obscure since the criteria for healing are not presented.

Dr. Ahlgren also objects to our preference for one-stage exchange operations compared with two-stage procedures. It is perfectly correct that our material does not allow any meaningful statistical analysis concerning function in these two groups. We, however, believe that most readers of our paper would be aware that our recommendations are based on bacteriological considerations and the experience that two-stage procedures are technically more complicated and cause more strain on the patients.

We thank the Editor and Dr. Ahlgren for the opportunity to clarify some of the points in our paper.

REFERENCE

- Hamblen, D. L. & Sisher, W. D. (1981) Girdlestone excision arthroplasty as a salvage procedure for failed total hip replacements. The Second AOA International Symposium, Boston, Massachusetts, May 27-30.