We welcome letters to the Editor concerning articles which have recently been published. Such letters will be subject to the usual stages of selection and editing; where appropriate the authors of the original article will be offered the opportunity to reply.

Letters should normally be under 300 words in length, double-spaced throughout, signed by all authors and fully referenced. The edited version will be returned for approval before publication.

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The influence of immediate surgical treatment of proximal femoral fractures on mortality and quality of life

Sir,

We read with interest the article by Dorotka, Schoechnner and Buchinger¹ in the November 2003 issue entitled 'The influence of immediate surgical treatment of proximal femoral fractures on mortality and quality of life: operation within six hours of the fracture *versus* later than six hours'.

We are concerned about the internal validity of this observational study. Due to lack of randomisation, known and unknown differences could not be balanced.

The study does not mention the outcome in different age groups. Younger patients with hip fractures are brought to hospital early when compared with elderly patients especially those who live alone. The two groups studied differed with respect to age and walking ability, with those operated on earlier being younger and presumably fitter. While this difference was not statistically significant, had a larger sample of patients been used it would have reached statistical significance and the conclusions of the paper would be different. Furthermore, the survival curve demonstrates a difference in mortality which becomes apparent one to two months after surgery. This was some time after the initial surgery, an indication that the differences in outcome were due to the general health of the patients, not the timing of surgery.

The study is certainly commendable in demonstrating that it is possible to operate on these patients shortly after the injury which suggests this may have a beneficial effect on outcome. Now, we must set about evaluating the effects of the timing of surgery in a more rigorous fashion.

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1. Dorotka R, Schoechnner H, Buchinger W. The influence of immediate surgical treatment of proximal femoral fractures on mortality and quality of life: operation within six hours of the fracture *versus* later than six hours. *J Bone Joint Surg [Br]* 2003;85-B:1107-13.

Author's reply

Sir,

We appreciate the interest of Messrs Pervez and Parker in our study. We cannot confirm that younger patients with hip fractures are brought to hospital earlier than elderly patients as there were four patients in each of our study groups who were less than 60 years of age, with the youngest patient being in the delayed group. The two groups differed minimally with respect to age (77.1 *vs* 79.3 years) and walking ability; however, the differences were not statistically significant. Physical health graded by the American Society of Anesthesiologists (ASA) was identical (3.2 *vs* 3.3) in both groups. We agree that a more rigorous method for evaluating the effects of the timing of surgery must be established. Further studies with a prospective and randomised design and a larger number of patients are required. However, the number of patients is a limiting factor. In order to achieve statistically significant differences with respect to age between our groups, a sample size of 468 in each group would be needed, and in order to calculate statistically significant differences with respect to the ASA grade 1570 subjects would be needed.

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Arthroplasty of the elbow in haemophilia

Sir,

We read with interest the article by Chapman-Sheath, Giangrande and Carr¹ in the November 2003 issue entitled 'Arthroplasty of the elbow in haemophilia'. We would agree that there is a lack of available information on arthroplasty of the elbow in haemophilia. We compliment the authors on the largest published series of cases with short-to-medium follow-up. However, the article lacks sufficient information on evaluation of total elbow replacement (TER) in this condition. We would like to mention a few points.

The indications for TER are not clearly mentioned. The authors have quoted a reference stating, "excision of the head of the radius combined with arthroscopic or open synovectomy has yielded consistently good results with reduction in pain, an increased range of movement and a decrease in the frequency of the joint bleeds".² They have suggested that replacement arthroplasty may become necessary if destruction of the joint continues. It is unclear why the authors recruited patients for elbow replacement without considering less invasive procedures.

All the patients in this study were satisfied with the outcome. No mention has been made regarding the criteria of satisfaction used for the assessment. It would be interesting to know if the patient with septic loosening was satisfied with the result.

Similarly, the criteria used for assessment of pain have not been described. The authors have mentioned post-operative pain relief

in only six cases. The text lacks detail about one case. We imagine that the elbow which had septic loosening was omitted from the study.

As other studies have shown a high complication rate and poor outcome^{3,4} and the authors themselves consider total elbow replacement as an experimental procedure, we feel that it may not be appropriate to perform TER without comparing its results with excision of the radial head and synovectomy for similar indications.

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2. Rodriguez-Merchan EC, Galindo E, Magallon M, Cage J, Villar A. Resection of the radial head and partial open synovectomy of the elbow in the young adult with haemophilia. *Haemophilia* 1995;1:262-6.
3. Luck J, Kasper C. Surgical management of advanced haemophilic arthropathy: an overview of 20 years' experience. *Clin Orthop* 1989;242:177-88.
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Author's reply:

Sir,

We thank Messrs Choudhary and Ahmed for their interest in our paper. The indications for total elbow replacement are clear, with pain and significant disability arising from loss of movement in the elbow joint. An alternative treatment is excision of the radial head and synovectomy and indeed we have performed this in a number of cases. In the series, we reported the amount of joint destruction and pain clearly arising from the whole joint which made this option very unlikely to be successful. The one case which developed infection and loosening had pain prior to the revision surgery but has done very well since and the clinical result is recorded in the paper.

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Arthroscopic debridement in the treatment of the infected total knee replacement

Sir,

I read with interest the article by Dixon, Parish and Cross¹ in the January 2004 issue entitled 'Arthroscopic debridement in the treatment of the infected total knee replacement'. The authors have discussed a timely and worthwhile topic.

A critical issue in the salvage of prostheses in patients with infected knee replacements is the time interval between the diagnosis of infection and the washout.² The authors have commented on the time interval between the arthroplasty and arthroscopic debridement but not the time interval between the diagnosis of infection and debridement. This is likely to have a significant

impact on the percentage of prostheses retained. The criteria for establishing the diagnosis of infection before arthroscopy have not been detailed. In a study describing the successful control of infection, the basis of diagnosing infection pre-operatively needs to be clarified.

The mean interval between the arthroplasty and arthroscopic washout was 35 months and this has been labelled as 'chronic' infection. Perhaps, an alternative term could be 'late infection' with the terms acute and chronic being reserved for the duration of infection in the joint. The selection criteria for patients for arthroscopic washout is unclear and in the absence of a control group, the applicability of the results to patients presenting with infected knee replacements is doubtful.

I do not think that the evidence presented in this paper is convincing enough to change clinical practice.

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1. Dixon P, Parish EN, Cross MJ. Arthroscopic debridement in the treatment of the infected total knee replacement. *J Bone Joint Surg [Br]* 2004;86-B:39-42.
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Authors' reply:

Sir,

We thank Mr Agarwal for his interest in our paper and raising some valid points. Infection in TKR is not always an easy diagnosis to make and has a spectrum of presentation. Our criteria for making the diagnosis were based on a high clinical suspicion plus erythrocyte sedimentation rate/C-reactive protein and a positive joint aspirate/biopsy either prior to or at arthroscopic debridement. Of the 15 TKRs in our study five had a positive aspirate prior to arthroscopic debridement. The longest delay between positive aspirate and arthroscopic debridement was six days. However, we feel that the time from definitive diagnosis to debridement is misleading in terms of how long the infection has been present within the knee, as most patients in our study had been symptomatic prior to positive culture and probably had ongoing infection although we cannot prove this. We take Mr Agarwal's point regarding "chronic" and "late" infection but feel that the two terms are not mutually exclusive. The point at which a TKR becomes infected is often very difficult to ascertain and we feel that although infection may be "late", the majority in this study were also "chronic".

We would like to emphasise that the technique described is much more than just an arthroscopic washout. It is a thorough debridement and synovectomy of the joint and demands a level of expertise in knee arthroscopy.

This technique is useful in that it is relatively non-invasive, can be diagnostic and can be curative.

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