

CORRESPONDENCE

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.

REAMED OR UNREAMED NAILING FOR CLOSED TIBIAL FRACTURES

Sir,

We read with interest the paper by Court-Brown et al¹ in the July 1996 issue (1996;78-B:580-3) comparing the use of the AO Unreamed Tibial Nail (UTN) with the Grosse-Kempf reamed tibial nail in the treatment of Tscherne type-C1 tibial diaphyseal fractures. They concluded that they could not support the use of the unreamed tibial nail in these fractures because of delayed time to union, an increased incidence of nonunion and malunion and a high rate of cross-screw breakage.

We wish to raise two points in regard to this study. First, Figure 2 is shown as an example of cross-screw fracture with distal migration of the nail into the ankle. It is used to support the contention that the UTN is unsuitable in the treatment of *diaphyseal fractures*. This figure shows a long oblique juxtametaphyseal fracture with the UTN singly locked distally. This does not represent most diaphyseal fractures of the tibia and we do not think that the authors are entitled to draw their conclusions on this basis.

Secondly, it has been recommended that when using the UTN early weight-bearing should not be encouraged and that it should be preceded by dynamisation² or by removal of the cross-screws.³⁻⁴ In following a protocol 'not recommended by the manufacturer' the authors are not using the implant in its appropriate context and therefore we feel that their conclusions may be invalid. The UTN is a useful device when used selectively and we do not think that there is yet sufficient evidence to abandon it.

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1. Court-Brown CM, Will E, Christie J, McQueen MM. Reamed or unreamed nailing for closed tibial fractures: a prospective study in Tscherne C1 fractures. *J Bone Joint Surg [Br]* 1996;78-B:580-3.

2. Haddad FS, Desai K, Sarkar JS, Dorrell JH. The AO unreamed nail: friend or foe. *Injury* 1996;27:261-3.

3. Angliss RD, Tran TA, Edwards ER, Doig SG. Unreamed nailing of tibial shaft fractures in multiply injured patients. *Injury* 1996;27:255-60.

4. Boenisch UW, de Boer PG, Journeaux SF. Unreamed intramedullary nailing: fatigue of locking bolts. *Injury* 1996;27:265-70.

Sir,

We read with interest the article in the July 1996 issue by Court Brown et al¹ entitled 'Reamed or unreamed nailing for closed tibial fractures' (1996;78-B:580-3). These authors have contributed considerably to the understanding of the healing of tibial fractures but we are concerned about two points which they raise.

We believe that there is a difference in the healing pattern between reamed and unreamed fixation, but that the differences in these authors' patients were caused specifically by five patients in one group who failed to heal. Their Figure 2 shows a very low oblique tibial fracture treated by an unreamed tibial nail with a single locking screw. The fracture was unstable and would not be held to length by the single screw which would be expected to break. We do not think that most surgeons would have used this implant here and therefore question whether it is correct to use such a patient to analyse the healing of what were presented as matched fractures.

In Table II, six of the eight parameters which evaluate recovery of function show significantly more improvement in the unreamed than the reamed group, although the authors state that healing is slower in the unreamed group. We would expect recovery of function to run parallel with healing and find these statements contradictory.

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1. Court-Brown CM, Will E, Christie J, McQueen MM. Reamed or unreamed nailing for closed tibial fractures: a prospective study in Tscherne C1 fractures. *J Bone Joint Surg [Br]* 1996;78-B:580-3.

Author's reply:

Sir,

Both letters comment on our treatment of a low oblique tibial fracture using one distal cross-screw. This is not a particularly low fracture or indeed usually very difficult to treat. Experience over the last ten years has suggested that if a larger reamed nail is used one cross-screw is adequate for this type of low-velocity fracture. Clearly, Smith and Matthews are correct in stating that it is an inappropriate construct with an unreamed nail and we have now reverted back to the use of reamed nailing.

We accept that we did not follow the manufacturer's recommendations regarding early weight-bearing. We do not believe that given the success of reamed intramedullary nailing in allowing early weight-bearing it is now reasonable to deny this to patients with these relatively simple fractures. There is no evidence that dynamisation affects bone union and it is interesting that

Haddad et al,¹ quoted by Tennent et al, reported that six of 16 patients who were not dynamised showed screw breakage, a true failure rate of 37.5%.

The last point made by Smith and Matthews is of particular interest. The similarity of outcome shown in our Tables I and II indicates that intramedullary nailing is a useful method of treatment regardless of whether reamed or unreamed nails are used. We believe that the concept that recovery of function runs in parallel with bone union is a hangover of the days of cast management and that this is no longer the case with adequate modern fixation.

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1. Haddad FS, Desai K, Sarkar JS, Dorrell JH. The AO unreamed nail: friend or foe. *Injury* 1995;27:261-3.

MANAGEMENT OF PERTHES DISEASE OF LATE ONSET IN SOUTHERN INDIA

Sir,

I read with interest the article in the July 1996 issue of the Journal by Joseph et al¹ entitled 'Management of Perthes disease of late onset in Southern India' (1996;78-B:625-30), but would like to know if any complications were encountered in this series of 48 children undergoing osteotomy to improve containment of the femoral head. I note that the text describes implant loosening with some increase in varus angulation in only one hip and yet the illustration in Figure 6b, outlining the progress of an eight-year-old boy, clearly shows that the plate has broken. The authors state that the next illustration indicates that there has been some healing of the head although the plate is different. Has the plate been removed and changed or is this a different patient?

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1. Joseph B, Srinivas G, Thomas R. Management of Perthes disease of late onset in Southern India. *J Bone Joint Surg [Br]* 1996;78-B:625-30.

Author's reply:

Sir,

I thank Mr Eyres for his letter.

In the original radiographs illustrated in Figures 6a, 6b and 6c the name of the patient can be read on all three. These have been coded as II.92, 2.93 and 2.94 respectively, at the time of our study. It is clearly evident that there is a serrated shadow of a radio-opaque plate placed for locating the patient identification tag. This, unfortunately, has come to overly the distal end of the plate. I am sure that it will be appreciated that this serrated marker extends all the way down to the bottom of the X-ray film and is distinctly separate from the femoral shaft at this point. I apologise for selecting a radiograph with this confusing artefact but it happened to be one of the best cases to illustrate the sequence of union of the osteotomy site.

I have not encountered a single instance of plate breakage up to the present. The illustration of implant loosening was submitted along with the original manuscript but was later deleted at the

behest of the Editor. Apart from the two specific complications cited no others were encountered.

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Footnote: Dr Joseph has submitted the original radiographs to the Journal for inspection. We agree with his interpretation and have no doubt as to their authenticity.

SURGICAL RESECTION OF PRIMARY SOFT-TISSUE SARCOMA

Sir,

We read with interest the article in the July 1996 issue by Goodlad et al¹ entitled 'Surgical resection of primary soft-tissue sarcoma' (1996;78-B:658-61). The message from this excellent paper about soft-tissue sarcoma is quite clear. Gone are the days when it is acceptable simply to shell out a mass without knowing what it is. Any suspicious musculoskeletal mass should be staged and biopsied before excision. Clinicians must be wary of any mass which is larger than 5 cm, has become painful, is increasing in size or is deep to the deep fascia.

Our experience is similar to that of Goodlad et al in that approximately 60% of the patients presenting to us after an initial operation elsewhere had residual tumour.

The article does not address the question as to whether the presence of residual tumour is related to appropriate staging studies before the original excision. Did the staging studies after the first excision and before reoperation show whether residual tumour was present? Our studies suggest that in a significant number of cases CT or MRI carried out six weeks after the operation, when all the bruising and haematoma have resolved, fails to show any residual tumour, although at reoperation this may be found microscopically and sometime macroscopically.

We advocate wide re-excision in any patient who has had an injudicious excision of a soft-tissue sarcoma. We fervently hope that the incidence of these inappropriate operations will decrease with time and more widespread understanding.

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1. Goodlad JR, Fletcher CDM, Smith MA. Surgical resection of primary soft-tissue sarcoma: incidence of residual tumour in 95 patients needing re-excision after local resection. *J Bone Joint Surg [Br]* 1996;78-B:658-61.

Authors' reply:

Sir,

Thank you for allowing us to reply to the letter by Messrs Grimer, Carter and Tillman. There is increasing and substantial evidence to show that rare conditions such as soft-tissue sarcoma are better treated in specialist centres, the duty of which is to publish the findings.

To answer the specific questions in the letter, with few exceptions none of the patients had staging studies at the referring hospital. The less than satisfactory preoperative assessment of soft-tissue sarcoma was the subject of a presentation from our unit

at a recent meeting of the British Orthopaedic Association.¹

We performed staging studies before the second operation in almost all cases. The scans were positive in less than 5%, and most examples of residual tumour were microscopic. This shows the limitations of scanning studies in this context and supports the statement that a normal scan does not mean 'no tumour' but rather 'that within the limitations of our investigations we cannot find any tumour'. We have been uncomfortably aware of these limitations and have been investigating the role of PET scans in the diagnosis, staging and follow-up of soft-tissue sarcoma.

Such studies are dependent on our colleagues continuing to refer these cases which hopefully will allow us to provide the optimal service.

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1. **Clasby R, Goodlad JR, Fletcher CDM, Smith MA.** Soft tissue sarcomas: a regional audit of 378 cases. *J Bone Joint Surg [Br]* 1996;78-B:Suppl 1:58.

THE EFFECTS OF PARTICULATE POLYETHYLENE AT A WEIGHT-BEARING BONE-IMPLANT INTERFACE

Sir,

We read the article entitled 'The effects of particulate polyethylene at a weight-bearing bone-implant interface' by Allen et al,¹ which was published in the January 1996 issue with interest.

The model proposed by Allen et al is certainly useful, although it eliminates movement of the implant and places the implant in a weight-bearing area. However, the study has a considerable drawback in that the authors have not ruled out infection by aerobic and anaerobic cultures which are the most important tests in the diagnosis of periprosthetic infection.² Infection itself may result in bone resorption and membrane formation and it cannot be ruled out by histopathological studies only.

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1. **Allen M, Brett F, Millett P, Rushton N.** The effects of particulate polyethylene at a weight-bearing bone-implant interface. *J Bone Joint Surg [Br]* 1996;78-B:32-7.
2. **Paprovsky WG, Bradford MS, Jablonsky WS.** Cementless revision total hip arthroplasty. In: Callaghan JJ, Dennis DA, Paprosky WG, Rosenberg AG, eds. *Orthopedic knowledge update: hip and knee reconstruction*. Rosemont: AAOS, 1995;II:215-25.

Author's reply:

Sir,

We thank Drs Karahan and Esemeli for their comments.

We agree that in the clinical setting the diagnosis of sepsis is rarely, if ever, made solely on the grounds of a histopathological examination. However, it is well recognised that the results of bacteriological cultures can be potentially misleading. Padgett et al¹ recently compared the sensitivity and accuracy of intraoperative cultures from tissue samples with histological examination as a means of diagnosing infection around total joint replacements. In a study of 138 consecutive revision arthroplasties of the hip there were 42 positive cultures, but only one joint ultimately developed sepsis. The infected hip was the only one to show histological evidence of acute inflammation, while the remainder all showed signs of chronic inflammation consistent with aseptic loosening. The overall positive predictive value of cultures was 2.4% in this series of hips, a result which prompted the authors to conclude that 'positive intraoperative cultures are an unreliable predictor of sepsis and that permanent histologic sectioning is a more useful tool in determining sepsis'.

Although the use of aerobic and anaerobic cultures might have made the study more relevant clinically, it is unlikely that they would have been of significant benefit in the accurate diagnosis of infection in these animals. None of the rats received antibiotic therapy for more than 24 hours after operation and no antibiotics were used after the intra-articular injections. We are confident that if sepsis was responsible for the bone resorption in our model, we would have seen evidence of acute inflammation. Mirra et al² found the histological changes of acute inflammation in all of 15 patients with positive joint cultures and recommended that 'all tissues in which polys are noted by the pathologist must be cultured'. None of the specimens from our pilot study showed evidence of polymorphonuclear leucocyte infiltration, so none were cultured.

The small size of the knee joint in the rat makes harvesting of sterile samples of tissue difficult. If one of the specimens had produced a positive culture it is likely that this would have been the result of contamination. Rather than eliminating animals from the study on the basis of a possible false-positive result, we prefer to culture samples when the histological appearances have provoked suspicion. It may be more thorough to perform both cultures and tissue examination concurrently, but we feel that our regimen of histological assessment followed by bacteriological cultures if indicated is appropriate for the diagnosis of sepsis in our animal model.

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1. **Padgett DE, Silverman A, Schjowicz F, Simpson RB, Rosenberg AG, Galante JO.** Efficacy of intraoperative cultures obtained during revision total hip arthroplasty. *J Arthroplasty* 1995;10:420-6.
2. **Mirra J, Amstutz HC, Matos M, Gold R.** The pathology of joint tissues and its clinical relevance in prosthesis failure. *Clin Orthop* 1976;117:221-40.