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.

Fractures involving splitting of the humeral head

Sir,

We read with interest the article by Chesser et al\(^1\) entitled “Fractures involving splitting of the humeral head” in the April 2001 issue. Although we welcome the highlighting of these uncommon and complex injuries, we take issue with the broad interpretation of the diagnostic criteria used in this paper. We believe that a number of fractures included in this series fall outwith the criteria for true “head-splitting fractures” and are more consistent with impression fractures. The distinction is important because the pathology, prognosis and management are very different.

According to most authoritative texts,\(^2,3\) a head-splitting fracture is so defined by the gross disruption of the anatomical head, and thus the articular surface, of the proximal humerus. This deprives articular fragments of their vascular supply and results in a high incidence of subsequent avascular necrosis. We believe that the examples depicted are not true head-splitting fractures. Figure 1 shows the trough line or double shadow described by Cisternino et al\(^4\) which is correctly described in the text as representing an impaction fracture from a posterior dislocation. However, the caption ascribes this appearance to a “splitting fracture” and this is incorrect. We interpret Figures 2 and 5 as showing neglected caption ascribes this appearance to a “splitting fracture” and this is impaction fracture from a posterior dislocation. However, the


Authors’ reply:

Sir,

We thank Messers White and Robinson for their interest in our article. Terminology can be confusing and we specifically described “fractures involving splitting of the humeral head” and not “head-splitting fractures”. As we have stated in previous correspondence relating to this article,\(^7\) Neer described the head-splitting fracture with the articular surface fragmented into many disconnected pieces.\(^5\) We describe fractures including splitting of the humeral head involving the articular surface and in seven of the eight cases reported that there was only one fracture line and in no case articular comminution. It is incorrect to describe these as “large reverse Hill-Sachs lesions” since a Hill-Sachs lesion is by definition a compression fracture with “a separate avulsed fragment from the humerus practically never present”.\(^3\)

We agree that careful adherence to accepted classification systems is essential if meaningful comparisons are to be drawn from reported results. T. O. WHITE, BMedSci, AFRCs C. M. ROBINSON, BMedSci, FRCS Ed (Orth) Royal Infirmary of Edinburgh Edinburgh, UK.

1. Chesser TJS, Langdon IJ, Oglivie C, Sarangi PP, Clarke AM.
2. Bigliani LU.
3. Bigliani LU.
4. Norris TR.
5. Cisternino SJ, Rogers LF, Stufflebam BC, Kruglik GD.
6. Stableforth PG, Sarangi PP.
7. Kristiansen B.
8. Ko JH, Yamamoto R.

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Simple treatment for torus fractures of the distal radius

Sir,

We read with interest the article in the November 2001 issue by Davidson et al.1 entitled ‘Simple treatment for torus fractures of the distal radius’. This is a simple but very relevant message with respect to identifying and treating a subgroup of paediatric fractures. We agree that these fractures are benign.

The authors stated that in their study the injuries were seen in the fracture clinic at a mean of 1.1±3.6 days after the injury. This implies that the patients were seen between 2.5 days before the injury and 4.7 days after the injury!

They have also stated that one of the 201 fractures was actually not a torus fracture but a greenstick fracture. In a prospective trial with specific interest in torus fractures of the distal radius conducted at a paediatric hospital 0.5% of the fractures were ‘misdiagnosed’. When this is translated to a general fracture clinic the percentage would in all probability be higher. In the present medicolegal situation this can be a potential for litigation. In the same vein, the authors stated that more patients from the ‘splint group’ did not attend the follow-up clinic. When patients do not attend the clinic they do so at their own risk and the health-care personnel cannot be held responsible for their actions. Finally, the authors have not mentioned how many patients in their ‘splint group’ actually asked for a further clinic appointment.

We entirely agree that torus fracture of the distal radius is a separate entity with little chance of any significant complication and also that there is a place for these fractures to be treated in a splint in the compliant patient with understanding parents.

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Author’s reply:

Sir,

We thank Messers Kumar and Kamath for their comments in regard to our article. In response to their first point we would like to thank them for highlighting the problem with using means and standard deviations with non-parametric data!

We agree with the comments regarding ‘misdiagnosis’ of the fracture. Our study is only applicable to simple torus fractures. It does not apply to greenstick fractures. It is precisely for this reason that a visit to the fracture clinic is necessary after injury to confirm the diagnosis. It is essential that any person treating these injuries, in accordance with our protocol, can recognise the difference between the two patterns of fracture. We also suggest that if there is any doubt in diagnosis between the two, a more ‘traditional’ approach to treatment should be used.

For the duration of our study all patients in both groups were given a follow-up appointment at three weeks. No patients in either group required any further follow-up visit after this.

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Pyomyositis of the iliacus muscle in a child

Sir,

I read with interest the case report by Peckett et al.1 in the January 2001 issue entitled ‘Pyomyositis of the iliacus muscle in a child’. Recently, a nine-year-old girl presented to us as a case of possible septic arthritis of the hip. Further investigations including MRI, the diagnosis proved to be pyomyositis. Ultrasound of the hip was negative and the blood parameters suggested acute infection. The positive blood cultures and appropriate systemic antibiotics gave resolution of the symptoms in five days.

As suggested by the reports of Peckett et al.1, Macnicol2 and Papadopoulos et al.3, we endorse the use of MRI for the early definitive diagnosis of suspected infection around the hip or the pelvis. Pyomyositis appears to be increasing in prevalence in temperate climates. Orthopaedic surgeons are usually involved in the initial decision-making and ultimate care of these patients. The diagnosis is often delayed because of lack of familiarity and the paucity of symptoms on initial evaluation. Other primary diagnoses usually considered included muscle strain, synovitis, early Perthes’ disease, thrombophlebitis and neoplasm thus contributing to the diagnostic delay. Pyomyositis of the obturator internus,4 adductor,5 and psoas6 giving similar diagnostic confusion has also been reported.

MRI confirms whether the inflammatory process is localised to the muscle or is secondary to an infection in the bone.7

M. B. RAJESH, FRCS
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confirms the value of MRI in the investigation of such cases which allowed early diagnosis and resolution of the condition with appropriate systemic antibiotic treatment.

It may well be that pyomyositis is increasing in prevalence in temperate climates and we believe that it is important therefore that orthopaedic surgeons are aware of this differential diagnosis which we have shown can readily be diagnosed in its early stages by MRI.

We suspect that before the availability of MRI similar cases probably progressed to the formation of an abscess around the hip which required surgical drainage leading to increased morbidity. With greater clinical awareness of this condition and more readily available MRI pyomyositis will be detected at an early stage eliminating the need for surgical treatment.

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L. A. APTHORP, FRCR
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Treatment of isolated injuries of the lunotriquetral ligament

Sir,

I read with interest the article in the September 2001 issue by Shin et al entitled ‘Treatment of isolated injuries of the lunotriquetral ligament’ and wish to thank the authors for establishing that isolated lunotriquetral arthrodesis has an unacceptable rate of complications and an overall poor outcome.

The authors performed a power calculation to show that the size of their samples would allow the detection of a difference of 1.5 standard deviations between any two of the three groups. A power analysis is traditionally used in prospective studies to determine the size of the samples required to detect a predetermined effect, with a given probability, usually 0.8. To use such a calculation in reverse to determine the size of the difference for a given sample is unorthodox. A difference of 1.5 standard deviations between groups is a very large effect, using the standard Cohen’s d ratios of 0.2, 0.5 and 0.8 standard deviations to indicate small, medium and large effects respectively. These figures would indicate that this study is underpowered, leading to potentially significant results being dismissed.

For example, the authors state that the DASH score for arthrodesis was worse than that for the other groups, but that it was not statistically significant. This is probably because the number of patients studied was too small. It is also stated that the three treatment groups were statistically similar for a number of factors including the mechanism of injury, the grip strength and the range of movement, etc. This again could have been due to the lack of power of the study and not the inherent similarity of the groups.

The main difference between the groups is the length of follow-up available. The mean follow-up was 6.7, 10.5 and 16.6 years for the repair, arthrodesis and reconstruction groups, respectively. This implies that the standard treatment for this injury changed over time and in effect the authors are using historical controls. This is of course a valid means of study, but not one that is clearly stated in the methods.

The conclusion was that ligament repair or reconstruction should be the operation of choice for this type of injury. That may be the case, but as with most retrospective studies, a properly controlled, adequately powered study, with non-operative management as one of the groups, is required to demonstrate this rigorously.

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Authors’ reply:

Sir,

We thank Mr Wright for his letter. In a retrospective study, the use of a power analysis is quite appropriate since the size of the sample is predetermined by the number of records available for review. A power calculation helps to define the threshold at which differences between groups are not likely to be detected because of sample size. Furthermore, dismissal of all of our results on the basis of low statistical power is not justified. There was sufficient power to detect several factors which significantly affected the outcome. Mr Wright is correct in recognising that other variables in which the observed differences were less than 1.5 standard deviations may be significant with larger clinical numbers, a type-II error.

Another concern expressed was variation in group characteristics. We reported that the three treatment groups (arthrodesis, ligament reconstruction and ligament repair) were statistically similar with respect to age, hand dominance, the mechanism of injury, chronicity, workers’ compensation status, the pretreatment range of movement and grip strength. In our paper, preoperative differences in range of movement and grip strength of the wrist between groups were evaluated using analysis of covariance. This served to minimise any potential effect as a result of possible preoperative differences in these variables.

Although withholding treatment for an identified medical problem may be required to provide complete assurance of the results of intervention, our study is not unique in lacking this control group. This is a recognised problem with retrospective reviews in general. Ultimately, it was our desire to provide the most thorough analysis possible given the limitations of a retrospective study. Our conclusions are based on these results, without speculation or personal bias. The factors identified as significant are valid and important for clinicians to consider in practice. Future prospective studies may allow the identification of other significant variables.

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Prevention of pulmonary embolism by a foot sole pump

Sir,

We write with reference to the article by Asano et al in the November 2001 issue entitled ‘Prevention of pulmonary embolism by a foot sole pump’. I have some concerns with regard to the methodology of the study.

First, pulmonary embolism was defined as a new defect detected by pulmonary perfusion scintigraphy which was performed before and at one week after the operation with comparison of the two recordings. It is recognised that fat and bone-marrow embolic events occur during hip arthroplasty,
especially associated with the preparation of the femur and the insertion of the femoral component.\textsuperscript{2,3} Such embolic events could result in defects detected by perfusion scintigraphy, and their occurrence would not have been influenced by the use of foot pumps. It might have been more appropriate if the baseline reading had been performed early in the postoperative period rather than before hip surgery.

Secondly, the authors rightly acknowledged that perfusion defects may occur as a result of local hypoventilation. It is common practice in assessing pulmonary embolism to perform combined ventilation/perfusion scans to distinguish matched from unmatched defects. This was not performed in this study nor was there any mention of the incidence of postoperative respiratory problems in the control and treatment groups.

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Author’s reply:

Sir,
We thank Mr Charalambous for his comments and we agree that fat and bone-marrow embolic events occur during hip arthroplasty and that our defects may include them. However, we disagree that a baseline reading should have been performed early in the postoperative period for three reasons. First, it is difficult to perform scintigraphy just after surgery when the condition of the patient is not stable. Secondly, deep-vein thrombosis begins during surgery and a new defect early in the postoperative period does not necessarily mean that it is the result of a fat and marrow embolic event. Finally, we wished to know the usefulness of a foot pump for the reduction of pulmonary embolism as a whole.

Perfusion defects may occur as a result of local hypoventilation. It is possible that if we had included such cases that the incidence of pulmonary embolism would have been lower than we showed. However, a foot pump cannot prevent local hypoventilation and a reduction of the incidence of new defects means that a pulmonary embolism has been prevented. The fact that we did not use ventilation scans does not deny the usefulness of a foot pump. In addition, there were no respiratory problems among the control and treatment groups, and there were no abnormal signs on radiographs of the chest.

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