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.

The behavioural response to whiplash injury

Sir,

We have studied the paper in the July 1997 issue by Gargan et al. entitled ‘The behavioural response to whiplash injury’ and have three comments.

First, the scaled General Health Questionnaire (GHQ-28) was devised as a screening tool for detecting hidden psychiatric morbidity in the community and has not been validated for patients with neck injuries. The presence of neck pain will have a direct influence on the GHQ-28 score, particularly regarding somatic and social dysfunction. Secondly, no criteria were given to explain how patients were classified into the four categories of symptom severity. Thirdly, we are not clear as to what form of psychological mechanism the authors were invoking to explain the purported link between severity of neck injury and outcome.

We were also concerned to find at least two children (aged 10 and 12 years) included in the study and question the appropriateness of subjecting minors to certain questions in the GHQ-28, particularly those regarding suicidal intent. We feel that this study cannot support the authors’ conclusions because of fundamental methodological flaws.

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Authors’ reply:

Sir,

We thank the correspondents for their interest. In reply to their points, no psychological questionnaire has been validated specifically for patients with whiplash injury. In view of the sudden onset of pain, questionnaires for chronic pain are not appropriate.

In using the GHQ-28 we were interested in comparing the response found after whiplash injury with that of the general population to the normal vicissitudes of life. Of patients with a whiplash injury 30% had occipital headache acutely and 15% after two years. We accept that two of the GHQ-28 questions make reference to head symptoms.

The criteria for disability have been defined in a previous paper. We made no attempt to investigate psychological mechanisms in detail but simply to identify whether psychological factors were present which might indicate further lines of research.

We accept some of the correspondents’ criticisms, but we do not feel that the methodology is fundamentally flawed or the conclusions invalid.

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C. MAIN, PhD
Hope Hospital
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Neurological deterioration after posterior wiring of the cervical spine

Sir,

I wish to draw attention to the paper by Lundy and Murray in the November 1997 issue regarding neurological deterioration after posterior wiring of the cervical spine.

I have been involved in collecting details of morbidity in relation to internal fixation of the spine for spinal societies both in the UK and France. It is clear that whatever method of fixation is used (wire, cable, hook, etc), if metal is introduced into the spinal canal where it is narrowed then there is a serious risk of creating a neurological deficit. I wish to draw particular attention to Figure 1 in the article of Lundy and Murray. It can be seen that wires have been introduced into the spinal canal at the very level of an unreduced fracture dislocation. This carries with it a high risk of causing neurological damage.

It would appear that not all surgeons in this difficult field are aware of this important matter of detail.

J. DOVE, FRCS
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Fractures of the base of the middle phalanx of the finger

Sir,

I was impressed with the article in the September 1997 issue entitled ‘Fracture of the bone of the middle phalanx of the finger’ by Seno et al. I am, however, concerned by elements of the
Sir et al\textsuperscript{2} compared the treatment of pilon fractures by conservative means, internal fixation and banjo traction. Traction gave significantly better results and internal fixation was very complex. Seno et al\textsubscript{1} stated that open anatomical reduction of the articular surface, bone grafting and rigid internal fixation for early mobilisation are sometimes needed. It is difficult to see how this can be applied to the more difficult fracture, although good results may follow internal fixation of type-1 fractures. Their figures suggest that surgical treatment generally gives only poor or fair results. The series of Stern et al is the largest and they achieved good results in 62%, based on the criteria of Seno et al.

Joints in the upper limb appear to be less dependent upon accurate anatomical reduction than those in the leg since they are not continuously weight-bearing. The principles of open reduction and accurate internal fixation therefore do not need to be so closely adhered to. There will be instances when internal fixation is the best choice, but in most cases, dynamic traction should be considered first.

G. GIDDINS, FRCS Orth, FRCS Ed, EDHS, DBA
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Bath, UK.


Author’s reply:

Sir,

Thank you for your comments on our article. As in type-1 and type-2 fractures type-3 fractures may also be divided into three divisions, avulsion, split and split-depression. The so-called pilon fracture may coincide with the split-depression fracture of type 3 according to our classification. Our experience of type-3 cases was limited, however, and we were not able to subdivide it as clearly as in type 1 or type 2.

The surgical outcome was poor because of our initial incompetence with internal fixation for type-3 fractures. Our technique of open reduction and internal fixation with bone graft was developed mainly for fractures of types 1 and 2. Our overall general objective has been to achieve anatomical reduction of both weight-bearing and upper-limb joints.

We have no experience of dynamic traction. Our conclusion that anatomical and stable fixation was necessary for good surgical results is mainly based on our results with type-1 and type-2 fractures.

H. HASHIZUME, MD
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MRI after operative reduction for developmental dysplasia of the hip

Sir,

The article entitled ‘MRI after operative reduction for developmental dysplasia of the hip’ by McNally et al\textsuperscript{1} in the September 1997 issue is only of theoretical value to surgeons who treat this problem. After the application of a spica cast there is no need to check concentric reduction of the femoral head by MRI because other cheaper, rapid and reliable methods are available. The surgeon must know whether the hip is reduced or not, both clinically and radiologically, before the patient leaves the operating room. If a clinically stable concentric reduction has been obtained at operation, redislocation of the hip during application of the cast is unlikely in experienced hands.

I believe that arthrography is the best method for checking concentric reduction before and after application of the cast in hips in which the capsule has not been opened. An experienced surgeon can easily detect the relationship between the femoral head and the acetabulum in a capsule which contains residual contrast medium even in the presence of a cast. I am uncertain about the cost and reliability of MRI. It may be better to use a radiolucent cast if there is uncertainty with plain radiography. It is usually difficult to obtain adequate scans without sedation in infants and when they are recovering from anaesthesia they need close observation.

H. ÖMEROĞLU, MD
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Authors’ reply:

Sir,

We thank Dr Ömeroğlu for his comments. Our suggestion that MRI may have a role after open or closed reduction of a dislocated hip in a child arises from the difficulty in interpreting plain films in the presence of a spica cast. Samuelson et al\textsuperscript{1} described the use of plain tomography to overcome this problem and others have suggested ultrasound and CT. While plain radiography may help in many cases, the issue is not as straightforward as Dr Ömeroğlu suggests.

He states that it is usually difficult to obtain adequate scans without sedation in infants. We disagree; sedation was not used in any of the infants in our study and images of diagnostic quality were obtained on all occasions. It has been our experience with MRI in children that most can be examined without sedation. We agree with Dr Ömeroğlu that they should not be transferred to the MRI unit until they have undergone an appropriate period of observation after operation. We are not advocating that MRI is carried out while they are still under the effects of anaesthesia, merely that children are often more placid in the 24 hours after an anaesthetic, which adds to the ease of acquiring satisfactory images without sedation.

We appreciate that we are fortunate in having ready access to high-quality and inexpensive MRI. The cost of each examination is £150 (approximately US$225). While this is more than the cost of a single radiograph, we feel that it offers the surgeon an accurate image without using ionising radiation. The cost differential is reduced further if multiple plain films are needed and it is considerably cheaper than removing a cast to replace it with a radiolucent one. With time, we believe that these facilities will become more widely available and MRI will become the method of choice for imaging.

E. G. McNALLY, FRCR
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Fracture on removal of the ACE tibial nail

Sir,
I read with interest the article in the May 1997 issue by Takakuwa et al entitled ‘Fracture on removal of the ACE tibial nail’.

The high incidence of posterior fracture described by the authors when removing this nail is disturbing. We have implanted 53 ACE tibial nails in varying types of fracture and to date have removed 28 without refracture. The ACE tibial nail is relatively rigid since it is designed for nonreamed application. If inserted near the tibial tubercle it is forced into a three-point position with contact on the dorsal diaphysis. We prefer an insertion point near the edge of the tibial plateau which also makes removal easier.

Because of the rigidity of the material a thinner nail can be used, which is especially beneficial when using the unreamed technique. We used 40 × 8 mm and 10 × 9 mm diameter nails with no breakage of either nail or locking bolts. In our experience the use of nails of larger diameter is limited. Over-reaming of the tibia to accommodate 11, 12 or 13 mm nails is unnecessary. We dynamise the nail if callus formation has not occurred by 12 weeks, and have had no cases of pseudarthrosis.

Callus formation occurs rapidly with intramedullary nailing and is mature by ten months. We remove the nail 12 months after operation and have not experienced any difficulty. ACE nails are made of a proven titanium and have aerospace surface treatment which improves the material strength. Bone growth occurs rapidly and early removal of the implant is advisable to avoid possible complications due to encapsulation and overgrowth. If early removal is not possible it is advisable to leave the implant in place.

G. LOB
H.-J. ANDRESS
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Author’s reply:

Sir,
We have used the same technique for tibial nailing for many years and have not previously experienced problems with other types of nail. We were very careful when reaming and did not use excessive force. At removal, care was taken to ensure that the knee was in maximal flexion. The resistance encountered during removal and the appearance on CT have led us to believe that the shape of the slot may be important. In retrospect, we realise that some of our cases could have been managed with smaller nails, and we must consider their earlier removal.

We are still using the ACE 8 and 9 mm nails effectively, but continue to find that larger sizes may be necessary. Our report was presented as a warning of the possible complications which may arise from the incorrect selection of an implant and, of the timing of removal of the nail and the necessity for adherence to the correct technique.

M. TAKAKUWA, MD
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A locking nail for fractures of the humerus

Sir,
With regard to the article entitled ‘A locking nail for fractures of the humerus’ in the July 1991 issue,1 further experience with this nail has made me change the views expressed in my paper. The Seidel nail does not offer absolute stability since the fins do not achieve as sound a grip on the bone compared with a locking screw.

In order to achieve proper alignment of the fracture fragments it is necessary to make an incision through the rotator cuff. This inevitably leads to damage to the cuff either because of the primary incision or by the nail backing out proximally. The lesions produced may have a significant influence on shoulder function.

When we reviewed the 19 active patients in 1991 we did not assess their shoulders and this should have been addressed.

It is clear that only screws can provide stability, thus requiring distal locking or retrograde insertion of the nail. This introduces a risk of interference with the function of the elbow or further fracture of the humerus.

H. HABERNEK, MD
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Austria.


Reamed or unreamed tibial nailing for closed tibial fractures

Sir,
We read with interest the article entitled ‘Reamed or unreamed nailing for closed tibial fractures’1 in the July 1996 issue and the ensuing correspondence in the July 1997 issue.2

Mr Court-Brown raises a number of important issues in his reply to Mr Harrington’s letter. He states that there are five papers which show that reamed femoral nailing is better than unreamed nailing. We would be grateful if he would provide the references.

We understand that Mr Court-Brown believes that it is unethical to deny weight-bearing. We would question, however, how ethical it is to perform a randomised trial using an implant in a manner which the manufacturers state is contraindicated. Their recommendations were based on sound mechanical studies and suggest that implant breakage is inevitable with early weight-bearing. We are surprised that the Ethical Committee allowed this study to proceed.

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P. BROWNSON, DM, FRCS Ed(Orth)
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Author’s reply:

Sir,
I note the points made by Mr Moran and Mr Brownson. Currently, there are a number of papers which demonstrate the advantages of
reaming. Tornetta and Tiburzi\textsuperscript{1} undertook a prospective randomised study and showed that reamed femoral nailing led to faster healing in fractures of the distal third of the femur. They could see no advantage in nailing without reaming.

Giannoudis et al\textsuperscript{2} performed a retrospective study comparing reamed and unreamed AO femoral nails and showed a significantly longer time for union in the unreamed group. Bone et al\textsuperscript{3} had similar findings but also demonstrated a greater requirement for exchange nailing if unreamed nails were used. Shepherd et al\textsuperscript{4} and Le et al\textsuperscript{5} had similar conclusions after comparing reamed and unreamed femoral nailing.

There is now overwhelming evidence that reamed nailing is preferable to unreamed. Indeed, it is difficult to see why surgeons would wish to use a mechanically inferior implant which does not allow early weight-bearing and is associated with a lower incidence of union and a higher requirement for secondary surgery.

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Closed treatment of displaced middle-third fractures of the clavicle gives poor results

Sir,

In the paper in the July 1997 issue by Hill et al\textsuperscript{1} entitled ‘Closed treatment of displaced middle-third fractures of the clavicle gives poor results’, their finding of a 15% rate of nonunion contrasts significantly with the 0.1% found by Neer\textsuperscript{2} notwithstanding inclusion of paediatric cases. They state that 10 of their 52 patients had a reduction but they do not say how many of the eight patients with nonunion had closed reduction.

It is my experience that in displaced, oblique fractures, such as seen in Figure 1 of the article, the tip of the medial fragment can buttonhole through the deep fascia, trapping soft tissue between the surfaces of the fracture, which may be detected by dimpling of the adjacent skin. This soft-tissue entrapment can be released manually under local anaesthesia by injecting the fracture haematoma with xylocaine and retracting the shoulders. The fracture can then be treated by a figure-of-eight strap and perhaps a sling.

If this problem is not treated early, swelling of the soft tissue may obscure the dimple and the need for fascial disengagement is not realised.

I would also recommend that a prospective series of such closed reduced fractures be evaluated for the rate of nonunion before advising that middle-third fractures be operated on.

J. M. GOSSARD, MD
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Author’s reply:

Sir,

We thank Dr Gossard for his letter. Two of the ten patients who had an attempted closed reduction subsequently developed nonunion of the fracture and in a third the fracture healed but with an unsatisfactory result. In our study nonunion was seen equally in fractures undergoing closed reduction and in those which did not.

We wish to emphasise that our paper involved only fractures of the middle third of the clavicle with displacement of 100% or more. We attempted to define factors which might influence nonunion in such patients and found that only shortening of more than 2 cm or more was significant. Our recommendations for internal fixation applied only to this group.

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