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 teaching of trauma and orthopaedic surgery to the undergraduate in the United Kingdom

Sir,
I have noted the correspondence in the November issue from Ramachandran about the teaching of trauma and orthopaedic surgery to undergraduates to the UK. There will always be time restraints on clinicians at the registrar and consultant level. Nevertheless, medical students have to be trained and educated in our specialty. It is for this reason that we established WorldOrtho (www.worldortho.com) three years ago and have put out the second edition of the CD ROM covering its content. This includes over 3000 illustrations of patients, as well as articles, lectures, radiographs, specimens and quizzes aimed at medical students and registrars. It provides a fundamental and thorough orthopaedic education. We recommend this resource to your students and trainees.

E. SHERRY, MD, FRACS, FA (OrthA)
The University of Sydney
Penrith, New South Wales.


The outcome of treatment of trigger thumb in children

Sir,
We read with interest the article by Dunsmuir and Sherlock in the July issue entitled ‘The outcome of treatment of trigger thumb in children’. The subject of this article is important as surgery, especially in young children, should be avoided unless absolutely necessary.

We understand that a high proportion of trigger thumbs present with a fixed flexion posture, but the exact number in the study was not given. What proportion, if any, presented with the symptom of triggering, and what proportion had a fixed flexion deformity at presentation?

We note that the patients were not randomly allocated to either treatment or observation groups. Could it be that the 53 children in the deferred group had a less degree of triggering, so that the option of surgery was not impressed on the parents as much as it was when a child had a significant trigger thumb with fixed flexion on presentation?

We are not aware of any recognised classification of the severity of triggering, even although this condition varies in its severity from patient to patient. Would it not have been possible to group patients according to whether the deformity was correctable by gentle manipulation or not?

Five of the six infants presenting at less than six months of age ultimately required surgery and there was a higher spontaneous rate of recovery in infants over 12 months (57.9%). Should we therefore not be treating these ‘congenital’ (<6 months) trigger thumbs more aggressively, and is there a place for manipulation alone in these cases?

Until a prospective, randomised study is performed it is difficult to draw a conclusion that can be applied to our daily practice.

N. EMMS, FRCS
S. SCOTT, FRCS
Aintree University Hospital, Liverpool, UK.


Authors’ reply:

Sir,
We thank the correspondents for their interest in our paper. Our paper is a retrospective review of practice in our hospital. The data obtained in case notes frequently varies in its completeness. We are therefore unable to comment on the exact proportion of children with fixed flexion deformities at presentation or with deformities correctable by manipulation. Most, however, had fixed flexion with only a small proportion having true triggering.

Nor can we comment specifically about the severity of the condition in our deferred group of children. We have no reason to believe that the spread of severity of the condition varied between the deferred and operated groups. There was, however, a higher proportion of children with bilateral trigger thumbs in the operated group. This may be indirect evidence of parents being more insistent on surgical treatment when both hands are affected.

Five of the six infants first seen at less than six months of age ultimately required surgery. We believe that only one of these was truly congenital, presenting at one month. The remainder we believe to have acquired trigger thumbs. Despite the high operative rate in this subgroup, we still feel that an aggressive operative policy in these children is not warranted. We have no evidence of postoperative fixed flexion deformities in our study group, even in those infants presenting after three years of age. With this in mind we feel that a conservative approach should continue to be the norm with these younger children, to allow the maximum time for spontaneous resolution of the problem.

We have been unable to find any evidence to support the use of manipulation to grade this condition. We feel that any anecdotal evidence for the use of manipulation to treat trigger thumbs merely reflects the higher than anticipated natural spontaneous rate of resolution of this condition.

We accept that our study displays some of the problems asso-
Nonunion of the femoral diaphysis

Sir,
I read with interest the article by Giannoudis et al in the July 2000 issue entitled ‘Nonunion of the femoral diaphysis: the influence of reaming and non-steroidal anti-inflammatory drugs’. The authors tried to establish a relationship between the use of NSAIDs and nonunion, but their study was retrospective, and it is therefore not possible to form any causal relationship. They analysed only 32 patients’ charts in a retrospective manner with doubtful statistical tests and unbelievable p values (0.000001) to assess risk factors for nonunion after fractures of the femoral diaphysis, and compared them with the charts of 67 patients whose fractures healed without delay.

The link is obvious. Usually, nonunion is accompanied by severe pain and NSAIDs are the most effective analgesic for musculoskeletal pain. There is therefore a higher demand for NSAIDs by patients with nonunion and a correlation between the use of NSAIDs and nonunion is significant, and indicates good clinical practice. Prolonged use of NSAIDs points to nonunion. If the number of radiographs were counted in patients with nonunion and compared with those in patients with unremarkable healing of the fracture, there would doubtless be an even stronger correlation. Would Giannoudis et al ban radiological imaging, knowing the danger of x-rays?

J. PFÖRTNER, MD
Holzminden, Germany.


Author’s reply:

Sir,
I thank Dr Pförtner for his letter and interest in our paper. I am sure that we would all agree that there are some limitations in retrospective reviews, and our study is no different from others. However, the paper was reviewed by at least two expert statisticians who did feel that it was sound, although the limitations described are indeed there. I also note the difficulty in defining union, and I think that this is present in all papers which discuss the union of fractures of the diaphysis in the presence of an implant in which all the measurements of stiffness are useless. The most important feature is that after statistical review it became clear that the effects of the NSAIDs swamped all of the other issues that we were trying to examine and appeared to be the important factor. Clearly, a properly conducted prospective, randomised control trial is required and I look forward to that.

With regard to the second point about timing and the use of NSAIDs, it seemed that the earlier they were given the greater the effect. Also, although not reported in this paper, we noted a delay in healing in patients with fractures which healed but who took NSAIDs. I agree that patients with nonunion may continue to take analgesics for longer than others, but most NSAIDs were given early in the course of treatment before long-term pain from nonunion became a relevant feature. I agree that the cause and effect cannot be separated in a retrospective study. What we have identified at the moment is what I believe to be a true association between the use of NSAIDs and nonunion, which requires further investigation.

R. M. SMITH, MD, FRCS
St James’s University Hospital
Leeds, UK.

Relapse in staged surgery for congenital talipes equinovarus

Sir,
I read with interest the article entitled ‘Relapse in staged surgery for congenital talipes equinovarus’ by Uglow and Clarke in the July 2000 issue. I appreciate and agree with their concern for problems of wound healing in the surgical management of club feet, the need for a reproducible classification for pre- and post-operative comparison, and the use of separate plantar medial and posterolateral incisions, as described by Carroll, McMurtty and Leete. It was uncertain how they managed to correct the composite deformities by using an initial limited medial release which included structures such as the talonavicular and calcaneocuboid capsules, but excluded the crucial tibialis posterior, flexor hallucis, flexor digitorum longus, tendon Achilles, and the anterior portion of the deltoid ligament. Unless these structures, commonly found to be very tight in operable club feet, are divided, it is difficult to determine the adequacy of any limited medial release. Also, it may not be possible to reduce the navicular on to the talus and to restore the talocalcaneonavicular relationship in the horizontal, coronal and sagittal planes, which are considered to be essential elements of successful surgical correction of club feet. If these are to be divided through a posterolateral incision, after two to four weeks, despite an adequate, staged release, it would still be difficult to obtain a satisfactory talocalcaneonavicular reduction, with a medial incision in an advanced stage of healing. Single-stage surgery allows the precise assessment of the adequacy of soft-tissue release, which in turn is essential if we are to obtain a satisfactory radiological correction. This is the reason why all such procedures described, including those by Carroll et al, McKay and Turco are recommended as single-stage procedures to allow simultaneous multiplanar, bony and soft-tissue correction. The authors, however, have given neither direct reference to the radiological outcome of the staged surgery as described by them, nor to its effect on relapse.

K. B. MUKHERJEE, Diplomate, NB(Orth), D Orth
St Stephen’s Hospital, Delhi, India.

Authors’ reply:

Sir,

We thank Dr Mukherjee for his interest in our paper and the issues which he has addressed.

The radiological outcome of our series and the effect on relapse, together with the functional results, have been published separately. The overall rate of relapse in our series was similar to that of the other series mentioned in this article. We therefore believe that the results of staged surgery are comparable with those of other authors.

We wish to reiterate that by classifying the deformities appropriately it is possible to identify in which group of patients the rate of relapse is unacceptably high. It is for these patients that further methods should be explored and Dr Mukherjee’s comments may be of relevance. Until, however, other series are published with a compatible classification, it is not possible to compare treatment regimes. At present, staged surgery produces comparable results to other peer-reviewed series and may even be advantageous for grades II and III as classified by Dimeglio et al.

M. G. UGLOW, FRCS (Tr & Orth)
N. M. P. CLARKE, FRCS
Southampton General Hospital, UK.


Fix and flap: the radical orthopaedic and plastic treatment of severe open fractures of the tibia

Sir,

I read with interest the article in the September 2000 issue by Gopal et al entitled ‘Fix and flap: the radical orthopaedic and plastic treatment of severe open fractures of the tibia’. The authors and the members of their team are to be congratulated on the high rate of limb salvage which they were able to achieve.

They employed ‘immediate radical wound debridement outside the zone of injury’ before flap coverage. Have they any comment on the relative amount of debridement which they found to be necessary? ‘Radical’ debridements, ‘outside the zone of injury’ have been recommended by other authors, but perhaps a more aggressive debridement is necessary to do what Mr Gopal and his co-authors are proposing. I realise that an assessment of the amount of tissue debridged is difficult to make. Can they comment on how much tissue they had to remove to allow immediate placement of internal fixation and flap coverage and if, compared with debridements done in the past, this new, more aggressive approach required the removal of more tissue, less, or about the same amount?

I was surprised at the statement that “modern techniques of management combining the skills of experienced orthopaedic and plastic surgeons can consistently restore excellent limb function in a very high proportion of patients”.

I can find very little data regarding functional outcome. The authors state that all grade-IIIc fractures “united with excellent function”, and, except for the four amputations, “all other patients had good functional outcomes, walking freely on united fractures, with no evidence of infection at final follow-up”. There are no data, however, on the range of joint movement, return to work, or walking capability. Similarly, there is no information in the paper concerning the impact of the injury on the patients’ quality of life.

Without such data, there is no way of assessing functional outcome. I think that most trauma surgeons would agree that, even if a mangled extremity is salvaged, ultimate function is far from excellent. I have seen very few patients with limb salvage who feel that they have had an ‘excellent’ outcome.

A. J. STARR, MD
University of Texas Southwestern Medical Centre
Dallas, USA.


Author’s reply:

Sir,

I am pleased to receive these comments from Dr Starr. I am sure that he has a very wide experience of this sort of injury. He is correct in stating that an assessment of tissue debridement is very difficult. Our experience is that most wounds are inadequately debrided and contused; dead and dying tissue is often left behind in wounds which have not been adequately extended. We remove all devitalised bone and soft tissue. Bony fragments with good soft-tissue attachments are maintained and muscle with minor contusion does not need to be removed. We would tend to err on the side of a slightly more radical debridement and aim to reduce the risk of infection in severe open fractures. In some patients the debridement involves little muscle and is only skin, but if in the wrong place, this can require a flap as the only feasible method of covering the area. It is particularly applicable to the lower tibia where a lower-grade injury inevitably becomes grade IIIB because of the lack of options for freely available local soft tissue. On balance, we now believe that the ability to cover a defect reliably with viable muscle is the essential factor which allows us to have the confidence to debride the wound adequately. This is critical for the low rate of infection described in this series.

With regard to functional outcome, I completely agree with Dr Starr’s comment. I am certainly not aware of any patients who have major problems from the orthopaedic point of view, but I realise that the surgeon’s assessment is not always as valuable as that of the patient. The functional assessment of a large number of these patients is almost complete and will be submitted for publication in the near future. This will include the relevant scores relating to quality of life.

R. M. SMITH, MD, FRCS
St James’s University Hospital
Leeds, UK.