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.

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Modern concepts in the treatment of hallux valgus

Sir,

We read the article in the August 2005 issue by Robinson and Limbers’ entitled ‘Modern concepts in the treatment of hallux valgus’.

It highlights the pathogenesis of hallux valgus as described by Stephens, in which they describe the response of the medial and lateral heads of flexor hallucis brevis (FHB). However, the diagram shown in Figure 1 does not show the two distinct heads of FHB as described in the text and as shown in recognised orthopaedic and anatomical texts, but rather it shows a single muscle belly and tendon unit with a single insertion bypassing the sesamoids. We feel this error may lead to confusion for those reading the article.


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Sir,

I read the article in the August 2005 issue by Robinson and Limbers’ entitled ‘Modern concepts in the treatment of hallux valgus’ with interest. While complimenting the authors on an otherwise excellent article, I was disappointed on two fronts. Firstly, they did not address the problem of adolescent hallux valgus and secondly, they decried the Wilson osteotomy.

The article by Keogh et al, which they cited, records the operation as being technically uncomplicated and yielding 89% satisfactory results.

A completely shattered tibia

Sir,

We read the article in the November 2005 issue by John et al. entitled ‘A completely shattered tibia’ with interest. We note the outcome of treatment for the complex injury described. This case illustrates the difficulties of successfully treating severe IIIb tibial fractures.

We would like to make the following points. First, presented with such an injury, nearly every orthopaedic surgeon would treat it in their own way. However, there are several options that the authors do not mention. Most involve traction to keep the tibia out to length, followed by external bracing once the fracture is ‘sticky’ enough (i.e. traction for several weeks followed by immobilisation in plaster, pins and plaster or a Sarmento functional brace). This treatment is described in the literature for IIIb fractures of the tibia with good results.

Secondly, the authors’ described treatment has actually followed a similar plan. We note that the intramedullary nail shown in Figure 2 passes around the fracture fragments (not through them as described), and that the afflicted limb was immobilised in plaster for at least eight months.

Lastly, in Figure 3 two large sinuses are visible on the patient’s shin. The patient has chronic osteomyelitis and may yet require further procedures or amputation. One wonders if the use of bone graft in the presence of potentially infected non-union at two operations may have contributed to this. The literature describes the accuracy of MRI scans in determining the extent of osteomyelitis: such an investigation may help plan future treatment for the patient described.

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Authors’ reply

Sir,

We thank Dr Bismil and colleagues for their interest in our case report. As mentioned by them, any orthopaedic surgeon would treat such a fracture in his or her own way. The senior author (AD) was aware of the good results of nailing in similar comminuted fractures.1 We did have in mind the various treatment options, however, as mentioned in our report, we declined to use the option of calcaneal pin traction, as it would require additional posterior splinting and prolonged immobilisation. The interlocked nail, in the form of an internal splint, enabled non-weight bearing mobilisation of the patient once the initial pain subsided. It also avoided the possibility of additional complications associated with prolonged skeletal traction and hospitalisation.

In such grossly comminuted fractures, delineating a proper cortex and medullary canal among the multiple fragments, and hence passing a nail through the centre of the entire shattered diaphysis is impossible. The purpose of the internal splint is to bring the bone and extremity back to length, achieve axial and rotational alignment, and provide stability. This purpose is served by ensuring that the nail is seated in the centre of the proximal and distal epiphysio-metaphyseal blocks and subsequently locked. In such a case, the apparent position of the nail with respect to the individual comminuted fragments of the mid diaphysis need not be a concern.

Regarding Dr. Bismil’s concerns of potential infection, we would like to state that during both instances of bone grafting, there were no external signs of infection. The possibility of future infection at a nonunion site should not discourage the surgeon from undertaking bone grafting.

In our country where there are no mandatory insurance policies in health care, the entire cost of treatment has to be borne by the patient. A significant number of people carry on their activities of daily living in the presence of stigmata of chronic osteomyelitis. Patients younger than 45 years of age tend to do well.3 Mere evidence of stigmata of chronic osteomyelitis in a sensate and functional limb would not always require surgery, let alone an amputation.4


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Venous thromboembolism associated with hip and knee replacement over a ten-year period

Sir,

I read the article in the December 2005 issue by Howie et al1 entitled ‘Venous thromboembolism associated with hip and knee replacement over a ten-year period: a population-based study’ with interest. The authors have found no evidence of improvement in the incidence of venous thromboembolism or of fatal pulmonary embolism with the increased use of DVT chemoprophylaxis in hip and knee arthroplasty. The authors suggest that their data do not imply that anti-thrombotic prophylaxis and earlier mobilisation regimes have no effect. Their data must also imply that the ever-increasing use of chemoprophylaxis has made no difference to the rate of fatal pulmonary embolism. Is the data on length of stay for the hip and knee replacement procedures available over the ten-year period? Their data on day-case and in-patient cataract surgery have shown a significant difference in the venous thromboembolic rate with early mobilisation and discharge.

Early post-operative mobilisation after hip and knee replacements has been accepted as the reason for the decrease in incidence of venous thromboembolic disease. Is it possible therefore, with the data available so far, that we need to pause and review our continuing use of routine DVT chemoprophylaxis for hip and knee replacement surgery?


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