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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J Bone Joint Surg [Br] 2005;87-B:884-6.

Predictive value of the duration of sciatica for lumbar discectomy

Sir,

I read with interest the article by Ng and Sell¹ in the May 2004 issue entitled "Predictive value of the duration of sciatica for lumbar discectomy". It is a rigorous assessment of this much debated question, but to call it a prospective, cohort study, despite the fact that the duration of symptoms varies at recruitment, may lead readers to draw a possibly erroneous conclusion.

It is the nature of sciatica from disc prolapse that patients may improve at any time, with a gradual decrease in the incidence of patients experiencing sufficient improvement to obviate the need for surgery. The necessary corollary of this is that the longer a patient has had sciatica, the nearer is he or she to the worse end of the spectrum of disease. This may be the entire explanation of the worse outcome - by waiting longer, one is not predicting a worse result from surgery, but merely selecting a worse group of patients.

What is needed is a cohort study with recruitment based on an intention to treat (by any means - conservative or operative), not on an intention to operate. My feeling is that waiting over a year may indeed be associated with a worse outcome, but unless the dropout rate of patients improving spontaneously during the waiting time is known, it cannot be proven.

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1. Ng LCL, Sell P. Predictive value of the duration of sciatica for lumbar discectomy. *J Bone Joint Surg [Br]* 2004;86-B:546-9.

Author's reply:

Sir,

We thank Mr Brown for his interest in our paper. In answer to his comment, we would consider it unlikely that readers of the Journal would draw an erroneous conclusion on the data presented. The title of the paper makes it clear that the cohort in question is

an operative one. Our article contributes to the literature regarding the optimum timing for operation in radicular pain.

The perfect answer to a question often needs to be balanced against the practical realities of clinical practice. The dropout rate to follow-up of a prospective cohort of non-operatively treated patients would be so high that the suggested comparison based on intention to treat could not be achieved in our clinics. A better level of evidence would be a randomised, controlled trial.

Readers of our paper will have noted that patients still improve in terms of pain after a year of symptoms, but that decline in disability as indicated by the outcome measures used showed less change with time, perhaps suggestive of the development of bio-psycho-social factors with chronic symptom duration. It is the authors' feeling, that this is the major factor, but it remains to be proven.

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Internal fixation of complex fractures of the proximal humerus

Sir,

We read with interest the paper in the August 2004 issue entitled "Internal fixation of complex fractures of the proximal humerus" by Gerber, Werner and Vienne¹ and would like to raise the following points.

They describe the treatment of 34 shoulder fractures. There were nine different types of fracture and at least ten types of operation, undertaken by different surgeons with varying post-operative physiotherapy regimens. In some cases the same type of fracture pattern was treated with different operations. With so many variables we feel it is difficult to draw any valid conclusions from the results.

The aim of operative treatment was to achieve reduction of the fracture, and therefore a better functional outcome. We note that the four malunited fractures achieved a mean subjective shoulder value score of 88.75%. Those fractures without malunion had a mean subjective shoulder value score of 87.1%. The role of accurate reduction, therefore, seems still to be unproven with regard to function.

We would welcome the author's comments on these points.

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1. Gerber C, Werner CML, Vienne P. Internal fixation of complex fractures of the proximal humerus. *J Bone Joint Surg [Br]* 2004;86-B:848-55.

Author's reply:

Sir,

We like to thank Messrs Gibbs and Ricketts for their interest in our paper.

They correctly note that 34 fractures of nine different types treated with ten technical variants were reported.

Taking the usual classification systems, the number of different fracture types was large. Unfortunately, all these classification systems have failed to show that a fracture belonging to one particular group has a distinctly different prognosis, requires a different treatment, or has a different outcome. We have reported the results in articular fractures of the proximal humerus which have, as a common denominator, a high risk for the development of avascular necrosis of the head segment. We feel this article argues that, according to the type of fracture, the type of bone and the type of patient, there is probably not a universally best method of treatment and that, therefore, different types of reduction and internal fixation techniques might be advantageous in apparently similar cases. The fractures were treated by a single surgeon particularly experienced in shoulder problems who decided, according to the intra-operative stability obtained, which post-operative regimen was possible without jeopardising the stability of the operatively-obtained reduction.

Previous publications have shown that operative treatment of such fractures does not yield better results than conservative treatment. These publications have failed to document whether a satisfactory reduction of the fragments was obtained and whether the fractures healed in anatomical alignment. It was the purpose of our paper to document that if satisfactory reduction is obtained and maintained until healing, a satisfactory outcome can be expected. It appears that with a satisfactory reduction and healing in an anatomical or near anatomical position, patients almost invariably do well. The group of patients which had a reduction which might have been considered satisfactory for other authors, but which were considered to be non-anatomical in this study, was fortunately so small that no conclusions can be drawn. We agree, therefore, with Mr Gibbs that from the present series it is not possible to state the outcome of non-anatomical reduction.

Nonetheless, we feel that our study reports in a consecutive series that if operative reduction and internal fixation are undertaken with the goal of restoring the anatomy of the proximal humerus, the clinical results are consistently good and that, therefore, efforts to obtain anatomical reduction and healing by open reduction and internal fixation appear justified. The article also suggests that if a paper criticises open reduction and internal fixation for fractures of the proximal humerus, authors would have to be invited to report on whether they have obtained and maintained anatomical reduction or whether their surgical results were unsuccessful in terms of anatomical reduction.

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Reconstruction of the posterior cruciate ligament

Sir,

We read with interest the informative paper "Reconstruction of the posterior cruciate ligament" in the May 2004 issue.¹ It was correctly noted that "It is uncommon to see a patient presenting

with an acute, isolated injury to the PCL".¹ This is indeed true for Western countries. It is of interest, however, that in Ho Chi Minh City (Saigon), Vietnam, my experience during the last ten years has been different. There the main means of individual transportation is the motor bike. A condition locally known as "Honda knee" is very common. In crowded and chaotic traffic conditions, two machines often meet nearly head to head in close quarters. The left knee of each driver hits that of the opposite, avulsing the PCL of each without other significant knee injury. Usually a bone fragment is present at the distal end of the avulsed ligament and a simple posterior approach and screw fixation suffice to correct the problem. One or more such are seen at the trauma hospital each week. The results of these repairs are uniformly good and the diagnosis is rarely missed owing to the frequency of the injury.

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1. Dowd GSE. Reconstruction of the posterior cruciate ligament. *J Bone Joint Surg [Br]* 2004;86-B:480-9.

Author's reply:

Sir,

Dr Raisbeck presents an interesting point with regard to PCL injuries in Saigon. There is no doubt that the injury is recognised in that part of the world partly because of a high index of suspicion. He states that these injuries occur commonly, and also because radiographs show a bony avulsion. It is correct to point out that bony avulsion of the PCL is amenable to internal fixation using a posterior approach as described in the article published by the *Journal of Bone and Joint Surgery*.

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New concepts in orthopaedic thromboprophylaxis

Sir,

I read with interest the review article by Warwick¹ in the August 2004 issue entitled "New concepts in orthopaedic thromboprophylaxis". This was certainly a heavily-researched and well-written review article on a difficult subject. However, the author's recommendation for thromboembolism prophylaxis following hip arthroplasty neglects to mention that mechanical prophylaxis with a sterile thigh-high sequential compression device on the operated limb may easily be used during the surgical procedure. This is the crucial time when thrombi begin to develop in the limb during arthroplasty.

Both Woolson² and Hooker, Lachiewicz and Kelley³ published series demonstrating the efficacy of mechanical prophylaxis using thigh-high sequential compression devices intra-operatively, and continued post-operatively without chemoprophylaxis. It is not necessary to wait to begin the use of foot pumps in the recovery room after hip or knee replacement.

In addition, Westrich et al⁴ showed that foot pumps were mechanically inferior to both thigh-high compression devices and rapid inflation calf compression devices in terms of the volume and velocity of venous blood augmented by these devices. We have

recently completed a prospective randomised study of two types of pneumatic compression devices after knee arthroplasty and found that rapid inflation was associated with a lower prevalence of thromboembolism.⁵
doi:10.1302/0301-620X.87B6.16571

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1. **Warwick D.** New concepts in orthopaedic thromboprophylaxis. *J Bone Joint Surg [Br]* 2004;86-B:788-92.
2. **Woolson ST.** Intermittent pneumatic compression prophylaxis for proximal deep venous thrombosis after total hip replacement. *J Bone Joint Surg [Am]* 1996;78-A:1735-40.
3. **Hooker JA, Lachiewicz PF, Kelley SS.** Efficacy of prophylaxis against thromboembolism with intermittent pneumatic compression after primary and revision total hip arthroplasty. *J Bone Joint Surg [Am]* 1999;81-A:690-6.
4. **Westrich GH, Specht LM, Sharrock NE, et al.** Venous haemodynamics after total knee arthroplasty: evaluation of active dorsal to plantar flexion and several mechanical compression devices. *J Bone Joint Surg [Br]* 1998;80-B:1057-64.
5. **Lachiewicz PF, Kelley SS, Haden L.** Two mechanical devices for prophylaxis of thromboembolism after total knee arthroplasty: a prospective, randomized study. *J Bone Joint Surg [Br]* 2004;86-B:1137-41.

Author's reply:

Sir,

I would like to thank Mr Lachiewicz for his interest in my paper.

I would agree that the closer we can bring prophylaxis to surgery, the more likely it is to be beneficial. It is possible to use both foot pumps and sequential compression devices in the operating theatre. The cuffs can be excluded with standard surgical drapes

and the air hoses can be excluded using an arthroscopic sheath. For pragmatic reasons, some surgeons may choose to start these devices in the recovery room rather than during surgery, albeit with a theoretically reduced efficacy.

Professor Lachiewicz refers to papers by both Woolson¹ and Hooker, Lachiewicz and Kelley² showing the efficacy of mechanical prophylaxis without chemoprophylaxis. However, these studies do not address the important issue of thrombi which form after discharge from hospital. They also do not address the well-recognised issue of compliance with all mechanical devices which becomes more apparent a few days after surgery when the patients begin to mobilise.

The relevance of the volume and velocity of venous blood which is augmented by mechanical devices, as demonstrated elegantly by Westrich et al,³ has not been correlated with clinical outcome. I look forward to seeing robust clinical studies that address this point in due course.

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1. **Woolson ST.** Intermittent pneumatic compression prophylaxis for proximal deep venous thrombosis after total hip replacement. *J Bone Joint Surg [Am]* 1996;78-A:1735-40.

2. **Hooker JA, Lachiewicz PF, Kelley SS.** Efficacy of prophylaxis against thromboembolism with intermittent pneumatic compression after primary and revision total hip arthroplasty. *J Bone Joint Surg [Am]* 1999;81-A:690-6.

3. **Westrich GH, Specht LM, Sharrock NE, et al.** Venous haemodynamics after total knee arthroplasty: evaluation of active dorsal to plantar flexion and several mechanical compression devices. *J Bone Joint Surg [Br]* 1998;80-B:1057-64.

Errata

J.-P. Whittaker, G. Smith, N. Makwana, S. Roberts, P. E. Harrison, P. Laing, J. B. Richardson. Early results of autologous chondrocyte implantation in the talus

J Bone Joint Surg [Br] 2005;87-B:179-83.

It is regretted that Figures 2a and 2b were transposed. The correct Figures 2a and 2b are shown below:

Fig. 2a

Fig. 2b

Case 10. Arthroscopy showing a lateral talar osteochondral defect a) pre-operatively (1.3 cm²) and b) at 12 months after ACI with a smooth graft site which is white in colour and slightly raised and softer than the surrounding cartilage.