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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\$2.00 *J Bone Joint Surg [Br]* 2005;87-B:1165-6.

Computer-assisted total knee replacement

Sir,

I would like to draw your attention to a potential pitfall in the reporting of one of the parameters measured by the Perth CT protocol for total knee replacements as reported by Chauhan et al¹ in the August 2004 issue. This potential error is also relevant to the calculation of the Perth Alignment Index (PAI).² The issue relates to the measurement of the axial views, femoral rotation and femorotibial mismatch and the impact of asymmetrical prostheses on these measurements.

If a femoral component is asymmetrical, as occurs in the Genesis II (Smith & Nephew, Memphis, Tennessee), it is important that the measurement of its position, in relation to the transepicondylar axis, be made on the inner surface of the posterior condylar flanges of the prosthesis, and not on the outer surface (Fig. 1). There is a 3° difference between the two readings caused by a build-up on the lateral part of the implant. If the femoral component is symmetrical it does not matter which surface is used for the calculation.

Femorotibial mismatch is determined by superimposition of the underside of the tibial component onto the image of the femoral component. The relationship between the transverse diameter of the tibial base plate and the geometry of the tibial stem is consistent and easily interpreted in symmetrical prostheses. A perpendicular to the transverse axis of the tibial base plate can be constructed without difficulty. However, in asymmetrical tibial components, such as the Genesis II and the Profix, the relationship between the stem and the base plate is variable, size-dependent and not easily understood (Fig. 2). In such cases a femorotibial mismatch of zero does not necessarily indicate perfect placement. So far the author has been unable to determine the ideal femorotibial relationship for this prosthesis.

The implication of these findings is that comparisons of prostheses which are not symmetrical should probably exclude femorotibial mismatch as an investigative parameter, at least until the ideal placements are understood. In such cases the PAI should be modified to a five-component index (coronal femoral, sagittal femoral, axial femoral, coronal tibial and

sagittal tibial). It is suggested that this now be called the modified PAI.

It is important to note that all the publications to date which have reported on the results of the Duracon (Stryker Corp) prosthesis remain unaffected as this (in common with the majority of prostheses) is a symmetrical implant.

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J. M. SIKORSKI, MD
Hollywood Private Hospital,
Nedlands, Western Australia.

1. Chauhan SK, Clark GW, Lloyd S, et al. Computer-assisted total knee replacement: a controlled cadaver study using a multi-parameter quantitative CT assessment of alignment (the Perth CT protocol). *J Bone Joint Surg [Br]* 2004;86-B:818-23.

2. Sikorski JM. Computer-assisted revision total knee replacement. *J Bone Joint Surg [Br]* 2004;86-B:510-14.

Patient preferences in knee prostheses

Sir,

I read with interest the article by Pritchett¹ entitled 'Patient preferences in knee prostheses'. The purpose of the study was to assess the outcome of various kinds of knee prostheses by "providing information only on patients' preference". Over the past decade, the measurement of patient preferences has become a well-established research area using sophisticated methodology in order to deliver valid results. The title of Pritchett's paper could be misleading as his study did not use this methodology but instead measured preferences by asking the single question, 'Which is your better knee overall?'.

There are several critical methodological issues that need to be considered in interpreting the results of this study. First, patients' characteristics at baseline remain unclear. Secondly, the fact that two knees (as opposed to one knee) were assessed per patient was not taken into account in the analysis. Thirdly, multivariate analysis was not performed. Fourthly, patients with an unfavourable outcome in one of their operated knees were excluded from the analysis without analysing the relationship between excluded and included cases. Fifthly, the statistical analysis was unclear. Specifically, how did the author define 'large size effect', and more importantly, to which measurement was he referring? Did the "four-point difference detected by the analysis"¹ refer to the Knee Society Score? If so, how did the author establish the clinically important difference of four points? Finally, how do these scores relate to patients' preference?

Table IV provided the percentages of patients' preferences but it is left to the reader to interpret the data as no tests of significance are given. Patients' explanations for their preferences are stated but not further interpreted, thus their meaning remains unclear.

The aim of Pritchett's paper is highly valuable considering the importance of patient-based outcome assessment as one of the many ways of improving the quality of medical care. However,

without the use of sound methodology, this strategy is doomed to fail.

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A. ROPOSCH, MD
The Hospital for Sick Children and University of Toronto
Toronto, Canada.

1. Pritchett JH. Patient preferences in knee prostheses. *J Bone Joint Surg [Br]* 2004;86-B:979-82.

Author's reply:

Sir,

I thank Dr Roposch for his interest in our paper. I appreciate this opportunity to clear up the points raised. Several discussions and podium presentations shaped the choice of the title. There are many sophisticated methods and instruments in outcomes research. This paper made no attempt to assess implant wear, loosening or other variables. A patient could prefer a knee that will have a higher chance of failure over time. I tried to write this paper to provide data and interpretation in a form useful for both patients and surgeons. A significant goal was to limit the number of variables and therefore questions addressed.

A multivariate analysis was not performed and other instruments such as the Oxford knee score or Short Form SF-36 were not used to capture additional baseline information. The Knee Society score is commonly used and simple to apply. It has been validated in many publications.

Because of a fair or poor result from either of their knee replacements 21 patients were excluded from analysis. This represents 2.7% of the 778 knees replaced. While unfavourable cases are always of interest, the small numbers and the lack of a consistent reason for the result do not affect the conclusions reached.

A 10-point difference in knee scores can reasonably be regarded as clinically significant. Some may consider a 5-point difference important. The size of the study groups was large enough statistically to detect a difference in knee scores of ≥ 4 points. This means that there were no clinically important differences in knee scores between the different designs. Also, the knee scores did not relate to a patient's preference.

Table IV showed that patients did not have a preference between posterior cruciate-retaining and posterior cruciate-substituting prostheses. Patients preferred retention of their anterior cruciate ligament when possible, but had an equal preference for medial or lateral pivot prostheses.

Like many discussions, my paper raises as many or more questions than it answers. It remains unclear why patients prefer one knee over another. Could it be proprioception, rotation, stability, shape, temperature or even sound?

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J. W. PRITCHETT, MD, FACS
University of Washington,
Seattle, USA.

Long-term disability after neck injury

Sir,

I read with interest the paper by Joslin, Khan and Bannister¹ in the September 2004 issue entitled 'Long-term disability'. The Bristol group has previously pointed out how poor the modern treatment of supervised neglect, combined with analgesia, physiotherapy,

chiropractic and osteopathy is in preventing chronicity and its now £3 billion a year cost to the insurance industry.

I can only ask myself, would we treat bad ankle sprains in the same way? I have been surprised as to the similarities of symptoms in all claimants I have seen. They are mostly genuine.

In my opinion, a radical reappraisal is necessary. We have to accept that the anterior neck muscles are too weak to protect the neck from a hyperextension injury and until neck restraints are programmed to follow the head around, the whiplash condition induced by rear end impact will stay with us. The authors have shown that patients with demonstrable cervical injuries on radiographs or MR scans who are treated in a hard collar for six to eight weeks seem to escape chronicity. It is my observation that claimants confined to bed with other injuries also seem to escape severe chronicity.

Perhaps our treatment is wrong? Clearly soft collars are of no benefit and it would not be practicable to put everyone to bed for six weeks but is it not time that all but the mildest neck sprains were treated in a custom-made hard cervical collar which extended inferiorly (front and back) to immobilise the neck and upper thoracic segments properly and allow the injury to heal?

This would probably not need to be worn only during the day. Physiotherapy and exercises should start six to eight weeks later once healing has taken place.

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A. O. RANSFORD, FRCS
Royal National Orthopaedic Hospital,
London, UK.

1. Joslin CC, Khan SN, Bannister GC. Long-term disability after neck injury: a comparative study. *J Bone Joint Surg [Br]* 2004;86-B:1032-4.

Authors' reply:

Sir,

We thank Mr Ransford for his interest in our paper. Although we have not addressed treatment, we agree entirely with his sentiments. It is certainly true that the current treatment regime of supervised neglect, analgesia and early mobilisation is ineffectual in a large number of patients. While the use of soft collars after whiplash injury has been investigated and discounted, only one study¹ has considered the use of a rigid cervical collar following such an injury. In a prospective randomised trial, patients immobilised for four weeks suffered no adverse stiffness, returned to work sooner and had less pain than those treated by early mobilisation. A period of immobilisation following cervical injury enables soft tissue healing and improves long-term outcome in certain cases. The difficulty is to differentiate those patients with significant injury who would benefit from immobilisation from patients with simple muscular sprains.

We believe that further investigation is warranted to establish the benefit of rigid cervical immobilisation following whiplash injury.

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C. C. JOSLIN, MRCS
S. N. KHAN, FRCS(Orth)
G. C. BANNISTER
Southmead Hospital,
Bristol, UK.

1. Gurumoorthy D, Twomey LT, Batalin NJ. A prospective study of acute whiplash injury and its clinical management. *Whiplash Associated Disorder World Congress* 1999.