

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.

Pain related to the psoas muscle after total hip replacement

Sir,

I read with interest the article in the September 2002 issue by Jasani, Richards and Wynn-Jones¹ entitled 'Pain related to the psoas muscle after total hip replacement'. I entirely agree with the authors that when investigating a painful hip following total hip replacement (THR), causes other than infection and loosening of the prosthesis have to be considered. However, I would like to highlight psoas haematoma as another cause of groin pain, particularly in patients on anticoagulant medication.

Although this complication of anticoagulant therapy is well recognised, its importance in relation to THR has only been recently reported.² Early clinical diagnosis can be established by looking for weakness of the hip flexors and a sluggish quadriceps reflex following compression of the femoral nerve by the haematoma inside the psoas sheath. The diagnosis may be confirmed by a CT scan. Prompt intervention by stopping the anticoagulation may lead to a good outcome. If not, surgical decompression will be required.

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1. **Jasani V, Richards P, Wynn-Jones C.** Pain related to the psoas muscle after total hip replacement. *J Bone Joint Surg [Br]* 2002;84-B:991-3.
2. **Ankarath S, Campbell P.** Psoas haematoma presenting as hip pain. *Orthopaedics* 2001;24:689-90.

Authors' reply:

Sir,

We thank Mr Ankarath for his interest in our article. We agree with his comments that a psoas haematoma is a recognised cause of groin pain in patients on anticoagulants both with and without hip replacements.¹

Our article, however, demonstrated the distinct clinical entity of impingement between the psoas muscle and the implant itself. None of our patients had any evidence of femoral nerve compression or were on anticoagulants. Pain from a psoas haematoma is associated with weakness and is due to femoral nerve compression

rather than a direct impingement of the psoas on the total hip implant as described in our report. We found specifically that our patients had no evidence of femoral nerve compression, but had features implying psoas pain, such as pain on lifting a leg out of a car.²

We appreciate Mr Ankarath's comment, however, that a psoas haematoma in its own right is another possible differential diagnosis in patients with groin pain after total hip replacement.

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1. **Ankarath S, Campbell P.** Psoas haematoma presenting as hip pain. *Orthopaedics* 2001;24:689-90.
2. **Jasani V, Richards P, Wynn-Jones C.** Pain related to the psoas muscle after total hip replacement. *J Bone Joint Surg [Br]* 2002;84-B:991-3.

A ten-to-15 year follow-up of the Cementless Spotorno stem

Sir,

We read with interest the article in the March 2003 issue by Aldinger et al¹ entitled 'A ten-to-15-year follow-up of the Cementless Spotorno stem.'

We are encouraged by the long-term results that this femoral component is producing. Our centre has had similar success, particularly when used in patients under 50 years of age.² However, the authors describe the implant as double-tapered. We consider the implant to be a triple-taper as originally described by Blaha, Spotorno and Romagnoli.³ The stem is a wedge-shaped taper in three planes, frontal, sagittal and transverse.³⁻⁵ This is an important aspect of stem design and geometry, since the biochemical principle which contributes to the success of the stem is a three-dimensional taper which facilitates primary stabilisation in the proximal femur at the time of stem insertion prior to any secondary stabilisation by osseointegration. We believe that the three-dimensional taper also has a role in limiting subsidence by providing a more anatomical taper-lock.

The impact of stem design on the long-term outcome in cementless total hip arthroplasty is critical and we believe that the success of this implant is directly related to its design.

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1. **Aldinger PR, Breusch SJ, Lukoshek M, et al.** A ten-to-15-year follow-up of the Cementless Spotorno stem. *J Bone Joint Surg [Br]* 2003;85-B:209-14.
2. **Bailie AG, Doran E, Nixon JR.** Long-term results of a cementless self-locking tapered femoral stem in total hip replacement in young patients. *The British Hip Society Annual Meeting* 2003.
3. **Blaha JD, Spotorno L, Romagnoli S.** CLS press-fit total hip arthroplasty. *Techniques Orthop* 1991;6:80-6.
4. **Spotorno L, Romagnoli S, Ivaldo N, et al.** The CLS system: theoretical concept and results. *Acta Orthop Belg* 1993;59 Suppl 1:144-8.
5. **Romagnoli S.** Press-fit hip arthroplasty: a European alternative. *J Arthroplasty* 2002;17(4 Suppl 1):108-12.

Authors' reply:

Sir,

We thank Mr Bailie and Professor Nixon for their comments. They are absolutely right. The CLS stem is tapered in all three planes. We used the terminology as stated in the German manual by the manufacturer, Sulzer Orthopaedics (now Centrepulse), where the stem is described as a double taper.

We also believe that this design feature is of critical importance. Similar to those of Bailie, Doran and Nixon,¹ our results were particularly impressive in patients under 55 years of age with a survival rate of 97% (95% to 100%) after 13 years.^{2,3} The triple-tapered design of this stem might also contribute to the absence of severe stress shielding and distal cortical hypertrophy as shown in a continuous prospective seven-year study using DXA.^{2,4}

We believe that the low e-modulus titanium, the triple taper and the grit-blasted surface are the three key factors for the success of this implant.

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1. **Bailie AG, Doran E, Nixon JR.** Long-term results of a cementless self-locking tapered femoral stem in total hip replacement in young patients. *British Hip Society Annual Meeting* 2003.
2. **Aldinger PR, Thomsen M, Mau II, Ewerbeck V, Breusch SJ.** Cementless Spotorno tapered titanium stems: excellent 10 to 15 year survival in 141 young patients. *Acta Orthop Scand* 2003;73-3:253-9.
3. **Aldinger PR, Breusch SJ, Lukoschek M, et al.** A ten-to-15-year follow-up of the cementless Spotorno stem. *J Bone Joint Surg [Br]* 2003;85-B:209-14.
4. **Aldinger PR, Sabo D, Pritsch M, et al.** Pattern of periprosthetic bone remodelling around stable uncemented tapered hip stems: a prospective 84-month follow-up study and a Median 156-month cross-sectional study with DXA. *Calcif Tissue Int* 2003; Online first.

The effect of knee flexion on the popliteal artery and its surgical significance

Sir,

We read with interest the article in the March 2003 issue by Shetty et al¹ entitled 'The effect of knee flexion on the popliteal artery and its surgical significance'. We compliment them on their clear illustrations and would agree with the need to identify the movement of the popliteal artery (PA) with knee flexion and its clinical relevance. The 50 patients in this study appear to be a heterogeneous group who may or may not have knee pathology. This is clinically relevant as it is likely to represent a different population of patients from those who might undergo total knee replacement (TKR) or high tibial osteotomy.

We published a study showing similar findings in patients with arthritic knees prior to undergoing primary TKR, and also in patients following knee replacement. In none of our subjects undergoing a primary TKR (45 knees) did the popliteal artery move towards the tibia with knee flexion.²

We would agree with their conclusions but for different reasons. The fibrosis and thickening of the capsule and synovium in the arthritic knee, which is clearly seen during joint replacement surgery, is a more realistic reason for this movement of the PA away from the joint than the effect of gravity which will also be acting when the knee is extended, or uncoiling of the middle genicular vessels. When the knee is flexed, this hypertrophied tissue will displace the PA away from the knee joint. The study by Zaidi, Cobb and Bentley³ was on subjects with normal knees which will

not have had this soft tissue hypertrophy, and as those results suggest, there is no consistent trend in a knee, which is not arthritic. It is a shame that the bulk of the popliteus was not studied in more patients as this may be relevant in normal knees.

The sample population of a study must be a true representation of the population at risk if correct conclusions are to be drawn.

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1. **Shetty AA, Tindall AJ, Qureshi F, Divekar M, Fernando KWK.** The effect of knee flexion on the popliteal artery and its surgical significance. *J Bone Joint Surg [Br]* 2003;85-B:218-22.
2. **Farrington WJ, Charnley GJ, Harries SR, et al.** The position of the popliteal artery in the arthritic knee. *J Arthroplasty* 1999;14:800-2.
3. **Zaidi SHA, Cobb AG, Bentley G.** Danger to the popliteal artery in high tibial osteotomy. *J Bone Joint Surg [Br]* 1995;77-B:384-6.

Author's reply:

Sir,

We thank Messrs Farrington and Charnley for their comments, and accept their point that our sample was not selected to be representative of the type of patient who undergoes either total knee replacement or high tibial osteotomy. The mean age of our sample group was over 55 with the median age being much higher, and we would therefore expect many of the patients to have had, incidentally, mildly thickened posterior joint capsules.

We did not feel that the effect of gravity was a major factor, nor the uncoiling of the possibly sclerotic and inelastic genicular vessels. The limited number of axial MR images studied showed the effect of flexion on popliteus, which we believe is the key factor. We are extremely interested in the hypothesis offered that a similar effect is produced by shortening and thickening of the posterior capsule.

We appreciate these comments and agree that more work is needed to study the effect of knee flexion on the anterior popliteal structures.

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Impingement syndrome associated with whiplash injury

Sir,

We read with interest the article in the April 2003 issue by Chauhan, Peckham and Turner¹ entitled 'Impingement syndrome associated with whiplash injury'. Many patients with whiplash injuries claim compensation for them; the concept of secondary gain following whiplash associated injuries is well known.² The authors did not specify if patients with ongoing or planned litigation following the whiplash injury were excluded. The patients' perception may be that any evidence of concomitant shoulder pain after the injury may strengthen the case for compensation. We feel that the clinical tests used, although valid for the assessment of patients with impingement syndrome, may be unreliable if there are issues of secondary gain. Investigations, such as radiographs and MRI, may provide objective criteria for impingement syndrome. Furthermore, psychological assessment may be helpful in improving the selection of patients for inclusion in the study.

Further details on the length of time which elapsed following the whiplash injuries were not documented. This may be important as it has been suggested that residual disability following whiplash injuries decreased steadily to 8% at the end of four years.³ A prospective study, excluding patients with ongoing or planned litigation,

tion, with MRI and radiographs, may provide evidence or otherwise of impingement-type pain after whiplash injuries.

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1. **Chauhan SK, Peckham T, Turner R.** Impingement syndrome associated with whiplash injury. *J Bone Joint Surg [Br]* 2003;85-B:408-10.
2. **Evans RW.** Whiplash injuries. In: Macfarlane R, Hardy DG, eds. *Outcome after head, neck and spinal trauma*. Oxford: Butterworth Heinemann 1997:359-72.
3. **Murray PA, Pitcher M, Galasko CSB.** The cost of long-term disability from road traffic accidents, four-year study: final report. *Transport Research Laboratory Project* 1993:45.

Author's reply:

Sir,

We thank you for your interest in our article, and the questions you have raised. Of the 43 patients with impingement-type pain in our study group, nine were still in the process of litigation at the time of the study. Two were in the eventual good outcome group, five in the moderate and two in the poor. As stated in our paper, all patients were seen for the first time only after a period of six months had elapsed following their index injury. Any patient who had initial shoulder pain from his/her accident was excluded from the study, as this would have indicated direct injury to the rotator cuff, and it was, in fact, only those patients who had a delayed onset of pain who were targeted.

Impingement syndrome is a clinical diagnosis. Neer's sign and test when used in combination is, we believe, a reliable diagnostic indicator. We accept that MRI provides additional information, but the diagnosis should not be based on this or radiographs alone. We do, however, agree that a prospective study with MRI would provide further useful information regarding this topic. However, we would disagree that patients with ongoing or planned litigation should be excluded, as these provide an interesting, comparative group in terms of outcome.

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The provision of services for spinal disorders

Sir,

The editorial by Mr Gardner¹ and the letter in response by Mr Dowling's team from Dublin,² highlight some of the major problems that exist in orthopaedics and neurosurgery at present regarding the management of back pain and its associated disability.

Unfortunately, the editorial was written at a time when the effect of Primary Care Trusts (PCTs) on the local provision of healthcare was unknown. Recent experience has shown that local health administrators in the National Health Service (NHS) have little understanding of the importance of chronic disabling conditions such as lumbar spine pain. Because there are few tangible 'targets' that can be met by treating people with back pain, the subject is dismissed. When surgery is undertaken, it is often perceived as expensive, time-consuming and, depending on the institution, 'too complex' for the local health economy. When individual PCTs have responsibility for the health needs of only 175 000 to 200 000 people, any single PCT cannot understand the need for possibly expensive treatment, which is targeted at populations of 500 000 to 1 000 000 people. When such a service happens to be based in the hospital over which they have financial control nominally, they feel threatened. In Northampton, this has resulted in the disintegration of the integrated Spinal Assessment Unit, based on the Middlesborough model. Alan Gardner may have been correct that, in the past, the NHS was ideally suited to co-ordinate back pain services, but the latest in a long line of meddlesome reforms has meant that this is no longer true.

The UK, like every industrialised society, needs a robust system to handle back pain, effectively and quickly. This country seems unable to implement such a system through political incompetence in healthcare management. I would urge all of your readers outside of the UK to be alert to any similar changes that their governments want to introduce, lest similar catastrophes occur.

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1. **Gardner ADW.** Editorial: the provision of services for spinal disorders. *J Bone Joint Surg [Br]* 2002;84-B:313-4.
2. **Mofidi A, Sedhom M, Cassells M, et al.** The provision of services for spinal disorders. *J Bone Joint Surg [Br]* 2003;85-B:463.