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.

Superolateral wear of the acetabulum

Sir,

I read with interest the topic for debate in the March 1998 issue by Murray and O’Connor entitled ‘Superolateral wear of the acetabulum’. I agree that flexion and extension occur about a transverse axis and that internal rotation about a vertical axis during the stance phase will shift the site of maximum sliding velocity slightly laterally. I wonder if this is the most important reason for changing the axis of rotation laterally and distally. This can only happen with adduction of the leg or with a fixed leg in the stance phase due to a drop of the contralateral hip with some degree of Trendelenburg gait. I suggest that this could cause a much more superolateral redirected wear component than any rotation of the hip during the gait cycle.

This is supported by the finding of a slight Trendelenburg gait pattern in patients after total hip replacement. Analysis of adduction and abduction in patients after total hip replacement would appear to give a more likely explanation of the consistent superolateral wear patterns.

S. EHRENDORFER, MD
Vienna, Austria.


Author’s reply:

Sir,

I am pleased that our article has generated some debate and that Dr Ehrendorfer agrees that a combination of extension and internal rotation causes the axis of rotation to pass laterally and distally and therefore could contribute to superolateral wear.

He suggests that the Trendelenburg gait after total hip replacement is the main cause of superolateral wear. Although a Trendelenburg gait may contribute to superolateral wear it does not explain why this is associated with higher wear rates than superomedial wear. With a Trendelenburg gait the loads across the hip are likely to be lower than normal, and therefore the wear rate in a superolateral direction would actually be lower not higher. It is for this reason that I do not think that a Trendelenburg gait is the most important cause of superolateral wear.

D. MURRAY, FRCS
Nuffield Orthopaedic Centre
Oxford, UK.

The anatomy of acute scaphoid fractures

Sir,

I read with interest the study entitled ‘The anatomy of acute scaphoid fractures’ by Compson in the March 1998 issue. In Table II he mentions that 11 of the prospective 44 acute fractures were unclassifiable and, in his results, states that in 11 of the 47 retrospective cases it proved impossible to define fully their three-dimensional pattern. I think that this needs clarification or correction.

S. EHRENDORFER, MD
Vienna, Austria.


Author’s reply:

Sir,

I thank Dr Ehrendorfer for his letter. I agree with him that I have made a mistake. In the section on which he comments, the paragraph should begin “11 of the 44 prospective cases” rather than “the 47 retrospective cases” as has been printed. I apologise for this error, which I missed despite the fact that I had read it many times.

J. P. COMPSON, FRCS Orth
King’s College Hospital
London, UK.

Effect of particulate cobalt, chromium and cobalt-chromium alloy on human osteoblast-like cells in vitro

Sir,

We write concerning the article by Allen et al entitled ‘The effect of particulate cobalt, chromium and cobalt-chromium alloy on human osteoblast-like cells in vitro’ in the May 1997 issue. We feel that calling MG-63 cells “osteoblast-like cells” infers a degree of homology with human osteoblasts which is perhaps not warranted. We have compared the in vitro behaviour of MG-63 cells with that of human osteoblasts (hOB) and found that the MG-63 cells neither expressed alkaline phosphatase, when assessed qualitatively, nor showed mineralisation after three weeks of culture in an enhanced medium; both are features of hOB cells. Other authors such as Clover and Gowen and Jukkola et al have also noted that the differentiated function of these cells was “not very representative of bone cell culture.” It may be more appropriate to refer to these cells as ‘human osteosarcoma-derived cells’ and extrapolate from results with the necessary caution.

M. G. McALINDEN, BSc, MPhil, FRCS
D. J. WILSON, PhD
M. T. GIBSON, BMEdSci
Queen’s University of Belfast
Belfast, UK.


Author’s reply:

Sir,

We thank Mr McAlinden and colleagues for their letter. We were also unable to detect alkaline phosphatase activity in MG-63 cells and would agree that the cells do not reflect all aspects of the osteoblast phenotype. There are many inconsistencies in the literature regarding the classification of these cells. In their paper, Clover and Gowen refer to MG-63 as both “human osteosarcoma cell lines” (title) and “osteoblast-like cells” (abstract). In the introduction and discussion to our paper, we refer to the MG-63 and SaOS-2 cell lines as osteoblast-like osteosarcoma cells; in hindsight, it would have been more accurate to have used the same terminology in the title of the paper.

M. J. ALLEN, MA, VetMB, PhD, MRCVS
SUNY Health Sciences Centre
Syracuse, USA.


Diagnostic value of intra-articular anaesthetic in primary osteoarthritis of the hip

Sir,

I write with reference to the interesting paper of Crawford et al entitled ‘Diagnostic value of intra-articular anaesthetic in primary osteoarthritis of the hip’ in the March 1998 issue.1

The findings show that the relief of symptoms by the instillation of local anaesthetic into the joint has a positive predictive value of >96% for intra-articular pathology amenable to arthroplasty and not a sensitivity of this value as stated by the authors.

Sensitivity is a measure of the probability of a patient with the condition testing positive, i.e., true-positives/true-positives + false-negatives. Specificity is the probability of a patient who does not have the condition testing negative, i.e., true-negatives/true-negatives + false-positives.

Positive predictive value is the probability of a patient who tests positive having the disease, i.e., true-positives/true-positives + false-positives. Similarly, negative predictive value is the probability of a patient who tests negative not having the disease, i.e., true-negatives/true-negatives + false-negatives.

The ‘best-case scenario’ is that all those who do not have relief of pain with an intra-articular injection of local anaesthetic do not have osteoarthritis and the ‘worst-case scenario’ is that they all do and would benefit from total hip replacement (THR). Taking these two cases as extremes:

true-positives = 32 or 33 (relieved by injection and benefited from surgery)
false-positives = 0 or 1 (relieved by injection but no benefit from surgery)
true-negatives = 4 to 9 (no relief from injection and did not benefit from THR or benefited from treatment of alternative diagnosis)
false-negatives = 0 to 5 (no relief from injection and would have benefited from THR).

This is a relatively specific test (80% to 100%) with a high positive predictive value (96% to 100%) and a sensitivity of between 87% and 100%. The negative predictive value, however, may be as low as 44%.

In conclusion, those with a positive test can confidently be expected to benefit from THR, but the interpretation of a negative result should be more circumspect. Up to 65% of patients testing negative could have osteoarthritis of the hip and benefit from surgery.

P. ALLCOCK, FRCS G
Raigmore Hospital
Inverness, UK.


Author’s reply:

Sir,

We thank Mr Allcock for his correct comments on terminology and his analysis which supports the conclusions of our paper. Intra-articular local injection of anesthetic is very useful in identifying patients with hip disease. From our data, however, we cannot be certain, as we believe was made clear in our paper, as to how reliable this test is at excluding it.

R. W. CRAWFORD, FRACS Orth
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Oxford, UK.