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.

Percutaneous repair of ruptured tendo Achillis

Sir,

I read with much interest the article in the September 1999 issue by Webb and Bannister entitled ‘Percutaneous repair of the ruptured tendo Achillis’.

The authors describe a technique without failures and practically no complications. Their patients returned to work at four week and to sport after 16 weeks.

Table I reports the rate of reruptures in non-operatively-treated patients. These are as high as 35% (Persson and Wredmark), 30% (Edna) and 29% (Inglis et al). Mention is made of the complications of non-operative treatment, such as a lengthened tendon and reduced power of the calf muscles.

After reading the article I concluded that the simple, cheap and rapid conservative treatment of eight weeks in an equinus cast should be abandoned. The authors, however, surprisingly state that the comparison of their results with historical controls (as those in Table I) should be interpreted with caution. How do they justify this statement?

A. S. VIVALDI
Paderewski 1554
Santiago, Chile.


Author’s reply:

Sir,

We thank the author for his interest in our paper. We reported good results in our series of patients who had percutaneous repair for ruptures of tendo Achillis. We expressed caution in comparing our results with historical controls, since these results show a trend towards improving outcomes with all types of treatment.

J. WEBB, FRCS Orth
Avon Orthopaedic Centre
Bristol, UK.

The use of ultrasound in determining the initiation of treatment in instability of the hip in neonates

Sir,

We read with interest the article in the September 1999 issue by Holen et al entitled ‘The use of ultrasound in determining the initiation of treatment in instability of the hip in neonates’.

In this study, unstable hips have been graded as mild, moderate and severe based on the Barlow and Ortolani manoeuvres. These clinical tests are either positive or negative and an artificial grading system appears to introduce a qualitative element which does not exist.

Although the authors have stated that a “dynamic evaluation of instability” of the hips was carried out using ultrasound, a purely morphological technique for diagnosing unstable hips (femoral head coverage, FHC) has been used and the results of dynamic testing have not been presented.

Clinical tests for instability of the hip are highly specific (100%) but lack sensitivity (around 60%). Dynamic sonographic assessment is more sensitive than clinical examination alone in diagnosing instability of the hip. If dynamic evaluation of the hips was performed, there was a correlation between sonographic assessment of stability and morphological variables such as coverage of the femoral head?

J. S. SAMPATH, FRCS Ed
R. W. PATON, FRCS, FRCS Orth
Blackburn Royal Infirmary
Blackburn, UK.


Author’s reply:

Sir,

We thank Messrs Sampath and Paton for their comments.

With regard to our three clinical grades of instability, even if we had a high correlation between them and the femoral head coverage (FHC), we found, as stated in the paper, that this classification gave little information of clinical value, and is therefore unnecessary. We recommend that the clinical examination should be classified either as a negative, or a positive Barlow or Ortolani test.

We did not perform any measurements in our dynamic evaluation, because in our opinion, this is operator-dependent, the force used is difficult to predict, and standardisation is impossible. Mills and Shar1 stated that ultrasonographic evaluation should be based on reproducible and quantifiable images and this supports our view. This paper is part of a prospective, randomised trial comparing ultrasound and clinical screening of the hips in newborn infants. More than 15 000 infants have been included. If ultrasound is to be used for screening purposes, subjective assess-
ment of stability of the hip is hardly sufficient. Reproducible measurements are needed using the morphological methods either Graf or ourselves. In this study, one basic measurement was performed, the FHC with the hip slightly flexed, in the neutral position. This is similar to the standard coronal view described by Graf.

In our opinion, the most important factor in ultrasonographic evaluation of the hip in newborn infants is the coverage of the femoral head by the bony acetabular roof. Our ultrasound method measures this coverage. Therefore we are of the opinion that dynamic measurements would contribute little as the only method of ultrasonographic examination of the hip. We agree with Engesaeter et al, however, who stated that ultrasound examination of the hip should also include assessment of stability.

K. J. HOLEN, MD, PhD
Trondheim University Hospital
Trondheim, Norway.


Chlorhexidine and chondrolysis in the knee

Sir,

I read with interest in the November 1999 issue the article by van Huyssteen and Bracey entitled ‘Chlorhexidine and chondrolysis in the knee’. A previous case of chondrolysis using 0.02% chlorhexidine has been reported in a 41-year-old man after surgery to the knee. Reading their paper I was unsure as to the number of arthroscopies in which chlorhexidine had been used as the irrigation solution. Was this a standard practice or was it only used for three cases, and if so was its use stopped after a problem was noted?

I entirely agree with the authors’ conclusions that chlorhexidine should not be used as the irrigation solution for arthroscopy, but question their statement that it should not be used in traumatic wounds in which articular cartilage is exposed. Since two articles have been published in the past two years on chondrolysis associated with the use of chlorhexidine during arthroscopy a study specifically addressing this issue would be of interest.

A. D. READING, FRCS
Houston, UK


Author’s reply:

Sir,

We thank Mr Reading for his comments. Chlorhexidine 0.02% was used as the irrigation solution during only three consecutive arthroscopically-assisted repair procedures on the anterior cruciate ligament (ACL) during 1989. This constituted a change in practice by the operating surgeon compared with the preceding cases. The use of chlorhexidine in this way was then stopped after the disastrous outcomes in these three patients.

The aggressive destructive arthritis of early onset seen in these cases has never previously been described as a complication of any form of procedure on the ACL. Chlorhexidine, even in the low concentration of 0.02%, has a very damaging effect on the articular cartilage of the knee when used as an irrigation solution during an arthroscopic procedure. It may be that the irrigation solution has a longer time in contact with the articular cartilage during arthroscopy compared with during open surgery when the articular surfaces are exposed. However, until further studies conclusively demonstrate otherwise we advise that chlorhexidine 0.02% should not be used for irrigation when articular cartilage is exposed.

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