

criteria for the techniques of ultrasonography; how many of these were actually fulfilled by these observers? General experience with neonatal ultrasound scanning is not sufficient to interpret hip images properly: a few "discussions before the study" are not enough, however expert the paediatric radiologist or orthopaedic surgeon. The type of machine and its tuning are also vital to the Graf technique; it is more dependent on perfect images than the Harcke technique for reliable, consistent and reproducible results. 3) Femoral head measurements must be performed in standardised positions of flexion and rotation of the hip since the head is not perfectly spherical. The ingenious but questionable use of Mose rings on ultrasonograms done in 90° of flexion in some hips and surely 85° or 100° in others must introduce more problems of accurate measurements.

We agree with the authors that dynamic screening of the hip is very important, especially in the neonate, before soft-tissue contractures have developed to preclude the direction of instability. Dynamic scanning by an untrained hip ultrasonographer, however, is even more open to questions of reliability.

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#### Authors' reply:

Sir,

Thank you for the opportunity to reply to the letter from Drs Joseph and Meyer. They fail to appreciate the central point of our study. We tried to question the reproducibility of selected measurements, not to produce absolute values. The observers were being tested, not the standard of the scans: scans were not compared, the observers were. We do not believe that the static scans which we used were technically poor, but even if they were, the measurement technique should give better agreement between observers.

There is still a controversy about the technique of ultrasonic examination of the infant hip; some protagonists show an almost evangelical zeal. Our technique was that described by Clarke et al (1985) as the 'coronal-flexion' view, the only difference being that we scanned the infant lying on its side rather than on its back. The end points of the scan followed the principles of Graf with regard to the acetabulum in the 'standard plane' from the ilium and the acetabulum. Their shape does not depend on the position of the femoral head. Graf (1986) states that "the technique for evaluating the acetabulum on sonograms is position-independent . . . it does not matter how the infant is positioned or what position is assumed by the upper end of the femur."

Our measurements 1 to 7 inclusive were independent of the shape of the femoral head. Measurement 8 (the beta angle) may be influenced by the shape and position of the proximal femoral epiphysis but this was the only measure to be consistent, and measurement 9 uses the shape of the acetabulum to predict the putative geometrical centre of the femoral head.

Even Graf states that "Without doubt the measuring technique is subject to certain sources of error . . . it must therefore not be overrated". We invite Drs Joseph and Meyer to repeat our experiment and look forward to seeing their results.

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## KNEE REPLACEMENT IN HAEMOPHILIA

Sir,

In your May 1993 issue (1993; 75-B:368-70) Gregg-Smith et al described six cases of septic arthritis in severe haemophilia. We agree with much of their paper, especially that the orthopaedic complications of haemophilia should be treated in specialised centres. Our experience, however, leads us to question their contention that there is a high risk of secondary infection in HIV-positive men undergoing total knee replacement (TKR).

Of a series of 15 primary and two revision TKRs performed for haemophilic patients in this hospital since 1983 only one has become infected, and this was five years after the primary operation following a haemarthrosis that occurred at the same time as a dental infection.

Infection with HIV has profound immunological consequences, but it does not appear to predispose to secondary joint infection *per se*. Eight of our patients were HIV-positive at the time of operation; in one the TKR became infected. His CD4 count was  $0.35 \times 10^9/l$  preoperatively, and has remained stable for five years since his TKR. In four others who had CD4 counts of  $> 0.4 \times 10^9/l$  at the time of operation, there was no deterioration in the count after TKR. Two men with CD4 counts of  $< 0.2 \times 10^9/l$  died three years after their TKR (Birch et al 1993).

In a series of 27 TKRs, Weidel (1993) reported three men with secondary infection, all associated with poor hygiene or poor home treatment technique. All were HIV-positive and had CD4 counts above  $0.4 \times 10^9/l$ . Gregg-Smith et al report that all four of the men who were HIV-positive had CD4 counts of  $0.2 \times 10^9/l$  or less. Only one of them had a knee replacement.

We do not think that they provide sufficient evidence to suggest that TKR is unsuitable for all HIV-positive men with haemophilia. This operation has greatly improved the quality of all our patients' lives mainly because of relief from pain. In HIV-positive men with low CD4 counts, however, it should be stressed to the patients and their families that progression of HIV disease is possible.

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