Prevention of deep-vein thrombosis after total knee replacement

Sir,

The article by Blanchard et al.1 in the July 1999 issue entitled ‘Prevention of deep-vein thrombosis after total knee replacement’ is of particular interest and their findings raise several issues.

They state that there was no statistically significant difference in the prophylactic effect of the A-V Impulse System and low-molecular-weight heparin (LMWH, Fraxiparine), with respect to the potentially more dangerous proximal, deep-venous thrombosis (DVT), both treatments being associated with lower levels than would have been expected in the absence of effective prophylaxis. There was, however, a significantly higher incidence of total DVT in the A-V Impulse System group. This is not surprising since, by the authors’ own admission, elastic stockings were not used in patients in the A-V Impulse System group. This is contrary to the manufacturer’s recommendations. Other studies2,3 have shown that when used in conjunction with graduated elastic compression, the A-V Impulse System is as effective as LMWH in the prophylaxis of both total and proximal DVT. In particular, the much larger study by Warwick et al.2 showed that there was no statistically significant difference between the two treatments in terms of the incidence of total and proximal DVT, but that there were fewer soft-tissue side-effects associated with the Impulse System.

There is some ambiguity in regard to those patients discontinued from the A-V Impulse System group. Blanchard et al. state that 16 patients in this group were discontinued, and since 63 were originally allocated to it, then 47 should have been available for objective DVT analysis. In the event, however, a total of 59 patients was included for objective DVT analysis. This suggests that in accordance with the protocol described by the authors, 12 patients discontinued from the mechanical group must therefore have gone on to receive LMWH before assessment of DVT. Neither the dose of LMWH to which these patients were exposed nor the rationale for the scientific acceptability of this practice is indicated in the paper.

One patient in the LMWH group had a major haemorrhage after operation which required a transfusion of 5.5 litres of blood. It is surprising that the authors attach relatively little importance to a bleed of this magnitude and severity. They conclude that the incidence of troublesome bleeding in the LMWH group, a matter of concern for all orthopaedic surgeons, was “less important than anticipated” and yet state that the 95% confidence interval (0.04 to 8.3) does not exclude the possibility of a higher frequency of severe bleeding.

In the light of recent debate concerning the justification for thromboprophylaxis, when the incidence of pulmonary embolism is known to be much lower than previously thought, the appearance of such complications is of paramount importance. It is imperative that this balance between risk and benefit is considered when selecting the appropriate method of prophylaxis for high-risk patients.

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Authors’ reply:

Sir,

The issues raised by Dr Goldberg are well taken. First, the aim of our study was to compare the efficacy and safety of two antithrombotic regimens in total knee replacement (TKR) namely, a once-daily subcutaneous injection of nadroparin, a low-molecular-weight heparin (LMWH) and the use of a novel intermittent compression system of the foot, the A-V Impulse System, marketed by Dr Goldberg’s company. Elastic stockings were not part of the study. It is possible that the combination of the A-V Impulse System with stockings would be more efficacious than the compression system alone but this remains to be established in an appropriate controlled, randomised study.

Secondly, the results were analysed on an intention-to-treat principle, which means that patients who discontinued one prophylactic regimen were still considered as being in the group to which they had been assigned initially in a random fashion. Of course, they had prophylactic LMWH after discontinuation of mechanical prevention, which should have had a positive rather than a negative influence on the results in the A-V Impulse System group. Finally, it is true that the only case of severe bleeding (1.5%; 95% CI 0.04 to 8.3) which occurred during the study was in the LMWH group. This adverse event should not obscure the fact that patients allocated to mechanical prophylaxis had twice as much evidence of DVT on phlebography than those given LMWH, a prevalence of 64.6% which is close to that reported with no prophylaxis. The definite superiority of LMWH compared with the foot pump system in TKR has also recently been reported in a small Swedish study.4

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References


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Polyethylene wear, osteolysis and acetabular loosening with an HA-coated hip prosthesis

Sirs,

We read with interest the article by Røkkum et al. in the July 1999 issue entitled ‘Polyethylene wear, osteolysis and acetabular loosening with an HA-coated hip prosthesis’. The results of the hemispherical hydroxyapatite-coated (HA) screw cup are poor, as has recently been reported by the authors, but the reason for this is not addressed in their article. However, it may be explained by a number of factors. The survey included a design of screw cup which in several studies has been shown to give higher rates of revision than those of porous-coated and cemented cups. Moreover, the size of the femoral head was 32 mm. This together with the poor quality of the polyethylene (PE) and poor geometry between the head and liner, in addition to micromovements between the implant and surrounding tissue will accelerate global resorption of the HA coating. By contrast, in a stable situation bone ingrowth seems to protect against resorption of HA.

The title of the article and the text give the impression that the HA coating used was also thicker than recommended in the literature. Furthermore, it was applied to an implant with a smooth surface. This has already been shown to be a risk for coating delamination in contrast to application on a porous-coated surface. Finally, it is unlikely that the entire HA coating delaminated, but micromovements between the implant and surrounding tissue will accelerate global resorption of the HA coating. By contrast, in a stable situation bone ingrowth seems to protect against resorption of HA.

The title of the article and the text give the impression that the HA coating and PE particles caused accelerated wear, osteolysis and loosening of the cup. In order to draw these conclusions, however, a control group without HA on the cup is required.

We believe that the excessive PE wear and loosening of the cups are most likely caused by the design of the screw cup in combination with poor quality of the PE. The effect of HA particles on third-body wear and the formation of osteolytic lesions has not been studied. It could be hypothesised that HA coating prolonged the survival of the cups compared with a non-HA-coated cup. The results of the article are related to one type of implant and no conclusions on HA-coated prostheses in general can be made. We advocate that quality reports should be available for each batch of prostheses delivered, and that HA coating should not be used to improve a poor design of implant.

We thank Dr Overgaard and Dr Soballe for their comments. They find that the reason for the poor results with the HA-coated screw cup is not given in our article and propose a number of explanations. Most of these (32 mm femoral heads, polyethylene quality, internal cup geometry, wear between liner and shell, thickness of the HA coat) however, were addressed in our discussion.

We do not agree that the use of all screw cups should be abandoned, although an unsatisfactory outcome has been reported with several types, all with the common feature of a very smooth external surface. Osseointegration improves with rougher surfaces and this has been proposed to explain partially the beneficial bone response to HA. The superiority of porous-coated hemispherical cups compared with screw cups may depend on the difference in the surface roughness. We had poor results with a hemispherical HA-coated press-fit cup, which was identical to the screw cup which we reported, apart from the threads. Recently, this difference was confirmed by the Norwegian Arthroplasty Register. We believe that the threads improve the primary stability, leading to increased bone ingrowth with the HA-coated screw cup compared with the identical unthreaded cup.

Radiologically, all our screw cups showed extensive bony incorporation without lines or migration and this was a consistent finding throughout most of the examinations. Even the sudden loosening of the cup occurred without the presence of lines. This is not consistent with instability of the cup in which lines would have been expected to form during the years of observation. The complete loss of HA from the retrieved cups may be explained by global resorption of the coat or delamination.

Extensive metallosis without worn-through polyethylene, scoring of the steel heads and disappearance of HA from parts of the stems uncovered with bone led to the suggestion of third-body wear with third bodies composed of HA particles. Further research is required to establish the exact relationship between particles and excessive PE wear and osteolysis with HA-coated prostheses.

Authors’ reply:

Sirs,

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Is diagnostic arthroscopy of the hip worthwhile?

Sir,
We read with interest the article by Baber et al in the July 1999 issue entitled ‘Is diagnostic arthroscopy of the hip worthwhile?’.

There were two points which we found confusing. First, 154 patients were diagnosed as having ‘idiopathic hip pain’ on the basis of a normal radiograph. It is clear from Table II that the final diagnosis in many of these cases, such as osteoarthritis, labral tears and osteochondral defects, is definable by other investigations such as CT or MRI. Therefore should this diagnosis have been made on the basis of clinical examination and plain radiography alone?

Secondly, in Table I a range of preoperative and postoperative diagnoses is given. Before operation seven patients were diagnosed as having acetabular dysplasia, four Perthes’ disease and one protrusio acetabuli, but afterwards none of these diagnoses remained. All were based on a clinical history and examination combined with the presence of radiological abnormalities. While we concede that arthroscopy of the hip may alter the primary diagnosis surely these clinical diagnoses should have remained as the secondary diagnosis.

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Sheffield, UK.


Authors’ reply:

Sir, We thank Messrs Chell and Flowers for their comments on our paper and will deal with their points in turn.

Of the patients in our series 124 had had MRI, CT or arthrography before their arthroscopy. All the available information, including the results of the scans, was used in reaching the preoperative diagnosis. The routine use of these tests is not part of our standard preoperative assessment. The reason is that we consider it necessary that the result of an investigation should alter the patient’s management and thus, in this series, avoid an arthroscopic procedure.

The purpose of our paper was not to investigate clinical outcome, but to examine the alteration of diagnosis, and the potential for therapeutic intervention. In the future, arthroscopy of the hip needs to be substantiated by studies on clinical outcome.

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Osteoid osteoma

Sir, We read with interest the article by Campanacci et al in the September 1999 issue entitled ‘Osteoid osteoma’ and have some comments regarding the imaging of these lesions.

Not all patients need a preoperative bone scan. Despite the fact that a typical double-density sign for the nidus has been described in the literature, thin-section CT and dynamic CT are more specific in differentiating a nidus and sequestration seen in chronic osteomyelitis. Therefore bone scans are only required if the plain radiograph fails to guide the CT scan to the area of interest. If the patient has no typical history or the thin-section CT scan (≤3 mm slice thickness) identifies a nidus which may be explained by sequestration after chronic osteomyelitis, dynamic CT is the technique of choice. The hypervascular nidus shows a pattern of enhancement similar to that of adjacent major arteries in comparison with the relatively hypovascular Brodie’s abscess. When the nidus has been localised the next step is CT-guided radiofrequency ablation under general anaesthesia. Results have shown that the technique is cost-effective and promising with a low morbidity. If a CT-independent intervention such as intralesional excision or wide resection is planned MRI or dynamic MRI may be indicated to reduce CT-related radiation in young patients. Although the high sensitivity of MRI in detecting reactive changes in bone marrow and soft tissues may cause various erroneous diagnoses, such as malignant tumours or stress fractures, an experienced radiologist should be able to localise the nidus in most cases. If the MRI fails to confirm the diagnosis CT is warranted.

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THE JOURNAL OF BONE AND JOINT SURGERY


Author's reply:

Sir,

We thank Drs Böttner, Wörtler and Link for their comments. We do not at present have enough experience of CT-guided radiofrequency ablation, even if we believe that it is a reliable technique for treatment of osteoid osteomas or Brodie’s abscesses.

A precise preoperative study of the lesion is mandatory regardless of the treatment chosen. We agree that sometimes a bone scan may be unnecessary, but it is still helpful for diagnostic purposes when symptoms and/or standard radiographs are not clear enough to guide the thin-section CT scan.

In our experience MRI is equal or inferior to CT in demonstrating the nidus. It is very sensitive for showing the perilesional inflammatory reaction, and this may obscure the diagnosis. Moreover, if MRI does not show the exact site of the lesion, CT is indicated. We prefer to use the latter as the primary investigation.

In any case treatment with radiofrequency requires CT, and therefore young patients are exposed to CT-related radiation if treated by this technique.

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Infections associated with dental procedures in total hip arthroplasty

Sir,

I read with interest the article in the January 1999 issue by LaPorte et al. entitled ‘Infections associated with dental procedures in total hip arthroplasty’. Their findings show that infection of total hip arthroplasty (THA) may be more common than previously suspected. They suggest that patients with systemic disease, or those undergoing extensive dental procedures, should be considered for prophylactic treatment with antibiotics before having dental treatment.

This may well be sensible advice, but their paper does support an alternative strategy to try to prevent late infection of THA. In the discussion they report that none of the patients had an active dental infection at the time of their oral procedure, but in Table I one patient (case 1) had multiple extractions for pain, another (case 2) had root canal therapy on multiple roots and a third (case 3) had treatment for periodontal disease.

Diseases such as caries, apical abscess, cyst or periodontitis are due to neglect of the teeth and may many years to develop although symptoms may arise overnight. Both caries and periodontitis are endemic in the elderly population. Periodontal disease in particular is a chronic, progressive condition which in severe cases may give rise to a transient bacteraemia every time the patient brushes his teeth or even grinds his teeth together.

Surely a more logical strategy to prevent infection of THA would be to eliminate all dental sepsis before surgery so that the risk of transient bacteraemia after surgery because of dental sepsis and its treatment could be minimised.

Orthopaedic surgeons are meticulous in their surgical technique to avoid any risk of sepsis at the time of surgery. It is therefore surprising that they are prepared to insert a prosthesis in the face of gross dental sepsis.

I would suggest that all patients listed for joint replacements should have a full dental examination. All dental pathology should be treated and eliminated before surgery and regular dental check-ups instituted after surgery.

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Authors’ reply:

Sir,

We appreciate the comments made by Dr Rees concerning our article. We agree that the best prevention would be for patients to have regular visits to their dentists to achieve excellent dentition. Nevertheless, any patient having extensive procedures or who is immunocompromised should have prophylactic antibiotics before undergoing dental treatment. We think that his comments are certainly constructive with regard to the avoidance of infections in patients with total joint replacement.

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Degenerative spondylolisthesis: developmental or acquired

Sir,

I read with interest the excellent paper by Love, Fagan and Fraser in the July 1999 issue entitled ‘Degenerative spondylolisthesis.’ They observed that it is by far most common at the L4-5 level and four or five times more so in women than in men. The discussion reviewed various explanations for the gender difference, but finally they agreed that no clear-cut interpretation was available.

I am curious as to why they did not consider the reason for the gender difference on the basis of the wider female pelvis. Certainly, walking for about 40 years with the pelvic rotation relative to the lumbar spine would create greater stresses to the facet joint at the pelvic level in the female with the greater lever arm. The facet changes are a degenerative process secondary to uncompensated overload. The stresses would seem to be greater in the female.

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Radiological factors influencing femoral and acetabular failure in cemented Charnley total hip arthroplasties

Sir,
I refer to the article of Ritter et al.1 titled ‘Radiological factors influencing femoral and acetabular failure in cemented Charnley total hip arthroplasties’ published in the November 1999 issue. I believe that it is badly titled.

Radiological factors do not ‘influence … failure’. They may indicate the potential for failure, or be signs of increased risk of failure, but they are merely shadows on film, and should only influence the surgeon concerned to do better next time.

F. H. OLIN, MD
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Editor’s reply:

Sir,
I am most grateful to Dr Olin for pointing out my misuse of language. At least the Journal has one close reader!

There is no doubt that he is right and that my attempt to shorten a somewhat long title produced etymological inaccuracy. It would have been much better to have replaced ‘influencing’ by ‘in the assessment of’.

I hang my head in shame, and hope to do better next time.


Idiopathic scoliosis in twins studied by DNA fingerprinting

Sir,
Since epidemiological studies using samples of twins are potentially very valuable in disorders in which both environmental and genetic factors are suspected as being causative, we therefore read with interest the article by Inoue et al.1 in the March 1998 issue on idiopathic scoliosis in twins. We feel, however, that there are several weaknesses and methodological errors in the paper.

In studies on twins three types of concordance are commonly used: a) pairwise, b) casewise, and c) probandwise.2 Apparently, the authors estimated the pairwise concordance. This makes the issue of ascertainment very important. How were the twins in this study recruited? If only hospital records were used there is a high risk of overestimating the occurrence, especially in a small sample such as this (only 21 pairs). Furthermore, the rate of pairwise concordance does not predict risk on an individual level and cannot be compared with estimates of risk for other individuals (e.g., general population or ordinary siblings). Probandwise concordance is not sensitive to incomplete ascertainment and is therefore normally used in studies on twins, but this rate cannot be calculated based on the information provided in the paper.

The MZ and DZ twin groups in this sample were not statistically significantly different when applying 95% confidence intervals (MZ 92% [64 to 100], DZ 62% [24 to 91]). Thus, the statement that “Our findings suggest that there is a genetic factor in the aetiology of idiopathic scoliosis” appears to be unjustified. This does not mean that genetic factors may not play a role in idiopathic scoliosis. In the discussion the authors pool data from three reports and they do indeed find a statistically significant difference between the MZ and DZ rates (93% [77 to 99], 42% [20 to 67]) but again the deviation of the material is not reported for any of the papers, making it likely that there is the possibility of a strong selection bias. If the raw percentages of occurrence of disease are examined (93 and 42 for MZ and DZ twins, respectively) then idiopathic scoliosis may be an autosomal dominantly-inherited disease for which the expected percentages would be 100 for MZ twins and 50 for DZ twins. To estimate reliably the true contribution of genetic factors large population-based twin registers or familial studies using samples as starting points as in this paper are recommended. The incidence cannot be estimated as erroneously stated in the subtitle. This would require at least a longitudinal study.

We also question the relevance of the conclusions based on the measurements of the Cobb angle. In 1996 Loder et al.1 published a study on the variability of the Cobb angle in children with congenital scoliosis. They found that there should be at least a difference in the angle of 23° to ensure that any perceived difference was not due to variability in the measurements alone. Based on this it seems redundant and with little clinical significance to divide this small material into groups with increments of only 5°.

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