The tourniquet in total knee arthroplasty

Sir,

We read with interest the article by Wakankar et al.¹ in the January 1999 issue entitled ‘The tourniquet in total knee arthroplasty’.²

There are several points which we wish to raise. It is unclear how many surgeons, and of what grade, operated and where the surgery was performed. It is not stated what problems were encountered with bleeding when a tourniquet was not used. Although they conclude that the tourniquet can be used satisfactorily, more wound problems were observed in this group, including one wound still leaking at six weeks.

Concern was expressed about haemostasis and the ability to create an appropriate bone surface for applying cement when a tourniquet was not used. A combination of regional anaesthesia, exposure of the joint with the knee flexed and the use of intraoperative suction within 1 cm of the cut bone surfaces with lavage, creates an ideal environment and considerably reduces airborne debris.

If a tourniquet is used the cuff pressure should be restricted to 100 mmHg above the systolic pressure.³ Its use is strongly contraindicated in the presence of diabetes, peripheral vascular disease, rheumatoid disease, previous thromboembolism, active malignancy, multiply scarred legs, obese thighs and for revision surgery.

Since so many authors have attributed complications to the use of the tourniquet, such as nerve paralysis, vascular injury, circulatory changes on exsanguination with cardiac or respiratory problems, cardiac arrest, pulmonary oedema, increased rates of DVT and, recently, increased rates of embolism,⁴ does it not seem that the best practice is to avoid its use? We have shown that knee replacement surgery can now be performed with confidence without the use of the tourniquet and that many of the local and systemic complications of operating in a bloodless field can be avoided.⁴

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Authors’ reply:

Sir,

We thank Messrs Eyres, Abdel-Salam and Sharpe for their letter and have noted their comments with interest.

Our study had almost the same structure as that of Abdel-Salam and Eyres, but we used many strict exclusion criteria for the selection of patients such as rheumatoid arthritis, diabetes, etc., to reduce the confounding factors. Our results do not match those of Abdel-Salam and Eyres and the selection of patients may be one of the reasons. Both studies had small numbers of patients with the obvious limitations as a result.

All our patients were under the care of one consultant in one hospital and there were five surgeons (including two registrars and two associate specialists) following the predecided protocol. All trainee surgeons were supervised. When operating without a tourniquet surgeons considered bleeding to be a nuisance in seven knees and noted moderate to heavy bleeding in the form of constant oozing in six. In none of these cases did the tourniquet need to be applied and the surgery was completed satisfactorily.

All wounds had healed at the final review at four months. The patient with a small leaking wound had no evidence of deep infection and in our view this case does not raise concern regarding the use of a tourniquet especially since the wound had healed at four months. We would again draw attention to the fact that our patients had to satisfy exclusion criteria thereby reducing confounding factors. We entirely agree that surgery can be safely performed without a tourniquet especially in the presence of the many risk factors quoted; all patients with such problems were excluded from our study. In their absence, however, we have found it safe to use a tourniquet.

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total knee replacement (TKR) are at increased risk of popliteal and calf-vein thrombosis (CVT). Studies show that 12% to 32% of CVT may propagate, which increases the risk of pulmonary embolism. Duplex ultrasonography is still unreliable for diagnosing CVT and its use in this study may be partially responsible for the low incidence of DVT reported, which jeopardises the validity of the results and conclusions. We suggest that a more reliable diagnostic tool such as contrast phlebography should have been used.

Although the only DVT reported in the study was in the tourniquet group, and the difference was reported as insignificant, no mention was made of the numbers in either group.

The application of a tourniquet may impair venous drainage, damage the venous endothelium and confine coagulation reactants to below the tourniquet. Clinical evidence of the risk of DVT with a tourniquet in TKR is still inconclusive.\(^\text{6-8,9}\) The release of large venous emboli after deflation of a tourniquet during cemented intramedullary TKR has been shown to increase pulmonary vascular resistance and may contribute to fatal or nearly fatal intraoperative cardiac arrest.\(^\text{10-12}\) Moreover, Parmet et al.\(^\text{10}\) have shown that compared with TKR without a tourniquet, the use of a tourniquet places patients at a 5.33-fold greater risk of large emboli. We are concerned that, pending the availability of irrefutable evidence, based on adequate numbers and robust methodology, it may be dangerous to ignore the risk of venous thrombosis and potential fatal embolism associated with the continued use of a tourniquet in TKR.

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Authors’ reply:

Sir,

We thank Messrs Agu, Baher and Hamilton for their comments. Calf-vein thrombosis is a very controversial subject and there are reports both against and in favour\(^\text{1-3}\) of active treatment. Lotke, Steinberg and Ecker\(^\text{9}\) suggest that calf thrombi after total joint arthroplasty do not place a patient at risk.

When planning this study, we were limited to the use of Duplex ultrasonography as a method of diagnosing deep-vein thrombosis (DVT). Of the 77 patients in our study 51 (66%) had Duplex ultrasonography before and after operation; these included 25 and 26 patients from each group.

Many hospitals, including ours, have now given up the use of venous phlebography for diagnosing DVT and routinely use Duplex ultrasonography. This has the obvious implication that calf-vein thrombosis would not be adequately diagnosed and by default not treated. Whether this policy has led to increased numbers of deaths from pulmonary embolism remains unknown.

In our study, three patients died 16 weeks to 14 months after surgery. Two had had surgery without a tourniquet. All three had Duplex ultrasonography postoperatively which did not show evidence of DVT. Our small study did not show any serious consequences with the use of a tourniquet in total knee replacement in a controlled group of patients which satisfied exclusion criteria.

We entirely accept that contrast venography is the optimum method of diagnosis of DVT, especially in the calf. There have been several studies on this subject but reliable evidence concerning calf DVT is not forthcoming in the absence of a multicentre trial of a large number of patients with minimal confounding factors. Prophylaxis for DVT in our study included the use of low-dose Warfarin for four weeks and foot pumps for a variable postoperative period. We are not ignoring the risk of DVT and pulmonary embolism but we failed to confirm the findings of Parmet et al.\(^\text{10}\) in our small study.

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References

Sir,

We read with interest the article by Wakankar et al1 in the January 1999 issue entitled ‘The tourniquet in total knee arthroplasty’. We do not agree with their finding of the similarity of the intensity of early postoperative pain between those with the tourniquet applied and those without. We believe that tourniquet-induced limb ischaemia significantly increases the intensity of early postoperative pain in orthopaedic procedures in both the upper and lower limbs.2,3 Ischaemia induced by use of a tourniquet for longer than one hour leads to ultrastructural damage to the skeletal muscle distal to the cuff, which may be the first step towards muscular atrophy.4 This may explain the early decrease in the range of movement due to delayed recovery of muscle in the patients who had a tourniquet.

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Authors’ reply:

Sir,

We thank Drs Öméroğlu and Seber for their interest in our paper.

Pain is difficult to quantify and the requirement of analgesics is just one of the many inadequate ways of judging it. The number of patients in the study is small and although the average requirement for analgesics in the group with a tourniquet was higher, we found no statistical difference between the two groups. Dr Öméroğlu may well be correct in suggesting that tourniquet-induced ischaemia increases the intensity of early postoperative pain. The early decrease in the range of movements with the use of a tourniquet is perhaps due to some muscle injury, possibly at ultrastructural level. The final outcome showed no difference in knee flexion in the two groups, however, suggesting that there was full muscle recovery from any temporary damage.

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Acute fractures of the scaphoid

Sir,

I write with reference to the paper in the January 1999 issue by Hambidge et al1 entitled ‘Acute fractures of the scaphoid’.

The authors found extension to be significantly less in those wrists which had been immobilised in flexion and recommend that these fractures be splinted in extension. The difference in wrist extension between the groups is about 10°, which matches the normal difference in wrist extension between the right and left sides.2 There is no information as to whether the left or right scaphoids were fractured, but if most of the patients immobilised in extension had left-sided fractures, and those with immobilisation in flexion had right-sided injuries, then the difference will not be significant. It is of interest that the authors did not find a significant difference between the groups with reference to wrist flexion, and there is no significant difference in wrist flexion between the sides in normal subjects.2

The authors conclude that “immobilisation of the wrist is important for union, rather than the position”. In my opinion, the important factor is the immobilisation of the scaphoid, not the wrist.3

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Author’s reply:

Sir,

I thank Dr Günal for his comments and point out that, as mentioned in our paper, the two treatment groups were well matched for side of injury.

Of the 121 fractures, 58 occurred in the dominant wrist and the mean wrist extension in the uninjured wrists was 73° (SD = 10). When the dominant and non-dominant wrist injuries were analysed separately, the significant reduction in extension was still detected. Furthermore, for the 11 injured wrists with less than 45° extension at follow-up, the mean wrist extension in the contralateral uninjured wrist was 63° (SD = 15). We thus consider that the observed reduction in wrist extension after treatment of scaphoid fractures in a Colles’-type cast which holds the wrist flexed, is an accurate observation.

We agree that in order to achieve union of scaphoid fractures it is vital to immobilise the scaphoid bone. In our opinion the simplest, safest and most cost-effective method is to place the wrist in a plaster cast.

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Salvage of the head of the radius after fracture-dislocation of the elbow

Sir,

I read with interest the article in the March 1999 issue by Patel and Elliott1 entitled ‘Salvage of the head of the radius after fracture-dislocation of the elbow’ and congratulate them on their method using a tricortical autograft.

Fracture-dislocations of the elbow can be associated with injuries to the insertion of the biceps tendon. Since the bicipital...
tuberosity is just distal to the radial neck, it appears to have been involved in the original injury. It is also noted that this 61-year-old patient was able to obtain a useful arc of elbow flexion and extension, yet lacked 80° of forearm supination.

What was the condition of the biceps at the time of surgery? Was it reattached to the tricortical autograft? If not, this may explain the patient’s loss of forearm rotation, or is this due to the ipsilateral fracture of the distal radius?

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Authors’ reply:

Sir,

Thank you for the opportunity to reply to Dr Torosian’s letter. The patient which we described had a horrendous injury to the elbow and at the time of surgery the priority was to regain some stability at the elbow. No specific attempt was made to identify the biceps tendon.

At follow-up, rupture of the distal tendon of the biceps was not clinically apparent. We surmise that the limitation of rotation was partly due to scarring around the proximal radius and partly to the fracture of the distal radius.

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The pathology of bone allograft

Sir,

We read with interest the article in the March 1999 issue entitled ‘The pathology of bone allograft’ by Palmer et al.1 We have found similar results in a study of 123 iliac bone biopsies from potential bone allograft donors using light microscopy and histomorphometric analysis. Approximately 10% of biopsies showed severe osteoporosis. There was one case of a myeloproliferative disorder and one of bone-marrow aplasia. These conditions were present despite routine screening methods such as those outlined in the paper by Palmer et al. Although the clinical implications of transplanting severely osteoporotic bone are uncertain and there is debate as to what extent marrow abnormalities survive processing in a bone bank, we agree with the conclusion that occult findings are common and that histological examination of bone-allograft donors should be included in routine screening procedures by bone banks.

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Author’s reply:

Sir,

We thank Dr Siddiqui and his colleagues for their comments and would be interested to see their results on the histological examination of their bone biopsy specimens. It is obvious that occult findings of unsuspected pathology are not uncommon and that some form of histological screening is necessary to ensure safe standards of bone transplantation.

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