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.

TRANEXAMIC ACID REDUCES BLOOD LOSS AFTER KNEE ARTHROPLASTY

Sir,

We write regarding the paper in the May 1996 issue entitled ‘Fibrinolytic inhibition with tranexamic acid reduces blood loss and blood transfusion after knee arthroplasty’ by Benoni and Fredin (1996;78-B:434-40).

We are concerned that the objective of reducing blood loss was achieved using an antifibrinolytic agent without screening for any potential increase in thromboembolic complications. Tranexamic acid is contraindicated in patients with a history of thromboembolism and hypofibrinolysis is a consistent feature of familial thromboembolic disease. There is therefore at least a theoretical risk of iatrogenic complications. Venography was only performed in symptomatic patients. Most postoperative venous thromboses are asymptomatic, however, particularly the proximal ones (Oishi et al 1994). The small number of patients who had venography (18 of 86) invalidates any comparison of the true incidence of thrombotic complications.

The reason given for not screening was that previous research has failed to show that tranexamic acid has any thrombogenic effects after surgery (Becker and Borgstrom 1968; Gordon-Smith, Hickman and El Masri 1972; Hedlund 1975; Bekassy and Astedt 1990). The study of Gordon-Smith et al involved only 62 patients and that of Bekassy and Astedt relied on clinical presentation. Furthermore, these studies involved either animals or non-orthopaedic patients, and therefore cannot be applied to the situation after total knee replacement. Additional factors leading to a higher incidence of thrombosis include direct trauma to deep veins, endothelial damage after the use of a tourniquet (Zahavi et al 1980), a longer operation time, postoperative immobility and marked changes in the indices of thrombosis and fibrinolysis (Sharrock et al 1995).

The authors quote the paper of Eriksson et al (1988) showing delayed development of thrombosis with low-molecular-weight heparin (LMWH) speculating that this would protect against any thrombogenic effect of tranexamic acid given in the first three hours after surgery. First, this assumes that the effects of LMWH are not changed by the simultaneous administration of tranexamic acid, an assumption for which we can find no evidence, and, secondly, their conclusion was drawn from a sample of only ten patients who developed deep-vein thrombosis having undergone total hip replacement. Tourniquet release after total knee replacement results in an immediate rise in thrombin-antithrombin complex and d-dimer levels (Sharrock et al 1995), indicating that venous thrombosis begins during surgery, and this has been confirmed in a study using a femoral venous catheter (Parmet et al 1994). The investigation of Astedt, Liedholm and Wingerup (1978) which showed no decrease in fibrinolytic activity in vein walls is not relevant as it was performed on harvested superficial veins in patients on long-term therapy. The authors’ opinion therefore that it is highly unlikely that they would have been able to show any thrombogenic effect with tranexamic acid, is based either on work which has little relevance to total knee arthroplasty, or on studies which, because of the small numbers involved, would not show a significant difference. Under these circumstances, we feel that all patients in the study should have received adequate screening for venous thromboembolism, because the consequences of missing an increased incidence could be disastrous. Finally, the statement that tranexamic acid has little effect when given after heavy blood loss cannot be made: there was no control group.


**Authors’ reply:**

Sir,

We thank Drs Howes, Sharma and Cohen for their valuable comments on our article. We have shown that tranexamic acid, used during the early, hyperfibrinolytic, postoperative phase, reduces blood loss and the need for blood transfusion in knee arthroplasty. So far there are no reports of an increased incidence of deep-vein thrombosis (DVT), but we agree that a prospective study on tranexamic acid and the risk of postoperative thrombosis in orthopaedic surgery would be of interest. Our study aimed, however, to detect differences in blood loss. A study on the frequency of thrombosis calls for considerably greater numbers of patients.

In the dosage which we used, tranexamic acid is at therapeutic concentration in the plasma for approximately eight hours after operation (Benoni, Björkman and Fredin 1995), which is a period of hyperfibrinolysis with a high rate of blood loss.

Coagulation and fibrinolysis are stimulated by trauma. The release of d-dimers and the markers of coagulation found by Sharrock et al (1995) occurs after any major operation (Risberg 1985; Horow; Van Riper and Strong 1991; Dahl et al 1993), but this is not a proof of established DVT. The process is further augmented by the use of a tourniquet. We have analysed d-dimers and prothrombin fragments in 24 patients included in our study and found that tranexamic acid influenced d-dimers in the wound but not in the peripheral venous blood (Benoni, Lethagen and Fredin, unpublished data).

The study by Eriksson (1991) and more recent reports indicate that with standard prophylaxis with low-molecular-weight heparin (LMWH), DVT formation after arthroplasties is a prolonged process. In most cases it seems to start postoperatively during the period of fibrinolytic shutdown (Prins and Hirsh 1991), or later, up to several weeks after the operation (Bergqvist et al 1996; Planes et al 1996). We find it unlikely that tranexamic acid, given during the early hours of hyperfibrinolysis, will show any substantial effect on postoperative DVT formation. We have not found any reports which show interaction between tranexamic acid and LMWH. Our study was not primarily designed to investigate the effect of tranexamic acid after heavy blood loss had occurred. Figure 2 in our article clearly shows that tranexamic acid should be given early during the procedure in order to be effective.

We agree that postoperative DVT is a potentially serious complication but so are heavy blood loss and unnecessary blood transfusion.

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H. FREDIN, MD, PhD  
Malmö University Hospital  
Sweden.


**COMPARTMENT SYNDROME IN TIBIAL SHAFT FRACTURE MISSED BECAUSE OF A LOCAL NERVE BLOCK**

Sir,

We read with interest the article by Hyder et al entitled ‘Compartment syndrome in tibial shaft fracture missed because of a local nerve block’ (1996;78-B:499-500) since it highlights the critical result of a delay in diagnosis.

We have several concerns about this case. A triple nerve block is a technique by which local anaesthesia is introduced into the sheath surrounding the femoral nerve and encouraged to produce retrograde anaesthesia of the obturator nerve and lateral cutaneous nerve of the thigh. This blockade is a valuable means of pain relief in fractures of the neck or shaft of the femur and has been used in knee arthroscopy (Winnie, Ramamurthy and Durrañi 1973). It will only provide analgesia for the site of entry of the nail and the medial side of the leg. No other details of the anaesthetic technique are given but unless used in conjunction with a sciatic nerve block it would not be indicated for the relief of pain in the lower leg in the presence of a fracture.

The authors also state that the altered sensation in the patient’s foot varied in site. Unless there were gross anatomical abnormalities the only sensory branch at the level of the foot affected by a triple nerve block would have been the saphenous nerve which supplies an area of skin around the medial malleolus and the medial border. Sensory changes in the foot in the presence of a tibial fracture are more likely to be related to branches of the peroneal or tibial nerves.

The duration of action with the amount and type of anaesthetic used would have been approximately eight hours. Although sensory recovery of an anaesthetised segment may be patchy and erratic, we consider that the persistence of any symptoms between this time and 48 hours after injury when the pressure measurements were made should have immediately raised the alarm. By then there was gross motor weakness but there was no mention of symptoms with passive movement over the subsequent period.

It is of interest that epidural anaesthesia has also recently been blamed for masking the symptoms of compartment syndrome after elective corrective osteotomies (Price, Ribeiro and Kinnebrew 1996).

All surgeons should be very suspicious of compartment syndrome and we recommend that local anaesthesia should not be
used in trauma of the arm or the leg to avoid the possibility of conflicting opinion and diagnostic delay.

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G. HILL, FRCS
A. MAGIDES, FRCA
Princess Elizabeth Orthopaedic Hospital
Exeter, UK.


Authors’ reply:

Sir,

We thank Dr Magides and Messrs Hill and Eyres for their comments on our case report. There is no doubt that triple nerve or sciatic blocks are extremely effective for pain relief and can therefore mask the most important diagnostic feature of a compartment syndrome after fractures of the lower limb. There will also be alteration or reduction of sensation in the distribution of the nerve traversing the compartment which, in the case of the anterior tibial compartment, is the deep peroneal. This nerve supplies the dorsum of the foot and in particular the web between the first and second toes. The patient’s description of the loss of sensation, however, does not always conform to this small area. It is almost always more widespread, hence the confusion.

As far as we know, our patient had only a triple nerve block without a sciatic block. He complained of altered sensation sometimes on the medial aspect of the leg and sometimes at the instep, but his symptoms were not consistent.

There are four possible explanations for the absence of significant pain. First, there was a small possibility that sciatic block was in fact administered and not documented. Secondly, pain may have been controlled by the postoperative analgesia. Thirdly, the patient’s pain threshold may have been high. Lastly, the muscles of the anterior compartment may have been dead before operation and therefore painless. Pain in the compartment syndrome is caused by ischaemia of the muscles. If the condition is advanced and the muscles have died, the pain will not be an important feature. There will be paralysis. Our patient had lost dorsiflexion of the big toe before the fasciotomy.

Pressure measurements must be performed if there is any doubt as to the state of the compartment.

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S. KESSLER, MD
A. G. JENNINGS, FRCS
P. G. DE BOER, FRCS
Wilsden, Bradford, UK.

EFFECT OF FEMORAL OFFSET ON MOTION AND ABDUCTOR MUSCLE STRENGTH AFTER TOTAL HIP ARTHROPLASTY

Sir,

I wish to comment on the paper in the November 1995 issue by McGrory et al entitled ‘Effect of femoral offset on range of motion and abductor muscle strength after total hip arthroplasty’ (1995;77-B:865-9). The authors have shown a correlation between femoral offset and abductor power at the hip and have concluded that greater femoral offset allows greater abductor strength. This causal link, however, has not been proved in their paper. They have correctly used the statistical technique of multiple regression analysis (MRA) to try to quantify which combination of the many variables measured provides most influence on the dependent variable, abductor strength. Table III in their paper expresses the results of MRA, although they do not appear to have gone on to apply forward, backward or stepwise regression. In the table, the most significant variables in predicting muscle strength are patient gender, patient height and the range of flexion, using p < 0.05 as the cutoff (the level is not stated in statistical methods).

In MRA, a model is built up to include the variables that contribute most towards the dependent variable while taking into account interactions between the explanatory variables. Once a model is proposed, interaction between the variables should be checked for and an interaction term calculated if appropriate.

On the information given in the paper femoral offset would seem to add very little to explaining abductor strength (p = 0.74 in Table III). A tall male patient is likely to have a larger, wider pelvis and require a larger femoral component with a greater femoral offset. He is also likely to have a stronger power of abduction. Femoral offset is correlated with muscle strength but the relation and link are not causal.

T. J. W. SPALDING, FRCS Orth
RNH Haslar
Gosport, UK.


Author’s reply:

Sir,

We thank Mr Spalding for his comments. Whether or not strength is more closely related to femoral offset or to the abduction lever arm is arbitrary. The essential issue is whether technical matters within the control of the surgeon, or the offset of the design, can significantly influence the ultimate function of the joint. Our analysis has demonstrated and measured that which has been accepted as intuitively obvious. An increased lever arm which is obtained or defined by the femoral offset either inherent in the prosthetic design or its manner of placement directly correlates with abduction strength in a quantifiable fashion. The issue that has been raised is whether the femoral offset is a more significant variable than the abduction lever arm. We do not think that a detailed analysis of the relationship between these two variables is likely to be productive.

These two concepts are highly correlated and the specific quantification of each is dependent on the method of definition and the convention by which the measurement is made. We used accepted criteria of measurement for both the offset and abduction moment arm, but slight differences in measurement techniques will alter the data set and thus the subsequent analysis of statistical significance.

Furthermore, the various statistical methods may also impart different relative importance to one or the other variable. We had initially used a step-wise multiple regression analysis which showed that the abductor lever arm demonstrated statistical significance referable to abduction strength (p < 0.05). On acceptance of our paper the Journal of Bone and Joint Surgery recommended a single regression analysis, quite reasonably, because the latter implies a known hypothesis, and this was used
in the published manuscript. This method, however, altered the statistical significance of the correlation of the abductor moment arm with strength to be marginally, not statistically, significant at p < 0.06.

The abduction moment arm and the femoral offset are so closely correlated that changes in the method of measurement, the sample size and the specific statistical analysis will change the definition of statistical relevance between the two. This study quantifies this relationship and emphasises that the surgeon should pay specific attention to femoral offset both in the selection and in the implantation of the femoral component of a replacement.

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DISPLACED FRACTURES OF THE DISTAL FEMUR IN ELDERLY PATIENTS

Sir,
I read with interest the article entitled ‘Displaced fractures of the distal femur in elderly patients’ by Butt, Krikler and Ali (1996;78-B:110-4).

The authors state that the fractures were classified according to the AO system but unfortunately they do not provide the reader with this information.

I think that it is essential when comparing two different methods of treatment, to provide information as to the nature of the injuries being treated in the two groups, since it is unwarranted to draw conclusions without analysis of the specific fracture patterns involved.

J. D. WITT, FRCS, FRCS Orth
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Author’s reply:

Sir,
I thank Mr Witt for his comments. I enclose a table giving the classification of the fractures in the two groups according to the AO system (Table I). Although the fracture patterns in the operated group were more severe than in the conservative group I feel that we have shown that elderly patients with osteoporotic bone generally do better with operative treatment.

M. S. BUTT, FRCS
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Stourbridge, UK.

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