I am somewhat staggered that these authors were able to institute a bypass graft in 15 out of the 17 patients with arterial insufficiency. Overall I find evidence of arterial insufficiency in 13% of patients referred to my clinic (although this includes ulcers of all different sizes), but I have only ever successfully grafted one old lady. The reasons for this are several: in the first instance, many of these people are very frail and I would hesitate to inflict even a femoropopliteal bypass on them. In the second place, they very often do not complain of claudication and as just about half of the ulcers can heal with conservative treatment anyway, I would think that surgery is not indicated in these cases—once the ulcer has healed, the patient has no relative complaint. It is possible that they are so frail and immobile and do not walk enough to notice claudication, but I can only recall a primary complaint of claudication in a leg ulcer patient in about half a dozen instances. The other problem arises as to the nature of the grafting. Any operation on a leg with an open portal of infection must carry a risk, but Balaji and Mosley do not mention that there are complications with the procedure and it is possibly outside the scope of their paper. I would certainly have grave hesitation in introducing a prosthetic graft into such a limb.

While I believe that the future for treatment of leg ulcers will be at the cellular and microbiological level, this may be a long time coming and in the meantime we need practical solutions to this intractable problem which has been greatly facilitated by the work in this paper.

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A 5-year audit of outcome of apicectomies carried out in a district general hospital

We read with interest the recent paper by Lyons *et al.* (*Annals*, July 1995, vol 77, p273). We are concerned, however, that the apparently high success rate in this audit, based on a very small number of cases (less than half of those actually performed), together with an inappropriately detailed analysis of results, may be giving the wrong message.

We consider that the prescription of apical surgery should take due consideration of the scientific literature relating to conventional and surgical endodontic treatment and retreatment.

The literature suggests that conventional endodontic treatment can have a success rate varying between 70% and 97%, depending on the definition of success *(1)*, that preparation technique and position of the apical seal influence outcome *(1)*, that coronal leakage can contribute to failure *(2)*, and that the prognosis for success of non-surgical retreatment, when an unfilled canal is discovered and treated, is good *(3)*. Surgical retreatment in a root harbouring an untreated canal carries a questionable prognosis *(4)*. Therefore, surgical treatment is not always appropriate or justifiable.

To suggest that "periapical sepsis is refractory to orthograde treatment, it may also be refractory to retrograde treatment", without reference to the quality of the orthograde treatment, demonstrates a lack of appreciation of the pathological processes involved in endodontic failure.

We believe that there should be key involvement of clinicians appropriately trained in restorative dentistry in the decision-making process and initial treatment for endodontic cases referred to secondary care providers. The increase in resources required for this would be offset by a reduction in surgical resource usage, and an accompanying rise in the quality of patient care.

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Some clinical aspects of reconstruction for chronic anterior cruciate ligament deficiency

The excellent long-term review of outcome after reconstruction for anterior cruciate ligament deficiency (*Annals*, July 1995, vol 77, p290) concludes that a patellar tendon graft supplemented by a MacIntosh extra-articular reconstruction is a reliable technique. The author indicates that the data supports the inclusion of the MacIntosh procedure; however, an alternative conclusion may be drawn from these figures.

Take VI presents the Lysholm scores at 1, 3 and 6 years for each operation; if an arithmetical error in the MacIntosh only group is corrected (satisfactory results at 6 years should read 10 years) it suggests that the proportion of satisfactory results remains virtually unchanged between 1 and 6 years. In the other groups in which the MacIntosh procedure has been used as an additional reconstruction, the Lysholm scores for the patellar tendon reconstruction are also well maintained for 6 years; however, the results for prosthetic ligament reconstruction progressively deteriorate, possibly an indication of fatigue failure or fragmentation of the prosthesis. It would appear that the utilisation of a MacIntosh reconstruction does not prevent deterioration of knee function if the prosthetic graft fails, but it is also unclear whether the presence of the patellar tendon graft improves the outcome of this operation. In order to address this point, it is necessary to look at the outcome of the only objective measurement of knee stability performed in this study, that is the presence or absence of a pivot shift. In Table IV, approximately half of the patients who underwent a MacIntosh repair alone, or a prosthetic ligament reconstruction with a MacIntosh repair, redeveloped a pivot shift by 6 years; however,

patients undergoing a patellar tendon graft with a MacIntosh reconstruction did not redevelop a pivot shift (with the exception of one patient). As the pivot shift is the principal indicator of knee instability, and is the underlying cause of the patient's symptoms, this is the most important finding which strongly suggests that it is the presence of a patellar tendon graft which ensures a satisfactory outcome over a longer interval. This data does not support the use of a MacIntosh reconstruction to reliably abolish the pivot shift and casts doubt on the value of supplementing an autograft reconstruction of the anterior cruciate ligament with an extra-articular procedure.

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Author's reply

I am grateful to Richard Nutton for reading my article in such detail and for stressing the importance of the pivot shift sign. We are in complete agreement that the pivot shift is the principal indicator of instability and the underlying cause of symptoms.

There were eight knees with a satisfactory result (pivot shift negative and Lysholm score 77 or more) 6 years after MacIntosh reconstruction alone. Two of the ten patients with an adequate Lysholm score also had a positive pivot shift. There is no arithmetical error.

Mr Nutton is suggesting, I think, that reconstruction using patellar tendon alone is as effective as the combined procedure. If so, he may be right. Only a comparable series using patellar tendon alone could answer this question. I have no such series to report and can go no further than stating that anterior cruciate ligament (ACL) reconstruction using intra-articular patellar tendon combined with MacIntosh extra-articular reconstruction is a reliable technique.

Reconstruction using patellar tendon alone, usually the middle third, is probably the most commonly performed ACL reconstruction in use today and many thousands of these procedures have been performed around the world in the last decade. It is curious that the only 5-year results date from 1984 when Johnson *et al. (1)* reported a negative pivot shift in 68% of knees after 7.9 years. Absence of reported results usually means an operation does not work and that surgeons have abandoned it in favour of another unproven surgical adventure. Whatever happened to the xenografts and ligament prostheses that mysteriously disappeared from the market? And what are the surgeons who inserted them using instead?

Let me suggest to Mr Nutton that the long-term results of isolated patellar tendon reconstruction for chronic ACL insufficiency may be too awful to publish and that rumours of a 15% failure rate in the first year could be correct. We urgently need a credible report on the results of this procedure in chronic (not acute) ACL insufficiency more than 5 years after operation and with a follow-up of at least 70%. Until such a report is produced I will continue with the combined procedure.

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Tension-free mesh hernia repair: review of 1098 cases using local anaesthesia in a day unit

Kark *et al.* (*Annals*, July 1995, vol 77, p299) have added further evidence of the excellent results to be obtained with the tension-free mesh hernia repair when performed under local anaesthesia (LA). It is a pity that this method, used for over 30 years, has received scant attention by UK surgeons, despite some being early pioneers of the technique.

There is no doubt that the use of LA is the safest of all methods of anaesthesia for hernia repair. However Kark *et al.* reported 0.6% of patients with anterior thigh muscle weakness owing to femoral nerve palsy which followed what they described as standard local anaesthetic technique of inguinal nerve block and local infiltration, a method recommended in 'Clinical Guidelines on the Management of Groin Hernia in Adults' by the Working Party convened by The Royal College of Surgeons of England. This well-recognised complication is avoidable. It is due to the diffusion of LA on to the femoral nerve when attempting an unnecessary blind and unreliable external field block of the ileo-inguinal and hypogastic nerves.

For over 30 years I have used the simple local infiltration 'as you go' technique and have yet to see a femoral nerve paresis. It is the ideal anaesthetic technique for ambulatory hernia surgery and uses minimal dosages of LA. The skin and subcutaneous tissue are infiltrated along the line of incision. After dissecting down to the external oblique, LA is introduced under its aponeurotic fibres to distend the inguinal canal before its opening. The major nerves of the inguinal canal are thus directly anaesthetised. Additional LA is required at the internal inguinal ring to ensure anaesthetising the genitofemoral nerve and the inguinal sac. With this technique the LA is confined to the anatomical compartment of the inguinal canal well away from the femoral nerve. It is the technique now used by both the Shouldice and Lichtenstein Institutes.

Like many writers on the subject, their review of the history of mesh repair fails to acknowledge the work of surgeons whose contributions came well before Usher and Lichtenstein. It was Cumberland *(1)* of Australia who, in 1952, first reported the idea of a prefabricated pliable synthetic mesh for hernia repair and he deserves credit as the original pioneer. A commercial mesh was not available so he had it hand knitted by the occupational therapy department, and after performing experimental work on rabbits applied it to humans for ventral and inguinal hernia repair. He suggested the ideal characteristics required for an implantable mesh. His was a classical paper. Usher *et al.*, who made subsequent valuable contributions did not report on mesh until 1960 *(2)* when a manufactured mesh became available. Bellis *(3)* in 1969 was the first to report a large series repaired with mesh using a tension-free technique under LA and emphasising the day case ambulatory technique, thus preceding Lichtenstein and Shulman *(4)* by many years. Lichtenstein must be credited with popularising the approach