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Authors' reply:

Sir,

Thank you for the opportunity to reply to Mr Dove. He has raised an important issue affecting many orthopaedic surgeons. Some consensus committees regard orthopaedic surgery as a homogenous specialty with a uniformly high risk of thromboembolism. It is clear that several subspecialties, such as hand surgery and paediatric orthopaedics, have a very low incidence of deep-vein thrombosis and do not require the use of prophylaxis. We could not cover specific subgroups in detail, but we agree that spinal surgery should not be included in prophylactic policies designed for total joint replacement and fractures.

The consequences of anticoagulant-induced bleeding are considerably greater in spinal surgery. Anticoagulants should only be used in selected cases with a particularly high risk of deep-vein thrombosis, such as after previous thromboembolism. We use intraoperative intravenous Dextran 70 for spinal cases rather than heparin or low-molecular-weight heparin.

Mr Dove has quoted two significant papers which advocate mechanical prophylaxis in spinal surgery. Enhancement of venous blood flow by physical means is a safe and effective option. We support the wider use of physical methods of prevention in all areas of orthopaedic surgery, especially for patients with a high risk from bleeding complications.

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MANAGEMENT AFTER ARTHROSCOPY

Sir,

We were pleased that Birch et al in their recent article in the July 1993 issue entitled 'Anti-inflammatory drug therapy after arthroscopy of the knee' (1993; 75-B:650-2) included assessment of the efficacy of physiotherapy after arthroscopy, but were concerned about the design of their trial. To reflect current practice in this country, the NSAID group was given a specified seven-day course of drugs. The physiotherapy group had daily treatment continued to full functional recovery, which is unlikely to be standard in this country.

The frequency and content of physiotherapy should always be directed by the pathology: it should be very different after arthroscopy in patients with degenerative changes than in those with recent ACL rupture. Birch et al did not report the mode of treatment. Were the patients given electrotherapy, a defined, supervised exercise regime, reinforcement of home exercises or just counselled? They report the results of a non-existent, 'generic physiotherapy'. Their conclusion that such physiotherapy appears to be ineffective after arthroscopy, is meaningless.

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Birch NC, Sly C, Brooks S, Powles DP. Anti-inflammatory drug therapy after arthroscopy of the knee: a prospective, randomised, controlled trial of diclofenac or physiotherapy. *J Bone Joint Surg [Br]* 1993; 75-B:650-2.

Author's reply:

Sir,

Thank you for allowing us to reply to the letter from Beard and his colleagues. We regret that there was an error in our paper: outpatient physiotherapy was not daily, but varied according to the clinical condition of the patient. On average, patients in the physiotherapy group had 3.5 treatments (1 to 11), ranging from a single visit to repeat attendances over several weeks. We apologise for this error.

We were trying to assess benefit from the routine referral of patients for physiotherapy after arthroscopy. The actual treatments prescribed by our physiotherapists were dictated by the clinical condition of the patient; they included exercises and electrotherapy. We agree with Beard et al that physiotherapists are at liberty to prescribe whatever treatments they deem necessary for individual patients; we did not consider that this point needed specific mention as all physiotherapists and most surgeons are aware of it.

We were comparing two methods of management and did not therefore distinguish between different treatments, accepting that our physiotherapists would use the most appropriate methods for each case. This is not 'generic physiotherapy'.

Directed treatment is logical for both groups: a few of our patients with synovitis after a meniscal injury improved more quickly in the first two weeks with non-steroidals and some patients with poor quadriceps function or a poor range of motion benefited more from physiotherapy. The numbers were too small for statistical analysis but it seems probable that treatment should be carefully selected. This was intended to be the essential message of our study.

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SPINAL STENOSIS

Sir,

I commend Sanderson and Wood (1993; 75-B:393-7) for investigating the efficacy of surgery for lumbar spinal stenosis in elderly patients, and I believe that they are correct to conclude that "the results of local decompression for sciatica and neurogenic claudication in the elderly are good".

I would, however, like to have seen more use of statistics. The data were presented as numbers and percentages only – it would have been better to have given p values to account for the role of chance in the results. The only consideration of confounding variables was to give results based on diagnosis and then to list the number and percentage in each category. Their outcome measure was an ordinal scale and it would have been more effective to have used Mantel-Haenszel chi-squared testing to measure both the level of significance and to control for confounding.

The authors should have given more details of the different categories of outcome. They listed the variables, including pain, patient satisfaction, analgesic use, and walking distance but they failed to state how they defined excellent, good, or fair outcomes. These designations thus appear to be highly subjective and could lead to a reliability bias. Their findings require additional statistical tests before recommendations can be made.

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