Sanderson PL, Wood PLR. Surgery for lumbar spinal stenosis in old people. *J Bone Joint Surg [Br]* 1993; 75-B:393-7.

Authors' reply:

Sir,

The main purpose of our paper was to show that the often vague symptoms of spinal stenosis in the elderly should not be dismissed, because surgery can help. This group of patients is often ignored because the value of surgery is not appreciated. We accept that a chi-squared test could have been used to measure the significance of the various factors, but our criteria for the results were stated in the paper.

We used a simple outcome measure:

excellent - no leg pain;
good - less leg pain than before surgery; and
poor - no change or worse leg pain.

We also reported other data such as back pain, walking distance and the use of analgesia, as recommended by Turner et al (1992). This makes it possible for the reader to do his own statistical tests. We felt that the results which we gave justified our conclusion; 27 of the 31 patients were satisfied and 25 were improved on the criteria which we used.

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Turner JA, Ersek M, Herron L, Deyo R. Surgery for lumbar spinal stenosis: attempted meta-analysis of the literature. *Spine* 1992; 17:1-8.

PAIN RELIEF AFTER DISCECTOMY

Sir,

We read with interest the paper by Milligan et al in your September 1993 issue (1993; 75-B:769-71) showing that the use of bupivacaine significantly reduces morphine consumption after discectomy. They state in their discussion that hourly morphine consumption in the study group remained below that of the control group for the duration of the study. Inspection of the data presented in the paper does not support this.

Fig. 1

We have calculated the hourly consumption of morphine on the basis of the difference in the mean cumulative consumption: after eight hours there is no obvious difference between the two groups. This is clearly seen when their Figure 1 is reversed, plotting the amount of morphine remaining to be used against time (Fig. 1). We cannot assess statistical significance, but suggest that even a statistically significant reduction of under 3 mg in the total consumption over the last 12 hours is of little clinical relevance.

We also have reservations on the extent of the 'blinding'. The nursing staff who provided the formal assessment of pain control were blinded, but the surgeon and anaesthetist, who presumably continued their care of the patient into the postoperative period, were not. Pain assessment, even using visual analogue scoring, is notoriously subjective and patients who have consented to take part in such studies are often very receptive to clues as to which treatment they have received, and may unwittingly modify their responses.

Almost all of the excess morphine requirement of the control group occurred in the first six to eight hours, according well with the known plasma half-life of bupivacaine of just under three hours (Calvey and Williams 1991). In the absence of a blinded control infiltration, any subsequent morphine sparing could result from lack of blinding, the residual effect of the bupivacaine or the physical effect of tissue infiltration, rather than the suggested reduction of spinal cord hyperexcitability.

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Calvey TN, Williams NE. *Principles and practice of pharmacology for anaesthetists*. 2nd ed. Oxford. Blackwell Scientific Publications, 1991:241.

Milligan KR, Macafee AL, Fogarty DJ, et al. Intraoperative bupivacaine diminishes pain after lumbar discectomy: a randomised double-blind study. *J Bone Joint Surg [Br]* 1993; 75-B:769-71.

Authors' reply:

Sir,

We agree with Risdall and Johnston that wound infiltration with bupivacaine produced most of its benefits in the first 12 hours. This is to be expected since the passage of time will make it increasingly difficult to show a difference in postoperative pain relief.

We were particularly concerned about a possible 'rebound effect' causing increased morphine consumption when the bupivacaine ceased to be effective, one criticism of previous studies of wound infiltration (McQuay 1992). This rebound did not occur, the bupivacaine group reporting less pain despite their lower morphine consumption.

We agree that it might have been better to inject saline, for example, into the wound of the control group. Because neither the patients nor those carrying out the postoperative assessments knew what had been given, we do not consider that this invalidates our main conclusion: intraoperative wound infiltration with bupivacaine does provide significant improvement in postoperative pain relief.

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McQuay HJ. Editorial: pre-emptive analgesia. *Br J Anaes* 1992; 69:1-3.

HYPOTENSION DURING CEMENTED ARTHROPLASTY

Sir,

We read with interest the article by Wheelwright et al in your September 1993 issue entitled 'Hypotension during cemented arthroplasty' (1993; 75-B:715-23). We agree with their recommen-

dation that lavage and venting can be used to evacuate the contents of the medullary canal before cementing, but would like to emphasise one further point.

Modern cementing techniques involve a number of advances: lavage is only one. Intramedullary restriction devices are used by 78% of surgeons in the UK (McCaskie, Harper and Gregg, unpublished data, 1993) and are not only important for pressurisation (Oh et al 1978), but also compartmentalise the femur into the reamed proximal area and the unreamed distal marrow. The importance of the restrictor in its role in the reduction of embolic load has been described in patients undergoing arthroplasty by Ulrich and Heinrich (1990). It is not clear in the article by Wheelwright et al whether restriction was used: the above points are not emphasised in the discussion.

We consider that canal restriction is an important part of simulated cemented arthroplasty; its role should be evaluated before the results of this animal model are extrapolated to clinical practice.

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Oh I, Carlson CE, Tomford WW, Harris WH. Improved fixation of the femoral component after total hip replacement using a methacrylate intramedullary plug. *J Bone Joint Surg [Am]* 1978; 60-A:608-12.

Ulrich C, Heinrich H. Intrafemoral pressure in total hip replacement. In: Older J, ed. *Implant bone interface*. Berlin, etc: Springer-Verlag, 1990:95-8.

Wheelwright EF, Byrick RJ, Wigglesworth DF, et al. Hypotension during cemented arthroplasty: relationship to cardiac output and fat embolism. *J Bone Joint Surg [Br]* 1993; 75-B:715-23.

Authors' reply:

Sir,

McCaskie and his colleagues make two important points. They correctly emphasise the importance of surgical technique in determining the occurrence of embolic fat and they also caution against premature extrapolation from the animal model to clinical practice.

We aimed to characterise the acute haemodynamic changes caused by fat embolism. This is difficult to study in clinical practice because 'cement implantation syndrome' is very rare with modern techniques. Our aim was to study the effects of various surgical and anaesthetic manoeuvres on embolic load and secondary haemodynamic changes, and this can only be done in an animal model since lung histology is needed to confirm the pulmonary vascular obstruction responsible for the physiological changes.

We did not use an intramedullary restriction device; there is no evidence to suggest that compartmentalising the femur by a restriction device is effective in diminishing the embolic load. Ulrich and Heinrich (1990) showed that a venting hole above the restriction device may help to prevent the haemodynamic syndrome, but their echocardiographic data indicated that this technique did not abolish fat embolism.

Our previous studies suggested that lavage effectively reduces the embolic fat load; we are not aware of any experimental evidence indicating that canal restriction has a beneficial effect in these circumstances. Richards et al (personal communication, 1993) have used a similar model with a restriction device and found significant increases in pulmonary artery pressure and fat emboli in the lung parenchyma of experimental animals. A restriction device may limit the marrow exposed to increased pressure, but it does not prevent the syndrome.

We would agree that canal restriction may well be an important surgical element in the aetiology of the syndrome, but this remains unproven. We also agree that any data from an animal model must

be verified in well-designed clinical studies. We are pleased that our paper has stimulated such interest and that our research model may now be expanded by other investigators to simulate the clinical situation more closely.

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ULTRASOUND AND LEG LENGTHENING

Sir,

We read with interest the paper by Maffulli, Hughes and Fixsen in your January 1992 issue (1992; 74-B:130-2) on the ultrasonographic monitoring of limb lengthening. They state that a new cortex was detectable within seven weeks and that a medullary canal developed within seven to eight weeks after osteotomy.

We have recently evaluated new bone formation during limb lengthening using dual-energy X-ray absorptiometry (DEXA) (Eyles, Bell and Kanis 1993a) and compared these features with those obtained by ultrasonography and plain radiography (Eyles, Bell and Kanis 1993b). In the distraction gap, bone formation first appeared on ultrasound as echogenic islands which later coalesced to form a hyper-reflecting line through which no deeper image was visible. This correlated with a progressive increase in bone mineral content and density in the distraction gap. In the presence of a hyper-reflecting line, bone mineral density (BMD) was at least 0.4 g/cm² (or approximately 40% of control values).

These ultrasonic features were misinterpreted by Maffulli et al as signifying cortication of the regenerate when the bone-density measurements were clearly much lower. They also state that differentiation of a medullary canal was observed on ultrasound. We could not detect this with ultrasound, and the changes in BMD shown by DEXA (as judged by the restoration of a bimodal bone-density pattern across the bone) suggested that this was delayed for at least a further three months after the appearance of the hyper-reflecting line on ultrasound. Since, by definition, no ultrasound passes through the hyper-reflecting surface, we suggest that the medullary canal observed by Maffulli et al was an artefact due to the reverberation echo produced when ultrasound is reflected from a surface. Our reported experience was with a 7.5 MHz transducer, but we found that a 5 MHz transducer, as used by Maffulli et al, produced similar results but with poorer resolution.

Cortical and medullary differentiation cannot be detected by ultrasound; this overestimates the amount of bone formed in the distraction gap, and misinterpretation may lead to premature fixator removal, inappropriate rates of distraction, deformity or fracture. We do, however, agree that ultrasound can help to reduce the need for radiographs during limb lengthening, but the images must be interpreted with caution. We advocate the use of ultrasound in both longitudinal and transverse scanning modes in conjunction with DEXA to overcome these problems.

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