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Comment with regard to: Outcomes of a prospective cohort study on peri-radicular infiltration for radicular pain in patients with lumbar disc herniation and spinal stenosis (L. Ng et al.)

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In a patient presenting with clear-cut radicular pain, it is tempting to pursue non-surgical therapies directed towards the affected nerve. This would seem to be logical, especially in view of the increasing evidence that the genesis of such pain is not just nerve compression [1]. Amongst the techniques favoured in radicular pain syndromes is the injection of local anaesthetic and steroid around the nerve root, variously called peri-radicular infiltration, paravertebral block or nerve root block, a technique performed by anaesthetists as well as spinal surgeons. Whilst many practitioners can provide anecdotal evidence of efficacy, the literature is of little help: there are no controlled studies of reasonable quality. The study of Ng et al. in this issue is of interest because it does include carefully collected outcome data not only of pain and general function (Oswestry and Low Back Outcome Score), but also of the Dram score (Zung Depression Inventory and Modified Somatic Perceptions Questionnaire), in two cohorts of patients undergoing peri-radicular infiltrations for nerve root pain due either to lumbar disc herniation or spinal stenosis. The data provide details of the outcome that can be expected from the performance of a nerve root injection using a small volume of bupivacaine with 40 mg methyl prednisolone, on pain, distress and general function. Both groups get better,

but this is much more marked in the disc herniation group. The improvement seen in the spinal stenosis group is minimal, with only a two-point reduction in ODI and 7 mm on VAS at 6 weeks with some further improvement (to 6 and 12, respectively), on the VAS at 12 weeks. Given that the patients started off with VAS scores of 77 a reduction to 70 after 7 weeks is not satisfactory pain management. It has been estimated that one needs a 13-point change to detect clinically relevant pain relief [3].

The disc herniation group fare somewhat better, with VAS changes of 11 at 6 weeks and 20 and 12 weeks, but this is a condition with a natural history of resolution, and the fundamental question of whether the injection aided this recovery is not answered due to lack of controls. Here-with, the major problem with the study: the lack of a control group ultimately limits its value. The authors refer to the literature on epidural steroids, which, although by no means categorical, is supportive of an effect in acute lumbar disc herniation. The authors added contrast medium to the injectate and only used a small volume. They do not report on epidural spread, but spread of injectate into the epidural space has been described during injections of this type [2] which raises the question as to the site of action of the drug. If one makes the charitable assumption

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that the authors injections do provide some benefit to patients with lumbar disc herniation, the question then arises as to whether this technique is easier to perform, cheaper, or more effective than epidural injection. One aspect that the literature on epidurals in acute disc prolapse does not adequately address is the short-term acute pain relief obtainable from such injections. The pain of an acute disc can be terrible and because of its neuropathic nature difficult to

control even with strong analgesics. Effective relief of this pain is a rational end point of treatment even if the long-term outcome is unchanged. We thus do not need evidence of long-term benefit to justify a treatment if it can be shown to provide high-quality pain relief in the acute phase. Unfortunately, this study does not help in this respect. As always, there are more questions than answers: controlled studies are urgently needed.

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