Manipulative therapy and physiotherapy for persistent back and neck complaints

Sir,—Bart W Koes and colleagues have shown the benefits of manipulative therapy compared with physiotherapy, supporting the findings of an earlier trial conducted by the Medical Research Council. 1

Firstly, however, contrary to the statement in their paper, it is not possible to differentiate patients with disc herniation from those with other causes of back pain on the basis of a non-specific complaint and physical examination. Radiological investigations such as computed tomography, myelography, and magnetic resonance imaging are usually required to achieve this distinction.

Secondly, intervertebral discs start to degenerate in early adulthood, becoming symptomatic after fragmentation, with herniation through an intact annulus or impingement on the spinal canal. 2 As manipulative therapy entails small movements of high velocity, applying sudden stresses to chronically degenerating discs may precipitate protrusion of a disc. Manipulating the spines of patients with back pain of undiagnosed aetiology is not without risk, and known complications, although rare, range from injury to the cervical cord 3 to brain stem infarction. 4 Our experience includes two cases of compression of the cauda equina after chiropractic manipulation 5 in which the diagnosis was delayed, resulting in long term disability. As a result we endorse calls for further trials to elucidate the role of spinal manipulation in the management of low back pain and for a review of chiropractic training in the United Kingdom. 6

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Sir,—I am concerned about the inappropriate selection of patients and treatment in the study by Bart W Koes and colleagues. 1

The introduction mentions that, in the patients selected, no underlying disease could be established and the causes of the complaints remained unknown. Why? Were the assessors lacking the
competence to examine the spine fully enough to define which tissue or tissues were responsible for producing the pain, and it is not safe to attempt to manipulate patients with vertebralogenic pain of undiagnosed disc, sacroiliac, and facet lesions were also hip lesions, which may refer pain to the lower back? If no one was capable of diagnosing the lesions initially, they cannot have been capable of directing those patients with back or neck pain resulting from nuclear disc lesions to traction—surely the primary treatment for such patients.

The study does not state what exercises the physiotherapists dispensed. Back or neck pain resulting from disc lesions may well be worsened by conventional strengthening or mobilising exercises but may be helped by certain movements—for example, those designed by Robin McKenzie. The survey was too poorly constructed to reflect adequately what manipulative physiotherapists of today are capable of and the effective treatment patients should receive. ADRIAN F PEARCE

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SIR,—Bart W Koes and colleagues state, “Patients had to meet the following criteria: the complaint was non-specific—that is, no underlying disease could be established”; then in their discussion of why manipulation showed better results they state, “Finally, manipulative therapy may help to relax the function of the spine better than physiotherapy.” But there is no mention of any objective evidence of spinal dysfunction before treatment. We were unable to find any mention of the identification of spinal biomechanical derangement and the specific spinal manipulation used to correct that derangement. Instead, we get the impression that every patient with neck or back pain received the same non-specific manipulation. That is like goring everyone with heart trouble digitalis without due regard to its proper indication, dosage, and potency.

The trial showed that for persistent neck and low back pain non-specific spinal manipulation is superior to physiotherapy, treatment by a general practitioner, and placebo. The 65 patients given spinal manipulation seem not to have been screened or selected on the basis of criteria to determine that spinal manipulation was indeed the preferred treatment that would have had a lesional derangement to respond to a specific manipulative technique. Spinal manipulation is unlikely to have been the preferred treatment for all of the patients randomly assigned to manipulation. Thus if suitable patients had been selected for spinal manipulation and given the properly indicated manipulation the outcome would probably have been even better.

The authors refer to the study of Meade et al as having compared “non-specific” manipulative therapy with hospital outpatient treatments for low back pain. This is incorrect, as has been pointed out previously. Meade et al’s study was a randomised clinical trial comparing different manipulative techniques performed by chiropractors and physiotherapists. It showed that spinal manipulation by a chiropractor is more effective for low back pain of unknown aetiology than spinal manipulation performed by a physiotherapist.

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SIR,—Bart W Koes and colleagues’ trial of manipulative therapy and physiotherapy draws conclusions that cannot be substantiated on the basis of the data presented. The only reason for claiming that manipulative therapy and physiotherapy are superior to treatment by general practitioners and placebo treatment seems to be the observation that some patients treated by general practitioners given placebo treatment broke the rules of the protocol by transferring to a active intervention group. At 12 months 36% of the placebo group and 34% of the general practitioner group had transferred to one of the other treatments. We are given no further information, however, on the 64% and 66%, respectively, who either stayed in their allotted group or needed specialist or operative intervention (roughly the same numbers in each group).

The number of deviations from the treatment groups is not large enough to negate useful information from those persisting in a control group. If these patients had worse outcome measures than those in the active treatment groups I imagine that they should have been included in the analysis. It was not raises the suspicion that all four groups had outcomes that were not significantly different. If this was the case the conclusion would be that the form of intervention matters little but that these group of patients have a tendency to slow improvement over time with a high recurrence rate (63% in each group had received previous physiotherapy or manipulative therapy).

Perhaps the most important feature of therapy perceived by patients is contact with a caring therapist, and on the basis of their previous experience patients sought this as opposed to a single visit to their general practitioner. The large number of patients in the placebo group (receiving no physiotherapy) may mean that passive physiotherapy makes me wonder how “blind” these patients were, or perhaps sympathetic therapists had a low threshold for encouraging the transfer of patients from one therapist to another. Therapists who deal frequently with the type of patients described in this paper would be delighted if active intervention beyond human contact could unequivocally show benefit. Having read the study, I am no more confident that this is the case.

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AUThORS’ REPLY,—We agree with O J Lehmann and colleagues that to exclude disc herniation definitely radiological investigations should be carried out. In our study we selected patients with chronic complaints for whom no underlying pathology was established with standard diagnostic procedures. This means that patients under went radiological investigation only if underlying pathology was suspected. This is standard practice in the Nether lands, and we do not assume that in Britain all patients with persistent back complaints undergo examination.

Contrary to Adrian F Pearce’s suggestion, the general practitioners and the research assistant (an experienced physiotherapist and manual therapist) were well able to make diagnoses. In most patients with non-specific complaints spinal manipulations were performed; in a few cases patients with idiopathic herniation occurred. It can be established and thus the cause(s) of the complaints remain unclear. These complaints are thus usually labelled non-specific. Only patients with non-specific complaints were included in our study, those with an identified disc herniation or other clear underlying pathology were excluded. Furthermore, we did not want to include patients with acute complaints (less than six weeks’ duration). Many studies indicate that patients with acute symptoms within a few months are independent of the type of treatment, if any, given.1

It is correct that complications of (mostly cervical) manipulations have been reported.2 In our study no complications occurred. Manual therapy was performed by experienced physiotherapists who had studied during a
three to four year course. Also, manipulative thrusts are seldom applied by Dutch manual therapists. Finally, Pearce’s suggestion that traction or the McKenzie method is a valuable treatment for specific subgroups of patients with back complaints has still to be shown with properly designed randomised clinical trials.

T W Meade rightly points out that our randomised controlled trial is not totally flawless. Judged by our own criteria, it scores 55 on a scale of 100, which makes it still one of the best in the field. We also agree with Meade that a power of 60% is not spectacular, but in our opinion this influences only the interpretation of non-significant differences. We strongly disagree with Meade’s suggestion that pooling data from all randomised controlled trials on manipulative techniques, no matter what their methodological quality is, would be a good idea.

In answer to Philip Brien and Michael J Brien’s comments, our selection criteria were designed to select patients who were all suitable for physiotherapy, manual therapy, and continued care by their general practitioner. We thus included a relatively heterogeneous population of patients with persistent back and neck complaints. Of course this does not imply that all patients received identical treatment. Physiotherapists and manual therapists were free to adapt their treatment (within predetermined boundaries) to the perceived needs of each patient. We agree that within our population there could have been subgroups who were more suitable for treatment with manual therapy, but it was impossible to identify those subgroups successfully in advance. We have studied the outcomes in subgroups of patients labelled by the manual therapists as “very suitable” and “less suitable” during the first treatment session; there were no differences in outcomes between these subgroups (B W Koes et al, unpublished findings). Although we like the idea of defining clear indications for treatment with manual therapy, we think that much more research has to be conducted before this will be feasible.

Chris Deighton suggests that after six and 12 months we analysed the data in all four study groups and subsequently decided to report outcomes for only the manual therapy and physiotherapy groups. This is not true. About half the patients in both the placebo group and the general practitioner group sought other treatment after six and 12 months’ follow up. Therefore we would have had great problems in interpreting the outcome in these two groups. The suggestion to analyse and present the data on patients who stayed in the allotted groups (placebo and general practitioner) would be invalid owing to the obvious (self) selection related to outcomes. In our opinion, the high proportion of patients originally allocated to the placebo and general practitioner groups who sought other treatment clearly indicates better results from active treatment. Although we agree with Deighton that a large part of the treatment effect might result from the contact with a caring therapist, we showed in our paper reporting the short term results of the study that active treatment had consistently better results than the placebo.

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