competence to examine the spine fully enough to define which tissue or tissues were responsible for producing the pain, and is it not unsafe to attempt to manipulate patients with vertebrogenic pain of unknown origin? Am I to assume that among the undiagnosed disc, sacroiliac, and facet lesions there 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

General Hospital St Helier,
St Helier,
Jersey JE2 3QS

1 Koes BW, Bouter LM, van Mameren H, Essers AHM, Verstegen GMJR, Hofhuizen DM, *et al.* Randomised clinical trial of manipulative therapy and physiotherapy for persistent back and neck complaints: results of one year follow up. *BMJ* 1992;304:601-5. (7 March.)

SIR,—Bart W Koes and colleagues have now discovered for themselves¹ many of the difficulties of back pain trials which have formed the basis for their criticisms of other trials.² In their trial patients were selected “by general practitioners and by advertisements in the local press” so that, to begin with, it is difficult to assess the general applicability of their findings. At 60%, the power of their trial was not high, and any differences between the manipulative therapy and physiotherapy groups in the severity of the main complaint were actually less than the smallest difference considered to be clinically relevant. Furthermore, any significant differences there may have been seem to have come only from the analyses that used substitute measurements for missing values.

Koes and colleagues surmised that departures from allocated treatment in two of their management groups (general practitioner and placebo) may have indicated the superiority of the other methods (manipulation and physiotherapy) and abandoned a full intention to treat analysis. As, in consequence, they did not compare manipulation and physiotherapy with treatment by a general practitioner and placebo treatment they cannot conclude (as they did) that manipulation and physiotherapy were better.

Although there may indeed be a long term benefit of manipulative therapy over physiotherapy,³ I doubt whether Koes and colleagues are justified in drawing this conclusion from their findings. Though they are certainly to be commended for having attempted a trial themselves, whether they would have scored very highly on their own scale² is doubtful. In so far as their comparison of manipulation and physiotherapy was unbiased, however, their data would make a useful contribution to an overview of manipulative therapy compared with other techniques. This is likely to be more productive and clinically useful than further scored assessments of different trials based, as such reviews on this topic inevitably must be, on arbitrary and arguable criteria.

T W MEADE

MRC Epidemiology and Medical Care Unit,
Wolfson Institute of Preventive Medicine,
Medical College of St Bartholomew's Hospital,
London EC1M 6BQ

1 Koes BW, Bouter LM, van Mameren H, Essers AHM, Verstegen GMJR, Hofhuizen DM, *et al.* Randomised clinical trial of manipulative therapy and physiotherapy for persistent back

and neck complaints: results of one year follow up. *BMJ* 1992;304:601-5. (7 March.)

2 Koes BW, Bouter LM, Beckerman H, van der Heijden GJMG, Knipschild PG. Physiotherapy exercises and back pain: a blinded review. *BMJ* 1991;302:1572-6.

3 Meade TW, Dyer S, Browne W, Townsend J, Frank AO. Low back pain of mechanical origin: randomised comparison of chiropractic and hospital outpatient treatment. *BMJ* 1990;300:1431-7.

SIR,—In the report of their clinical trial 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 restore 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 a 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 giving 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 and that they had a lesion that would 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 chiropractic manipulation with hospital outpatient treatments for low back pain.² This is inaccurate, as has been pointed out previously.^{3,4} 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.

PHILIP BRIEN

South Orange,
New Jersey 07079,
USA

MICHAEL J BRIEN

St Barnabas Medical Center,
Livingston,
New Jersey

1 Koes BW, Bouter LM, van Mameren H, Essers AHM, Verstegen GMJR, Hofhuizen DM, *et al.* Randomised clinical trial of manipulative therapy and physiotherapy for persistent back and neck complaints: results of one year follow up. *BMJ* 1992;304:601-5. (7 March.)

2 Meade TW, Dyer S, Browne W, Townsend J, Frank AO. Low back pain of mechanical origin: randomised comparison of chiropractic and hospital outpatient treatment. *BMJ* 1990;300:1431-7.

3 Brien P. Chiropractors and low back pain. *Lancet* 1990;336:572.

4 Brien P. Letter to the editor. *J Manipulative Physiol Ther* 1991;14:541-2.

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.¹ 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 or given placebo treatment broke the rules of the

protocol by transferring to an 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 this would have been reported. The fact that 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 this 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 sham physiotherapy) who transferred to active 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 group to another.

Practitioners 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.

CHRIS DEIGHTON

Department of Rheumatology,
Dryburn Hospital,
Durham DH1 5TW

1 Koes BW, Bouter LM, van Mameren H, Essers AHM, Verstegen GMJR, Hofhuizen DM, *et al.* Randomised clinical trial of manipulative therapy and physiotherapy for persistent back and neck complaints: results of one year follow up. *BMJ* 1992;304:601-5. (7 March.)

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 underwent radiological investigation only if underlying pathology was suspected. This is standard practice in the Netherlands, 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 back pain, however, no underlying pathology 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 about 90% of these patients will recover within a few months irrespective 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 the subject 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.^{3,4}

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.⁵

BART KOES
LEX BOUTER
PAUL KNIPSCHILD
HENK VAN MAMEREN

University of Limburg,
6200 MD Maastricht,
Netherlands

1 Spitzer WO, Leblanc FE, Dupuis M, eds. Scientific approach to the assessment and management of activity-related spinal disorders. *Spine* 1987;7(suppl):1-59.

2 Assendelft WJJ, Bouter LM, Knipschild PG. *Chiropractie in Nederland*. Maastricht: Department of Epidemiology, University of Limburg, 1991.

3 Koes BW, Assendelft WJJ, van der Heijden GJMG, Bouter LM, Knipschild PG. Spinal manipulation and mobilisation for back and neck complaints: a blinded review. *BMJ* 1991;303:1298-303.

4 Koes BW, Bouter LM, Beckerman H, van der Heijden GJMG,

Knipschild PG. Physiotherapy exercises and back pain: a blinded review. *BMJ* 1991;302:1572-6.

5 Koes BW, Bouter LM, van Mameren H, Essers AHM, Versteegen GMJR, Hofhuizen DM, *et al*. The effectiveness of manual therapy, physiotherapy and treatment by the general practitioner for non-specific back and neck complaints: a randomised clinical trial. *Spine* 1992;17:28-35.

Perforation of gloves

SIR,—P W Richmond and colleagues' findings regarding punctures to gloves worn by accident and emergency staff are similar to results of a study in general surgery.² In both studies the perforation rate was 11% and there was a pre-disposition for punctures to occur on the non-dominant (left) index finger (38%¹ and 42%² of punctures respectively). Altogether 37%¹ and 49%² of punctures were not noticed by the wearer at the time of puncture, and there was no difference in puncture rate with seniority in either study.

Perhaps accident and emergency staff and staff of postmortem rooms, for whom similar findings have been reported,³ should consider wearing two pairs of gloves ("double gloving"); this reduces the number of incidents in which sharp objects penetrate as far as the skin from 11% to 2%.²

A M THOMPSON

Raigmore Hospital,
Inverness IV2 3UJ

1 Richmond PW, McCabe M, Davies JP, Thomas DM. Perforation of gloves in an accident and emergency department. *BMJ* 1992;304:879-80. (4 April.)

2 Matta H, Thompson AM, Rainey JB. Does wearing two pairs of gloves protect operating theatre staff from skin contamination? *BMJ* 1988;297:597-8.

3 Weston J, Locker G. Frequency of glove puncture in the postmortem room. *J Clin Pathol* 1992;45:177-8.

SIR,—We think that P W Richmond and colleagues may have underestimated the perforation rate of surgical gloves¹ by using the technique described by Church and Sanderson.²

The size of the perforation depends on the degree of distension of the glove. As either water or air is injected into the glove the palm distends more rapidly than the digits. The tip of each digit, where most perforations occur, distends least as the radius is least within this tip. This is an example of an important physical principle, the law of Laplace. This law states that the tension within the wall of a distensible hollow object is equal to twice the wall tension divided by the radius.

We suggest that perforations should be made at various points on the glove with a needle of known size and then either water or air should be injected into the glove to a known pressure, rather than volume, to detect these perforations. We assessed the accuracy of the technique described by Church and Sanderson² by perforating the tip of the index digit with a gauge 25 (diameter 500 µm) hypodermic needle. We were not able to show a leak.

This prompted us to modify the technique described by Ballbach *et al*.³ We improved the accuracy of the test by, firstly, perforating the palm of the glove (Regent Biogel) with a gauge 7/0 (diameter 220 µm) needle. It required about 500 ml of air, corresponding to a pressure of about 0.29 kPa, before perforations could be detected in the palm when it was submerged under water. Then 30-40 ml of air, corresponding to a pressure of about 0.74 kPa, had to be injected into each digit before perforations in the tip of the digit could be detected. We used this technique on 36 pairs of surgical gloves used during six cardiac surgical procedures. Twenty sites of leakage were found, 19 in the digits (eight in the tip) and one in the palm, when the first part of the test was performed. An additional 29 sites of leakage, however, were found during the second part of our test; 22 of these were in the tips of the digits. We used 10 pairs of unused gloves as controls, and they were all found to be intact in both parts of the test.

We suggest that both the technique to test for perforation of gloves in the clinical setting and its accuracy ought to be better defined, and the terms "perforation" and "leak" should not be used synonymously.

P S WONG
J E C WRIGHT

Department of Cardiothoracic Surgery,
London Chest Hospital,
London E2 9JX

P A WHITE

Department of Biomedical Engineering,
London Chest Hospital

1 Richmond PW, McCabe M, Davies JP, Thomas DM. Perforation of gloves in an accident and emergency department. *BMJ* 1992;304:878-80. (4 April.)

2 Church J, Sanderson P. Surgical glove punctures. *J Hosp Infect* 1980;23:84.

3 Ballbach RL, Beavin P, Walters SM. A study of testing methods for the detection of defects in disposable latex and plastic gloves. *J Assoc Off Anal Chem* 1972;55:1074-80.

Hospital admission and benzodiazepine use

SIR,—D Surendrakumar and colleagues rightly highlight the risk of subsequent dependence on benzodiazepines when these drugs are prescribed for hospital inpatients.¹

A recent audit of prescriptions for inpatients and at discharge undertaken in this acute geriatric department found that sedation at night as required had been prescribed for 18 of 40 patients: temazepam (nine patients), chlormethiazole (eight), and nitrazepam (one). Of these patients, six had used benzodiazepines previously (two regularly, four intermittently), including the patient prescribed nitrazepam. Twelve patients used some sedation at night during their inpatient stay, including three of the six previous users. Only two patients were prescribed benzodiazepines at discharge; both had used them long term previously.

Transient disturbance of sleep is common among elderly people admitted for acute illness, occurring in 30% of this sample. The hospital instituted a prescribing policy for benzodiazepines in 1990. In this department staff recommend using temazepam, chlormethiazole, or chloral hydrate if an elderly patient requires sedation at night and that any such prescription should be reviewed after five days. The audit suggests that a prescribing policy is effective in preventing patients being prescribed benzodiazepines at discharge unless they are long term users. I agree with Surendrakumar and colleagues that prescribing policies should be introduced more widely.

DUNCAN FORSYTH

Department of Geriatric Medicine,
Addenbrooke's Hospital,
Cambridge CB2 2QQ

1 Surendrakumar D, Dunn M, Roberts CJ. Hospital admission and the start of benzodiazepine use. *BMJ* 1992;304:881. (4 April.)

More injuries from "bouncy castles"

SIR,—In response to Gian Singer and Lawrence S Freedman's letter¹ I have analysed national figures for accidents involving "bouncy castles." The Department of Trade and Industry's leisure accidents surveillance system records all people injured in accidents other than home, industrial, or road traffic accidents who present to a sample of 11 hospitals throughout the United Kingdom, with 24 hour accident and emergency departments receiving at least 10 000 cases a year. One hundred and five such people were recorded, suggesting a national estimate of roughly 4000 people injured severely enough for them to present to hospital.