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during the first treatment session; there were no differences in outcomes between these subgroups (B W Koes *et al*, unpublished findings).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

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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.

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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

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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.

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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.

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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.

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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.

Thirty people were injured by falling off the castle (17 male, 13 female; five aged 0-4, 12 aged 5-9, 12 aged 10-14, and one aged 22). Parts of the body commonly injured were the foot, ankle, or toe (10 cases) and the arm, elbow, wrist, or hand (eight cases). Most injuries were minor bruising, cuts, or swelling (20 cases), but there were six fractures and three sprains or strains. Two children required admission for fractures to the arm or elbow. The locations of the bouncy castles varied: six were at fairgrounds and five at indoor sports centres.

Seventy five people were injured on the castle itself (31 male, 44 female; eight aged 0-4, 33 aged 5-9, 27 aged 10-14, and seven adults). Thirty two fell over and 14 were struck by another child, usually after the patient had fallen over. The arm, elbow, wrist, or hand (27 cases), the face and neck (nine cases), and the ankle or foot (15 cases) were commonly injured. Fifty injuries were minor, with nine fractures, 10 sprains and strains, and two dislocations. Fairgrounds (10 cases) were the most common site for the castle.

The youngest child injured was 2 and the oldest adult 41. Only one adult was not actually playing on the castle at the time of injury.

The severity of injuries resulting from people falling off bouncy castles could be reduced by surrounding the castles with impact absorbing matting. The frequency of other injuries could be reduced by controlling the number of children playing on the castle at any time. Adults were injured by doing somersaults, diving over the castles, and throwing one another around; they should contain their exuberance when playing on these castles.

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1 Singer G, Freedman LS. Injuries sustained on "bouncy castles." *BMJ* 1992;304:912. (4 April.)

Is the child father of the man?

SIR,—Roger J Robinson's editorial draws attention to the work done by David Barker and his team in relating coronary risk factors and cardiovascular risk in middle age to factors operating in the uterus and in infancy.¹ The inability of known risk factors measured in middle age to explain all that they might has been widely recognised. It has led, over the years, to many extravagant claims by research lobbies that, given funds, they would close the gap and supply the missing links, whether they are lipid subfractions, psychosocial factors, or DNA sequencing.

The Southampton group has rendered a service in opening up another dimension for environmental determination of cardiovascular risk and, by implication, for prevention. The editorial suggests that these findings have changed the paradigm for explaining regional variation in disease. Whether or not this is true, they cannot explain the other contemporary conundrum: the dramatic declines in mortality from coronary heart disease that are occurring in many countries. These support the role of environmental influences now.

The figure shows trends in mortality from coronary heart disease in men in the United States and in England and Wales, using a logarithmic scale to permit simultaneous comparison of eight different age groups. If the declining trends were caused by beneficial changes in exposures in early life they should occur in the youngest age group first and take three decades to work through from the 35 to the 65 year olds. In the United States the curves all began to turn over and plunge almost in unison in the middle to late 1960s (the blip in 1979 was due to a change in rules on coding disease). In England and Wales there is indeed a vague suspicion that older age groups are changing later,

Mortality from coronary heart disease (100 000 men in United States and England and Wales (from data of World Health Organisation analysed by cardiovascular epidemiology unit, Dundee University)).

but not enough to fit the birth cohort model described above. In general, within each age group, rates in England and Wales oscillated on a plateau between 1965 and 1975 before declining in the 1980s.

Despite the importance of the work done in Southampton cardiovascular risk can still be modified quite profoundly, even after subjects have graduated from the uterus and are out of nappies.

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1 Robinson RJ. Is the child father of the man? *BMJ* 1992;304:789-90. (28 March.)

Patent foramen ovale and subaqua diving

SIR,—Would the *BMJ* publish a study which suggested that hypertension is not a risk factor for stroke because the authors had found 26 hypertensive patients who had not had a stroke? Probably not. Yet Stephen J Cross and colleagues' report on the safety of subaqua diving for people with a patent foramen ovale is based purely on their observation that 26 divers with intracardiac right to left shunts on contrast echocardiography had not had decompression sickness.¹

Although such a shunt will usually correspond with a patent foramen ovale, not all people with a patent foramen ovale will have shunting and some shunts will be due to other mechanisms. Any comparison between the incidence of shunts during life and the incidence of patent foramen ovale in a necropsy study must be approximate.²

We know from larger studies that a significant proportion of divers who have never had decompression sickness have shunts.³ This is only to be expected. Shunts occur in a quarter to a third of the population, and there is no obvious reason why the presence of a shunt would affect recruitment of divers. Thus about 15 000 of the 50 000 divers in the United Kingdom might be expected to have shunts. Even if each of the 100 cases of decompression sickness annually in the United Kingdom occurred in divers with shunts there must be many divers with shunts who do not get decompression sickness, and other factors must also operate. Clearly, to show that shunts are a risk factor for decompression sickness by showing a reduced

incidence of shunts in unaffected subjects a study would need to be exceedingly large. It is far better to examine the incidence of shunts in affected people.^{4,5}

Cross and colleagues question "whether these shunts are relevant in subjects who have not had decompression sickness." Is hypertension a risk factor for stroke in someone who does not have a stroke?

The authors conclude that "the presence of a shunt in a diver without a history of decompression sickness may be irrelevant" and that "there is probably not a case for routine screening of divers for shunts" (my emphasis). If their data do not give a definite answer it is equally valid to conclude that the presence of a shunt may be relevant to divers who will one day get decompression sickness and that there is possibly a case for screening.

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- 5 Wilmshurst PT, Byrne JC, Webb-Peploe MM. Relation between inter-atrial shunts and decompression sickness in divers. *Lancet* 1989;ii:1302-6.

AUTHORS' REPLY.—About half of divers with decompression sickness are found to have right to left shunts.^{1,2} As Peter Wilmshurst states, roughly 15 000 of the 50 000 divers in the United Kingdom would be expected to have a shunt, yet there are only 100 cases of decompression sickness each year. Although a right to left shunt may increase the risk of a diver developing decompression sickness, this additional risk should not be exaggerated.

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- 1 Wilmshurst PT, Byrne JC, Webb-Peploe MM. Relation between interatrial shunts and decompression sickness in divers. *Lancet* 1989;ii:1302-6.
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Site distribution of colorectal cancer

SIR,—It is often stated that about 70% of colorectal tumours are within reach of a flexible sigmoidoscope. This has been reiterated in the ABC of Colorectal Diseases¹ but may be an overestimate. Widespread evidence suggests that the site distribution of colorectal cancer is altered in countries in which there is a high incidence of colorectal cancer.^{2,3}

We have examined the site distribution of the 487 histologically proved colorectal cancers newly diagnosed in Northern Ireland in 1990. This information was available from the database of the Northern Ireland colorectal cancer register. A total of 231 cancers (47.4%) occurred in the proximal colon and beyond the 60 cm range of the flexible sigmoidoscope.

Northern Ireland has one of the highest inci-