supported by a report by the Department of Health and Social Security's working group on back pain, which analysed many papers on this subject in terms of scientific validity and found only three on manipulative therapy that fulfilled its criteria.² In general these studies are akin to asking "Is appendectomy an effective treatment for abdominal pain?"

Breen, a chiropractor, suggested that "for those who use manipulative therapy in the management of back pain patients, a clear idea of the nature of pain and its possible sources is of paramount importance."³ Furthermore, "without rational hypotheses to address these questions treatment becomes incoherent and irrelevant to the problem." By way of example, mechanical and local inflammatory sources of pain may be amenable to manual therapy, but infective, metabolic, neoplastic, and systemic inflammatory causes most probably are not. Indeed, it is quite possible that acute and chronic conditions, whether in the neck or in the back, will all respond in different ways. In keeping with Breen's view we believe strongly that any future attempt to rationalise the role of manipulative therapy in the treatment of back pain should be preceded by an attempt to classify and segregate both site and cause.

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- 1 Koes BW, Assendelft WJJ, van der Heijden GJMG, Bouter LM, Knipschild PG. Spinal manipulation and mobilisation for back and neck pain: a blinded review. *BMJ* 1991;303:1298-303. (23 November.)
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- 3 Breen A. Sources of back pain due to mechanical disorders within the lumbar spine: a chiropractor's view. *Proceedings of the Society for Back Pain Research* 1990 May:9-11.

AUTHORS' REPLY.—We are pleased that three of the letters discussing our review are written by authors of trials that we listed in our "top 10."¹ The detailed comments on the methods we used to assess published trials show the considerable advantage of this method of review. Whereas traditionally authors of review articles implicitly apply their (unknown) criteria, we explicitly formulated and used our criteria, which obviously facilitates discussion.

R S MacDonald states that the trial of Hadler *et al*² was greatly undervalued in the review, but this trial scored 53 points, which was the second best score. MacDonald applied our criteria and weighting to Hadler *et al*'s trial and ended up with 90 points. J A Mathews assessed a study of which he was a coauthor³ and also obtained 90 points. Both of these scores seem to be based on a different use of our criteria and the use of additional information that was not included in the original articles.

The statement that random allocation has been carried out is, in our opinion, not sufficient to warrant all four points for this criterion. All studies included in our review were randomised controlled trials. Studies could earn points if there was a clear description of how the randomisation procedure had been carried out. For readers this is important.⁴ The points given for sample size are not meant as a reward for sufficient power. Consequently we do not agree that points should be given depending on whether a certain difference in outcome was significant. Our main reason for giving points for large sample sizes was the assumption that with larger numbers one has more assurance that (un)known prognostic indicators will be equally divided between the study groups.

MacDonald's suggestion that all 10 points should be given for relevant outcome measures when an author reports only one or two primary outcome measures is certainly interesting. Of course these primary outcome measures should be

chosen (and preferably published) before the data from the trial are analysed. Otherwise the reader has no assurance that the choice of primary outcome measures was not guided by the data analysis.

T W Meade and colleagues performed one of the best studies,⁵ but it still showed severe shortcomings.⁶ They think that we insisted on pragmatic trials, but this is obviously not the case because studies were also rewarded if they included an adequate placebo treatment. Meade and colleagues claim points for having carried out an analysis based on intention to treat. In our review, however, studies could earn points for this criterion only if in cases of more than 10% loss to follow up (which was the case after two years' follow up) and an alternative analysis had also been carried out correcting for withdrawals and missing values.

We do not agree with Mathews that these kind of reviews can be carried out only by clinicians (although three of us are clinicians). As long as explicit criteria are used that can be applied and checked by most readers we see no ground for this statement.

We agree with S M Hay and B Todd that the design of studies of this treatment is probably difficult to improve. A recent study from our department scored 55 points when assessed by the two reviewers (GJMGH and WJJA, in this case not blinded) who had made the assessments in our meta-analysis.⁷ We still think, however, that further trials are needed to determine the efficacy of spinal manipulation and mobilisation for well defined subgroups of patients with back and neck pain. Although the methods of reviewing that we used clearly need to be developed further, we recommend them for reviewing past and future studies.

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- 1 Koes BW, Assendelft WJJ, van der Heijden GJMG, Bouter LM, Knipschild PG. Spinal manipulation and mobilisation for back and neck pain: a blinded review. *BMJ* 1991;303:1298-303. (23 November.)
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- 7 Koes BW, Bouter LM, Mameren van H, Essers AHM, Verstegen GMJR, Hofhuizen DM, *et al*. Randomised clinical trial of manual therapy and physiotherapy for persistent back and neck complaints: results of one year follow up. *BMJ* (in press).

Glasgow coma scale and gag reflex

SIR.—With regard to A J Stanners's comments¹ on our paper on the relation between the Glasgow coma scale and the gag reflex,² the importance of other reflexes and mechanisms in protecting the airway is beyond dispute. The paper that we originally submitted to the *BMJ* discussed these more fully and included data on the cough reflex in the patients, which showed that the cough reflex was much less easily suppressed than the gag reflex.

The cough and swallow reflexes and other protective mechanisms are not, however, easy to

assess objectively, especially in an accident and emergency department, where sophisticated assessments such as speech therapy or video-fluoroscopy are unobtainable. Also, the conditions causing acute global cerebral depression that we described are not totally analogous to the more focal or chronic conditions referred to by Stanners, and some of the above reflexes may be attenuated in elderly or debilitated patients.

Though agreeing that a normal gag reflex does not necessarily indicate a safe airway, we think that the surprising feature of our data was the absence of the gag reflex in a group of fully conscious patients who would not normally be considered to be at any appreciable risk. We thus stand by our original assertion that in accident and emergency departments the gag reflex should be assessed independently of conscious level and used as an indicator of an airway at risk.

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- 1 Stanners AJ. Glasgow coma scale and gag reflex. *BMJ* 1991;303:1401. (30 November.)
- 2 Moulton C, Pennycook A, Makower R. Relation between Glasgow coma scale and the gag reflex. *BMJ* 1991;303:1240-1. (16 November.)

SIR.—A J Stanners would rely on videofluoroscopy in diagnosing aspiration in neurologically impaired patients.¹ This cumbersome investigation has largely been superseded in our hospital by direct observation of the larynx with a flexible fiberoptic nasendoscope while a bolus of milk is swallowed. "Milk nasendoscopy" is a quick and easy investigation that can be performed at the patient's bedside if required, with equipment and expertise that should be readily available in an ear, nose, and throat department. The patient is not irradiated, and the test can be repeated as often as required.

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- 1 Stanners AJ. Glasgow coma scale and gag reflex. *BMJ* 1991;303:1401. (30 November.)

Chorionic villus sampling

SIR.—As chairman of the Medical Research Council's working party on the evaluation of chorionic villus sampling and coordinator of its European trial of chorionic villus sampling and amniocentesis¹ we are writing to dissociate ourselves from D T Y Liu's assertion, made in his capacity as a member of the working party, that "participants were still on the learning curve and results will differ if the trial is repeated."²

Our report explicitly commented on the absence of any evidence that the somewhat greater rate of fetal loss attributable to chorionic villus sampling declined as the trial proceeded, as would have occurred if those performing chorionic villus sampling were still on the learning curve. Only centres satisfying specific criteria regarding their previous experience took part in the trial. Furthermore, most patients were recruited in centres with a particularly high level of interest and skill in chorionic villus sampling. Whether results would differ if the trial is repeated is thus entirely speculative. Findings from several individual centres included the rate Liu expects based on his personal experience, and there was no evidence of heterogeneity between centres. Had Liu entered patients into the trial he would more convincingly have been able to compare his results with those of others.

It is also misleading to suggest that the Medical Research Council's European trial "was not sub-

stantiated by the Canadian study." The Canadian findings were qualitatively similar to those from the European trial, their combined results suggesting an excess fetal loss rate of 3.2% (95% confidence interval 0.3% to 6.2%) in those investigated by chorionic villus sampling compared with amniocentesis.¹ It is for individual women and those responsible for their care to decide whether this risk is acceptable in order to achieve earlier diagnosis (or exclusion) of fetal abnormalities and the advantages of earlier termination if this is then indicated.

Some disappointment that chorionic villus sampling may not be quite as safe as amniocentesis is understandable, but if this is the case—and the evidence suggests that it is—those concerned need to know and attempts to try to explain the excess away are not in their interests.

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1 MRC Working Party of the Evaluation of Chorionic Villus Sampling. MRC European trial of chorionic villus sampling. *Lancet* 1991;337:1491-9.

2 Liu DTY. Chorionic villus sampling. *BMJ* 1991;303:1402. (30 November.)

3 Canadian Collaborative CVS-Amniocentesis Clinical Trial Group. Multicentre randomised clinical trial of chorionic villus sampling and amniocentesis. *Lancet* 1989;i:1-6.

Could an outbreak of poliomyelitis occur in the UK?

SIR,—S O Cameron and colleagues ask whether an outbreak of poliomyelitis could occur in the United Kingdom.¹ A patient with fatal acute poliomyelitis was treated recently in this hospital, but despite extensive microbiological investigation the causative organism is not known.²

Acute poliomyelitis was diagnosed only after postmortem examination and investigations performed during life had been inconclusive: the spinal cord was removed and examined four weeks later, after fixation in formalin. The spinal cord is not routinely examined at postmortem examination and, indeed, is probably examined very rarely in postmortem examinations performed for a coroner. It is therefore feasible that this case, although not proved to be due to poliovirus, could be one of several and that other fatal cases of poliomyelitis have escaped diagnosis.

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1 Cameron SO, Carrington D, Patterson W. Could an outbreak of poliomyelitis occur in the UK? *BMJ* 1992;304:52. (4 January.)

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Resuscitation and hypothermia

SIR,—Minerva¹ mentions a study in which none of 185 patients who had suffered a cardiac arrest outside hospital were discharged; all had received unsuccessful resuscitation outside hospital, which had been continued in the hospital emergency room.² What Minerva fails to mention is that hypothermic patients were excluded from this study; this is a vital point, as we recently found.

A patient was brought to Westminster Hospital, having been found unresponsive while in police custody. A police surgeon had diagnosed a cardiac arrest, and an ambulance crew with paramedical training had started resuscitation. The time from

the cardiac arrest to the start of resuscitation was over five minutes. On arrival at the hospital the patient had no pulse, was not breathing spontaneously, felt cold, and had fixed dilated pupils. Further resuscitation was continued in line with guidelines of the Resuscitation Council. The patient's core temperature was 29°C on a low reading thermometer.

As hypothermia is a contraindication to stopping resuscitation we decided to rewarm the patient by using extracorporeal bypass. For technical reasons this had to be instigated at a neighbouring hospital, and cardiopulmonary resuscitation was continued in the ambulance and into the operating theatre, where bypass was established via the femoral route. Within 10 minutes the patient developed a cardiac output. Cardiopulmonary resuscitation had been performed for over 60 minutes. The patient subsequently recovered completely.

Defibrillation is not likely to be effective if a patient's core temperature is less than 30°C, and resuscitation should not be abandoned until determined efforts to rewarm the patient have been made. Pupillary reactions may be misleading during resuscitation, especially after drugs have been given. The saying that you are not dead until you are "warm and dead" should be borne in mind by the person responsible for stopping resuscitation, especially at this time of year.

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1 Minerva. *BMJ* 1991;303:1410. (30 November.)

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Cost effectiveness of rehabilitation

SIR,—Daphne Gloag reported a conference on rehabilitation organised by the British Society of Rehabilitation Medicine.¹ Several papers have recently been published in American journals, dealing with the effectiveness of rehabilitation for head injury when it is delivered as a coordinated system—that is to say, as the patient's needs change the patient's management changes and he or she moves from an acute high dependency unit to more conventional inpatient neurorehabilitation, to a community based transitional unit, to job coaching, to return to employment or sheltered or supported living or whatever is appropriate.

Given this organisation of services, two groups of workers have clearly shown that the system is effective in terms of increasing the patient's productivity of life.^{2,6} Productivity is carefully defined, and the costs and benefits of different gains in productivity are defined. These workers show that an investment in appropriate rehabilitation can save many millions of dollars by reducing a patient's dependency, reducing the need for support, care, and attendance and (in the British context) the need for social security benefits.

In conclusion, there is now clear evidence of the effectiveness and cost effectiveness of skilled and appropriately delivered neurorehabilitation. I emphasise that the rehabilitation has to be skilled and appropriate.

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Chaos theory

SIR,—W J Firth's article on chaos theory was a good general introduction to the subject for clinicians.¹ There are, however, many other aspects of medicine in which chaos is relevant. For example, Rapp *et al* have developed a method of electroencephalographic monitoring that may predict impending epileptic fits as well as monitor the effects of drugs on the central nervous system.² The variations of dopamine concentrations in patients with Parkinson's disease have been shown to be chaotic and therefore amenable to mathematical manipulation.³ The mathematics of chaos theory has described the seasonal variations of measles epidemics; this calls for further research into the behaviour of other epidemics.

Some rhythmic processes in the body (insulin release and circadian and reproductive cycles) have already been re-evaluated by using the theory of chaos. In fact, almost any cyclic process is amenable to detailed mathematical analysis, which can help explain the underlying mechanisms and suggest ways for therapeutic intervention. This has been shown in the case of aging mechanisms. By using advanced mathematical formulas derived from the theory of chaos I have offered theoretical ways of altering some of the steps in protein modification that occurs with normal aging.⁴

Obviously, more research is needed into this fascinating new theory. The relevance of chaos in medicine needs to be more widely studied.

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Caring for larger lists

SIR,—G N Marsh writes about the possibility of increasing list sizes for general practitioners in the United Kingdom.¹ He should be aware, however, that experience in the United States shows that the general list size of between 2500 and 4000 patients cared for by community family physicians results, in the best of settings, in much preventive and long term care remaining undone. Even in the computer age, with efficiency of computerised practices (the rare situation), the number of elderly people and children who receive appropriate immunisations and the number of well managed diabetic and hypertensive patients falls far short of what have been set as goals for such care in general practice.

In the United States, particularly in times of economic hardship, patients visit physicians far less often than do patients in the United Kingdom; this results in fewer opportunities for practitioners to intervene prospectively in their care. Even in health maintenance organisations, where in theory there are no financial obstacles to obtaining