maintain operation of the system. When a system is upgraded it provides additional features that require the original cost-benefit analysis to be reassessed. Upgrading the equipment commonly results in faster throughput, better discrimination, and added diagnostic value—for example, that provided by angiography—with the result that it tends to be cost neutral on a per patient basis.

In their analysis Szczepura and colleagues calculated that changes in patients' management helped to reduce the costs of examination by £80 and, furthermore, that if half of the diagnostic procedures performed in addition to magnetic resonance imaging were dropped this could result in an additional saving of £62.

The table shows that if the equipment is operated efficiently during the normal working day and with the above assumptions magnetic resonance imaging can make a positive contribution to health care costs. More importantly, if the service is operated for longer each day—a policy that is becoming more widely adopted by units in the United Kingdom—this contribution can be considerable.

Szczepura and colleagues' study emphasises that equipment with high fixed costs can be justified only if it is to be operated efficiently. By assessing and quantifying the outcome in addition to the costs the authors have established a more rational basis on which decisions to purchase magnetic resonance imaging equipment can be made, which I believe should be more widely adopted.

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1 Szczepura AK, Fletcher J, Fitz-Patrick JD. Cost effectiveness of magnetic resonance imaging in the neurosciences. *BMJ* 1991; **303**:1435-9. (7 December.)

Spinal manipulation and mobilisation for back and neck pain

SIR,—In B W Koes and colleagues' review of two decades of reports of trials of spinal manipulation the methods score used seems not to have been sufficiently adapted to the special requirements of such trials.[1] This deficiency may best be seen by considering its application to one of the trials reviewed.

Hadler *et al* stated that patients were randomly allocated,[2] but in Koes and colleagues' paper they lost all four available points for randomisation by not having stated the method. All 12 points available for adequacy of group sizes were lost as a figure of 50 patients per group was not reached. But the trial showed a highly significant effect of treatment ($p=0{\cdot}009$). Target group sizes in protocols are merely estimates of the numbers that will be needed to show an effect of treatment; once a positive result of high significance has been obtained the estimate is superseded by reality. To penalise this trial for a demonstrably adequate group size is illogical.

The trial is not awarded the five points available for use of a placebo control as the sham manipulation employed involved laying on of hands and may thus have had some physical beneficial effect. If this had been so it would have led to an underestimate of the benefit from manipulation and therefore could not invalidate a positive result.

Ten points were available for using five different outcome measures, and yet Pocock *et al*, whom Koes and colleagues quote, advise deciding a priori on a small number of outcome measures and end points to avoid invalidating the significance tests used.[3] Other measures may be made as an exploratory feature of the design to compare the utility of different outcome measures, but this secondary function provides data for use in designing further trials and is not relevant when the trial's primary function of assessing outcome is being considered. Credit in this section should be given for the authors stating prospectively a small number of appropriate outcome measures. Hadler *et al* used one: the disability score designed by Roland and Morris and shown by them to be a more reliable and sensitive index of disability in back pain than measures such as pain experienced or spinal mobility,[4] for which Koes and colleagues would have awarded points.

Ten points were available if the five suggested outcome measures were measured blind. Hadler *et al* relied on a patient questionnaire administered over the telephone by someone unaware of the treatment, and their control patients had received the most realistic "sham" treatment of all reported trials. Therefore blind assessment was probably optimum and yet no points were awarded.

If the same categories and weighting that Koes and colleagues used were applied, the adjustments suggested above would increase the score for Hadler *et al*'s trial from 53% to 90% of the maximum. This confirms my view that this trial had the most sophisticated design reported to date and was greatly undervalued in the review.

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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. *BMJ* 1991;**303**:1298-303. (23 November.)
2 Hadler NM, Curtis P, Gillings DB, Stinnett S. A benefit of spinal manipulation as adjunctive therapy for acute low-back pain: a stratified controlled trial. *Spine* 1987;**12**:703-6.
3 Pocock SJ, Hughes MD, Lee RJ. Statistical problems in the reporting of clinical trials. *N Engl J Med* 1987;**317**:426-32.
4 Roland M, Morris R. A study of the natural history of back pain. Part 1: development of a reliable and sensitive measure of disability in low-back pain. *Spine* 1983;**8**:141-4.

SIR,—B W Koes and colleagues' review of trials of manipulation for back pain has two serious limitations.[1] Firstly, many of the criteria and methods are arbitrary and illogical. A less than homogeneous study population reflects the subjects seen in real life and may increase the applicability of results, particularly where (as in our case) minimisation is used to permit analyses for different groupings, an advantage that Koes and colleagues did not recognise.

It is not mandatory to avoid "cointerventions" (other treatments) in a pragmatic trial, as resort to other treatments may in day to day practice be the consequence of the approaches under comparison. Indeed, insistence on pragmatic trials and the simultaneous avoidance of cointerventions, which is what Koes and colleagues imply by their criteria, make it impossible to recognise the full implications of different policies. These inconsistent and debatable criteria seem to carry as much weight in the scoring system as trial size. Many trials in back pain set out to compare different active regimens in circumstances in which placebo treatment would be unethical, so that penalising these trials for the absence of a group treated with placebo is inappropriate. There are obvious difficulties in blinding patients in trials of different manipulative techniques.

Secondly, there are several inaccuracies. Our trial of chiropractic and hospital management for back pain—not specifically of manipulation—came near the top of the scores derived, but we refer to it here only to exemplify these errors.[2] There was a surprisingly high proportion (20%) of initial mistakes by the reviewers "usually . . . due to errors in reading," a problem already identified.[3] In the case of our own trial the authors did not recognise that we did establish homogeneous subgroups or that we described interventions in both the text and a table. Even more obviously, the authors failed to acknowledge our explicit statement that we analysed the results by intention to treat. One trial[4] was included in this and a previous review by Koes *et al*[5] because of its factorial design, a considerable advantage overlooked by the authors. Although almost identical criteria were used, it scored 38 on one occasion and 50 on the other, suggesting a serious degree of inconsistency in the review process.

Whether the term manipulation should be used, as Koes and colleagues used it, "to cover both manipulation and mobilisation" is at least arguable. Their conclusions about manipulation (thus defined) in back pain seem analogous to generalisations about antibiotics for sore throat.

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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.)
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 Meade TW. Effectiviteit van chiropractie en fysiotherapie bij behandeling van lage rugpijn. *Nederlands Tijdschrift voor Manuele Therapie* 1991;**10**:14-6.
4 Coxhead CE, Inskip H, Meade TW, North WRS, Troup JDG. Multicentre trial of physiotherapy in the management of sciatic symptoms. *Lancet* 1981;**i**:1065-8.
5 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. (29 June.)

SIR,—The blinded review of spinal manipulation and mobilisation by B W Koes and colleagues, from the department of epidemiology and biostatistics at the University of Limburg, is welcome for the attention it draws to a difficult problem and the meticulous scientific methods used.[1] The absence of clinical input, however, has led to erroneous judgments.

I wonder, for example, in respect of our own paper on what basis the authors gave it a score of 0 out of 5 for "mentioning good qualifications of manipulative therapist."[2] We clearly were too modest in assuming that membership of the Chartered Society of Physiotherapists and working and teaching in the department in which Cyriax pioneered the treatment under study were adequate. With similar modesty I have rescored our study and find that it now scores 90 and is top of the list. This is not a surprise as the study was mounted with a careful design and after advice from both clinicians and statisticians.

The serious message is that epidemiologists and statisticians may not be qualified to assess the merits of clinical papers and that their pronouncements may be misleading. I believe that such assessments should use appropriate clinicians.

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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.)
2 Mathews JA, Mills SB, Jenkins VM, Grimes SM, Morkel MJ, Mathews W, *et al*. Back pain and sciatica: controlled trials of manipulation, traction, sclerosant and epidural injections. *Br J Rheumatol* 1987;**26**:416-23.

SIR,—In their review of the most worthy papers on manipulation for back and neck pain Koes and colleagues have shown the unsatisfactory nature of almost all previous work.[1] We do not, however, agree with the suggestion that further attempts should be made to answer the same question by using this format. Of the papers reviewed by the authors, four achieved methodological scores of 50-60, and these were all comparatively recent. We think it unlikely that the quality of this type of study could improve dramatically, and this is