severity of health problems and the variety of options for care and care settings.

Secondly, concern about the use of QALYs still focuses on doubts about their adequacy as measures of health gain¹ and on the technical validity of comparisons of costs per QALY.² Though these problems may be overcome, we believe that the authors' conclusions are mistaken, even when considered within the logic of their own "economic" arguments.

The authors fail to acknowledge that the marginal cost per QALY changes with the extent of unmet need for a service. If services are provided first to those with greatest need the marginal cost per QALY will increase as provision is increased. As the point at which need is completely met is approached the marginal cost per QALY is likely to increase sharply because the marginal benefit derived from increasing provision declines. For example, the number of people in a population who need hip replacement is finite.³ Once services have reached the level at which all such people receive a replacement quickly the QALYs gained by increasing provision further will be zero and the marginal cost per QALY infinite. The only way around this problem is to estimate cost per QALY at each level of unmet need (not service provision as the authors suggest), based on an understanding of the distribution of severity of a health problem within the population.

It might be argued that for most services unmet need is so great that gain in QALYs can be assumed to remain constant over the range of any marginal increase in service provision. We believe that this view cannot be accepted uncritically and that the estimation of unmet need must form the basis of a strategy for planning and purchasing health services, even where maximising QALYs is the agreed objective. This can be achieved only by considering a wide variety of formal and informal information on the occurrence of health problems and the effectiveness, balance, and costs of services.² This is the business of needs assessment.

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- 2 NHS Management Executive. *Assessing health care needs: a DHA project discussion paper*. London: Department of Health, 1991.
- 3 Stevens A, Cabbay J. Needs assessment needs assessment. *Health Trends* 1991;23:20-3.
- 4 Harris J. QALYing the value of life. *J Med Ethics* 1987;13:117-23.
- 5 Drummond MF. Output measurement for resource allocation decisions in health care. *Oxford Review of Economic Policy* 1989;5:59-74.
- 6 Frankel S. Health needs, health care requirements and the myth of infinite demand. *Lancet* 1991;337:1588-90.

Cost effectiveness of magnetic resonance imaging

SIR,—A K Szczepura and colleagues present a valuable analysis of the costs of a magnetic resonance imaging service¹ but conclude that there is no evidence of a beneficial effect on their patients' quality of life. This conclusion is based on a cohort study without a control group. Mean quality of life scores are presented, based on responses to questionnaires completed at the time of the scan and six months later; the mean score for the whole group fell by 0.059, but there is no statistical analysis of these data to indicate significance, confidence intervals, or the statistical power of this study. Only 52% of the patients in the study completed questionnaires, and the possibility of sample bias is not discussed. Moreover, their data relate to a group of patients who had a wide variety of brain and spinal diseases.

It seems inappropriate for conclusions about the impact of radiological investigations on patients' quality of life to be drawn from an audit of this nature.

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AUTHORS' REPLY.—L J Abernethy is wrong to suggest that our study concludes that magnetic resonance imaging has no beneficial effect on patients' quality of life. The main focus of our evaluation was the impact of imaging on diagnosis, diagnostic certainty, and patients' management and the economic implications for the health service. In the service setting with which we were faced we were unable to set up a randomised controlled trial. To achieve our main objectives we chose a controlled observational methodology as the optimum means of ensuring adequate prospective control.¹ The lack of an external control group naturally meant that we were not able to identify with any certainty whether patients' quality of life changed because of magnetic resonance imaging. In the paper we state that "there was no indication that patients' quality of life improved after magnetic resonance imaging."

Furthermore, our study was restricted to neuro-science referrals, and large scale improvements in quality of life were not expected. The pattern we observed is similar to that reported by Dixon *et al.*² Both studies show a relatively high average quality of life for patients at the time of their scan and a slight deterioration six months later. In our study we also recorded the prognosis made by consultants at the time of referral. This predicted a slight deterioration overall, which was confirmed by the quality of life measurements.

Because our chosen methodology could not measure the impact of imaging on patients' quality of life we considered recruiting a contemporary control group of matched patients from other centres to obtain this information. Owing to financial constraints we had to reject this idea. Similar quasiexperimental studies, however, failed to recruit genuine control populations in earlier evaluations of new medical imaging technologies such as computed tomography.³ All this is fully explained in our report on the project¹ but could not be included in the condensed version published in the *BMJ*. Interestingly, the most recent document summarising the Australian trial of magnetic

resonance imaging (produced since our paper was submitted) concludes that further work is needed to measure the effects of magnetic resonance imaging on patients' quality of life "but that such data may be difficult to obtain."⁴ Our experience supports this.

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- 1 Institute of Medicine. *Assessing medical technologies*. Washington, DC: National Academy Press, 1985.
- 2 Dixon AK, Southern JP, Teale A, Freer CEL, Hall LD, Williams A, *et al.* Magnetic resonance imaging for the head and spine: effective for the clinician or the patient? *BMJ* 1991;302:79-82.
- 3 Russell I. The evaluation of computerized tomography: a review of research methods. In: Culyer AJ, Horisberger B, eds. *Economic and medical evaluation of health care technology*. Berlin: Springer-Verlag, 1983.
- 4 Szczepura AK, Fletcher J, Fitz-Patrick JD. *An evaluation of the introduction of magnetic resonance imaging (MRI) in a UK service setting*. Warwick: Health Services Research Unit, Warwick Business School, 1991. (Occasional paper No 1.)
- 5 Hailey DM, Crowe BL, McDonald JG. *Assessing the place of MRI in Australia*. Canberra: Australian Institute of Health, 1991.

SIR,—A K Szczepura and colleagues¹ discuss the cost effectiveness of magnetic resonance imaging in the neurosciences. I wish to highlight the sensitivity of the cost per patient to changes in the throughput of patients; reinterpretation of the authors' data with some modifications leads to a far more favourable outcome for the use of magnetic resonance imaging.

The authors compared the annual costs in their unit in Coventry with those at other sites in the United Kingdom and in Australia and the United States. I have used the authors' data, with some modifications to take account of the current prices of equipment in the United Kingdom, to analyse the sensitivity of the cost of an examination to the number of patients scanned. The theoretical maximum throughput for magnetic resonance imaging is difficult to assess; in the private sector low throughput may result from an inability to attract referrals, while in the public sector it may result from underutilisation of equipment. I estimate that an achievable throughput approximates to one patient every 30 minutes with a 1.5 T system, falling to one patient every 45 minutes with a 0.5 T system. These figures are quite achievable, given that a typical lumbar spine examination, with imaging in two planes and up to date sequences, takes under 10 minutes at 1.5 T.

I have not included the cost of upgrading the system in this analysis as it is not necessary to

Estimated annual cost (£) of magnetic resonance imaging service in United Kingdom, based on 1991 prices

	Field strength*			
	Mid-field	Mid-field	Mid-field	High field
Patients per year	2 250	2 880	3 840	5 760
Patients per day	9	12	16	24
Operating hours	8	8	12	12
Annual capital costs:				
Equipment†	107 000	107 000	107 000	179 000
Accommodation‡	20 000	20 000	20 000	20 000
Total annual equivalent	127 000	127 000	127 000	199 000
Annual revenue costs:				
Staffing‡	105 000	105 000	155 000	155 000
Operating	48 000	48 000	48 000	48 000
Service and cryogenics	65 000	65 000	65 000	85 000
Variable	38 000	49 000	65 000	98 000
Total costs	256 000	267 000	333 000	386 000
Total annual cost	383 000	394 000	460 000	585 000
Direct cost/patient	170	137	120	102
Net effective cost§	89	56	39	21
Net effective cost	27	-6	-23	-41

*Mid-field includes 0.5 T and 1.0 T systems; high field is 1.5 T systems.

†Assumes that equipment has a seven year life and accommodation a 25 year life at 6% discount rate. Average prices are mid-field equipment £600 000, high field equipment £1.0m, and accommodation £250 000.

‡Staff costs increase to account for extended hours.

§Assuming improved management benefit from magnetic resonance imaging of £80.

||Assuming reduction in existing procedures, saving £62.

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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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 ($p=0.009$). 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 trial⁴ was included in this and a previous review by Koes *et al*¹ because of its factorial design, a considerable advantage over-

looked 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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- 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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- 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