satisfy the normal requirements for screening tests: each test should be cheap, easy to apply, reliably reproduced, sensitive, and specific. Although routine weighing satisfies the first two criteria, none of the quoted references suggests that it comes anywhere near to satisfying the remainder. If we are to make progress in detecting poor fetal growth we must begin to apply scientific principles to antenatal care. No longer is it sufficient to answer the question "Why should it not be done?" The important question is "Why should this be done?"

Dawes and Grudzinskas have shown that routine repeated measurement of maternal weight in all patients adds nothing to the reliable prediction of babies who are small for gestational age.² Further studies have confirmed this finding and indicated why the observation lacks sensitivity and specificity.^{3,4} If serial measurement of weight had never been a part of antenatal care there would not be any reason to introduce it now. This is not to imply, however, that measurement of maternal weight during pregnancy is never appropriate, merely that routine measurements are a waste of resources and may be misleading.

In the same issue as the editorial M G Dawes and colleagues highlight the confusion surrounding the perceived reasons for routine weighing in antenatal clinics.⁵ Those who currently apply this test gave 40 different reasons for doing so. Clearly there is no consensus, even among practising health care workers, over its application let alone its interpretation.

Routine serial measurement of maternal weight during pregnancy is not a valuable procedure and should be abandoned.

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- 1 Dimperio DL, Fentzen BH, Cruz A. Routine weighing during antenatal visits. *BMJ* 1992;304:460. (22 February.)
- 2 Dawes MG, Grudzinskas JG. Repeated measurement of maternal weight during pregnancy. Is this a useful practice? *Br J Obstet Gynaecol* 1991;98:189-94.
- 3 Dawes MG, Grudzinskas JG. Patterns of maternal weight gain in pregnancy. *Br J Obstet Gynaecol* 1991;98:195-201.
- 4 Lawrence M, McKillop FM, Durin JVA. Women who gain more fat during pregnancy may not have bigger babies: implications for recommended weight gain during pregnancy. *Br J Obstet Gynaecol* 1991;98:254-9.
- 5 Dawes MG, Green J, Ashurst H. Routine weighing in pregnancy. *BMJ* 1992;304:487-9. (22 February.)

SIR,—Diane L Dimperio and colleagues conclude that serial antenatal weighing should continue as a screening test for preterm delivery, low birth weight, and pre-eclampsia¹ but provide no evidence that it fulfils the criteria for a successful screening test.² Certainly the conditions they wish to identify are common and important enough for screening to be worth while, but there is no evidence of a silent period in the development of any of these conditions during which treatment is beneficial, with the possible exception of treatment with low dose aspirin for pre-eclampsia. Pre-eclampsia is better screened by measuring urine protein excretion or blood pressure, or both, than by serial weighing.

Even if there were effective interventions none of the papers quoted by the authors give data in a form such that the sensitivity and specificity of the test can be derived. They all simply describe weak correlations between low weight gain and various poor outcomes. Only Dawes and Grudzinskas have described the test characteristics of low weight gain (below the 10th centile) for predicting small for gestational age babies (sensitivity 19%, specificity 87%) and of high weight gain (above the 90th centile) for predicting high blood pressure (sensitivity 26%, specificity 80%).³ This performance is much worse than that of other variables measured in screening tests, such as fundal height, ultrasonographic measurements, blood pressure,

and urinary variables. Dimperio and colleagues provide nothing in their editorial to contradict Dawes and Grudzinskas's conclusions that routine weighing fulfils only two screening criteria (it is cheap and acceptable) and that it should stop.³

It is profoundly depressing that at a time when obstetricians are attempting to rationalise their management of pregnant women⁴ the editorial should respond to this excellent scientific evaluation of a common screening procedure in such an unscientific way.

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- 2 Grant A, Mohide P. Screening and diagnostic tests in antenatal care. In: Enkin M, Chalmers I, eds. *Effectiveness and satisfaction in antenatal care*. London: International Medical Publications, 1982:22-7.
- 3 Dawes MG, Grudzinskas JG. Repeated measurement of maternal weight during pregnancy. *Br J Obstet Gynaecol* 1991;98:189-94.
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AUTHOR'S REPLY,—Though one objective of antenatal care is to screen for problems, another is to provide teaching and anticipatory guidance that, if followed, result in optimum outcomes for both the mother and the newborn infant. Active promotion of healthy behaviours, such as consumption of a diet that provides adequate but not excessive energy, should be part of routine prenatal care. Methods of estimating the energy requirement of a pregnant woman exist but are time consuming, do not take into account individual variation, and have not been correlated with the outcome of pregnancy. A simple and more effective method of assessing energy sufficiency during pregnancy is weight gain. A comprehensive analysis of scientific data has resulted in guidelines for weight gain that are consistent with desirable outcomes for mothers and newborn infants.¹

As both inadequate and excessive gains in weight are associated with undesirable outcomes of pregnancy they should be avoided. Weight gain in the second half of pregnancy is especially correlated with fetal growth,² and thus assessments of weight after the initial booking continue to be essential for good care. Those providing care should routinely monitor weight gain to reinforce a positive pattern or intervene if the pattern is becoming abnormal. Women with poor weight gain should be assessed to determine why their energy intake is insufficient for their requirements. Unusually high weight gain may, but does not necessarily, reflect excess energy intake. When it occurs, assessment should determine whether it is a result of excess energy intake, abnormal fluid retention, or multiple pregnancy.

The routine of weighing the patient and subsequent counselling has two additional benefits: it relieves patients' anxiety about weight gain and introduces the topic of the overall nutritional adequacy of the diet. Promotion of good nutrition should be an important component of antenatal care. Women who receive intervention with emphasis on achieving an optimum weight gain and nutritional adequacy have improved outcomes compared with those who do not.³ Routine weighing of all women as part of antenatal care is valuable and should not be discarded.

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- 1 Institute of Medicine. *Nutrition during pregnancy*. Washington: National Academy, 1990.
- 2 Lawton FG, Mason G, Krystyna K, Ramsay I, Morewood G. Poor maternal weight gain between 28 and 32 weeks gestation may predict small-for-gestational-age infants. *Br J Obstet Gynaecol* 1988;95:884-7.
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Manipulative therapy and physiotherapy for persistent back and neck complaints

SIR,—Bart W Koes and colleagues have shown the benefits of manipulative therapy compared with physiotherapy,¹ supporting the findings of an earlier trial conducted by the Medical Research Council.²

Firstly, however, contrary to the statement in their paper, it is not possible to differentiate patients with disc herniation from those with other causes of back pain on the basis of a non-specific complaint and physical examination. Radiological investigations such as computed tomography, myelography, and magnetic resonance imaging are usually required to achieve this distinction.

Secondly, intervertebral discs start to degenerate in early adulthood, becoming symptomatic after fragmentation, with herniation through an intact annulus or impingement on the spinal canal.³ As manipulative therapy entails small movements of high velocity, applying sudden stresses to chronically degenerating discs may precipitate protrusion of a disc. Manipulating the spines of patients with back pain of undiagnosed aetiology is not without risk, and known complications, although rare, range from injury to the cervical cord⁴ to brain stem infarction.⁵ Our experience includes two cases of compression of the cauda equina after chiropractic manipulation⁶ in which the diagnosis was delayed, resulting in long term disability. As a result we endorse calls for further trials to elucidate the role of spinal manipulation in the management of low back pain and for a review of chiropractic training in the United Kingdom.^{7,8}

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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 Maurice-Williams RS. Disorders of the spinal nerve roots. In: Weatherall DJ, Ledingham JGG, Warrell DA, eds. *Oxford textbook of medicine*. Oxford: Oxford University Press, 1987:21.112-5.
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SIR,—I am concerned about the inappropriate selection of patients and treatment in the study by Bart W Koes and colleagues.¹

The introduction mentions that, in the patients selected, no underlying disease could be established and the causes of the complaints remained unknown. Why? Were the assessors lacking the

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.

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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.

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- 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.”¹ 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.

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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 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.

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- 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