

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.

Results were clinically trivial

EDITOR,—T W Meade and colleagues have provided further data from their study comparing chiropractic with hospital physiotherapy.¹ Unfortunately, their report is far from convincing. The "headline" advantage of chiropractic over hospital management at three years (29%) sounds impressive but refers to an improvement of three points on the 100 point Oswestry scale, or one and a half responses on the questionnaire. This difference may be statistically significant but is clinically trivial.

In their original article the authors noted that chiropractic was 50% more expensive,² and in the latest report they note that the chiropractic group had more treatments in the long term. The measured improvement does not seem to support such an expenditure.

The design of the study was criticised after the first paper was published.³ The patients initially presenting to a chiropractor were self selected on the basis that they believed that chiropractic would be effective (as they were expecting to pay for treatment). It is interesting that only the patients referred by chiropractors showed a significant advantage for chiropractic. Those initially recruited in hospital practice showed no significant difference. The study compares private practice with NHS treatment, with all the implications for environmental and personal factors that this brings. The follow up rate (70% and 77%) is inadequate and would preclude publication in some journals.⁴ Analysis by intention to treat does not obviate this deficiency. Important variables such as psychological disturbance are not addressed, although these are noted to have a greater impact on results than does treatment.⁵

It is disappointing that in the five years between the reports these and the other criticisms that were raised were not more fully addressed. Despite the acclamation of the first report in the popular media, I do not think that this study has advanced our understanding of the treatment of back pain in any useful way.

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1 Meade TW, Dyer S, Browne W, Frank AO. Randomised comparison of chiropractic and hospital outpatient management for low back pain: results from extended follow up. *BMJ* 1995;311:349-51. (5 August.)

2 Meade TW, Dyer S, Browne W, Townsend J, Frank AO. Low back pain of mechanical origin: randomised prospective comparison of chiropractic and hospital treatment. *BMJ* 1990;300:1431-7.

3 Low back pain: comparison of chiropractic and hospital outpatient treatment [letters]. *BMJ* 1990;300:1647-50, 1723.

4 Nachemson AL, Larocca H. *Spine* 1987;12:427-9.

5 Greenough CG. Recovery from low back pain: 1-5 year follow-up of 287 injury-related cases. *Acta Orth Scand* 1993;64(suppl 254):1-34.

Authors' reply

EDITOR,—Comparing like with like is a requirement for the randomly allocated groups of patients but not for the treatments, which can be as similar or different as circumstances dictate. We compared the general approaches of chiropractic and hospital treatment for two main reasons. One reason was the absence of evidence of the efficacy of chiropractic despite its growing popularity in the 1980s. The other reason was that those whom we consulted at the outset could not agree on which specific treatments and groups of patients should be included, and only a small number of physiotherapists are fully trained in manipulation.

We welcome Alison Wakefield and Martin Bull's endorsement of our recommendation for further trials. The training of physiotherapists to which they refer is necessary for these as well as for the increasing provision of specialist treatment by spinal therapists.

All the patients initially attending chiropractors who expressed a preference for chiropractic treatment were excluded from the trial, so C G Greenough's comment on this point is misplaced. Our first report described the overall cost effectiveness of chiropractic despite the somewhat higher cost of the treatment itself and also discussed the influence of psychological factors. The authors of both letters about our paper have been selective in their quotation of our results. They have overlooked the considerable improvement in pain due to chiropractic not only early on, when over 90% of patients were followed up and additional treatment had not been given, but at all the other follow up intervals. They also do not refer to the patients' considerably greater satisfaction with chiropractic than hospital treatment regardless of whether the patients initially attended chiropractors or hospitals. Our main conclusion that chiropractic has a part to play in the management of low back pain remains unaltered.

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Practical pulse oximetry

Saturation can be normal with dangerously high pCO₂

EDITOR,—We agree with C D Hanning and J M Alexander-Williams that pulse oximetry has a place in the management of hypoxic patients with respiratory failure and that hypercarbia is less damaging physiologically than hypoxaemia.¹ It is important to point out, however, that arterial carbon dioxide tension can become dangerously high while oxygen saturation remains normal, and this is a limitation of pulse oximetry. From the alveolar gas equations²:

$$\text{alveolar } P_{O_2} = \text{barometric pressure} \times (\text{inspired } O_2 - O_2 \text{ uptake/alveolar ventilation})$$

$$\text{alveolar } P_{CO_2} = \text{barometric pressure} \times (\text{mean inspired } CO_2 + CO_2 \text{ output/alveolar ventilation})$$

Therefore during hypoventilation the fall in alveolar oxygen tension (P_{O_2}) can be overcome by an increase in inspired oxygen while the alveolar carbon dioxide tension (P_{CO_2}) continues to rise.

For example, if a patient hypoventilates with an alveolar ventilation of 1.5 litres/min in room air the alveolar oxygen tension would be 4 kPa (assuming an oxygen consumption of 200 ml/min). If, however, the same patient was breathing 30% oxygen the alveolar oxygen tension would be 12.5 kPa, which is almost within the normal range. At this level of hypoventilation the arterial carbon dioxide tension, on the other hand, would rise to 13 kPa (two to three times normal) whatever the inspired oxygen concentration.

In patients who are hypoventilating while breathing supplemental oxygen, such as post-operative patients receiving intravenous or epidural opiates, the arterial carbon dioxide tension can climb dramatically in the face of a normal oxygen saturation. In this situation hyper-

carbia is life threatening, and as the oxygen saturation may be normal the pulse oximeter fails to warn of impending respiratory failure. Indeed, a normal saturation can lead to a false sense of security, and monitoring of respiratory rate and depth will be as valuable as pulse oximetry, if not more so, to detect impending carbon dioxide narcosis in these patients.

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1 Hanning CD, Alexander-Williams JM. Pulse oximetry: a practical review. *BMJ* 1995;311:367-70. (5 August.)

2 Nunn JF. *Applied respiratory physiology*. 4th ed. London: Butterworths, 1993.

Overnight oximetry is easy and useful

EDITOR,—Two aspects of C D Hanning and J M Alexander-Williams' review on pulse oximetry warrant further comment.¹ Firstly, the simplicity of operation of pulse oximeters may not be obvious to general readers. Not only can a pulse oximeter be used by nursing staff on the ward but patients themselves can easily be instructed in its use. This becomes important in assessing respiration during sleep. Patients can perform satisfactory overnight studies in their own home, with the advantages of being able to sleep in a familiar environment and not needing medical staff to supervise them.

Secondly, the article suggests that oximetry should be used only when sleep apnoea is suspected or the patient has important lung disease. It may also be useful, however, to assess the effect of sleep on cardiovascular disease. Clinicians are often faced with the problems of nocturnal angina or nocturnal arrhythmias, and oximetry can often give useful information and aid decisions on treatment.² We suggest that overnight oximetry should be included routinely in the investigations of these disorders.

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1 Hanning CD, Alexander-Williams JM. Pulse oximetry: a practical review. *BMJ* 1995;311:367-70. (5 August.)

2 Liston R, Deegan PC, McCreery C, McNicholas WT. Role of respiratory sleep disorders in the pathogenesis of nocturnal angina and arrhythmias. *Postgrad Med J* 1994;70:275-80.

Advance directives increase personal autonomy

EDITOR,—Ian Robertson's arguments against advance directives, or living wills, strike me as biased and specious.¹ The fact that most people who fail in attempts at suicide are later glad that they survived is irrelevant to advance directives. Most suicide attempts occur during mental states that later improve. Advance directives apply only in circumstances in which there can be no improvement.

Robertson also claims that terminally ill people do not commit suicide. This claim can be disputed. Most terminally ill people do not have the knowledge or the means to kill themselves and fear the consequences of a bungled attempt.

Robertson's argument that if there is brain damage the person occupying the body is not