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.

C G GREENOUGH
Consultant orthopaedic surgeon

Department of Orthopaedic
Surgery,
Middlesbrough General Hospital,
Middlesbrough TS5 5AZ

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.

T W MEADE

Director

SANDRA DYER

Statistician

WENDY BROWNE

Research nurse

MRC Epidemiology and Medical Care Unit,
Wolfson Institute of Preventive Medicine,
Medical College of St Bartholomew's Hospital,
London EC1M 6BQ

A O FRANK

Consultant in rheumatology and rehabilitation

Northwick Park Hospital,
Harrow,
Middlesex HA1 3UJ

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.

P M ROBBINS
Clinical fellow in anaesthesia

Harefield Hospital,
Harefield,
Middlesex UB9 6JH

M D BRUNNER
Consultant anaesthetist

Northwick Park Hospital,
Harrow,
Middlesex HA1 3UJ

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's 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.

PAUL C DEEGAN
Senior registrar

Department of Respiratory Medicine,
University Hospital,
Queen's Medical Centre,
Nottingham NG7 2UH

RICHARD LISTON
Clinical research fellow

Department of Geriatric Medicine,
Hope Hospital,
Salford M6 8HD

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

the person who wrote the advance directive is ridiculous. The courts are used to dealing with normal wills and have a good grasp of testamentary capacity. They accept an earlier will if later ones are seen to have been affected by mental illness or cerebral disorder. Clearly, the courts take the view that the person owning the estate was the person who existed before the onset of mental or cerebral illness. I consider that I own my body and my life. I would wish to specify the disposition of them while I am of sound mind, subject to rigid safeguards.

I am constantly saddened by the arrogance of those who wish to impose their beliefs and their analysis of situations on others. I have no wish to force Robertson to make an advance directive. I wish he would not try to deprive me of the right to do so.

C M TONKS
Consultant psychiatrist

Royal Masonic Hospital,
London W6 0TN

1 Robertson I. A will to live. *BMJ* 1995;311:457. (12 August.)

Prejudice against Basildon is unwarranted

EDITOR,—I imagine that the *BMJ* welcomes debate in its letter columns to guide its editorial policy, but I was disappointed by K Cleary's letter about classified advertisements, which focuses on examples of content that were irrelevant.¹ Cleary reasonably censures one inept medical recruitment department for errors in an advertisement and another for being economical with the truth concerning financial inducements. Cleary's prejudices about Basildon, however, are inappropriate. There is nothing wrong with working in Basildon, Hackney (as Cleary does), or anywhere else. Basildon Hospital is evolving into an important non-metropolitan teaching hospital for undergraduates and postgraduates. Its high profile advertisements are meant to be original and the content sincere. This month the chief medical officer will give the opening lecture at an expanded £500 000 postgraduate education centre with a new state of the art lecture theatre. A unique local professional trust fund advertises in the *BMJ* for research projects (applicants are mainly trainees) and awards grants of up to £25 000 a year.

At a time when I am extremely concerned about recruitment of consultant psychiatrists generally, I deprecate my psychiatric colleague's slur on the medical staff in this district, who take the recruitment of good consultants very seriously indeed.

M R LOWE
Chairman,
Basildon and Thurrock Consultant Staff Committee

Mental Health Service,
Basildon Hospital,
Basildon,
Essex SS16 5NL

1 Cleary K. Classified advertisements in the *BMJ*. *BMJ* 1995;311:1027. (14 October.)

Government health warnings may encourage adolescents to smoke

EDITOR,—Despite an intensive antismoking campaign aimed at young people and a decreasing prevalence of smoking among adults the prevalence of smoking in adolescents is increasing.^{1,2} I suggest that one possible contributor to this disturbing trend is government health warnings.

There is no evidence that health warnings deter adolescents from smoking,³ but there are several reasons why they may produce the opposite effect.

Adolescents take up smoking to rebel, to show proof of daring, to feel grown up, and to conform with their peers.³ Government health warnings make it easier for adolescents to achieve all of these. Health warnings are the recurring voice of a disapproving establishment. They emphasise the element of risk, they reinforce a subliminal association between smoking and adulthood, and they provide a banner under which, subconsciously, all adolescent smokers can unite.

The voluntary agreement between the government and the tobacco industry states that advertisements for cigarettes cannot show material that suggests it is daring to smoke or that would appeal to those who are rebellious, particularly the young. On these grounds, should not all cigarette advertisements carrying a health warning be banned?

Harefield Hospital,
Harefield,
Middlesex UB9 6JH

DAVID CUMMINS
Deputy clinical tutor

- 1 Diamond A, Goddard E. *Smoking among secondary school children in 1994*. London: HMSO, 1995.
- 2 Foulds J, Godfrey C. Counting the cost of children's smoking. *BMJ* 1995;311:1152-4. (28 October.)
- 3 Murray M. Smoking in childhood. In: Hart C, Bain J, eds. *Child care in general practice*. 3rd ed. London: Churchill Livingstone, 1989:144-54.

Misleading meta-analysis

One incorrect meta-analysis does not invalidate them all

EDITOR,—Guilt by association is dangerous practice in medicine as in law, yet James Le Fanu seeks to discredit the results of all meta-analyses¹ because of the incorrect result of one: small trials of intravenous magnesium in acute myocardial infarction suggested an effect not confirmed by a large trial.² The misleading suggestion was probably due to publication bias.² Meta-analyses are not immune to error and must be evaluated as critically as individual studies.

Le Fanu considers that passive smoking does not cause lung cancer. It is acknowledged that exposure to carcinogens has no threshold below which there is no effect. Linear dosimetry indicates an excess risk of about 20% in exposed compared with unexposed non-smokers (exposure from passive smoking is about 1% of that from actively smoking 20 cigarettes daily,³ which conveys about a 20-fold excess risk). Meta-analysis of the epidemiological studies, updated, gives about the same result.³ This consistency from two independent sources indicates that the estimate of 20% is about right. Variation in the distribution of the sites of cancers in the lung, with a higher proportion of peripheral cancers in passive smokers, is not surprising, as the source cited by Le Fanu indicated.

Le Fanu states that the effect of salt on blood pressure is "very small." Our analysis estimated that a reduction in daily salt intake of 3 g (30-40%) at age 50-59 lowers systolic blood pressure by 5 mm Hg, which is equivalent to a decrease of 22% in mortality from stroke and 15% in mortality from heart disease.⁴ The effect on blood pressure may seem small, but the effect on disease is great. The accuracy of the estimates of the effect of salt on blood pressure was confirmed by the precise prediction of the reductions in blood pressure (ranging from 2.6 to 25.2 mm Hg) in each of the 33 trials of salt reduction previously reviewed⁴ when the mean initial blood pressure, age, and reduction in salt intake in each trial were taken into account. Le Fanu attributes the blood pressure lowering in trials of salt reduction to a placebo effect; the precise prediction of the results of the double blind trials (not susceptible to placebo effect) refutes this possibility.⁵

Le Fanu's "biological common sense" might best be described as his personal opinion. Indeed, common sense supports the evidence. Breathing carcinogens from environmental tobacco smoke causes lung cancer but to a proportionately lesser extent than inhaling the carcinogens from active smoking. Salt, known to increase blood pressure experimentally, does so as expected in the general population.

M R LAW
Reader
N J WALD
Professor

Department of Environmental and Preventive Medicine,
Wolfson Institute of Preventive Medicine,
St Bartholomew's Hospital Medical College,
London EC1M 6BQ

- 1 Le Fanu J. Misleading meta-analysis. *BMJ* 1995;310:1603-4. (17 June.)
- 2 Egger M, Davey Smith G. Misleading meta-analysis. *BMJ* 1995;310:752-4. (25 March.)
- 3 Wald NJ, Nanchahal K, Thompson SG, Cuckle HS. Does breathing other people's tobacco smoke cause lung cancer? *BMJ* 1986;293:1217-22.
- 4 Law MR, Frost CD, Wald NJ. By how much does dietary salt reduction lower blood pressure? III. Analysis of data from trials of salt reduction. *BMJ* 1991;302:819-24.
- 5 Law MR, Frost CD, Wald NJ. Dietary salt and blood pressure. *J Hypertens* 1991;9(suppl 6):S37-41.

Subject to many potential biases

EDITOR,—I M Anderson and B M Tomenson's paper¹ updates our meta-analysis comparing drop out rates in patients taking selective serotonin reuptake inhibitors and tricyclic antidepressants² but excludes newer tricyclic drugs. Despite this difference, which would tend to favour the selective serotonin reuptake inhibitors, the results are nearly identical. The table compares drop out rates in the three published meta-analyses.^{1,3} Clearly, despite the different inclusion criteria, the results broadly agree: the difference is small and of dubious clinical and economic importance.

Drop out rates among patients taking selective serotonin reuptake inhibitors and tricyclic antidepressants in three meta-analyses

	Drop out rate (%)		
	Selective serotonin reuptake inhibitors	Tricyclic anti-depressants	Absolute difference (%)
Song <i>et al</i> ⁴	32.3	33.2	0.9
Montgomery <i>et al</i> ⁵ *	33.8	35.6	1.8
Anderson and Tomenson ¹	30.8	33.4	2.6

*Calculated from figures in paper, with correction for errors.

The small absolute difference found in these meta-analyses may be a poor estimate of the actual difference in drop out rates, for several reasons. Firstly, in several studies patients who were entered into the trials had already failed to benefit from a previous course of tricyclic drugs, and this can bias the results against these drugs. Secondly, seven relevant studies were excluded from Anderson and Tomenson's meta-analysis because the total number of drop outs was not reported. This may result in a bias if drop out rates were not reported because they did not show selective serotonin reuptake inhibitors to be significantly better tolerated. Thirdly, the different clinical settings of the trials may also bias the results. Finally, most studies have a follow up of four to six weeks; in practice, longer treatment is usually recommended.⁴

The broad agreement in the main, albeit crude, outcomes examined by the three meta-analyses shows the strength of our initial conclusion that the routine prescription of selective serotonin reuptake inhibitors as the first line treatment of depression cannot be justified by analysis of the available data.