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.

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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.

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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?

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- 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.

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- 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.
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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> ²	32.3	33.2	0.9
Montgomery <i>et al</i> ³ *	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.