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SIR: Dr Jones has suggested we have misquoted Christensen & Koldbaek (1982). Not so. Perhaps the insertion of two commas might help her understand the sentence "... found 43% of fits inadequate, in terms of EEG signs, when judged by clinical observation alone".

It is widely accepted that EEG monitoring provides the most accurate measure of seizure activity. For better or worse, we used it as the standard by which we judged clinical recognition. Thus, to be consistent, no EEG fit/clinical fit is interpreted as clinical misdiagnosis of a seizure.

A tautology is "saying the same thing twice over in different words" (*Oxford English Dictionary*). We fail to see how *changing* the definition of an EEG seizure and looking again at clinical seizures is tautological.

Dr Jones has read correctly our paper when she says that in the vast majority of cases EEG and clinical methods do agree. Disagreement in 8% of unilateral fits suggests to her that EEG monitoring of unilateral ECT is unwarranted. That is debatable. Ten of 17 patients given unilateral ECT had at least one EEG monitored fit of less than 25 seconds. If fit length is therapeutically crucial, we repeat our suggestion that the case for routine EEG monitoring is then greatly strengthened.

Dr Jones has encountered practical difficulties in EEG monitoring. Such difficulties may exist, but can be overcome. ECT is an important treatment, and it behoves all of us to examine closely our clinical practice.

ROBIN G. MCCREADIE

Department of Clinical Research
Crichton Royal Hospital
Dumfries DG1 4TG

Depression in attempted suicide

SIR: Ennis *et al* (*Journal*, January 1989, **154**, 41–47) state that our study of patients who had attempted suicide (Goldney *et al*, 1981) "reported the highest prevalence of depression". This is demonstrably not so, as two of the five other studies they quote in their Table II found an even higher prevalence of depression.

In considering the different sub-types of depression, there appear to be data missing from their Table I. Dr Ennis *et al* noted that our figure for the delineation of endogenous depression, 36%, was higher than those for affective disorder or endogenous depression detected by others, which were of the order of 10–17%. Their results are also appreciably greater than those previous results, but they have chosen to report them as indicating that "only" 31% were diagnosed with a major depressive episode.

They correctly acknowledge the problems of nosology of depression, and suggested that, "since only 8% of the sample met criteria for melancholia, bipolar illness or major depression with psychotic features, the rate of 'endogenous' depression can be assumed to have been low". This appears to be an unnecessarily restrictive assumption. Indeed, their figure of 31% is not dissimilar to the 36% of our subjects who were delineated as having "endogenous" depression, based on responses to the Levine-Pilowsky Depression (LPD) Questionnaire (Pilowsky & Boulton, 1970). At the very least, it can be asserted that the symptoms which contributed to that allocation of diagnosis by the LPD, and which were described in an earlier Australian study (Goldney & Pilowsky, 1979), are similar to those required for a DSM–III diagnosis of major depression.

The significance of individual symptoms in depression has long been debated. Pollitt (1971) postulated the concept of a "depressive functional shift" to describe the physiological symptoms which delineate depressive illness, and noted that it was an attempt "to find a nucleus of depressive illness; a timeless clinical index which, while being independent of culture and era, could be confidently assessed and communicated". He added that the value of the concept was that "a functional shift, however small, could be useful in distinguishing depressive illness from natural unhappiness". The symptoms employed by Dr Ennis *et al* to fulfil DSM–III criteria for a major depressive episode and those employed in our study to delineate endogenous depression are consistent with Pollitt's "depressive functional shift".

It is tempting to draw an analogy between angina and myocardial infarction, with the 'functional shift' of symptoms of depression being analogous to angina. Cardiologists have the benefit of electrocardiogram and enzyme changes to delineate the boundary between angina and infarction; psychiatrists as yet have no such instruments to provide precise delineation between the symptoms of the 'functional shift' and a depressive illness.

The above points are somewhat speculative. What is not speculative is the manner in which Dr Ennis *et al* have reported that "only" 31% of their subjects were diagnosed as having a major depressive episode, despite their figure being higher than a number of

other studies. A different interpretation of the results could be that no less than 31% of an essentially unselected group of subjects who had attempted suicide suffered from a syndrome of depression which carries a considerable degree of morbidity and mortality, and for which we have, for the most part, effective treatments.

The adequate treatment of depression remains the most effective way in which we as clinicians can assist suicidal patients. The word "only" in this context appears to be singularly inappropriate.

ROBERT D. GOLDNEY

Dibden Research Unit
Glenside Hospital
Adelaide
South Australia

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Molecular genetics and ethics

SIR: I would like to take up the issues aptly raised by Pelosi (*Journal*, October 1988, **153**, 570) and David (*Journal*, January 1989, **154**, 119) and suggest that the ethical implications of the new genetics are of immediate importance, not least because its practical implications have been quickly grasped by those such as insurance companies who play a significant role in the provision of health care in the Western world, if not yet in Britain.

Abuse of new technology is rarely the sole prerogative of doctors. (On the evidence of past enquiries, the public have more to fear from backward than enlightened practitioners, but that is another story.) Ethical abuses depend as much, if not more, on the ideological climate in which the advances occur. The real life counterparts of Drs Moreau and Frankenstein prosper under suitably unethical political regimes. Psychiatrists must ask themselves what implications the current ideological views of health care will have on the new genetics.

This is especially important in view of the fact that these advances promise more than they can, in the short term, deliver. They do not currently hold out any early hopes for treatment, nor do they, if the chromosome 5 studies are representative, imply a simple correspondence between genetic lesion and diagnostic category, an important caveat given the variety of impairments and prognoses across categories. Their preventitive power is constrained by the multiplicity of causes for mental illness and its link with further ethical questions surrounding procedures such as abortion. What they do offer, and what will no doubt be seized upon, is increased predictability of general morbidity within, and possibly outside, the affected pedigree.

The effect of all this in the current ethos may be unfortunate. In emphasising the predictable, and therefore inevitable, aspects of mental illness we do not put ourselves in a strong bargaining position for resources, but improve the position of those who might wish to further limit the amount of money spent on the mentally ill and see in the new discoveries both a reason for, and a potential means of, doing so. Genetic approaches to the mentally ill have an unpleasant social history, and given the above it is easy to see why.

Outside science fiction, ethical choices for doctors are not simply between hubris and humility but a more insidious business of resisting the carrots and sticks of ideology. In the present case the carrots may be more visible than the sticks: there are enough pedigrees, mental illnesses, and slots on the 23 chromosomes to occupy researchers well into the 23rd century. This is all the more reason for collective responsibility. This might entail: (a) strong guidelines on the use of predictive tests; (b) a monitoring of future research to ensure a balance with management issues, be they psychological, social, or physical; and (c) a reasoned debate on where these advances are leading, especially with regard to the everyday clinician and his patients. The most obvious source of such an initiative would be the Royal College of Psychiatrists.

Although some of the above smacks of Luddism, I hope it challenges enough to stimulate a discussion which, as your previous correspondents suggest, is overdue. Whatever history says about us, it will not accept the excuse that we were taken unprepared.

M. F. BRISTOW

St Bartholomew's Hospital
London EC1

Lipid-lowering drugs and violence

SIR: I was interested to see the letter from McLoughlin & Clarke (*Journal*, February 1989, **154**,