that this incidence was unexpectedly high, and the findings of Dr Cooper *et al* confirm this.

The overall incidence of psychiatric disorder in their study was 151/1000 in the post-natal year, which is only 14% higher than in a rather unsatisfactory Edinburgh control group aged 18–65 (not many women give birth after 50). However, as the authors concede, "there was a tendency for the onset of psychiatric disorder to arise in the first 3 months after delivery rather than evenly throughout the postpartum year". Indeed there was: 24% of the incidence was within a month of childbirth, 40% within 3 months and only 27% in the last 6 months of the post-natal year.

On the evidence presented it would be premature to write the obituary of post-natal depression.

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References

NEUGEBAUER, R. (1983) Rate of depression in the puerperium. *British Journal of Psychiatry*, **143**, 421–422.

PITT, B. (1968) "Atypical" depression following childbirth. *British Journal of Psychiatry*, **114**, 1325–1335.

SIR: Without doubt the study by Cooper *et al* (*Journal*, June 1988, **152**, 799–806) is one of the most thorough into the vexed question as to whether childbirth contributes to a genuine increase in non-psychotic morbidity. The results in fact showed no significant difference between the level of such morbidity in a group of puerperal women and that in a non-puerperal control group. The controls were not studied directly by the authors but were a subset of a general population sample of women studied by Surtees *et al* (1983) in Edinburgh. They were non-puerperal in that they had had no pregnancy or delivery during the previous year. The problem with this control group is that it may have contained women in their second or even third postnatal year still suffering from disorders which had had a post-natal onset.

The authors themselves acknowledge that such disorders may pursue a chronic course, and they refer to Pitt (1968), who found that 3.9% of his total sample of 305 women had depressive disorders which showed little or no improvement a year after initial assessment. Of particular relevance for an Edinburgh population is the finding reported by Wrate *et al* (1985) of a 3-year follow-up study of 103 mothers. Of 11 with postnatal depression, 7 (6.8% of the total sample) had disorders which lasted at least until the end of the first postnatal year and 2 mothers had disorders lasting for more than two years. Furthermore, Dr Cooper *et al* show that about one-third of their own puerperal cases are detectable at twelve months postpartum, and they presumably remained cases for at least part of the second postpartum year.

In clinical practice one certainly does see women in the second or, to a lesser extent, third year postpartum with non-psychotic disorders which have pursued a chronic persistent or relapsing course since delivery. Such women should be excluded from the control group in order to derive a better estimate of the psychiatric morbidity among the general female population of childbearing age; one which is independent of the effect of childbirth, although not independent of the effect of the stresses of childcare, and hence suitable for use as a control value.

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References

PITT, B. (1968) "Atypical" depression following childbirth. *British Journal of Psychiatry*, **114**, 1325–1335.

SURTEES, P. G., DEAN, C., INGHAM, J. G., *et al* (1983) Psychiatric disorder in women from an Edinburgh community: associations with demographic factors. *British Journal of Psychiatry*, **142**, 238–246.

WRATE, R. M., ROONEY, A. C., THOMAS, P. F., *et al* (1985) Postnatal depression and child development: a three year follow-up study. *British Journal of Psychiatry*, **146**, 622–627.

'Biological' Treatment

SIR: In his letter (*Journal*, September 1988, **153**, 405) commenting on my use of the word 'biological' in my article (*Journal*, May 1988, **152**, 657–659) on the prediction of response of depressed patients to treatment, Dr Van Kempen asks the question, "why not 'drug' or 'pharmacological' treatment?" The answer is to be found in the first paragraph on page 659, where it is indicated that 23 patients received ECT either alone or in combination with drugs. Perhaps I could have used the word 'physical' to cover this combination of treatments, but in considering the aetiology and treatment of psychiatric disorders it is accepted practice to categorise the factors as psychological, social, and biological.

On the broader issue of the meaning of the word 'biological', I take the point that it is generally held to imply a relation to the science of life in general, but the *Shorter Oxford English Dictionary* (3rd edition, 1973) gives two meanings: (a) "the study of human life and character", which is indicated as obsolete,

and (b) "the science of *physical* (my italics) life, dealing with organised beings or animals and plants, their morphology, physiology, origin and distribution".

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Journal Supplements

SIR: I must register most strongly my objections to your recent Supplement *Progress in Antidepressant Therapy* (*Journal*, September 1988, **153**). I was initially delighted that another supplement had arrived, especially with such a title – suggesting a review of recent trends in psychopharmacology. It was thus with dismay that I found that it was merely a prolonged advertisement for fluoxitene – presumably manufactured by Eli Lilly & Co., as a quarter of the contributors worked for that company.

While I do not doubt that this new drug will prove to be an effective (and expensive) antidepressant, it seems inappropriate that the launch of a new product should be given such official sanction as its own supplement in the *Journal* – especially as it shares its properties with more established drugs already on the market, most notably fluvoxamine (but also clomipramine and trazodone).

If the *Journal* continues to publish the proceedings of symposia, which I trust it will, I hope they will report those meetings that are not so actively involved in the marketing of a particular product, but rather the collection together of informed professionals. I am sure that Eli Lilly & Co. can afford to produce their own glossy advertising without the help of the *Journal* and thereby the tacit endorsement of the Royal College of Psychiatrists.

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The allegation that the *Journal*'s Supplement No. 3 is a "prolonged advertisement" for a particular product is untrue. This supplement is characterised, in fact, by carrying no advertising of any kind. It consists wholly of scientific information, much of it from outstanding scientific workers, which was peer-reviewed in accordance with the *Journal*'s usual standards. The fact that a number of papers refer to a particular drug does not alter this situation. In the same way, the *Archives of General Psychiatry* recently published five papers in one issue and two in a subsequent issue all referring to a particular drug without, so far as I am aware, its integrity being challenged.

The publication of this supplement followed the requirements laid down by the College's Council, and conformed to the highest ethical standards; the content of all three supplements so far published has been fully endorsed by the Journal Committee. Of the Journal's 12 000 subscribers (and considerably more readers), three have so far expressed disapproval of this particular supplement: none have been able to point to any fault in the content of any of the papers contained in it. If, as Dr Jelley suggests, the papers indicate that fluoxetine is the same in its properties as other drugs, that would be a rather peculiar form of 'advertising'.

This supplement does not in any way, directly or indirectly, offer "official sanction" or "tacit endorsement" to fluoxetine or any other drug; nor will any future issue do so. It is quite possible, however, that future supplements may contain more than one paper relating to a particular drug, provided that those papers are of appropriate scientific quality and conform to the *Journal*'s normal requirements.

HUGH FREEMAN

Editor, British Journal of Psychiatry

Prevalence of Dementia

SIR: Ineichen (*Journal*, 1987, **150**, 193–200) proposes that most of the reported prevalence rates of dementia in the elderly are too high, and in particular that the Newcastle rates are too high (Kay *et al*, 1964, 1970). However, the rate cited for the overall prevalence of dementia by Kay *et al* (1970) is shown as 8.8% (Table I of Dr Ineichen's paper), although the correct figure as given in the text is 6.2%. This is almost exactly the median of the rates arranged in order of magnitude in this Table. The Newcastle rate of 6.2% for age 65+ included cases which although mild (as opposed to severe) were regarded as definite.

Table II of Dr Ineichen's paper shows the rates reported for *severe* and *severe+moderate* dementia where authors make this distinction. The Newcastle rate is shown as 6.2% for severe dementia, but was actually 4.9% in the study cited (Kay *et al*, 1964). Moderate dementia was not distinguished but some cases which other authors might call moderate may have been rated as mild.

Dr Ineichen suggests that three recent UK studies which obtained low prevalence rates (mean = 2.9%) are to be preferred to earlier studies. However, the data cited in his paper do not appear to support this view. Of the seven studies with rates of severe dementia below 3% (Table II) (eleven others show rates $\geqslant 3\%$) three refer only to people in their 70th year, an