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

age when dementia is uncommon. In two studies, when the authors' rates of severe and moderate dementia are added together, the rates increase to 5.3% and 7.7%, the latter being higher than any other rate in the Table. In the study by Maule *et al* (1984) the rate of severe + moderate dementia is only 2.9%, but the *overall* rate is 8.6%, which is higher than the overall Newcastle rate of 6.2%.

This leaves only the London–New York cross-national study of Gurland *et al* (1983), in which the London rate of 2.5% compares with the New York rate of 5.8%, a striking difference for which there is no ready explanation; but as the authors point out, replication is needed. Finally, in the overall prevalence Table, the Melton Mowbray survey rate of 4.5% in a population aged 75+ is lower than expected (Clarke *et al*, 1986), but is based on one brief questionnaire.

There seems to be no good reason to think that dementia among the elderly at home is appreciably rarer than the Newcastle rates of 5–6%; in Newcastle, institutions added less than 1% to the rate, but the ratio of cases at home and in institutions will vary. In Finland, for instance, *severe* dementia is found in 3.8% of people aged 65+ at home and in 6.7% when institutions are included (Sulkava *et al*, 1985). The problem is that in the community the range of cognitive impairment is wide and degrees of severity are not yet adequately defined.

In fact, Kay elsewhere (Kay, 1972) reported the Newcastle rates as 6.2% ('severe') and 2.6% ('mild'). This just goes to show the state of confusion the grading of dementia is in.

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References

CLARKE, M., LOWRY, R. & CLARKE, S. (1986) Cognitive impairment in the elderly: a community survey. *Age & Ageing*, **15**, 278–284.

GURLAND, B., COPELAND, J., KURIANSKY, J., *et al* (1983) *The Mind and Mood of Aging: Mental Health Problems of the Community Elderly in New York and London*. London: Croom Helm.

KAY, D. W. K. (1972) Epidemiological aspects of organic brain disease in the aged. In *Aging and the Brain* (ed. C. M. Gaitz), pp. 15–27. New York: Plenum.

——, BEAMISH, P. & ROTH, M. (1964) Old age mental disorders in Newcastle upon Tyne. I. *British Journal of Psychiatry*, **110**, 146–158.

——, BERGMANN, K., FOSTER, E. M. *et al* (1970) Mental illness and hospital usage in the elderly: a random sample followed up. *Comprehensive Psychiatry*, **11**, 26–35.

MAULE, M., MILNE, J. S. & WILLIAMSON, J. (1984) Mental illness and physical health in older people. *Age & Ageing*, **13**, 349–356.

SULKAVA, R., WIKSTRÖM, J., AROMAA, A., *et al* (1985) Prevalence of severe dementia in Finland. *Neurology*, **35**, 1025–1029.

History of Depressive Disorder

SIR: The recent article by Berrios (*Journal*, September 1988, **153**, 298–304) suggests that depressive disorder became conceived as an emotional disorder in the 19th century. Not all the descriptions from that time would concur with this. Those of Griesinger (1867), Lewis (1889), and, a little later, Mendel (1908) suggest that a disturbance of energy may be the basis of the condition.

My own personal experience of the disorder over a period of 20 years and objective view of it during the past 10 years in general practice has led me to the conviction that depression is primarily a disorder of energy, both qualitative and quantitative, with mood changes being secondary. I favour the term dysenergia, which was used by Dioscorides in the 1st century AD, and which is no longer extant, to describe this.

In the early 20th century, when the term 'affective disorder' was gaining usage, caution was nevertheless exhibited by some writers. Craig (1912) observed that, "as with other disorders the mistake was made of naming the disease according to its most prominent symptom"; Bleuler (1923) wrote: "the disturbance of the affect represents merely the most conspicuous symptom of a general transformation of the psyche that cannot as yet be comprehended".

Over the past 40 years the idea that depression is a single system disorder of mood has become increasingly accepted. The biochemical evidence is conflicting, and I can find no phenomenological evidence to support it. I wonder if it is a long-standing assumption which needs examination.

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References

BLEULER, E. (1923) In *Textbook of Psychiatry* (ed. A. A. Brill). New York: Macmillan.

CRAIG, M. (1912) *Psychological Medicine*. London: J. & A. Churchill.

DISCORIDES, P. (1906) *De Materia Medica* (trans. M. Wellman). Berlin: Weidmannsche Buchhandlung.

GRIESINGER, W. (1867) *Mental Pathology and Therapeutics*. London: The New Sydenham Society.

LEWIS, W. B. (1889) *A Textbook of Mental Diseases*. London: Charles Griffin.

MENDEL, E. (1908) *Textbook of Psychiatry* (trans. W. C. Kraus). Philadelphia: F. A. Davis.

Supportive Analytical Psychotherapy

SIR: Holmes (*Journal*, June 1988, **152**, 824–829) proposes a carefully planned admixture of dynamic and supportive therapy which I suspect could only be