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Comparative Trial of a New Antidepressant

SIR: Few would argue with the point raised by Dunn (*Journal*, August 1987, **151**, 269) and Thompson (*Journal*, November 1987, **151**, 702–703) that a comparison of a new antidepressant with a placebo to establish that some efficacy exists is desirable. Clearly, ethical issues are a problem, and there is an extensive literature highlighting these (Klerman, 1986; Rickels, 1986). Practical issues are also a factor, and in a current clinical trial involving placebo control an eminent colleague withdrew his involvement because he felt that every patient should receive an active compound.

Studies previously carried out comparing fluoxetine, a tricyclic antidepressant, and a placebo (Stark & Hardison, 1985; Colin & Wilcox, 1985) confirmed that fluoxetine was more effective than placebo.

My study (Levine *et al*, *Journal*, May 1987, **150**, 653–655) was carried out to establish further evidence of efficacy and to compare the occurrence of side-effects with those of imipramine. The numbers involved allowed for any significant difference to be discriminated. A minimum score on the HRSD of 17 permitted less severe cases to be included. The suggestion by Thompson that 75 mg of a tricyclic antidepressant is "well accepted to be inadequate" is surely an idiosyncratic view, and is at variance with established practice over almost 30 years. Perhaps the as yet unpublished paper quoted in his letter will inform us otherwise.

The need for new antidepressants clearly exists when present compounds are only marginally more effective than placebo at any dose level, and future research will hopefully not be influenced by the "overcrowded market".

SIDNEY LEVINE

Oldham & District General Hospital
Rochdale Road
Oldham OL1 2JH

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Depressed Mood After Stroke

SIR: For Wade *et al* (*Journal*, August 1987, **151**, 200–205) to compare their finding that 20% of their stroke patients were definitely depressed and 10% probably depressed with Bergmann's (1982) reported rate of 4.4% for depressive illness and neurosis in 360 elderly women is of doubtful relevance. Reported rates of depression in community studies of the elderly vary enormously, and one could equally well argue on the basis of comparison with the work of Zung (1967) or Stenback (1979) that stroke protects against depression, as both these studies can be interpreted as demonstrating depression in well over 40% of elderly subjects. It is possible to quote studies which purport to show rates of depression in old age which vary from 48% right down to 1%. None of them help to interpret the findings of Wade *et al*, whose study is flawed by the lack of an age-matched control group, and the use of an instrument which was not developed in order to measure depression in stroke victims with an mean age of 70 but to measure the severity of the depressive syndrome in a group of patients already diagnosed as depressed whose mean age was in the mid-forties (Snaith *et al*, 1971).

DAVID AMES

The Royal Free Hospital
Pond Street
London NW3 2QG

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SIR: We thank Ames for stressing that the Wakefield Self-Assessment Depression Inventory was not developed for use with elderly people, and for pointing out that we did not have an age-matched control population. We discussed both points in the original paper, although we acknowledge that we did not refer to the population studies he mentioned.