use the full doses at which the efficacy of dothiepin was established. There is also inadequate discussion of the reasons for the more frequent withdrawals from dothiepin during the prophylactic phase. Is it possible that patients themselves recognised that dothiepin was no more effective than placebo?

There are studies that provide clear-cut evidence of the prophylactic efficacy of antidepressants in reducing the risks of new episodes of depression (reviewed by Montgomery & Montgomery, 1992). This is not one of them.

CASSIDY, S. & HENRY, J. (1987) Fatal toxicity of antidepressant drugs in overdose. *British Medical Journal*, **295**, 1021–1024.

MINDHAM, R. H. S., HOWLAND, C. & SHEPHERD, M. (1973) An evaluation of continuation therapy with tricyclic antidepressants in depressive illness. *Psychological Medicine*, **3**, 5–17.

MONTGOMERY, S. A. (1990) The methodology necessary to establish the long-term efficacy of antidepressants. In *Human Psychopharmacology: Methods and Measures* (eds I. Hindmarch & P. D. Stonier). Chichester: Wiley.

—— & MONTGOMERY, D. B. (1992) Prophylactic treatment in recurrent unipolar depression. In *Long-term Treatment of Depression* (eds S. A. Montgomery & F. Rouillon). Chichester: Wiley.

STUART A. MONTGOMERY
Academic Department of Psychiatry
St Mary's Hospital
Praed Street
London W2 1NY

AUTHORS' REPLY: If Montgomery thinks we "have not entirely understood [the] arguments" for separation of continuation therapy from prophylaxis, he has not read our paper carefully enough. We do discuss these arguments in some detail (p. 180; paragraphs 2 & 3) and explain why we did not apply the distinction strictly to our elderly patients, as might have been appropriate for a younger group. To reiterate and amplify the point, we were dealing with a population which has been consistently shown by research to contain only about 20% of people, or less, who will not experience a further episode of major depression; whereas the remaining 80% or so are liable either to suffer from chronic affective symptoms or one or more rapid relapses. The key question, therefore, is whether – the patient's physical state permitting – treatment should ever be stopped.

Having made the presumption that our study should have been conducted according to the design he prescribes, Montgomery employs inappropriate statistical methods to analyse it as if we had conducted it that way. In spite of our explanation (p. 177; first paragraph, second column), he does not appear to have understood the error of using cross-sectional analysis (χ^2 tests) on longitudinal data. His statements about significance have no valid statistical foundation. In fact, his assertion that "analysis at the six-month point makes sense" makes no sense at all. Our Table 2 clearly shows how relapses occurred over a two-year period, and the higher incidence in the placebo group is evident. We had hoped to make it abundantly clear that P values obtained from χ^2 tests were mentioned only to underline how misleading they can be when incorrectly applied (as, alas, they so frequently are in studies of this kind). The analysis which is appropriate to longitudinal data, such as these, is to fit a survival curve. In this instance a proportional hazards model was fitted and the survival curves speak for themselves. Clearly, it would have been preferable to have a larger sample since the consequent increase in power would have made it possible to detect a smaller difference between the treatment groups but, in the event, the difference was found to be large enough for the sample size to be acceptable.

Montgomery accuses us of giving an eccentric definition of relapse of one point more than the recommended cut-off. Where does he get this idea from? There is no inconsistency between having a criterion MADRS response < 11 and a definition of relapse > 10 because, provided the individual scores are integers (which they are), the two sets are mutually exclusive and exhaustive. The mean MADRS score on relapse was 24.01.

There was no discussion of suicide or serious side-effects in the interests of brevity. There were no suicides during the trial. Nor were there serious side-effects, which were carefully monitored throughout in accordance with international standards. However, if it were to give Dr Montgomery some peace of mind, none of the psychiatrists taking part in this trial have an 'axe to grind' about tricyclics in general, or dothiepin in particular. The important point is the maintenance of effective pharmacological treatment, without which the great majority of this group of patients can expect their relatively short remaining life-span to be blighted by continuous or recurrent illness.

ROBIN JACOBY
The Maudsley Hospital
London SE5 8AZ

DANIEL LUNN
Worcester College
Oxford
OX1 2HB

Sex and schizophrenia: vive la différence

SIR: Lewis provides an interesting review of the literature investigating the sex differences in

https://doi.org/10.1192/S0007125000181772 Published online by Cambridge University Press