based on the false premise that each time point is of separate interest in its own right. In reality, the primary hypothesis is more global (across all measurements over the follow-up period, is there a tendency for superior functioning in the group with home-based treatment?). While the descriptive data shown by the authors would tend to favour this hypothesis, their analyses appear to be biased towards the null hypothesis of no difference. We therefore think that it is important that the authors use a simple summary statistic approach or global test as described by Frison & Pocock (1992) and Pocock *et al* (1987*b*). Furthermore, instead of presenting many tests involving subgroups, they should test for interaction with some relevant variables. Statistical tests for interaction assess directly whether a prognostic factor affects the difference in treatments (Pocock, 1983).

Another issue that the authors could address is the fact that in both the intervention group and the control group, there appears to be a significant *trend* to improve on clinical and social measures with each subsequent time point, but the trend appears to be stronger in the intervention group. The authors could therefore use a test which allows for the examination of a treatment time interaction. This would reduce the number of tests, and be compatible with what the data tell us.

Time in hospital. The authors' decision to include baseline measures for endpoints as a covariate in some of the analyses is to be commended. For example, if there are large differences on a measure at the pre-treatment baseline assessment, then these differences need to be adjusted for in assessing the effect of treatment at follow-up; the results are not readily interpretable if such baseline differences are not taken into account in assessing the treatment effect. As time in hospital is arguably the most important outcome measure, we were disappointed to note that the authors did not discuss the fact that important baseline differences between the two groups on first admission status may have affected the results (73% of DLP patients were first admissions, versus only 57% of controls; $\chi^2=5.4$; $P=0.02$). Because readmission risk is increased with each subsequent admission (Mortensen & Eaton, 1994), the reported results may be confounded, as controls are more likely to be readmitted regardless of any treatment difference. Furthermore, the higher proportion of prevalent cases with established chronicity in the control group may have confounded other treatment associations as well.

The data provided by Marks and colleagues reveal large differences between the median and mean time spent as in-patients in both groups, indicating that this measure is skewed. As the reported *t*-tests are invalidated in the presence of significant skewness, we would like to suggest an appropriate transformation or the use of non-parametric methods, taking into account confounding by baseline admission status.

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BJP, **166**, 80–86. In authors names and details, Robert A. Burns should read Roger A. Burns, MRCPsych.

BJP, **166**, 263. In the letter *ECT seizure threshold and fluoxetine*, the correct initials for the authors are R. I. Tobiansky and G. G. Lloyd.

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