*Brit. J. Psychiat.* (1979), **134,** 220–224

# *Correspondence*

## SIMULATED AND REAL ECT

DEAR SIR,

There can be little doubt that the paper by Lambourn and Gill (*Journal*, December 1978, **133,** 514–19) will be widely quoted by those who, from whatever motive, seek to denigrate the therapeutic effect of ECT. However the unobservant reader who simply accepts the authors' conclusions will be seriously misled, and we request space to present the data in a different form so that the issues may be clear.

Over a period of two weeks, 16 depressed patients were treated with ECT and a further 16 were subjected to the procedure termed simulated ECT. The immediate outcome of the two groups was not remarkably different; in each group 11 patients responded well (+++ or ++) and 5 patients responded poorly (+ or 0). It is from this observation that the authors draw their major conclusion, that the induction of a convulsion plays an unimportant part in the therapeutic procedure. (The authors' statement in their Results section is, of course, an error: overall, 10 not 5 patients failed to make an immediate favourable response). However, the study does not end at this point, for the referring clinicians required that 6 of the patients in the active treatment group and 7 of the patients in the simulated group should have further ECT at the end of the two-week trial period. At this stage presumably all the patients were receiving active treatment, for the authors do not state otherwise.

The issue now becomes slightly clouded by the fact that the authors have managed to 'lose' the data (or the patients) of three in each group. However, the outcome, after a further month, of the 11 patients who had now received active treatment and for whom data were available is sufficiently impressive (see table on p. 224). Thus 10 out of the 11 patients now receiving active ECT (5 of them for the first time) improved.

We think that no one would deny that a part of the total effect of ECT, and indeed of every other medical procedure, may be regarded as a placebo effect, although suggestion alone cannot account for the total variance of this effect. Some patients will have improved since they were coming to the end of their depressive illness anyway, in others the diagnosis will have been in error, and so on. It should therefore come as no great surprise that the recovery rates after the initial two week procedure were similar. What is important is that, at the end of the period, when all these extraneous factors had exerted their effect, such a large proportion of patients responded favourably to real ECT.

It is necessary to stress a further point. The authors quote our work (Barton *et al*, 1973) but seem to have missed the point in planning their own study. The point is this: that a large proportion (38 per cent in our study) of patients who recover with ECT do not show evidence of this recovery until *after six* applications have been given.

J. L. BARTON

*University of Missouri-Columbia,*
*School of Medicine, Missouri Institute of Psychiatry,*
*St Louis, Missouri 63139, U.S.A.*

R. P. SNAITH

*Leeds University,*
*Department of Psychiatry,*
*15 Hyde Terrace, Leeds LS2 9LT*

### Reference

BARTON, J. L., MEHTA, S. & SNAITH, R. P. (1973) The prophylactic value of extra ECT in depressive illness. *Acta Psychiatrica Scandinavica*, **49,** 386–92.

DEAR SIR,

The report of 'A Controlled Comparison of Simulated and Real ECT' (*Journal*, December 1978, **133,** 514–19) is important; not least because it will enter the political arena as ammunition for those who actively oppose ECT in any circumstances. As the authors state, ECT 'is accepted as a highly effective therapy, particularly for depressive psychosis . . .', and many, probably most, experienced psychiatrists are convinced that there are some clinical syndromes where ECT leads to a dramatic and sometimes life-saving improvement, which cannot be explained by a placebo effect.

What are we to conclude from this study? Unfortunately, it is difficult to come to any conclusion, as we are told very little about the 32 patients. The classification of affective disorder is not an area in which there is wide agreement, and 'a diagnosis of