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Old versus new ECT

SIR: Scott *et al* (*Journal*, March 1992, **160**, 360–364) report a randomised trial comparing efficacy of traditional and modern electroconvulsive therapy (ECT) devices. Eleven out of 17 patients (65%) receiving constant-voltage ECT and 9 out of 14 (64%) receiving constant-current ECT attained 'recovery' defined by an HRSD score of less than 8. The authors stated that "... the likelihood of recovery ... [was] virtually identical in the two groups ... the policy of bilateral suprathreshold modern ECT monitored by EEG [electroencephalogram] is as efficacious as traditional ECT". They must have given some thought to sample size, since they state it "was too small to attempt to identify any potential subsets of depressed patients who may respond preferentially to ECT". A reader's misgivings about the small size of the whole study could be allayed by the authors' attention to this issue.

The *British Medical Journal* requires, in my opinion rightly, confidence intervals to support the main conclusions of a study. Use of confidence intervals at this instance reveals clearly how little one can be sure that the difference is negligible. A 95% CI for 11 out of 17 is 38% to 86%; the corresponding interval for 9 out of 14 is 35% to 87% (Lentner, 1982). The difference in recovery rates is determined only to within a margin of error of 34%: an approximate 95% CI for the difference is −33% to +34%. It is clear that a clinically important difference, in favour of either of the methods of administering ECT, is not ruled out.

LENTNER, C. (ed.) (1982) *Geigy Scientific Tables. Vol. 2. Introduction to Statistics, Statistical Tables, Mathematical Formulae*. 8th ed., pp. 89–104. Basle: Ciba-Geigy.

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AUTHOR'S REPLY: The critique of the interpretation of our findings rests on several assumptions. Firstly, it is assumed that we assess the clinical efficacy of ECT upon recovery rate alone. This is not so, and Newcombe makes no mention of the other measures of clinical outcome after the course of ECT. Secondly, it is assumed that we base our interpretation on the results of this randomised comparative study alone. This is not so; for example, the recovery rate after brief-pulse ECT was identical (64%) to that in an earlier study that used the same definition of recovery (Scott *et al*, 1986). Thirdly, the critique takes no account of the existing literature that we cited. When comparative studies of traditional and brief-pulse ECT are designed to take account of stimulus intensity, no difference in clinical efficacy is found. Finally, the extreme limits of the 95% confidence intervals for the recovery rates are quoted to suggest that we may have failed to detect a "clinically important difference". Gardner & Altman (1989), the statistical advisers to the *British Medical Journal*, stated "regardless of the width of the confidence interval, the sample estimate is the best indicator of the population value". Table 1 showed that none of the estimates (means) of clinical outcome after ECT (final HRSD score, final Montgomery & Asberg score, number of ECT prescribed, and recovery rate) showed any difference of clinical significance.

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I agree that confidence intervals are valuable indicators of the precision of a sample estimate and if I were writing the paper today I would include these as a matter of routine. Undoubtedly, a larger sample of patients would have led to narrower confidence intervals and allowed us to test other hypotheses about traditional ECT. As we explained in the paper, this would also have exposed more depressed patients to a treatment known to be associated with more severe confusion, anterograde amnesia and, to a lesser extent, a greater risk of the loss of personal memories.

GARDNER, M. J. & ALTMAN D. G. (1989) *Statistics with Confidence*, p4. London: British Medical Journal.

SCOTT, A. I. F., WHALLEY, L. J., BENNIE, J. P., *et al* (1986) Oestrogen-stimulated neurophysin and outcome after electroconvulsive therapy. *Lancet*, *i*, 1411–1414.

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Daily living programme

SIR: Muijen *et al*'s preliminary account of an intensive community support scheme as an alternative to prolonged or revolving-door hospital admissions made interesting reading (Muijen *et al*, *Journal*, March 1992, **160**, 379–384). They rightly draw attention to the worrying finding that the study group in the Daily Living Programme included two patients who later killed themselves and another man charged with murder. In assessing such treatment programmes we must, of course, bear in mind that other such controlled studies have not attracted such tragedies and so it would be unwise to draw general conclusions about the worthiness or desirability of such a programme.

However, it would be valuable to have further information regarding these three violent episodes. One wonders about their diagnoses, severity of illness and although contact was 'regular', was it frequent and of what kind was it? How early in the treatment programme did the events occur and if early on, were mean figures added in (to cover for the rest of the treatment phase as if the suicides were still included) on the assumption that the suicides were a tragic, but random event?

While such programmes are in an early phase and before they are generally introduced we need to ensure that such unfortunate events were indeed random (and not dependent upon whether a patient is assigned to a hospital or community treatment group). Any cost-savings of such a community treatment package (if as well run as the Maudsley's) seem to be slight, and the justification may, therefore, rest on qualitative rather than quantitative factors.

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AUTHOR'S REPLY: Dr Green asks about the tragedies in our study. Among our 187 Daily Living Programme (DLP) patients and controls seen for 18 months after trial entry, the DLP had three, and the control group two, deaths from self-harm. The deaths were 4, 5 and 14 months after trial entry; all had had brief in-patient stays. The control deaths were 7 and 12 months after trial entry following admissions of 4 and 3 months' duration. The difference of three versus two deaths does not begin to approach statistical significance. (Further details will appear in our final report.)

Many studies have noted a high suicide rate in psychotic patients, and there is no easy way to reduce this. The risk remains high despite intensive in-patient and/or community care. When monitored, suicides occur surprisingly often even in patients on section and on 24-hour 'special' observation by nurses. There is no evidence as yet that standard in-patient care actually reduces suicide rates in the long-term. Evaluation has to be even-handed between innovative and standard models of care. It rarely is.

The DLP patient's homicide took place more than seven weeks after admission for self-cutting. During five days of in-patient observation he was wary but coherent and rational. Repeated examination found no signs of depression or psychosis. With the ward team's agreement he was discharged into intensive community care, and was seen repeatedly at his home and the DLP base despite his evasiveness. He managed well, and was found a job after having been unemployed for several years. He had not been sectionable at any time. Two days after last being seen he suddenly killed a child. In custody he revealed previously concealed paranoid delusions.

Had the above patient remained an in-patient well beyond the national mean duration for such cases he could have discharged himself, been lost to out-patient follow-up, and become an ex-patient for whom district services could disclaim responsibility. Community care teams like the DLP which rightly do assertive outreach, however, continue their responsibility and thus their vulnerability to blame for adverse events. With whom does the responsibility

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