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Correspondence

Correspondents should note that space is limited and shorter letters have a greater chance of publication. The Editors reserve the right to cut letters and also to eliminate multitudinous references. Please try to be concise, strictly relevant and interesting to the reader, and check the accuracy of all **references in Journal style.**

PREDICTORS OF RESPONSE TO REAL AND SIMULATED ECT

DEAR SIR,

I would like to register my disquiet concerning the methods, arguments and conclusions presented by the Northwick Park research team (*Journal,* March 1984, **144,** 227–37). This research team continues to maintain that the advantage of real over simulated ECT is short-lived, but failed to mention whether ECT dispersed or resolved delusions in a step-wise or linear gradient, and whether the anti-delusional action is longer lasting then the anti-depressant action. If it is, ECT could be seen as possessing substantial benefits.

Further, the Northwick Park research team seemed to operationally define the presence of delusions by the quality and quantity of irrational material expressed at interview. The researchers are remiss in that they have not presented any reliability estimation of their *ad hoc* total delusional score. This score was the sum of the expressed extensiveness of the delusions multiplied by the perceived conviction by which each delusion was held. Therefore, what it may have measured is a combination of the ability to verbalise illogical ideas, garrulousness, and an underlying predisposition to be convinced by one's own automatic thoughts. The researchers refer to the work of Kantor & Glassman (1977); their stipulation that depressives not recovering prior to specific treatment are probably deluded implicitly carries with it the implication that many delusions are not overt, are not obvious, and are not necessarily elicited by close psychiatric questioning.

The Northwick Park research team also continue to obscure how atypical their sample was, and how different it was from the corpus of previous published research on this subject. They attempt to do this by presenting their sample as being selectively drawn from a less selective sample. But this argument is logically flawed, because the final sample studied remains different despite different inclusion and exclusion criteria having been applied. More devastatingly, their second argument, based on the assertion that the most relevant criterion for suitability for ECT is a specific response to real ECT, is clearly *ex post facto,* patently judgmental, and solipsistic. Their third argument, based on the fact that the geographical area from which their sample was selected has a very low usage rate of ECT, does not disprove Kendell's (1981) hypothesis that many of the patients treated in this trial were inappropriate candidates for any form of ECT. It merely indicates that real and severe affective disorder is not randomly distributed throughout the country.

If the entire Northwick Park ECT trial sample was atypical, and I believe that it undoubtedly was, then the smaller subsamples responding differentially to real or simulated ECT were probably even more atypical. Therefore, discriminant predictors may be statistical artefacts, and the failure to find sensible predictors and to replicate the more representative preceding work may be due to methodological errors. The knowledge that there is only a 50/50 chance of any given patient actually receiving a genuine treatment must certainly influence normal referral patterns. Therefore, further new research should be directed to ascertaining predictors of good response, relapse, and poor outcome for clinically suitable and typical patients.

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Drs Crow and Johnstone reply

We read Dr. Weeks' letter with concern. Some phrases ("failed to mention", "are remiss in that they have not", "continue to obscure", "failure to find sensible predictors") suggest to us that his position may be influenced by considerations beyond the substance of the reports of the Northwick Park trial.

In response to the points he raises:—

1. We are puzzled by the distinction he makes between the step-wise and linear dissolution of delusions. We doubt whether existing instruments of assessment (or indeed the inherent variability of psychiatric symptoms) allow such fine theoretical distinctions to be applied in practice.

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The problem in testing his assertion that the response of delusions to ECT is longer-lasting than that of other features of depression is that the Hamilton scale which was used in our study (and most other recent studies) of ECT does not include an assessment of delusions. However, information on the time course of the effects of real vs. simulated ECT in patients with delusions is available in Tables II and III of our paper. The differences between the groups which are present at the end of the 4-week trial period are substantially diminished one month later and absent at 6 months. We find it difficult to see how conclusions other than those which we have drawn concerning the magnitude and duration of the effects of ECT, whether in patients with delusions or the sample as a whole, can be derived from these findings.

2. We have described (*Journal,* March 1984, p.228, column 2, para. 2) how the delusion score we used was derived from the P.S.E. ratings. Degree of conviction was assessed on the standard 0, 1, 2 scale. The reliabilities of P.S.E. ratings in general have been examined (Wing, Cooper, Sartorius 1974). No items in the P.S.E. are specifically concerned with "ability to verbalise illogical ideas, garrulousness, and an underlying predisposition to be convinced by one's own automatic thoughts" which Dr. Weeks supposes have contaminated our delusion score. We doubt whether verbal facility per se biasses the assessment of delusions when the P.S.E. is used in the recommended manner.

3. We remind readers who are interested in Dr. Weeks' suggestions concerning the nature of our trial sample that the patients were selected by (i) the Newcastle criteria for both endogenous depression and responsiveness to ECT, (ii) the criteria for depressive illness used in the MRC (1965) trial and (iii) the Feighner (St. Louis) criteria for primary depressive illness, and invite them to re-examine Table IV of our recent paper. This shows the mean score of our trial sample in relation to the predictive scales of Hobson, Roberts, Carney, Mendels, Kendell and Hamilton. We particularly draw attention to the findings in relation to the series of patients studied by Kendell at the Maudsley Hospital. On the scale he devised a higher score was associated with better response to ECT. In his sample those with a diagnosis of neurotic depression (ICD 314) had a mean score of −1.9, with manic-depressive illness depressed type (ICD 301) 9.5, and melancholic illness (ICD 302) 9.7. The mean score of our sample was 15.2 with a standard deviation of 10.9. We doubt Dr. Weeks' assertion that the spread of real and severe affective disorder across the country departs significantly from a random one but this observation (together with those of Table IV in our paper) does not support the view that the patient sample of the Northwick Park trial was less severely depressed than those treated with ECT elsewhere.

4. We view Dr. Weeks' assertion that "the entire Northwick Park ECT trial sample was atypical" as a personal conviction unrelated to the evidence we have presented and one which is appropriately placed in the context of his subsequent unreferenced declaration of faith ("I believe it undoubtedly was"). Clinical investigators can do no better than to present the findings from their studies in terms of the assessment and rating scales which are available and in as full and accurate a way as possible. This we have attempted to do.

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SPONTANEOUS SECOND FIT AFTER ECT

Dear Sir,

We would like to invite comment from your readers upon an unusual clinical phenomenon in the course of the treatment of a 17 year old girl suffering from a severe psychotic depressive illness. The girl became progressively depressed and deluded over a period of three to four months and eventually refused to eat or drink. She was unable to give informed consent and she was, therefore, treated with ECT twice weekly under Section 58 of the Mental Health Act, 1983. After the second treatment (right unilateral ECT Duo-plus mark 4 machine setting 2) the patient had a second bilateral tonic clonic fit with a divergent squint to the left lasting for one minute and occurring one minute after the electrically induced fit ceased. The patient made an uneventful recovery and she had no further episodes during her subsequent six treatments. The anaesthetic procedure was unremarkable (suxamethonium 50 mgs; Brietal 50 mgs; full preoxygenation). At the time she was on no medication and there was no history of cerebral insult or epilepsy. Neurological examination and EEG were normal and a CT Scan was reported as showing normal ventricles with an enlarged superior cerebellar cistern and cistern of the velum interpositum, but otherwise normal.

Recent evidence (Christensen & Koldbaek, 1982) with EEG monitored ECT has shown that disturbed electrical activity continues after the visible tonic clonic phase ceases. Blumenthal (1955) reports in a series of 51 cases the occurrence of spontaneous fits within a few hours of administering ECT. The questions that concern us are how unusual is it for a second fit to occur within a minute of ECT and whether this can be explained in terms of a spontaneous second fit or continuation of the original electrically induced

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