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# *Correspondence*

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## 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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### References

KANTOR, S. J. & GLASSMAN, A. H. (1977) Delusional depressions: natural history and response to treatment. *British Journal of Psychiatry,* **131,** 351–60.

KENDELL, R. E. (1981) The present status of electroconvulsive therapy. *British Journal of Psychiatry,* **139,** 165–283.

### 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.