one case is excluded at a time and the number of tests is equal to the number of cases. This is the "leaving one out" or "jacknife" method (Lachenbruch & Mickey, 1968). Alternatively one can generate a random sample with the same number of variables and cases, and estimate the random component of the classification function to use as the base-line for significance tests. Another possibility is to use "bootstrap" methods, where the sample is duplicated a number of times and then a number of random samples are drawn to gain an estimate of the bias present in any given sample of the same size (Efron, 1982; McLachlan, 1980).

I would urge the Clinical Research Centre to carry out a re-analysis of their data, and to publish their revised findings. In the meantime, clinicians should set aside their recommendation that ECT is an appropriate treatment for delusional depression; it may turn out that this otherwise well designed study indicates that the effect of ECT is almost wholly a placebo effect.

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Dr Crow and Colleagues Reply

DEAR SIR,

Dr Stuart claims to have detected "serious statistical errors" in the analysis of the Northwick Park ECT trial. Some of his assertions are based upon a failure to separate our original report of the overall therapeutic effect (Johnstone *et al*, 1980), which we regard as the most reliable information we have of the magnitude of the therapeutic effect, from our later analysis (*Journal*, March 1984, **144**, 227–237) of possible predictors of response to real ECT. The conclusions of the latter paper are much less certain as will be apparent from the summary which indicates that "The limited size of the sample does not allow firm conclusions."

Concerning the former paper Dr Stuart writes that "the overall effect was very weak". The size of the effect is apparent in the figure in our original paper and the difference between the groups over the course of the four-week trial was significant at the 1% level. None of Dr Stuart's criticisms appear to be relevant to this conclusion although this may not be apparent to the casual reader of his letter.

Dr Stuart's criticisms are, however, relevant to our analysis of potential predictors of ECT response. Here we are surprised that Dr Stuart regards our paper as having made anything more than tentative suggestions concerning predictors. Thus in addition to the above statement in the summary, the Discussion includes the following:

"Extensive examination of this data did not show clear-cut predictors of response to real or simulated ECT, but this was not unexpected in view of the relatively small sample size". Our main conclusions concerning predictors are included in the subsequent sentences:

"Firstly: we found little support for our previous suggestion . . . that the predictors of response to ECT are merely the predictors of satisfactory response to treatment or even a generalised tendency to satisfactory outcome. . . .

Secondly, the analysis does not support the view that a predominance of endogenous features is a specific predictor of a response to real ECT. The most salient predictor of response to real ECT is probably the presence of delusions. . . . Retardation may be relevant but it may also be associated with response to simulated ECT."

The burden of Dr Stuart's letter is that a different method of statistical analysis would have given a different and more certain conclusion.

We doubt that this is the case but think rather that there is an overriding limitation in the size of our sample. Our sample was primarily collected to test for an overall therapeutic effect of ECT, not to test for interactions. No amount of statistical sophistication can get from a study more information than is contained in the data originally included. Our sample ($n=62$ completors) took 3½ years to accumulate but as a basis for prediction of differen-

https://doi.org/10.1192/S000712500020901X Published online by Cambridge University Press

tial response may be regarded as very small. Thus sub-grouping of patients results in wide confidence limits on any estimate of a treatment effect.

Fortunately some remedy to this limitation is at hand. Since the publication of our paper another study (the Leicester trial) has been completed. This trial had some comparabilities of design to our own and the outcome was in certain respects similar. By combining the samples of the two trials it may be possible to arrive at a firmer conclusion concerning the possible predictors of response to real ECT, e.g., whether they include delusions or retardation separately or together, or some other clinical feature. With the collaboration of the Leicester workers such an analysis is now in hand.

We hope that in due course it will contribute an answer to the question that interests Dr Stuart, other clinicians and ourselves, of what are the reliable predictors of response to real ECT.

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Reference

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What Price Psychotherapy?

DEAR SIR,

The nub of Dr Ryle's argument (*Journal*, August 1985, **146**, 209–210) in his criticism of Professor Shepherd's advocacy of controlled evaluation of psychotherapy is that psychotherapists find the question "Is psychotherapy effective?" inappropriate. This is true insofar as psychotherapy makes no pretention to being a specific therapy and it is then correctly placed among other activities—teaching, parenting—for which Dr Ryle judges the question of effectiveness to be unproductive: a list which, from this point of view, could be extended to include friendliness, kindness and mutual succour. However, where a specific effect, which exceeds that of placebo, is claimed the validity of the claim is crucial: a matter of public concern, for example, when NHS funding of psychotherapy is under consideration.

The issue is fudged by Dr Ryle's inclusion of psychiatry among activities where he judges the question of effectiveness is not appropriate. Many specific therapeutic methods incorporated within psychiatry, somatic, psychological and social, have been either submitted to controlled evaluation or the need is acknowledged. Broadly (e.g. excepting behaviour therapy) the effectiveness of psychotherapy *qua* therapy has not been demonstrated although this has been attempted (Candy *et al*, 1972) and failed for lack of patients judged by psychotherapists as suitable for controlled comparison. Thus their conviction that psychotherapy has a self-evident specific therapeutic effect and the attitude to evaluation it engenders in psychotherapists are obscurantist and explain the commendable persistence of Professor Eysenck and Professor Shepherd in dragging the matter into public view—which Dr Ryle so deplores. That, given the will, the effectiveness of psychotherapy can be weighed in the balance is evident from the study of Sloane *et al* (1975). It is to be hoped that Dr Ryle will see the necessity and wisdom of encouraging such efforts.

It is also worth pointing out that Prioleau's (1983) paper is a re-analysis of studies cited in a report purporting to demonstrate the effectiveness of psychotherapy but which, incredibly, included placebo effect as a mode of therapy. His paper arranged this material so that the effect of psychotherapy was *compared* with placebo effect. The shortcoming of the Prioleau study, which Dr Ryle cites, arises from the dearth of material on which to judge the effectiveness of psychotherapy. He suspects that the attention it has received is attributable to its conclusions. He is right. These tentative as they are, cannot be brushed under the carpet without consigning the credibility of psychotherapy to the dust heap also.

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Munchausen's Syndrome

DEAR SIR,

We should like to report an unusual case history which seems to illustrate an emerging syndrome—Munchausen's syndrome presenting with psychiatric illness.

The patient, a 34 year-old man, presented at a

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