Brit. J. Psychiat. (1970), 117, 715–19

Correspondence

THE BEARING OF TREATMENT ON THE CLASSIFICATION OF THE AFFECTIVE DISORDERS

Dear Sir,

In the September, 1970, issue of the *Journal* Drs. Roth, Kerr *et al.* examined 154 patients suffering from depression. They then observed the patients' response to ECT, tricyclic antidepressants, MAOIs, or sedatives and tranquillizers.

They also noted that 16 patients died in the follow-up period.

In the article they did not say how far this response followed ECT, tricyclic antidepressants, MAOIs, or sedatives and tranquillizers. I would be grateful if this could be made clear.

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Dear Sir,

In reply to Dr. T. O. Clark's letter we would like to make two comments.

Our survey was not confined to depressive states, but included patients suffering from both depressive illnesses and anxiety states, one of the main aims of the project being to examine the relationship between them.

Of the sixteen deaths, twelve were due to physical illness (Kerr *et al.*, 1969) and four patients committed suicide. In no case was psychiatric treatment considered to contribute directly to the fatal outcome.

Clair Gurney,
Martin Roth,
T. A. Kerr,
Kurt Schapira.

References

Kerr, T. A., Schapira, H., and Roth, M. (1969). 'The relationship between premature death and affective disorder.' *Brit. J. Psychiat.*, 115, 1277–82.

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PERCEPTION OF HIDDEN FIGURES BY NEUROTIC AND SCHIZOPHRENIC PATIENTS

Dear Sir,

I wish to thank Dr. Romney for his letter in the July issue of the *Journal* (p. 125), drawing the attention of Mr. Hutt and myself to the work he has done using the Gottschaldt Figures. Of the two references quoted I had read the published article, but not, understandably I think, the unpublished work. However, I must object to the tendentious statement, at the end of the letter, that the published article (1) confirmed that the Gottschaldt Figures Test is 'an almost pure measure of general intelligence'. A reader who is not in a position to evaluate inferences from factor-analytic studies would surely take this to mean that the Gottschaldt is of no more interest, in the study of schizophrenic thinking, than any intelligence test, which is certainly not true, as our article (2) showed. The notion of a 'pure measure of intelligence' has no meaning in an absolute sense (after all 'intelligence' is just a way of referring to the convenient fact that skills tend to be positively correlated), and in the context of Dr. Romney's study it only means that in that study the Gottschaldt was correlated with the intelligence tests and not systematically correlated with the other measures. This, of course, is simply a reflection of what measures were used, and different 'factors' would have appeared if different tests had been included.

We certainly found a substantial correlation between the Gottschaldt and our intelligence measure in our non-psychotic group, but the whole point of the article was that this correlation did not appear in the schizophrenics, who were much worse than the other group on the Gottschaldt while scoring at the same level on the intelligence measure. Dr. Romney's finding, in the unpublished work (3), that the difference between neurotics and schizophrenics on the Gottschaldt disappeared when intelligence was partialled out, is therefore factually at variance with our findings. This is presumably because of the difference in the subjects used, ours being all acutely ill schizophrenics.

The main point of interest of our article, which may not have come across clearly in its abstract form, was that acute schizophrenics, whatever their intelligence, *all* did badly on the Gottschaldt. In many years of work on this topic, I have never found this degree of consistency with any other test purporting to measure schizophrenic thinking.

T. G. Crookes.

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References

1. Romney, D. (1969). 'The validity of certain tests of overinclusion.' *Brit. J. Psychiat.*, **115**, 591–2.
2. Crookes, T. G., and Hutt, S. J. (1970). 'Perception of hidden figures by neurotic and schizophrenic patients.' *Brit. J. Psychiat.*, **116**, 335–6.
3. Romney, D. (1967). *Aspects of Cognitive Dysfunction in Nuclear Schizophrenics and their Parents and Siblings.* Unpublished Ph.D. thesis, University of Newcastle Library.

TREATMENT OF PHOBIC PATIENTS WITH ANTIDEPRESSENTS

Dear Sir,

My earlier letter, in addition to earning me a magisterial rebuke from yourself ('Dr. Mawson expects too much'...), has clearly made Dr. Freeman very angry indeed. I have therefore re-read it carefully in an attempt to understand why. The results of this exercise would clearly not justify publication had Dr. Freeman contented himself with attacking me personally. Unfortunately he passes from the argument *ad hominen* to the argument *ad institutionem* and also chooses to ascribe the vices which he believes my letter to illustrate ('intellectual arrogance' and 'neglect of practical and humane considerations') to 'the development of academic psychiatry'; thereafter his targets proliferate, coming to include 'academic assessors', 'the pursuit of methodological purity', 'scientific sophistication' and even, at least by implication, the Dunlop Committee on the safety of drugs! It is possible that the prejudices thus revealed are shared by a significant proportion of psychiatrists and it would therefore seem important to identify the real issues at stake and discuss them fully and, if possible, dispassionately.

An initial step is to identify these issues which are not basic to the dispute. The principal of these is the question of whether or not MAIOs are in fact effective in the treatment of phobias. Despite Dr. Freeman's supposition to the contrary I too 'actually treat patients', and my experience of treating phobic patients with MAIOs leads me to believe that they do produce a striking response in at least a proportion of cases. Thus the difference between Dr. Kelly and his colleagues and myself, within this narrower context, is that I know I only believe whereas they believed they knew. (I am glad to see, from their courteous and temperate reply to my previous letter, that they no longer consider that 'to carry out a trial using a placebo appears unjustifiable' and instead state that 'it now seems justified to carry out a double-blind controlled trial of phenelzine versus placebo'.)

Perhaps the real and important issues can be expressed as four questions.

(1) When should a statement in the form 'Treatment with X, has been shown to result in Y ($p = < 0\cdot001$) be afforded more respect than one taking the form 'The authors' extensive experience has shown that treatment with X is highly effective in producing Y'—unembellished with probability values?

A statement in the first form purports to be a scientific statement, and is likely to be accepted by most readers as really meaning that there is less than a one in a thousand chance that X did not 'result in' Y. It may be helpful here to consider the following passage: 'In our general impressions far too great weight is attached to what is marvellous .... the scientific man takes care to base his conclusions on actual numbers. General impressions are never to be trusted. Unfortunately when they are of long standing they become fixed rules of life and assume a prescriptive right not to be questioned. Consequently those who are not accustomed to original inquiry entertain a hatred and horror of statistics. They cannot endure the idea of submitting their sacred impressions to cold blooded verification.'(1) Francis Galton was writing nearly 100 years ago: the fact that his remarks still have some relevance is illustrated by the applicability of the last sentence of the quotation to the anguish expressed by Dr. Freeman in connection with his fluphenazine trial and those 'academic assessors'. In the main, however, the point which Galton expressed so well has been heeded, but the result has not been altogether an unmixed blessing.

On the one hand there is the loss, lamented by Dr. Sutherland in your columns two years ago, of the subjective, anecdotal or speculative type of article, putting forward hypotheses, formulations or models, dealing with 'soft' and often intrinsically unquantifiable data, and perhaps based on detailed but uncontrolled observations of small numbers of cases. To exclude such articles is also to exclude much of the subject matter of our specialty and to deny the value of the methods of, for example, Freud or Piaget. (It is ironic, in the context of the current dispute, to recall that Dr. Sutherland referred to your preference for research ... "dominated by the