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BEHAVIOUR THERAPY

DEAR SIR,

A clinician even vaguely familiar with the literature and practice of "behaviour therapy" cannot help but be dismayed at an article like that of Marks and Gelder in your July 1965 issue, "A Controlled Retrospective Study of Behaviour Therapy in Phobic Patients". Although one must be impressed by the care exercised in matching treatment and control patients in terms of deviant behaviour, age, and so forth, there is absolutely no control in terms of actual treatment. To be specific, on p. 564 the authors point out that the behaviour therapy patients often received as wide a variety of ministrations as relaxation-hypnosis, systematic desensitization, barbiturates, and, yes, two E.C.T.s and one leucotomy. How can one overlook this utter disregard of the most elementary and basic criteria of experimental design? All the numbers in the world (e.g. duration of treatment, outcome of treatment on a five-point scale, etc.) are meaningless as a result.

A further criticism is in the use of the term "behaviour therapy" to refer to Meyer and Gelder's technique of gradual *in vivo* exposure or "practical retraining" as they call it. It is especially puzzling to see this unjustified generalization of the phrase "behaviour therapy" in the same article which, in its first paragraph, points up the widely differing nature of psychotherapeutic techniques which are subsumed, for better or for worse (and, in my opinion, for worse) under the rubric of behaviour therapy or conditioning therapy. I have myself been involved in a treatment programme quite similar, in parts, to Meyer and Gelder's (Lazarus, Davison, and Polefka, 1965); we never considered our successful therapy as any sort of vindication of "behaviour therapy", however.

Let me commend Marks and Gelder for their emphasis on the importance of non-desensitization or non-practical retraining factors in treating even relatively simple neurotic disorders. After controlled experimental studies have established the actual conditioning bases of "behaviour therapy techniques" —and this kind of work has only just begun, references below—we will do well to examine any "non-learning" factors of which, I suspect, most practitioners considering themselves behaviour therapists are keenly aware. Arnold Lazarus of South Africa has stressed these non-specifics for several years now. On the other hand, it seems premature to *assert* that learning principles cannot be found to account for aspects of therapy which go beyond the desensitization couch or the syringe loaded with apomorphine.

Ambitious attempts at evaluating various therapies are surely to be encouraged and reinforced. However, it is misleading to publish articles which are so unsatisfactory on methodological grounds. As a fellow "behaviour therapist", I can only hope that investigators like Drs. Marks and Gelder will be more careful in specifying the referents of their terms.

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DEAR SIR,

Dr. Davison's comment on the design of our study misses the point. It was a retrospective inquiry; we collected all the phobic patients who had received behaviour therapy in this hospital from 1960 to 1963. Since we found that it had been customary clinical practice to use behaviour therapy as part of a wider plan of treatment (which sometimes included drugs and occasionally E.C.T.) we collected a control group, with similar clinical features, who had received a similar amount of drugs and E.C.T. The one patient who had a leucotomy and behaviour therapy was matched by a similar patient who had had a leucotomy but no behaviour therapy. Comparison of the two groups revealed the contribution of behaviour therapy over and above that of the other treatments.

The design undoubtedly shows the effect of an active treatment: for example, it shows up the useful effect of behaviour therapy in less severe phobias, and of modified leucotomy in severe agoraphobia (to be published). We cannot accept, therefore, that our findings result merely from poor design.

Dr. Davison has decided that practical retraining should not be called behaviour therapy. Unfortunately he has not provided his definition of the latter. Practical retraining is an accepted form of behaviour therapy, used for many years, notably by Gwynne Jones; it falls within the definition of behaviour therapy adopted in a recent book on the subject, and the method is included as such in that book (*Experiments in Behaviour Therapy*, edited by H. J. Eysenck, 1964, pages 1 and 57).

Behaviour therapy can be expected to help only in certain disorders. The task ahead is to identify these, while realizing that behaviour therapy is but one part of general psychiatric treatment, which it cannot be expected to replace.

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THE BI-ACROMIAL DIAMETER OF WOMEN

DEAR SIR,

Dr. F. D. Kelsey (*Journal*, December 1965, page 1162) draws attention to the variation in the bi-acromial diameter of women, as documented by different observers. The Board of Trade figures (1957) were lowest—35·0, while in his own series the mean was 35·3, S.D. 1·65, the patients being older on average than those measured by Coppen and Rey (1959), whose means were higher still—35·9, S.D. 1·2. He postulates a slight decline with age, and warns of sources of error in measurement due to the degree of tension or relaxation of the subject. Having recently measured 100 controls taken from the general population, it may be of interest to communicate my own figures, which to my surprise were higher than any of the above. The women, aged 18 to 45, had a mean B.A.D. of 36·8, S.D. 2·9. Two measurements were made in each subject by means of a Harpenden anthropometer. It is not plausible that regional factors are entirely responsible for the markedly broader shoulder girths of my Mancunian subjects, and the most likely explanation would appear to be individual variability in the method of measuring. The subject's stance, her state of muscle tone and the degree of pressure applied by the operator must all be taken into consideration. It is probably unwise to regard the figures of different authors as comparable, nor should any one series be accepted as the norm. But rather should the present practice continue of each investigator providing his own control sample.

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SLEEP PATTERNS AND REACTIVE AND ENDOGENOUS DEPRESSIONS

DEAR SIR,

In reply to the letters of Kay and Garside, I apologize for what appears to have been my misunderstanding of Kiloh's (1963) personal communication. It would seem, then, that the most reasonable conclusions we can reach at the present time are that (1) Objective recordings and observations do not provide evidence supporting the clinical position concerning sleep patterns. (2) The evidence obtained concerning patients' *reports* about their sleep is conflicting. It is of interest to note that the loading of the clinical feature "early awakening" on the bipolar factor reactive *vs.* endogenous depression is only 0·227 in the most recent Newcastle study (Carney, Roth, and Garside, 1965), compared with the loading of 0·692 in the earlier study (Kiloh and Garside, 1963), the correlations of the feature with diagnosis in the two studies being 0·271 and 0·831 respectively. In the more recent Newcastle study neither "early awakening" nor "initial insomnia" are included in the ten clinical features selected as giving the best prediction of diagnosis of type of depression. The findings of Mendels (1965) also give no support to the suggested diagnostic validity of sleep patterns.

I am happy to see that Garside has now quoted us (Costello and Selby, 1965) correctly, and I really do not see how a statement that findings suggest no difference between reactive and endogenous depressions is the same as claiming on the basis of negative findings that the null hypothesis has been confirmed.

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[This correspondence is now closed.—ED.]

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