

claims" which have been made for behaviour therapy. An example comes from the book of which he is co-author (p. 266): "A rough estimate based on published large-scale reports suggests that something in the region of 80 per cent. of patients treated were apparently cured or markedly improved." We agree much more with the following quotation, given in his letter, and taken, rather surprisingly, from the same book: "The routine use of these methods is undoubtedly not yet feasible; it must await further improvement of techniques and definitive evidence of superiority over other available techniques."

We are, of course, familiar with the work of Lang and Lazovik and of Lazarus. We did not include them or Paul's study (1) in our review because they did not deal with psychiatric patients, but with volunteers sought out by the authors. The relevance of these studies to psychiatric patients has yet to be established.

The main point of our paper seems not to be understood by Professor Eysenck, viz. that every treatment has indications and contraindications. Of course, skill is important, but the most skilful therapist will obtain poor results when he treats unsuitable patients. The practical question is to delineate those conditions which can be successfully treated by therapists of moderate experience. This we have attempted to do.

We stated in our paper that there was disagreement on 3 per cent. of *all* assessments of the extracts from the case notes at the five points in time. However, Professor Eysenck asks about the reliability of assessments of the final outcome; the correlation of two independent ratings of final outcome was 0.85. After a third independent rating of disagreements, the correlation was 0.94.

As to his suggestion that our series concerned "early self-training results", it may be noted that 94 per cent. of patients in our series were treated after 1960, the year in which the book edited by him, *Behaviour Therapy and the Neuroses*, appeared, and about five years after interest in behaviour therapy began in his department.

We do not share the view that graded retraining in the actual phobic situation is a "discarded (method) which has failed to establish itself". Our results showed, on the contrary, that in suitable cases—the circumscribed phobias—the method was useful.

Dr. Snaith rightly points out that few patients had desensitization in imagination with deep relaxation. We emphasized this in our paper and commented on the slight evidence that patients treated in this way did rather better. We must point out once more that our case material was not directly comparable with

Professor Wolpe's. The paper did not set out to disparage his claims, but to examine objectively results obtained mainly with retraining methods, using adequate control groups and follow-up.

We have recently used desensitization in imagination with relaxation in two prospective investigations with phobic patients. The results will be published. Our findings were, briefly, that desensitization of the phobia in imagination by reciprocal inhibition does not improve results in patients with severe agoraphobia, but does produce long-term results with less severe and extensive phobias which are significantly better than those of two forms of psychotherapy. Again the need for selection of cases is apparent.

Dr. Rachman suggests that we compared the results of behaviour therapy with "conventional psychotherapy". In fact most of our controls had conventional psychiatric management, not psychotherapy. The number receiving psychotherapy is stated in the article.

We certainly do not wish to discourage others from using the treatments which Professor Wolpe pioneered. Our interest, like Dr. Snaith's, is in identifying conditions for which this is the treatment of choice, and we think it important to stress that, in our experience, not all neurotic conditions can be expected to respond. Careful selection of patients is essential in this, as in any other treatment.

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#### REFERENCE

1. PAUL, G. L. (1964). "Effects of insight, desensitization and attention-placebo treatment of anxiety." Ph.D. Thesis, University of Illinois.

#### TRIAL OF OXYPERTINE FOR ANXIETY NEUROSIS

DEAR SIR,

In the paper by Robinson, Davies, Kreitman, and Knowles, "A Double-blind Trial of Oxypertine for Anxiety Neurosis" (*Journal*, June 1965, pp. 527-529), the ultimate comment made was, "The IPAT Anxiety Scale does not appear to be a valid technique for the assessment of anxiety states." I would like to challenge this mildly arrogant statement.

The two features of the study which the authors interpreted in reaching this conclusion were: (a) the IPAT Anxiety Scale did not correlate with the Modified Hamilton Anxiety Scale, but (b) it did correlate with the Neuroticism Factor on the EPI;

the implication being that it is not a measure of anxiety but probably is a measure of neuroticism.

A legitimate distinction can be made between anxiety as a personality "trait" and anxiety as a psychiatric "state". A person of marked anxious personality disposition need not necessarily be suffering from an anxiety state in the neurotic sense; conversely a person could suffer from an anxiety state who normally is not of an anxious disposition. Therefore any degree of correlation between a test that measures anxiety as a trait (which the IPAT Anxiety Scale mainly does) and a test that measures anxiety as a state (which the Modified Hamilton Anxiety Scale probably does) need not be expected. Of course, where there was both a marked anxiety state and a normally high anxious disposition a degree of correlation would occur; but I would suggest, since these two features are relatively independent, that few people would fit into that category.

To lend some force to this suggestion I would like to quote some slight work of my own. Using the Foulds' Symptom Sign Inventory (which measures, *inter alia*, anxiety states) I found that on a ten-point scale the mean number of symptoms was only 3.91 with a definite skew towards the lower end of the scale; on the other hand anxiety scores derived from the 16 Personality Factor Questionnaire (which is the same scale as the IPAT Anxiety Scale) were fairly normally distributed, but with a slight skew towards the higher end of the scale. On a small sample of the same patients who were seen after treatment the anxiety symptoms had almost completely disappeared, whereas the anxiety scores had not altered very much—a mean decrease of 0.7 (sten) in fact. Although Cattell and his associates claim that the anxiety scale could be used as a "temperature chart", this is not meant in the sense that a dramatic change in the anxiety level must be expected after treatment, rather as Cattell himself says, "we all experience higher and lower states with changing circumstances, but there is also evidence that some people vary about levels which are typically different for them from the central tendency in others. We then speak of 'characterological anxiety', i.e. a trait" (*The Scientific Analysis of Personality*). Thus a complete remission of anxiety symptoms might well be expected after treatment, but there would be a limited decrease in trait anxiety, probably only down to the characteristic level. If this is so, lack of correlation between the Modified Hamilton Anxiety Scale and the IPAT Anxiety Scale is not surprising.

The high correlation between the EPI Neuroticism Factor and the IPAT Anxiety Scale is not a very

valid indication that the IPAT is not measuring anxiety. Two scales correlating highly does not necessarily mean that they are measuring the same thing, e.g. a colleague of mine (J. J. Kear-Colwell) found a high correlation,  $r = 0.68$ , between hostility (as measured on the Foulds' Hostility Scale) and anxiety (as measured by the IPAT Anxiety Scale). It would be more rational to say that hostility and anxiety are related, a fact confirmed by extensive clinical observation also, than to say these are both measures of the same thing and then rely on being very partisan to decide which test is best. Incidentally, the Taylor Manifest Anxiety Scale was also accorded the same treatment because it correlated with the MPI Neuroticism Factor. Also there is some query whether there is an anxiety component in the EPI Neuroticism Factor. But without embarking upon a lengthy discussion about factorial composition it is reasonable to say that psychological entities can be correlated but separate.

In conclusion, I would like to state that I have not wittingly tried to uphold that the IPAT Anxiety Scale is a valid measure of anxiety, only that the conclusion of the authors that it did not "appear to be" is not warranted by their results, or the interpretation of their results. The question of the validity of the IPAT Anxiety Scale is a matter of very precise experimentation related to that problem alone. At the risk of appearing partisan, I would suggest that the work of Cattell and his co-workers has been too thorough to be dismissed in a cavalier fashion.

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#### WORK OF A PSYCHIATRIC EMERGENCY CLINIC

DEAR SIR,

Dr. John Brothwood in his paper on "The Work of a Psychiatric Emergency Clinic" states: "It is pertinent to ask what part might be played by a Psychiatric Emergency Clinic in an integrated service."

The question might be answered from the experience at St. Clement's Hospital, E.3, where integration was made possible by the transfer of statutory obligation for Mental Health from the London County Council to the London Borough of Tower Hamlets.

At St. Clement's Hospital the observation ward was converted into an Early Treatment Unit (Benady and Denham, 1963). Emergency referrals