to a small figure. As long as we remain in ignorance on these points, however, it is almost impossible to form any accurate judgment of the outcome of the "experiment". In my *Handbook of Abnormal Psychology* (1960) I discussed at some length desirable and necessary criteria for outcome assessments, and Lazarus (1961) has demonstrated how such procedures can be objectified in the case of phobic disorders.

I feel that it is justifiable to conclude from Marks and Gelder's review that when an outdated and experimental type of behaviour therapy is applied to phobic patients by inexperienced novices without any training in behaviour therapy, and the outcome compared with traditional methods by means of a subjective estimate of unknown reliability, it is found that at no point is behaviour therapy inferior, and in relation to phobias other than agoraphobia it is superior. We would not at any point have considered these early self-training results worthy of exhumation, and the studies examined by Marks and Gelder were certainly not designed to prove or disprove any claims on behalf of behaviour therapy; it is surprising and welcome to find that even under these conditions behaviour therapy did no worse, and in some connections rather better, than traditional methods of therapy. Certainly the result suggests that a similar study, using up-to-date methods and a highly reliable method of assessment, carried out on the performance of trained and experienced behaviour therapists, would show very much better results. One such experiment is in progress at the moment in my Psychology Department, and preliminary results seem to bear out this prognosis.

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DEAR SIR,

In their retrospective study of the effects of behaviour therapy (*Journal*, July 1965, pp. 561–573) Drs. Marks and Gelder concluded that this technique produced results which were equal to (and in certain cases, better than) those yielded by conventional psychotherapy. Their report may, however, give rise to certain mistaken impressions. I feel that they do not stress sufficiently the fact that in the majority of their cases the type of behaviour therapy administered consisted of an early, rudimentary procedure (practical re-training). Professor Wolpe, whose results are discussed in their paper, virtually discarded this method more than ten years ago in favour of ideational desensitization and other lesser techniques. A direct comparison between the Maudsley results and those of Wolpe, Lazarus and others is therefore neither feasible nor fair. As I have attempted to argue elsewhere,* the clinical and experimental results so far available are, in the main, consistent with Wolpe's findings. Furthermore, the few patients in the Marks and Gelder series who received "Wolpeian" treatment appear to have responded rather better than those treated by practical re-training.

I understand that Drs. Marks and Gelder are currently assessing the effectiveness of the Wolpeian technique, and their findings on this topic are awaited with interest.

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* RACHMAN, S. (1965). "The current status of behaviour therapy." *Arch. gen. Psychiat. (Chic.)* (in the press).

DEAR SIR,

We do not appear to disagree fundamentally with Dr. Snaith. We accept that patients with agoraphobia differ in many ways from other phobic patients and this is precisely why we divided our group in this way. We are continuing to examine these differences in further case material, but think it premature to conclude that anxiety neurosis underlies all agoraphobias.

Many advocates of behaviour therapy still maintain that *all* neuroses are collections of maladaptive learned responses and that all can be treated by deconditioning. This may be true only for certain neurotic syndromes. For this reason, like Dr. Snaith, we consider that results in different neurotic syndromes should be reported separately.

Professor Eysenck asks about the "considerable

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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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;