Brit. J. Psychiat. (1965), 111, 1007–1013

Correspondence

BEHAVIOUR THERAPY

DEAR SIR,

In their article on behaviour therapy (*Journal*, July 1965, p. 561) Marks and Gelder conclude that "in complex agoraphobias, behaviour therapy on the lines of graded practical re-training was not particularly useful". The authors make no distinction between agoraphobia and the "simple" phobias so far as underlying psychopathology is concerned; they state that together they form a "fairly clearly defined syndrome". Later in their paper they cast doubt on whether phobias, especially agoraphobia, are simple learned habits, but they do not consider the possibility that "agoraphobia" and simple phobias may be fundamentally different conditions.

The term agoraphobia (meaning literally fear of the market-place) was coined by Westphal in 1872 and popularized by Freud. At first Freud clearly recognized that the symptom occurred in the setting of anxiety neurosis (1), but subsequently, especially following the analysis of "Little Hans" (2), this fact was largely lost to sight and "agoraphobia" came to be regarded more as a defined psychiatric syndrome like the "simple" phobias. Perhaps, had Benedikt's term, Platzschwindel (dizziness in public places) (3), been preferred to "agoraphobia" this confusion might never have arisen.

The fact is that the term agoraphobia is a thoroughly bad one; not only does it lead to difficulties in distinction from true phobias, but it does little to describe the widespread fears of all situations of "insecurity" occurring in the setting of generalized free-floating anxiety. The intensity of the "agoraphobia" usually fluctuates with that of the underlying anxiety neurosis, and when a remission of the latter occurs the agoraphobia usually disappears as well.

It follows, then, that the only rational way to treat this symptom is to treat the underlying anxiety state itself, and that to even expect cure of "agoraphobia" by an approach directed only to the symptom is as illogical as to expect aspirin to cure appendicitis.

So far as the results collected by the authors in "other phobias" are concerned, the details recorded concerning treatment are far too meagre to enable the reader to concur with their pessimistic outlook as to the effectiveness of behaviour therapy. Apparently in this group Wolpe's recommended technique of desensitization by reciprocal inhibition of the phobia with deep mental and physical relaxation was not used at all. At any rate the authors only refer to "graded exposure to the feared situation", with no mention at all of measures taken to inhibit the anxiety. It is surely unfair to collect and publish results disparaging Wolpe's claims if his methods are not used.

The founders of behaviour therapy have almost certainly made overreaching claims in asserting that all neurotic symptoms are based on maladaptive learned responses. Drs. Marks and Gelder would seem now to be exposing the weaknesses of this claim. But it would be a pity if their paper discouraged other workers from experimenting with this mode of therapy in order to establish for what neurotic manifestations, properly applied, it may be the treatment of choice. My own experience leads me to believe that in the true phobias behaviour therapy can give results very much better than the authors imply.

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3. BENEDIKT, M. (1870). "Über Platzschwindel." *Allg. Wien. med. Ztg.*, 15, 488.

DEAR SIR,

The recent report by Marks and Gelder (*Journal*, July 1965, p. 561) of their controlled retrospective study of behaviour therapy raises several interesting points regarding methodology and conclusions. It also raises certain questions concerning evaluation and comment upon past work. My discussion will be short, as more extensive comments on many of these points are available elsewhere (Eysenck, 1964; Eysenck and Rachman, 1965).

(1) Marks and Gelder complain that "few published reports justify the claims which have been made for behaviour therapy. Most are of single cases, or of a few cases without controls." Unfortunately they do not quote the "considerable claims" which they say have been made for behaviour therapy; it is

therefore impossible to know what they have in mind. One wonders whether they would or would not agree with the summary statement given by Rachman and myself (1965) in our recent textbook: "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." Whatever their answer, however, they are certainly wrong in giving the impression that single case studies and uncontrolled treatments of large series of cases are the only available evidence. Several controlled experiments have been quoted in my *Experiments in Behaviour Therapy* (1964), and even in the specific area of phobic patients and their treatment there is the excellent and well-controlled experiment of Lazarus (1961), to say nothing of the exceptionally important work of Lang and Lazovik (1963). It is on these and similar studies that claims for behaviour therapy are based, not on "single case reports". Readers of Marks and Gelder's report might easily get the wrong impression.

(2) As the authors point out, "all patients received graded retraining in meeting their feared situations, and only a few also received desensitization in imagination while in a state of relaxation-hypnosis". It is this latter method, of course, which is characteristic of Wolpe's work; the former harks back more to Herzberg (1941). No reasonable comparison is therefore possible with Wolpe's published figures for recovery, or those of Lazarus and others (Eysenck and Rachman, 1965). The retraining method was used purely on an experimental basis, and has now been given up because of its poor showing in favour of Wolpe-type desensitization. The review does not deal with an actively practised method, therefore, but with a discarded one which has failed to establish itself. There are still interesting theoretical questions regarding the reasons for the superiority of one method over the other (if such superiority is, in fact, capable of being established experimentally), but from the point of view of assessing modern behaviour therapy and its effectiveness the Marks and Gelder study is certainly irrelevant.

(3) It is important, in assessing the efficacy of any form of therapy, to know something of the experience and training of those who carried out the treatment. Marks and Gelder discuss this point in relation to the comparative lack of success of behaviour therapy with agoraphobic patients: "Lack of skill on the part of therapists is an alternative explanation, but results showed that experienced therapists obtained no better results. It might be argued that lecturers and senior lecturers in a University Department of Clinical Psychology were unskilled, but if this is so, who then is to be regarded as skilled?" This argument is not only factually wrong (the Department in question is one of Psychology; there is no University Department of Clinical Psychology in the University of London); it is also somewhat disingenuous. Skill in behaviour therapy is in part a function of knowledge of modern learning theory, and in so far as this is concerned the senior members of a Psychology Department may certainly be presumed to possess this knowledge. It also involves training in behaviour therapy and experience in the exercise of the skills acquired during this training; both of these were lacking in the persons concerned at the time that the treatment reviewed by Marks and Gelder was undertaken. A psychiatrist is not considered to be a trained psycho-analyst by virtue of his medical training; he requires special training and experience before his therapeutic work can be regarded as relevant to any claims which might be made on behalf of psychoanalysis. Precisely the same is true of behaviour therapy, and the answer to Marks and Gelder's somewhat rhetorical question must be: Someone with adequate grounding in learning theory who has undergone a special training process under an acknowledged behaviour therapist.

(4) Marks and Gelder do not at any time give an estimate of the *reliability* of their assessments of final outcome. This is a very important point which has not received the attention it deserves. Let us assume that behaviour therapy is 100 per cent. effective, and psychotherapy 0 per cent. effective; let us also assume that the method of assessment has zero reliability. Under these conditions an experiment such as that described by Marks and Gelder would fail to show any superiority in outcome for behaviour therapy. It is not suggested that their method of assessment had no reliability at all, but it is well known that psychiatric assessments of this type are not very reliable (Eysenck, 1960), and any considerable reduction in reliability would automatically reduce any difference in outcome between procedures to a much lower figure than the true one. Retrospective studies of the type considered cannot, of course, overcome this difficulty; it is for this reason, among others, that in our view retrospective studies throw much less light on the subject they are meant to illuminate than they are designed to do. There are, in fact, two sources of unreliability involved in such studies, one of which only is subject to assessment. We have in the first place the unreliability of the original report; it is difficult to see how this could be ascertained or overcome, unless it be by a separate follow-up procedure. We have, in the second place, the unreliability of the *assessment* of the original report; this could, of course, be studied and reduced

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 pıeliminary 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