LEARNING THEORY AND BEHAVIOUR THERAPY*

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IT would probably be true to say that the present position in the psychiatric treatment of neurotic disorders is characterized by the following features. (1) With the exception of electroshock, the only method of treatment at all widely used is psychotherapy. (2) In practically all its manifestations, psychotherapy is based on Freudian theories. (3) With the exception of intelligence testing, psychological contributions consist almost entirely in the administration and interpretation of projective tests, usually along psycho-analytic lines. I have argued in the past, and quoted numerous experiments in support of these arguments, that (1) there is little evidence for the practical efficacy of psychotherapy,† whether strictly Freudian or "eclectic" (8, 17); (2) that Freudian theories are outside the realm of science because of their failure to be consistent, or to generate testable deductions (10); and (3), that projective tests are so unreliable and lacking in validity that their use, except in research, cannot be defended (16).‡ I shall not here argue these points again; the evidence on which these views are based is quite strong, and is growing in strength every year. I shall instead try to make a somewhat more constructive contribution by discussing an alternative theory of neurosis, an alternative method of treatment, and an alternative way of using the knowledge and competence of psychologists in the attempted cure of neurotic disorders. It need hardly be emphasized that the brief time at my disposal will make it inevitable that what I have to say will sound much more dogmatic than I would like it to be; I have to ask your indulgence in this respect, and request you to bear in mind all the obvious qualifying clauses which, if included in this paper, would swell it to three times its present size.

Few psychiatrists are likely to deny that all behaviour ultimately rests on an inherited basis, but even fewer would be prepared to assert that environmental influences played no part in the genesis and modification of behaviour. Once we

* This paper was delivered on 3 July, 1958 to a meeting of the R.M.P.A., and its style inevitably bears traces of the fact that it was originally prepared for verbal presentation. It was followed by another paper, delivered by Mr. Gwynne Jones, giving concrete examples of the application of behaviour therapy from our own experience. Some of these are discussed in his article published in this Journal (29), and it is suggested that readers interested in the theories here advanced may like to consult this article in order to obtain some notion of the practical methods emanating from these theories. A more detailed discussion of many theoretical points that arise may be found in "Dynamics of Anxiety and Hysteria" (15), as well as several of my previous books (7, 9, 11).

† When I first suggested that the literature did not contain any kind of unequivocal proof of the efficacy of psychotherapeutic treatment, this conclusion was widely criticized. Since then, however, Dr. Weinstock, Chairman of the Fact-Finding Committee of the American Psychoanalytical Association, has explicitly stated in a lecture delivered at the Maudsley Hospital that his Association made *no claims of therapeutic usefulness for psychoanalytic methods*, and in this country Glover (21) has equally explicitly disavowed such claims. On this point, therefore, leading psychoanalysts appear to share my views to a considerable extent.

‡ This fact is also beginning to be more widely realized, and it is symptomatic that such well-known departments as that belonging to the New York Psychiatric Hospital have followed the lead of the Institute of Psychiatry and discontinued the routine use of projective techniques like the Rorschach.

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are agreed that learning and conditioning are instrumental in determining the different kinds of reaction we may make to environmental stimulation, we will find it very difficult to deny that neurotic reactions, like all others, are *learned* reactions, and must obey the laws of learning. Thus, I would like to make my first claim by saying that modern learning theory (24), and the experimental studies of learning and conditioning carried out by psychologists in their laboratories (38) are extremely relevant to the problems raised by neurotic disorders (41). If the laws which have been formulated are, not necessarily true, but at least partially correct, then it must follow that we can make deductions from them to cover the type of behaviour represented by neurotic patients, construct a model which will duplicate the important and relevant features of the patient, and suggest new and possibly helpful methods of treatment along lines laid down by learning theory. Whether these methods are in fact an improvement over existing methods is, of course, an empirical problem; a few facts are available in this connection and will be mentioned later. It is unfortunate that insistence on empirical proof has not always accompanied the production of theories in the psychiatric field—much needless work, and many heart-breaking failures, could have been avoided if the simple medical practice of clinical trials with proper controls had always been followed in the consideration of such claims.

How, then, does modern learning theory look upon neurosis? In the first place, it would claim that neurotic symptoms are *learned patterns of behaviour* which for some reason or other are *unadaptive*. The paradigm of neurotic symptom formation would be Watson's famous experiment with little Albert, a nine months old boy who was fond of white rats (44). By a simple process of classical Pavlovian conditioning Watson created a phobia for white rats in this boy by standing behind him and making a very loud noise by banging an iron bar with a hammer whenever Albert reached for the animal. The animal was the conditioned stimulus in the experiment, the loud fear-producing noise was the unconditioned stimulus. As predicted, the unconditioned response (fear) became conditioned to the C.S. (the rat), and Albert developed a phobia for rats, and indeed for all furry animals. This latter feature of the conditioning process is of course familiar to all students as the generalization gradient (38); an animal or a person conditioned to one stimulus also responds, although less and less strongly, to other stimuli further and further removed from the original one along some continuum.

The fear of the rat thus conditioned is unadaptive (because white rats are not in fact dangerous) and hence is considered to be a neurotic symptom; a similarly conditioned fear of snakes would be regarded as adaptive, and hence not as neurotic. Yet the mechanism of acquisition is identical in both cases. This suggests that chance and environmental hazards are likely to play an important part in the acquisition of neurotic responses. If a rat happens to be present when the child hears a loud noise, a phobia results; when it is a snake that is present, a useful habit is built up!

The second claim which modern learning theory would make is this. People and animals differ in the speed and firmness with which conditioned responses are built up (39). Those in whom they are built up particularly quickly and strongly are more likely to develop phobias and other anxiety and fear reactions than are people who are relatively difficult to condition (15). Watson was lucky in his choice of subject; others have banged away with hammers on metal bars in an attempt to condition infants, but not always with the same success.

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Individual differences must be taken into account in considering the consequences of any course of attempted conditioning. Nor is the degree of conditionability the only kind of individual variability with which we are concerned. Learning theory tells us that the amount of reinforcement following any action determines in part the amount of conditioning that takes place (43). Thus the louder the noise, the greater the fright of the infant, and the greater the fright, the stronger the phobia. But different children have different types of autonomic system, and the same amount of noise produces quite unequal amounts of autonomic upheaval in different children. Consequently, autonomic reactivity must also be considered; the more labile or reactive the child, the more likely he is to produce strongly conditioned fear reactions, anxieties, and phobias. The individual differences in autonomic reactivity and in conditionability have been conceptualized as giving rise to two dimensions of personality, namely neuroticism and introversion respectively (11). The more autonomically reactive, the more prone will the individual be to neurotic disorders. The more easily he forms conditioned responses, the more introverted will his behaviour be. Combine introversion and neuroticism, and you get the dysthymic individual, the person almost predestined to suffer from anxieties, conditioned fears and phobias, compulsions and obsessions, reactive depressions and so forth.

But this is only part of the story. Many conditioned responses are unadaptive, and consequently may embarrass the individual and even drive him into a mental hospital if sufficiently intense. Yet other conditioned responses are obviously necessary and desirable; indeed, many of them are indispensable for survival. It has been argued very strongly that the whole process of socialization is built up on the principle of conditioning (35); the overt display of aggressive and sexual tendencies is severely punished in the child, thus producing conditioned fear and pain responses (anxiety) to situations in which the individual is likely to display such tendencies. He consequently refrains from acting in the forbidden manner, not because of some conscious calculus of hedonic pleasure which attempts to equate the immediate pleasure to be gained from indulgence with the remote probability of later punishment, but because only by not indulging, and by physically removing himself can he relieve the very painful conditioned anxiety responses to the whole situation. Anxiety thus acts as a mediating drive, a drive which may be exceedingly powerful by virtue of its combination of central, autonomic, skeletal, and hormonal reactions. This mediating role of anxiety, and its capacity to function as an acquired drive, have been subjected to many well conceived experimental studies, and the consensus of opinion appears to leave little doubt about the great value and predictive capacity of this conception (34).

Let us now consider an individual who is deficient in his capacity to form quick and strong conditioned responses. He will be all the less likely to be subject to phobias and other anxieties, but he will also be less likely to form useful conditioned responses, or to become a thoroughly socialized individual. When this lack of socialization is combined with strong autonomic drive reactions (high neuroticism), such an individual is likely to show the neurotic symptomatology of the psychopath or the hysteric, and indeed, in our experimental work we have found that, as predicted, dysthymic patients and normal introverts are characterized by the quick and strong formation of conditioned responses, while psychopaths and normal extraverts are characterized by the weak and slow formation of conditioned responses (12, 14, 15). Thus the deviation from the average in either direction may prove disastrous—too strong

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conditioning easily leads to dysthymic reactions, too weak conditioning easily leads to psychopathic and hysterical reactions. The logic of this whole approach leads me to postulate two great classes of neurotic symptoms which between them exhaust in principle all the possible abnormal reactions with which you are all familiar. On the one hand we have *surplus conditioned reactions*, i.e. reactions acquired along the lines I have adumbrated, and where the reaction is unadaptive, even though originally it may have been well suited to circumstances. On the other hand we have *deficient conditioned reactions*, i.e. reactions normally acquired by most individuals in society, which are adaptive, but which because of defective conditioning powers have not been acquired by a particular person. It is necessary to emphasize that surplus conditioned reactions and deficient conditioned reactions are due to an interplay between such individual factors as conditionability and autonomic lability, on the one hand, and environmental conditions on the other. There will be no socialization for an individual who cannot form conditioned responses at all, but conversely, there will be no socialization for a person growing up on a desert island, however powerful his conditioning mechanism may happen to be. In this paper I have no time to deal with differences in the conditioning forces of the environment, and their relation to such factors as social class, but they should certainly not be forgotten.

Many other testable deductions, apart from the differential conditionability of dysthymics and hysterics, follow from such a formulation. Some of these deductions can be tested in the laboratory, and examples have been given in my book, *The Dynamics of Anxiety and Hysteria*. But others can be tested clinically, and for the sake of an example I shall give just one of these. I have shown how psychopathic reactions originate because of the inability of the psychopath, due to his low level of conditionability, to acquire the proper socialized responses. But this failure is not absolute; he conditions much less quickly and strongly than others, but he does condition. Thus where the normal person may need 50 pairings of the conditioned and the unconditioned stimulus, and where the dysthymic may need 10, the psychopath may require 100. But presumably in due course the 100 pairings will be forthcoming, although probably much later in life than the 10 of the dysthymic, or the 50 of the normal person, and then he will finally achieve a reasonable level of socialization. If this chain of reasoning is correct, it would lead us to expect that the diagnosis "psychopath" would by and large be confined to relatively young people, say under thirty years of age; after thirty the course of life should have brought forth the required 100 pairings and thus produced the needed amount of socialization. As far as I can ascertain, clinical psychiatric opinion is in agreement with this prediction.

How does our theory compare with the psychoanalytic one? In the formation of neurotic symptoms, Freud emphasizes the traumatic nature of the events leading up to the neurosis, as well as their roots in early childhood. Learning theory can accommodate with equal ease traumatic single-trial learning, for which there is good experimental evidence (26), but it can also deal with repeated sub-traumatic pain and fear responses which build up the conditioned reaction rather more gradually (42). As regards the importance of childhood, the Freudian stress appears to be rather misplaced in allocating the origins of *all* neuroses to this period. It is possible that many neurotic symptoms find their origin in this period, but there is no reason at all to assume that neurotic symptoms cannot equally easily be generated at a later period provided conditions are arranged so as to favour their emergence.

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The point, however, on which the theory here advocated breaks decisively with psychoanalytic thought of any description is in this. Freudian theory regards neurotic symptoms as adaptive mechanisms which are evidence of repression; they are "the visible upshot of unconscious causes" (37). Learning theory does not postulate any such "unconscious causes", but regards neurotic symptoms as simple learned habits; there is no neurosis underlying the symptom, but merely the symptom itself. *Get rid of the symptom and you have eliminated the neurosis.* This notion of purely symptomatic treatment is so alien to psychoanalysis that it may be considered the crucial part of the theory here proposed. I would like to explore its implications a little further later on.

From the point of view of learning theory, treatment is in essence a very simple process. In the case of surplus conditioned responses, treatment should consist in the extinction of these responses; in the case of deficient conditioned responses, treatment should consist in the building up of the missing stimulus-response connections. Yet this apparent simplicity should not mislead us into thinking that the treatment of neurotic disorders offers no further problems. It is often found in scientific research that the solution of the problems posed by applied science is as complex and difficult as is the solution of the problems posed by pure science; even after Faraday and Maxwell had successfully laid the foundations of modern theories of electricity it needed fifty years and the genius of Edison to make possible the actual application of these advances to the solution of practical problems. Similarly here; a solution in principle, even if correct, still needs much concentrated and high-powered research in the field of application before it can be used practically in the fields of cure, amelioration, and prophylaxis.

What are the methods of cure suggested by learning theory? I shall give two brief examples only, to illustrate certain principles; others have been given by G. Jones (29). One method of extinguishing the neurotic response X to a given stimulus S is to condition another response R to S, provided that R and X are mutually incompatible. This method, called "reciprocal inhibition" by Wolpe (45, 46), harks back to Sherrington (40) of course, and may be illustrated by returning to our rat phobic little boy. Essentially what Watson had done was to condition a strong sympathetic reaction to the sight of the rat. If we could now succeed in establishing a strong parasympathetic reaction to the sight of the animal, this might succeed in overcoming and eliminating the sympathetic response. The practical difficulty arises that, to begin with at least, the already established conditioned response is of necessity stronger than the to-be-conditioned parasympathetic response. To overcome this difficulty, we make use of the concept of stimulus gradient already mentioned. The rat close by produces a strong conditioned fear reaction; the rat way out in the distance produces a much weaker reaction. If we now feed the infant chocolate while the rat is being introduced in the far distance the strong parasympathetic response produced by the chocolate-munching extinguishes the weak sympathetic response produced by the rat. As the conditioned parasympathetic response grows in strength, so we can bring the rat nearer and nearer, until finally even close proximity does not produce sympathetic reactions. The sympathetic reaction has been extinguished; the phobia has been cured. This is in fact the method which was used experimentally to get rid of the experimentally induced fear (27), and it has been used successfully by several workers in the field of child psychiatry. More recently Herzberg (23) in his system of active psychotherapy, and more particularly, Wolpe (46) in his

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psychotherapy by reciprocal inhibition, have shown that these principles can be applied with equal success to the severe neuroses of adult men and women—substituting other methods, of course, for the chocolate-munching, which is more effective with children than with adults!

As an example of the cure of deficient conditioned responses, let me merely mention *enuresis nocturna*, where clearly the usual conditioned response of waking to the conditioned stimulus of bladder extension has not been properly built up. A simple course of training, in which a bell rings loudly whenever the child begins to urinate, thus activating an electric circuit embedded in his bedclothes, soon establishes the previously missing connection, and the extremely impressive list of successes achieved with this method, as compared with the very modest success of psychotherapeutic methods, speaks strongly for the correctness of the theoretical point of view which gave rise to this conception (36).

We thus have here, I would suggest, an alternative theory to the Freudian, a theory which claims to account for the facts at least as satisfactorily as does psychoanalysis, and which in addition puts forward quite specific suggestions about methods of treatment. I have called these methods "behaviour therapy" to contrast them with methods of psychotherapy.* This contrast of terms is meant to indicate two things. According to psychoanalytic doctrine, there is a psychological complex, situated in the unconscious mind, underlying all the manifest symptoms of neurotic disorder. Hence the necessity of therapy for the psyche. According to learning theory, we are dealing with unadaptive behaviour conditioned to certain classes of stimuli; no reference is made to any underlying disorders or complexes in the psyche. Following on this analysis, it is not surprising that psychoanalysts show a preoccupation with psychological methods involving mainly *speech*, while behaviour therapy concentrates on actual *behaviour* as most likely to lead to the extinction of the unadaptive conditioned responses. The two terms express rather concisely the opposing viewpoints of the two schools. Table I presents, in summary form, a tabulation of the most important differences between Freudian psychotherapy and behaviour therapy.

What kind of answer would we expect from the Freudians? I think their main points would be these. They would claim, in the first place, that conditioning therapy has frequently been tried, but with very poor results; aversion therapies of alcoholism are often mentioned in this connection. They would go on to say that even where symptomatic treatments of this kind are apparently successful, as in enuresis, the symptom is likely to return, or be supplanted by some other symptom, or by an increase in anxiety. And, in the third place, they

* The growth of the theoretical concepts and practical methods of treatment subsumed in the term "behaviour therapy" owes much to a large number of people. Apart from Pavlov and Hull, who originated the main tenets of modern learning theory, most credit is probably due to Watson, who was among the first to see the usefulness of the conditioned paradigm for the explanation of neurotic disorders; to Miller and Mowrer, who have done so much to bring together learning theory and abnormal human behaviour; to Spence, whose important contributions include the detailed analysis of the relation between anxiety and learning; and to Wolpe, who was the first to apply explicitly some of the laws of learning theory to the large scale treatment of severe neurotics. If there is any novelty in my own treatment of these issues it lies primarily: (1) in the pulling together of numerous original contributions into a general theory and (2) in the introduction into this system of the concepts of neuroticism and extraversion/introversion as essential parameters in the description and prediction of behaviour. I would like to emphasize, however, that this contribution could not have been made had the ground work not been well and truly laid by the writers quoted above and by many more, only some of whom are quoted in the bibliography.

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TABLE I

Freudian Psychotherapy	*Behaviour Therapy*
1. Based on inconsistent theory never properly formulated in postulate form.	Based on consistent, properly formulated theory leading to testable deductions.
2. Derived from clinical observations made without necessary control observation or experiments.	Derived from experimental studies specifically designed to test basic theory and deductions made therefrom.
3. Considers symptoms the visible upshot of unconscious causes ("complexes").	Considers symptoms as unadaptive conditioned responses.
4. Regards symptoms as evidence of *repression*.	Regards symptoms as evidence of faulty learning.
5. Believes that symptomatology is determined by defence mechanism.	Believes that symptomatology is determined by individual differences in conditionability and autonomic lability, as well as accidental environmental circumstances.
6. All treatment of neurotic disorders must be *historically* based.	All treatment of neurotic disorders is concerned with habits existing at *present*; their historical development is largely irrelevant.
7. Cures are achieved by handling the underlying (unconscious) dynamics, not by treating the symptom itself.	Cures are achieved by treating the symptom itself, i.e. by extinguishing unadaptive C.R.s and establishing desirable C.R.s.
8. Interpretation of symptoms, dreams, acts, etc. is an important element of treatment.	Interpretation, even if not completely subjective and erroneous, is irrelevant.
9. Symptomatic treatment leads to the elaboration of new symptoms.	Symptomatic treatment leads to permanent recovery provided autonomic as well as skeletal surplus C.R.s are extinguished.
10. Transference relations are essential for cures of neurotic disorders.	Personal relations are not essential for cures of neurotic disorder, although they may be useful in certain circumstances.

would claim that even if in some cases the therapies suggested might be successful, yet in the great majority of cases psychoanalysis would be the only method to produce lasting cures. Let me deal with these points one by one.

There is no doubt that conditioning treatment of alcoholism has often been tried, and that it has often failed. I have no wish to take refuge in a *tu quoque* argument by pointing out that alcoholism has been particularly difficult to treat by any method whatever, and that psychoanalytic methods also have been largely unsuccessful. I would rather point out that learning theory is an exact science, which has elaborated quite definite rules about the establishment of conditioned reflexes; it is only when these rules are properly applied by psychologists with knowledge and experience in this field that the question of success or failure arises. Thus it is quite elementary knowledge that the conditioned stimulus must precede the unconditioned stimulus if conditioning is to take place; backward conditioning, if it occurs at all, is at best very weak. Yet some workers in the field of alcoholism have used a method in which the unconditioned stimulus regularly preceded the conditioned stimulus; under these conditions learning theory would in fact predict the complete failure of the experiment actually reported! Again, the time relation between the applica-

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tion of the conditioned stimulus and the unconditioned simulus is a very important one; it is controlled to very fine limits of hundredths of a second in psychological experimentation, and it has been universally reported that conditioning in which any but the optimal time relation is chosen is relatively ineffective. Taking eye-blink conditioning as an example, it is found that a time interval of about $\frac{1}{2}$ second is optimal, and that with intervals of $2\frac{1}{2}$ seconds no conditioning at all takes place (31, 32). No attention seems to have been paid to these points by most workers on alcoholism, who apply the conditioned and unconditioned stimuli in such a vague way that it is often impossible to find out what the actual time relations were. This lack of rigour makes it quite impossible to adduce these so-called experiments as evidence either in favour or against conditioning therapy (19).

How about the return of symptoms? I have made a thorough search of the literature dealing with behaviour therapy with this particular point in view. Many psycho-analytically trained therapists using these methods have been specially on the lookout for the return of symptoms, or the emergence of alternative ones; yet neither they nor any of the other practitioners have found anything of this kind to happen except in the most rare and unusual cases (35). Enuresis, once cured by conditioning therapy, remains cured as a general rule; relapses occur, as indeed one would expect in terms of learning theory under certain circumstances, but they quickly yield to repeat treatment. So certain of success are the commercial operators of this method that they work on a "money back if unsuccessful" policy; their financial solvency is an adequate answer to the psychoanalytic claim. Nor would it be true that alternative symptoms emerge; quite the contrary happens. The disappearance of the very annoying symptom promotes peace in the home, allays anxieties, and leads to an all-round improvement in character and behaviour. Similar results are reported in the case of major applications of behaviour therapy to adults suffering from severe neurotic disorders; abolition of the symptom does not leave behind some mysterious complex seeking outlet in alternative symptoms (35). Once the symptom is removed, the patient is cured; when there are multiple symptoms, as there usually are, removal of one symptom facilitates removal of the others, and removal of all the symptoms complete the cure (46).

There is one apparent exception to this rule which should be carefully noted because it may be responsible for some of the beliefs so widely held. Surplus conditioned reactions may themselves be divided into two kinds, autonomic and motor. Anxiety reactions are typical of the autonomic type of surplus conditioned reactions, whereas tics, compulsive movements, etc., are typical of motor conditioned reactions. What has been said about the complete disappearance of the symptom producing a complete disappearance of the neurosis is true only as far as the autonomic conditioned reactions are concerned. Motor reactions are frequently activated by their drive-reducing properties *vis-à-vis* the historically earlier conditioned autonomic responses (35); the extinction of the motor response without the simultaneous extinction of the conditioned autonomic response would only be a very partial cure and could not be recommended as being sufficient. As pointed out at the end of the previous paragraph, "removal of *all* the symptoms completes the cure", and clearly removal of the motor conditioned response by itself, without the removal of the autonomic conditioned response is only a very partial kind of treatment. Behaviour therapy requires the extinction of all non-adaptive conditioned responses complained of by the patient, or causally related to these symptoms.

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But how frequently does this type of treatment result in cures? Again I have made a thorough search of the literature, with the following outcome. G. P. treatment, not making use of psychotherapy in any of its usual forms, results in a recovery of about two seriously ill neurotics out of three (4). Eclectic psychotherapy results in a recovery of about two seriously ill neurotics out of three (8). Psychotherapy by means of psychoanalysis fares slightly worse, but results are at a comparable level (17). Results of behaviour therapy of seriously ill neurotics, as reported by Wolpe, are distinctly superior to this, over 90 per cent. recovering (46). This difference is highly significant statistically, and it should be borne in mind that the number of sessions required by behaviour therapy is distinctly smaller than that required by psychotherapy, whether eclectic or psychoanalytic. (Wolpe reports an average of about 30 sittings for his cases.)

These results are encouraging, but of course, they must not be taken too seriously. Actuarial comparisons of this kind suffer severely from the difficulty of equating the seriousness of disorders treated by different practitioners, the equally obvious difficulty of arriving at an agreed scale for the measurement of "recovery", and the impossibility of excluding the myriad chance factors which may effect gross behaviour changes of the kind we are here considering. I would not like to be understood as saying that behaviour therapy has been *proved* superior to psychotherapy; nothing could be further from my intention. What I am claiming is simply that as far as they go—which is not very far—available data do not support in any sense the Freudian belief that behaviour therapy is doomed to failure, and that only psychoanalysis or some kindred type of treatment is adequate to relieve neurotic disorders. This Freudian belief is precisely this—a belief; it has no empirical or rational foundation. I have no wish to set up a counter-belief, equally unsupported, to the effect that psychotherapy is doomed to failure, and that only behaviour therapy is adequate to relieve neurotic disorders. What I would like to suggest is simply that a good case can be made out, both on the theoretical and the empirical level, for the proposition that behaviour therapy is an effective, relatively quick, and probably lasting method of cure of some neurotic disorders. This case is so strong that clinical trials would appear to be in order now to establish the relative value of this method as compared with other available methods, such as psychoanalysis, or electroshock treatment. Even more important, I think the evidence would justify psychiatrists in experimenting with the method, or rather set of methods, involved, in order to come to some preliminary estimate of their efficiency. I have noted with some surprise that many psychotherapists have refused to use such methods as conditioning therapy in enuresis, not on empirical grounds, but on *a priori* grounds, claiming that such mechanical methods simply could not work, and disregarding the large body of evidence available. Even in long-established sciences *a priori* considerations carry little weight; in such a young discipline as psychology they are quite out of place. Only actual use can show the value of one method of treatment as opposed to another.

There is one point I would like to emphasize. Freud developed his psychological theories on the basis of his study of neurotic disorders, and their treatment. Behaviour therapy, on the contrary, began with the thorough experimental study of the laws of learning and conditioning in normal people, and in animals; these well-established principles were then applied to neurotic disorders. It seems to me that this latter method is in principle superior to the former;

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scientific advance has nearly always taken the form of making fundamental discoveries and then applying these in practice, and I can see no valid reason why this process should be inverted in connection with neurosis. It may be objected that learning theorists are not always in agreement with each other (24), and that it is difficult to apply principles about which there is still so much argument. This is only very partially true; those points about which argument rages are usually of academic interest rather than of practical importance. Thus reinforcement theorists and contiguity theorists have strong differences of view about the necessity of reinforcement during learning, and different reinforcement theorists have different theories about the nature of reinforcement. Yet there would be general agreement in any particular case about the optimum methods of achieving a quick rate of conditioning, or extinction; these are questions of fact, and it is only with the interpretation of some of these facts that disagreements arise. Even when the disputes about the corpuscular or wavular nature of light were at their height, there was sufficient common ground between contestants regarding the facts of the case to make possible the practical application of available knowledge; the same is true of learning theory. The 10 per cent. which is in dispute should not blind us to the 90 per cent. which is not—disagreements and disputes naturally attract more attention, but agreements on facts and principles are actually much more common. Greater familiarity with the large and rapidly growing literature will quickly substantiate this statement (38).

It is sometimes said that the model offered here differs from the psychoanalytic model only in the terminology used, and that in fact the two models are very similar. Such a statement would be both true and untrue. There undoubtedly are certain similarities, as Mowrer (35) and Miller and Dollard (5) have been at pains to point out. The motivating role of anxiety in the Freudian system is obviously very similar in conception to the drive-producing conditioned autonomic responses of learning theory, and the relief from anxiety produced by hysterical and obsessional symptoms in Freudian terminology is very similar to the conditioned drive-reducing properties of motor movements. Similarly, a case could be made out in favour of regarding the undersocialized, non-conditionable psychopathic individual as being Id-dominated, and the dysthymic, over-conditionable individual as being Super-Ego dominated. Many other similiarities will occur to the reader in going through these pages, and indeed the writer would be the first to acknowledge the tremendous service that Freud has done in elucidating for the first time some of these dynamic relationships, and in particular in stressing the motivating role of anxiety.

Nevertheless, there are two main reasons for not regarding the present formulation as simply an alternative differing from the psychoanalytic one only in the terminology used. In the first place, the formulation here given differs from the Freudian in several essential features, as can be seen most clearly by studying Table I. Perhaps these differences are most apparent with respect to the deductions made from the two theories as to treatment. Psychoanalytic theory distrusts purely symptomatic treatment and insists on the removal of the underlying complexes. Behaviour theory on the other hand stresses the purely symptomatological side of treatment and is unconvinced of the very existence of "complexes". It might, of course, be suggested that there is some similarity between the Freudian "complex" and the "conditioned surplus autonomic reaction" posited by behaviour theory. That there is some similarity

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cannot be denied, but no one familiar with psychoanalytic writings would agree that the Freudian complex was not in essence a very different conception from the conditioned autonomic response, both from the point of view of its origins, as well as from the point of view of the appropriate method of extinction.

This brings me to the second great difference between the two models. What the Freudian model lacks above all is an intelligible objectively testable *modus operandi* which can be experimentally studied in the laboratory, which can be precisely quantified, and which can then be subjected to the formulation of strict scientific laws. The stress on such a mechanism, namely that of conditioning, is the most noteworthy feature of the model here advocated. It is entirely due to the great body of research which has been done in connection with the elaboration of laws of modern learning theory that we are enabled to make fairly precise deductions resulting in different methods of treatment for patients suffering from neurotic disorders, and it is with respect to this feature of the model that the relevant case histories and accounts of treatment should be read (28, 33, 47).

It has sometimes been suggested that the criticisms which I have levelled against the psychotherapeutic schools because of their failure to provide adequate control groups to validate their claims regarding the curative properties of their methods, could justifiably be levelled against the accounts given by those who have used behaviour therapy and reported upon the effects achieved. Such a criticism would not be justified for two reasons. In the first place the cases quoted are *illustrative of methods*, not *proofs of psychotherapeutic efficacy*; the only case in which claims regarding relative efficacy have been made contains a statistical comparison with the effects of psychoanalytic treatment of similar cases (46). In the second place the concept of "control" in scientific experiments is somewhat more than simply the provision of a control *group*; the control in an experiment may be *internal*. As an example, consider the experiment reported by Yates (47) on the extinction of four tics in a female patient by means of a rather novel and unusual method, namely that of repeated voluntary repetition of the tic by massed practice. Precise predictions were made as to the effects that should follow, and these predictions were studied by using the fate of some of the tics as compared to the fate of other tics submitted to dissimilar treatment. Thus, practice for two tics might be discontinued for a fortnight, while practice on the other two would go on. By showing that the predictions made could thus be verified, and the *rate of extinction* of the tics varied at will in accordance with the experimental manipulation for such variables as massing of practice, a degree of control was achieved far superior to the simple assessment of significance produced in the comparison of two random groups submitted to different treatments. It is by its insistence on such experimental precision and the incorporation of experimental tests of the hypotheses employed, even during the treatment, that behaviour theory differs from psychotherapy.

There is one further method of pointing up the differences between the two theories and of deciding between them; I mention this matter with some hesitation because to many psychiatrists it seems almost sacrilegious to use animal experimentation in the consideration of human neurosis. However, Fenichel himself (18, p. 19) has quoted "experimental neuroses" as support for the Freudian conception of neurotic disorders, and it is with respect to these experiments that the contrast between the psychoanalytic and our own model may be worked out most explicitly. Fenichel maintains that the model of

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psychoneurosis "is represented by the artificial neuroses that have been inflicted upon animals by experimental psychologists. Some stimulus which had represented pleasant instinctual experiences or which had served as a signal that some action would now procure gratification is suddenly connected by the experimenter with frustrating or threatening experiences, or the experimenter decreases the difference between stimuli which the animal had been trained to associate with instinct gratification and threat respectively; the animal then gets into a state of irritation which is very similar to that of a traumatic neurosis. He feels contradictory impulses; the conflict makes it impossible for him to give in to the impulses in the accustomed way; the discharge is blocked, and this decrease in discharge works in the same way as an increase in influx; it brings the organism into a state of tension and calls for emergency discharges.

"In psychoneuroses some impulses have been blocked; the consequence is a state of tension and eventually some 'emergency discharges'. These consist partly in unspecific restlessness and its elaborations and partly in much more specific phenomena which represent the distorted involuntary discharges of those very instinctual drives for which a normal discharge has been interdicted. Thus we have in psychoneuroses, first a defense of the ego against an instinct, then a conflict between the instinct striving for discharge and the defensive forces of the ego, then a state of damming up and finally the neurotic symptoms which are distorted discharges as a consequence of the state of damming up—a compromise between the opposing forces. The symptom is the only step in this development that becomes manifest; the conflict, its history, and the significance of the symptoms are unconscious".

Hebb (22) has laid down certain requirements for attempting to demonstrate that experimental neurosis occurs in animals and Broadhurst (2, 3) has examined the literature, and particularly that referred to by Fenichel, from this point of view. Here is his summary.

> "How does the large body of American work stand up to such an assessment? For the purposes of a recent review (3), the available literature was examined in the light of Hebb's criteria. Noteworthy among this is the work of the group headed by Liddell (1), one of the pioneers of conditioning methodology in the United States, who has used principally the sheep as his experimental subject; of Gantt (20), whose long term study of the dog 'Nick' is well known; and of Masserman (30), who has done extensive work using cats. This is not the place to enter into the details of this evaluation, which is reported elsewhere (3), but the overall conclusion which was reached was that there are few instances in all this work of any cases of experimentally induced abnormalities of animal behaviour which meet all of Hebb's criteria. Let us take, for example, the work of Masserman, whose theoretical interpretation of abnormal behaviour need not concern us here except to note that it was the basis upon which he designed his experiments to produce "conflict" between one drive and another. What he did was this. He trained hungry cats to respond to a sensory signal by opening a food box to obtain food. Then he subjected them to a noxious stimulus, a blast of air, or electric shock, just at the moment of feeding. The resulting changes in behaviour—the animals showed fear of the situation and of the experimenter, and refused to feed further—he identified as experimental neurosis. But the behaviour observed fails to fulfil more than one or two of Hebb's criteria, and, moreover, certain deficiencies in the design of his experiments make it impossible to

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draw any satisfactory conclusions from them. Thus Wolpe (45) repeated part of Masserman's work using the essential control group which Masserman had omitted—that is, he gave the cats the noxious stimulus alone, without any "conflict" between the fear motivation thus induced, and the hunger which, in Masserman's animals, operated as well—and found that the same behaviour occurred. It hardly needs to be said that a fear response to a threatening stimulus is not abnormal and cannot be regarded as an experimental neurosis."

It is clear from the studies cited that Fenichel is quite wrong in claiming that "experimental neurosis" is in any way analogous to the Freudian model of human neurosis. It appears, therefore, that in so far as these studies are relevant at all they can be regarded as demonstrating nothing but simple conditioned fear responses of the kind called for by our theory. It is perhaps worthy of note that the failure of psychoanalysis to use control groups in the human field has extended to their work with animals, as in the case of Masserman quoted above. Fenichel's easy acceptance of data congruent with his hypothesis is paralleled by his failure to mention data contrary to the psychoanalytic viewpoint. By taking into account all the data it seems more likely that a correct conclusion will be reached.

I would now like to return to some of the points which I raised at the beginning of this paper. I argued then that the special knowledge and competence of psychologists in mental hospitals was largely wasted because of concentration on, and preoccupation with, Freudian theories and projective types of test. I would now like to make a more positive suggestion and maintain that by virtue of their training and experience psychologists are (or should be) experts in the fields of conditioning and learning theory, laboratory procedures, and research design. In suitable cases, surely their help would be invaluable in diagnostic problems, such as ascertaining a given patient's speed of conditioning, in the theoretical problem of constructing a model of his personality dynamics, and in the practical problem of designing a suitable course of behaviour therapy which would take into account all the available information about the case.* I am not suggesting that psychologists should themselves necessarily carry out this course of treatment; it would appear relatively immaterial whether the therapy is carried out by one person or another, by psychologist or psychiatrist. Both types of procedure have been experimented with, and both have shown equally promising results. Indeed, certain aspects of the therapy can obviously be carried out by less senior and experienced personnel, provided the course of treatment is reviewed periodically by the person in charge. Psychoanalysis lays much stress on what is sometimes called "transference", a devil conjured up only to be sent back to his usual habitat with much expenditure of time and energy (18). Behaviour therapy has no need of this adjunct, nor does it admit that the evidence for its existence is remotely

* It will be clear that the function here sketched out for the psychologist demands that he be furnished with the necessary tools of his trade, such as sound-proof rooms, conditioning apparatus, and all the other techniques for delivering stimuli and measuring responses on a strictly quantified basis (13). It is equally clear that such facilities do not exist in the majority of our mental hospitals. Until they do, the handicaps under which the clinical psychologist works at such institutions will be all but insurmountable, and no reasonable estimate of their potential usefulness can be formed. One might just as well employ an electroencephalographer and refuse to pay for the machine which he has been trained to use! It would be better to have a few, properly equipped departments than a large number of small, ill-equipped ones as at present. Even in the United States the position is bad; in this country it is worse. A relatively small capital investment would be likely to bear considerable fruit.

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adequate at the present time. However that may be, relinquishing the personal relationship supposed to be indispensable for the "transference" relation allows us to use relatively unqualified help in many of the more time-consuming and routine parts of behaviour therapy. In certain cases, of course, personal relationships may be required in order to provide a necessary step on the generalization gradient; but this is not always true.*

From a limited experience with this kind of work, carried out by various members of my department, I can say with confidence two things. The direct application of psychological theories to the practical problem of effecting a cure in a particular person, here and now, acts as a very powerful challenge to the psychologist concerned, and makes him more aware than almost anything else of the strengths and weaknesses of the formulations of modern learning theory. And the successful discharge of this self-chosen duty serves more than almost anything else to convince his psychiatric colleagues that psychology can successfully emerge from its academic retreat and take a hand in the day-to-day struggle with the hundred-and-one problems facing the psychiatrist. It seems to me that the tragic fratricidal struggle between psychiatrists and psychologists, which has so exacerbated relations between them in the United States, could easily be avoided here by recognizing the special competence of the psychologist in this particular corner of the field, while acknowledging the necessity of keeping the general medical care of the patient in the hands of the psychiatrist. I believe that most psychiatrists are too well aware of the precarious state of our knowledge in the field of the neurotic disorders to do anything but welcome the help which the application of learning theory in the hands of a competent psychologist may be able to bring.

* As an example of this we may quote a case reported by Graham White. This concerns a child who became anorexic after the death of her father. The therapist adopted the father's role in a variety of circumstances, ranging in order from play with dolls' teasets to the actual eating situation, and reinforced those reactions which were considered desirable. The theoretical rationale was that the father had become a conditioned stimulus on which eating depended.

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