

Behaviourism and Clinical Psychiatry.

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The term "behaviourism" is often used by clinical psychiatrists in a pejorative sense, to denote crude, over-simplified and animal-centred types of interpretation which fall far short of the complexity of human conduct, the cognitive interpretation deemed necessary for clinical cases, and the methods of therapy based upon these. Such comments are not usually based on any knowledge of the developments which have taken place in behaviourism generally, and the theory of conditioning in particular, since the early days of Watson, or even Skinner. Old-style methodological behaviourism has given way to neo-behaviourism, or even the modern version of dialectical behaviourism, and these modern theories include a great deal of cognitive behaviour as well as the recognition (as indeed did Pavlov's early work), that words can act both as conditioned stimuli and conditioned responses. In other words, criticism of modern behaviourism must be based on thorough knowledge of what it actually stands for, rather than vague shibboleths, relevant to a form of the theory 60 years out of date (Eysenck, 1982).

It is not always realised that the laws of conditioning and extinction provide an excellent interpretation of the acquisition and treatment of neurotic disorders of all kinds (Eysenck, 1980). Let us consider for a moment enuresis nocturna. This provides an excellent example of the different ways in which clinical psychiatrists and psychoanalysts, on the one hand, and behaviourists, on the other, regard a particular "symptom". Psychiatrists tend to regard enuresis as a symptom of a deeper underlying disorder; the clinician attaches fundamental causal importance to the deep-seated patterns of the child-parent relationships which are moulded from birth due to the complex interplay of unconscious causes from both sides. Complex interpretations are based on this foundation. Some psychiatrists believe that enuresis is a substitute form of gratification of repressed general sexuality; others regard enuresis as a direct manifestation of deep-seated anxieties and fears; yet others interpret it as a disguised form of hostility towards parents or parent substitutes which the victim does not dare to express openly.

For the behaviourist, enuresis is simply a failure for a conditioned response to take place, namely that between the enlargement of the bladder during sleep, and the conditioned response of waking up and going to the toilet. The anxieties and other emotional responses observed in enuretic children are the consequence, and not the cause, of the enuresis. Quite different methods of treatment follow from these alternative approaches. The psychoanalytic and psychotherapeutic approach is too well known to be discussed here; there is a large literature demonstrating its failure as compared with no treatment, or placebo treatment. The behaviouristic approach relies on the bell-and-blanket method of helping the child to form the conditioned response by making him sleep on a blanket which separates two metal surfaces which are linked with a battery and a bell; once the child begins to micturate, the urine acts as an electrolyte, makes a connection and the bell rings, waking up the child. This method has been found extremely successful, and is now used routinely practically universally.

Therapeutic success, however, is not the only criterion we can use to tell us whether or not the conditioning theory is correct. From our knowledge of the principles of conditioning we can make three quite specific predictions. The first of these is that there should be a high degree of relapse, because we are trying to establish a new conditioned response, and we

cease to establish a very weak CR, and it is known that such weak CRs extinguish quickly. This tendency to high relapse rate has indeed been found in many studies. How can it be avoided?

Here we come to our second prediction. It is well known that conditioned responses extinguish more quickly when established through a 100% reinforcement paradigm. Partial reinforcement, on the other hand, is much less subject to extinction, and it would seem to follow that if the bell-and-blanket regime were based on a partial reinforcement schedule (say 2 out of 3 nights reinforced), there would be a significant decline in the rate of relapse (Morgan, 1978). This has indeed been found to be so, strongly supporting the theory.

Another method which follows from the theory is that of *over-learning*. Here the child is made to drink a great deal of water before going to bed, at the point where the response is becoming established; this will prolong the period of conditioning, increase the number of conditioning trials, and hence will establish the habit more firmly. Again the evidence shows conclusively that this method reduces relapses significantly. In other works, the conditioning theory not only suggests a specific and successful method of treatment; it makes quite specific suggestions which can be experimentally tested and shown to be correct. This scientific method of using the theory to make these specific predictions can be compared with the purely argumentative and speculative method used by clinical psychiatrists to support the Freudian or allied views (Morgan, 1978).

It should further be noted that a fourth prediction is also verified. The anxieties, worries and depressed episodes characteristic of enuretic children tend to disappear with the disappearance of the enuresis, suggesting that indeed they were merely a consequence, and not a cause of the disorder. Once cured properly, i.e. through the use of over-learning, and, or partial reinforcement, and/or if necessary a repetition of the conditioning paradigm, enuresis remains cured and does not issue in substitute symptoms or a recurrence of the disorder (Morgan, 1978).

Work on enuresis is sometimes dismissed as dealing with a rather simple and elementary disorder, not comparable with the infinite complexity of proper adult neuroses. The same argument is sometimes made when dealing with the undoubted effectiveness of desensitization methods of therapy in relation to phobic disorders. Most of the treatments that have been reported in the literature are not of the relatively rare very simple mono-symptomatic types of phobias, but of fairly complex disorders demanding fairly complex types of desensitization. However, as a second example it may be more useful to take a type of neurotic disorder which has in the past proved extremely difficult to treat by psychoanalysis or any other type of psychotherapy, namely obsessive-compulsive hand-washing. The literature suggests, and our own records at the Maudsley and Bethlem Royal Hospitals confirm, that these disorders are extremely difficult to treat, and hardly ever yield to any of the orthodox types of psychotherapy, psychoanalysis, ECT, or even leucotomy. We thus have before us a very serious disorder which has an almost zero coefficient of recovery or treatment success. How would the behaviourist set about treating this type of disorder?

Our first step was to look at the animal literature to search for an animal analogue, and this was easily found in the work of Solomon and his colleagues (Solomon, Kamin & Wynne, 1953). They worked with dogs in shuttle-boxes, i.e. small rooms divided into two by a hurdle over which the animal could jump, each half of the room separately furnished with an electric grate which could be independently electrified to give the dog an electric shock through his paws. In addition there was a flickering light which served as the conditioned stimulus. The procedure for producing the neurotic reaction was to set the flickering light going, and to give

the dog a shock after ten seconds. The dog soon learned to jump into the other compartment, which was not electrified, once he had received the shock, and after a while he learned to jump to the CS, i.e. prior to a shock being given at all. In other words, the experimenter had by now produced a conditioned response to the flickering light, namely jumping from one half of the box to the other.

At this stage the electricity supply was disconnected, and the dog never received another shock. Nevertheless, he kept jumping across the hurdle each time the conditioned stimulus was put on, for hundreds and even thousands of times. Apparently the dog had acquired an obsessive-compulsive habit, namely jumping, which reduced his conditioned fears and anxieties, and hence was kept alive without any reinforcement. Thus we have here an apparent analogue for the obsessive-compulsive hand-washing of the human patient, substituting jumping over the hurdle for the hand-washing ritual. How can we cure the dog?

It proves as difficult to cure the dog of his jumping habit as it had proved to cure human patients of their hand-washing ritual. The most successful method was one of *flooding with response prevention* (Rachman & Hodgson, 1980). The experimenter increased the height of the hurdle to such an extent that the dog was unable to jump; he then put on the CS. The dog immediately showed strong signs of great fear, running around the chamber, jumping up on the walls, yelping, defaecating, urinating and quite generally being "flooded" with emotion. Gradually this emotional display died down, until finally the dog would lie quietly on the floor without any display of anxiety. In the language of the conditioning paradigm, extinction was being produced by this experimental paradigm, and the theory predicted that several repetitions of this procedure would finally extinguish the whole emotional response. This is indeed what happens; after several repetitions of the procedure the dog would not jump across the hurdle, even though the conditioned stimulus was put on, and the height of the hurdle reduced to make it easy for him to jump.

We decided to use the same paradigm for the treatment of human obsessive-compulsive hand-washers, on the assumption that the underlying dynamics were similar. Needless to say this assumption was severely criticised by clinical psychiatrists to whom we explained our procedures, and it was suggested that the method was quite inappropriate for human beings whose mental processes were much more complex than those of dogs.

The adaptation of the method was as follows (Rachman and Hodgson, 1980). The patient, after having given informed consent to take part in the study, was seated near a table which contained nothing but an urn with earth and rubbish in it. The psychologist would dig his hands into this rubbish and invite the patient to do the same. When the patient had done so, he showed a tremendous increment in anxiety and fear, very much as the dogs had done once the hurdle had been increased in height to make jumping impossible, particularly after it had been explained to them that they would not be allowed to go and wash their hands, but would be expected to stay in the room with their hands dirty. The original flooding with emotion could of course not be sustained, and after half an hour or an hour the patient, very much like the dog, showed a great reduction in fear and anxiety, thus again demonstrating the extinction considered necessary in the theory for a cure. Once the patient had reached this state of low anxiety, he was allowed to go and wash his hands, and the whole procedure was repeated on subsequent days, the theory predicting that in due course extinction would be complete, and the patient would be cured.

Simplistic as this prediction may appear, it was found that something like 90% of the patients so treated were in fact cured, or showed very considerable improvement. Follow-up failed to disclose relapses or symptom substitution, but rather showed a continued improvement in

sexual, work and family adjustment. Treatment and follow-up agreed in great detail with predictions made from the general theory which determined the treatment, and the difference in outcome between the usual psychoanalytic-psychotherapeutic failure, and the outstanding success of the behaviouristic method, seems to indicate that the conditioning theory was at least along the right lines. Many other examples could be given, and quite generally it has been found that behaviouristic methods of treatment are significantly superior to psychotherapeutic ones, particularly in serious cases of neurotic disorder (Kazdin & Wilson, 1978).

Quite generally, the evidence of over 500 separate studies is now fairly conclusive in showing that traditional methods, of psychotherapy and psychoanalysis do no better than placebo treatment, or even no treatment at all, whereas methods of behaviour therapy are significantly superior (Rachman & mWilson, 1980). This suggests that perhaps it might be worth the while of clinical psychiatrists to study very seriously the theoretical concepts of behaviourism and behaviour therapy, and in particular the theory relating experimental findings to the origins and methods of treatment of neurotic disorders. It might be suggested that surely a theory of neurosis, and a method of treatment based on it, which is unequivocally superior to traditional methods, deserves such careful study and should take pride of place in the armamentarium of clinical psychiatrists (Eysenck, 1978).

It is sometimes suggested that this summary of the evidence is incorrect, and certainly there are voices to suggest that psychotherapy is in fact extremely successful. Thus Smith, Glass, and Miller (1980) conclude their review of the effects of psychotherapy in the following way: "Psychotherapy is beneficial, consistently so and in many different ways. Its benefits are on a par with other expensive and ambitious interventions such as schooling and medicine, . . . psychotherapy benefits people of all ages as reliably as schooling educates them, medicine cures them, or business turns a profit. Different types of psychotherapy (verbal or behavioural; psychodynamic, client-centred, or systematic desensitization) do not produce different types or degrees of benefit." (Page 183-184). These claims seem more in line with the traditional beliefs of clinical psychiatrists, but a perusal of the evidence on which they are based discloses certain anomalies which will immediately strike the attentive reader. In the first place, the authors class "placebo treatment" as a type of therapy, rather than comparing the effects of therapy with placebo treatment. The average effect size of placebo treatment is almost identical with that of psychodynamic therapy; in other words there is no evidence here, as little as there is in the explicit study of the effects of psychotherapy as opposed to placebo therapy, of any beneficial effects of therapy as such (Eysenck, 1983; Prioleau et al., 1983).

Furthermore, Smith, Glass and Miller find that there is no relationship between the success of therapy and its duration; therapy lasting an hour is as effective as therapy lasting for many years! This is not the kind of result one would have expected following any of the theories of psychoanalysis or psychotherapy on which treatments are based.

In the third place, Smith, Glass and Miller found that the experience or background of the therapist was completely uncorrelated with the success of therapy; this again is not what one would have expected traditionally. Apparently new-comers with just a little instruction do as well as psychoanalysts of many years experience! This surely poses a serious problem to believers in the effects of psychotherapy.

But most important of all is the alleged finding that all different types of therapy are equally effective. This completely negates the specificity of effects which is claimed by any theory from psychoanalysis onwards; if methods not including the specific methodologies suggested by the theory are equally successful as those that do, then clearly we are dealing, not with

specific effects, but with a very general placebo effect, i.e. some form of suggestion mixed with reassurance, warmth and relaxation. Thus, all that Smith, Glass and Miller have succeeded in showing is that different types of psychotherapy are equally effective as placebo treatment in producing an amelioration of neurotic symptoms; this can hardly be called a vindication of psychotherapy! Worse than that, their statement that verbal methods are as successful as behavioural methods is untrue, even on their own showing; behavioural methods are significantly more effective than psychodynamic ones, although they try to argue their way out of this conclusion by means of a completely subjective and fallacious argument (Eysenck, 1983).

I have tried to show elsewhere that indeed these conclusions make good sense in terms of a conditioning paradigm which attributes all effective therapy to Pavlovian extinction (Eysenck, 1980). The usual methods of psychoanalysis and psychotherapy involve methods of desensitization in imagination, modeling, and sometimes flooding; they are less successful than behaviour therapy because they are used unintentionally and without knowledge of precisely what it is that the therapist is doing. Spontaneous remission also involves similar procedures adopted by friends, relatives, teachers, priests and others to whom the patient takes his troubles, and placebo treatment also tend to involve some of these elements. This is not the place to argue the point exhaustively, as has been done elsewhere (Eysenck, 1980); I think it may be claimed that the conditioning theory explains not only the origins of neurosis, but also the relative success of different methods of treatment, without involving anything but Pavlovian conceptions of conditioning and extinction (Eysenck & Rachman, 1965).

There have, of course, been criticisms of the conditioning theory (e.g. Rachman, 1977), and these criticisms are indeed very relevant to the original form of the theory given to it by Watson and Rayner (1920). However, there have been many improvements in the original theory, making use of recent developments in experimental and theoretical approaches to behaviourism in general and conditioning in particular (Eysenck, 1982), and these improvements in the theory should base his appraisal and criticism on the most recent version, rather than on older versions now very much out of date.

It is interesting to speculate why clinical psychiatrists on the whole have not adopted behaviouristic theories, but have stuck with the theories of a psychodynamic nature which are clearly false, and do not issue in successful methods of therapy (Eysenck, 1985). There is no experimental evidence on this point, but it is well known that new methods in medicine take a long time to establish themselves against the conservatism of those who have been trained in the older methods, and are not keen to look at the evidence favouring the new. In addition, of course, most psychiatrists of the older generation have been trained in methods of psychodynamics and psychotherapy, and do not relish the thought of having to learn entirely new theories and methods. Furthermore, of course, to become an expert in these methods requires a background in learning theory, conditioning methodology and hence a lengthy period of study and preferably experimental work in the laboratory; few Consultants would welcome the need for such retraining. All this is of course perfectly intelligible, but from the point of view of the patients' needs, and the doctor's responsibilities, it must be said that these are not acceptable reasons for rejection of methods of treatment which are clearly superior to those currently practiced. When a child presents with *enuresis nocturna*, or an adult with obsessive-compulsive hand-washing, the ethical doctrines associated with the Hippocratic Oath demand the use of the bell-and-blanket for the one, and the use of flooding with response prevention for the other; there is no excuse for using less effective methods of treatment when more effective ones are available.

The argument is often encountered that the doctrines of behaviourism de-humanise the patient,

and leave out of account important cognitive and other variables which are vital to an understanding of the disease, and its treatment. It is easy to say these things, but there is no evidence I know of which suggests that these objections are well taken (Latimer & Sweet, 1984). Neurotic disorders are essentially disorders of the paleo-cortex, not the neo-cortex, and Pavlovian conditioning is the language of the paleo-cortex. The relative inaccessibility of neurotic disorders to rational argument has often been commented upon; this is simply a function of the lack of communication between the neo-cortex and the paleo-cortex. There is a great deal of evidence to indicate that emotions can be conditioned and extinguished through Pavlovian procedures, rather than through rational verbal intercourse; this suggests that methods of treatment should also be based on Pavlovian concepts.

Patients of course do have cognitions, but these follow upon, rather than cause the conditioned responses. There is no evidence to suggest that the manipulation of cognitive processes by verbal means is anything like as effective as is behaviour therapy in its varied form (desensitization, modeling, flooding). It is noteworthy that those who criticise the behaviouristic approach have never taken seriously their task of going into experimental details, of looking carefully at the precise empirical evidence, or of advancing alternative cognitive theories for those of Pavlovian conditioning. What is usually offered is something quite different, namely an emotional appeal to humanistic ideals; this is not a scientific argument against behaviouristic theories and methods. Indeed, it must be obvious that clinical psychiatrists are not qualified to criticise or judge behaviouristic theories and practices because they lack the theoretical and empirical background which alone make such criticism scientifically meaningful.

The problem is, of course, aggravated by the old quarrels between medically trained people and non-medical psychologists, with the former arrogating to themselves all forms of "treatment", even in the absence of specific training relevant to the disorders in question. The methods of behaviour therapy have been very largely elaborated by non-medical psychologists on the basis of psychological experimentation in the laboratory; it is difficult to see why medical people ignorant of this background should be better equipped than psychologists to apply these methods, particularly when it is clinical psychologists who have had a great deal of experience and training along these lines, whereas clinical psychiatrists have very rarely possessed either background or training. Experience at the Institute of Psychiatry, and its associated hospitals (The Bethlem Royal and the Maudsley) suggest that a mutually agreeable basis for cooperation can be reached by having a large Department of Clinical Psychology to which appropriate cases of neurotic disorders can be referred for treatment. An alternative would be for clinical psychiatrists to be properly trained in the theory practice of behaviour therapy (Eysenck, 1975).

Other alternatives and choices may be possible; this is not a matter of science, but of administrative convenience, of training schedules and other decisions which are outside the scope of this article. The problem, however, is not one which is likely to go away, or which can be disregarded. In the interest of the patients alone decisions must be made, and these must be based on the facts, which indicate quite clearly the superiority of behavioural methods of treatment to those of traditional psychotherapy. It is unlikely that a brief article of this kind will convince the doubters, but it is hoped that they will at least read the references suggested, which will give them a wider overview over the difficulties raised for traditional clinical psychiatry by the emergence of behaviourists theories and methods of treatment. I believe that psychology has made an important and indeed fundamental contribution to psychiatry in the elaboration of behaviour therapy, and I hope that the influence of these behaviouristic methods will spread and be incorporate in the teaching and practice of clinical

psychiatry. How this can best be done is, of course, for clinical psychiatrists themselves to decide, hopefully on the basis of the large amount of factual material available.

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