

EDITORIAL

Psychological theories and behaviour therapy

There can be no doubt about the fact that behaviour therapy has won a firm place in psychiatry and clinical psychology. In a dozen years or so, the number of articles on the subject has risen meteorically from almost nothing to the point where, in 1972, it exceeded the total contribution of psychoanalysis (Hoon and Lindsley, 1974). Special commissions set up to pronounce on its value, such as that of the American Psychiatric Association, have reported very favourably (1973). What is more, the number of well-planned, well-conducted clinical and experimental studies comparing the results of behaviour therapy with those of no treatment, placebo treatment, and various forms of psychotherapeutic treatment is increasing, and demonstrates beyond cavil the efficacy of these new techniques of desensitization, flooding, modeling, reinforcement, aversive conditioning, and so on. Critics have almost ceased to argue that the methods do not work, or that they 'merely' cure symptoms; the failure of relapses and symptom substitutions to occur has by now been recognized through a variety of follow-up studies. Instead, critics are concentrating their fire on the fundamental proposition that behaviour therapy is an applied science, and that its methods derive from modern learning theory (Eysenck, 1959). They now claim that there is no unified theory of learning on which behaviour therapy is based; that the principles of treatment are not rigorously deduced from such (non-existent) theory: and that quite generally theoretical considerations do not play much part in the work of the clinician (Breger and McGaugh, 1965; Locke, 1971; London, 1972). What in fact is the relation between psychological theory and behaviour therapy?

A rational appraisal of the situation must take into account two sets of facts. The first relates to the present situation in psychology, and particularly in learning theory. It is certainly true that there is no unified learning theory, and that many alternative formulations are being put forward. It is certainly true that the principles of treatment are not 'rigorously' deduced from learning theory, but that instead well-established laboratory phenomena suggest modes of application which are tried out by clinicians, elaborated when successful, and abandoned or altered and improved when unsuccessful. When successful variants are discovered—for example, desensitization or 'flooding'—clinicians seldom worry overmuch about the theoretical background of these successful methods, but use them on a hand-to-mouth basis, as valuable tools in their therapeutic armamentarium. Thus it would at first glance seem as if the critics were right; psychological theory does not play as much part in the genesis of behaviour therapy methods as had originally been claimed.

The second set of facts, however, suggests that this would be an erroneous interpretation, because it views behaviour therapy from an impossibly idealistic angle, such that no applied science, even in the fields of physics and chemistry, would pass muster. The situation which obtains in psychology is precisely analogous to that obtaining in other sciences; there also unified theories are sadly lacking, 'rigorous' applications of firmly established principles turn out to be less rigorous than imagined, and practitioners often do not worry very much about the ultimate truth of the principles on which the methods they use are based. The famous case of the discovery of the planet Neptune, for instance, is often quoted as an inspiring example of rigorous deduction from unified theory leading to great discovery. When Herschel discovered Uranus, the slight deviations of this planet's path from prediction suggested that another planet might be present which affected the motions of Uranus; LeVerrier in France and Adams in England calculated the position of this hypothetical planet, and when astronomers in Berlin and London directed their telescopes to the precise spot indicated, there was Neptune. So much for the textbook story. Reality, alas, is somewhat different. The discovery of Neptune was possible only because an approximate estimate of its distance from the sun was available through Bode's Law, a rough-and-ready empirical rule showing a regularly increasing

distance from planet to planet as we go from the sun outwards; this 'law' cannot be deduced from Newton's equations, and is just an accidental property of our planetary system which would probably not obtain in other such systems. In addition, Bode's Law does not apply to Neptune; astronomers were very lucky that they looked for the planet when it was roughly at the distance predicted; six months earlier or later it would have been very far from the predicted spot, and would not have been found! Thus deduction, even in this classical case, was far from rigorous; it was based on an empirical rule which has no basis in theory; and successful prediction depended to a considerable extent on sheer luck. This is a more realistic point from which to view scientific prediction!

Nor does the notion of a 'unified theory' fare any better in this example. Newton's theory of gravitation postulates action at a distance, which was in fact disowned by Newton himself; Faraday and Maxwell later substituted a field theory, which was applied to gravitational problems by Einstein. Now, however, we are back at square one; 'action-at-a-distance electrodynamics' has been introduced, and Narlikar has shown that ADE fits in with Einstein's general theory of relativity and gives predictions which are indistinguishable from conventional field theory. There are many other examples of alternative theories with the same experimental consequences, but a very different conceptual basis: Newtonian and Lagrangian mechanics, for instance, and the Schroedinger and Heisenberg interpretations of quantum mechanics. The same may be true of cognitive and conditioning-type theories of learning; the fact that alternative interpretations exist does not excommunicate learning theory from science! It simply indicates that learning theory is similar, in this as in other respects, to what is common in the hard sciences.

Consider an example of the application of psychological theory to treatment. We can regard enuresis nocturna as a failure of conditioning to occur: the enlargement of the bladder, the conditioning stimulus (CS), does not produce the conditioned response (CR), of waking up and going to the toilet. We can mediate this conditioning process by means of the well-known bell-and-blanket method; this leads us to make three predictions. (1) Repeated application of the method should lead to the formation of the missing conditioned link, and the patient should learn to wake up in time and urinate in the toilet. This has been shown to be true over and over again. (2) The conditioned response, through failure of reinforcement to occur once it has become established, should extinguish again; we should have many relapses. This too has been demonstrated. (3) Extinction is known to be much less marked after partial reinforcement than after 100% reinforcement; we would predict that if the bell-and-blanket device was activated only on some of the trials, but not on others, relapses should occur much less frequently. This too has been demonstrated (Finley *et al.*, 1973). Can it really be denied that the discovery of the appropriate method owed much to well-known principles of learning theory? Much the same can be said for another successful method (over-learning) for reducing relapse (Young and Morgan, 1972).

But is it not true that contradictory results are often reported, both from laboratory work and from clinical experience? 'Flooding' methods—that is, extinction through response prevention—should work theoretically in eliminating phobic fears and obsessive-compulsive symptoms; some studies have shown the method to be very successful, others have shown it to make the patient worse. Is this not a grievous failure of theory to direct therapeutic work? The answer is that theory suggests the importance of a temporal parameter in making predictions. Short-term exposure to the CS (the phobic object, or the prevention of the obsessive-compulsive motor component) without reinforcement enhances the fear-anxiety component; long-term exposure reduces it. This has been found to be true both in animal (laboratory) experiments, and with clinical experiments on neurotic patients. The theory works perfectly well when it is properly applied; it seems to fail when important and relevant parameters are not taken sufficiently into account.

Criticisms of observed clinical facts as being contrary to theory are often based on a partial view of systematic theory. Thus it has often been observed that aversive conditioning of alcoholism, for instance, seems to imply 'backward conditioning'; in other words, the unconditioned stimulus (UCS) (nausea produced by drugs) precedes the CS (drinking alcoholic beverages). Backward conditioning, so it is said, is known not to work; it seems to follow that the clinical facts contradict

the theoretical formulation. But this is not true; the rule that backward conditioning does not work applies only when certain parameters of the experiment follow a given pattern. There is no backward conditioning when the CS is weak and the UCS strong; this is the typical pattern in American work. But when both CS and UCS are strong (as in the work of Asratyan in the USSR) or weak (as in the work of Dostalek in Czechoslovakia) backward conditioning has been demonstrated to be quite strong. Now the aversive conditioning situation closely resembles Asratyan's type of strong-strong association, and, consequently, backward conditioning would properly be predicted; it is only the failure to consider the parametric details of the theory which leads to erroneous prediction.

It will be apparent now what our answer is to the oft-repeated criticism that the theory underlying behaviour therapy is in a chaotic and incoherent state. The charge is true, but it would be equally true of most scientific theories. Ever since I began taking an interest in subatomic physics 40 years ago theories in that field have been in an incoherent state; yet the field has prospered and has had its unarguable applied successes. The same could be said of cryogenics, where one expert pointed out that it consumed theories at the rate of one every six months. We are not quite as lavish with our theories—perhaps we ought to be. Scientific theories are not Simon-pure, ethereal constructs of eternal beauty, Platonic ideas paraded at the mouth of the cave; they are tools of greater or lesser usefulness, to be discarded when they do not serve their function any longer, and replaced by better ones. As J. J. Thomson said, 'A theory in science is a policy rather than a creed'. Such theories as we have in psychology are far from perfect, but they have shown considerable usefulness. More than anything, perhaps, they serve to denote a policy; it is this policy that sets off behaviour therapy from its predecessors. That policy is to rely on laboratory experiments and general laws deriving from these; to deal with facts rather than with speculation; and to pay particular attention to the outcome problem—do patients actually get measurably better when I apply this, that, or the other general law? The imperfect state of the underlying theory will make certain that it never becomes a creed!

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