

EXPECTATIONS AS CAUSAL ELEMENTS IN BEHAVIOURAL CHANGE

H. J. Eysenck

Institute of Psychiatry, de Crespigny Park, London SE5 8AF

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Abstract — The present paper examines conceptual and methodological issues raised by the self-efficacy theory. The conceptual issues include the interrelationships of self-efficacy and other constructs such as competence, incentives, and cognitive appraisal to which it has been related and the interdependence of efficacy and outcome expectations. The assessment issues address whether the method of measuring self-efficacy contributes to the correlations obtained between expectations and performance and the need to validate self-efficacy measures. Finally, questions are raised about the scope of self-efficacy across target problems and therapy techniques. The paper suggests areas of research within each of the topics that may further elucidate and define the merits and limits of self-efficacy in predicting therapeutic change.

There are complex difficulties in the attribution of causal efficacy to elements in the experimental procedure, which bedevil progress in the physical sciences. There is much agreement on the physical facts giving rise to the concept of "gravitation", but even now there are at least three completely divergent theories of the causal phenomena underlying the physical manifestations. Newton's theory of "action at a distance" has recently been revived; Einstein's field theory comes perhaps nearest to being considered orthodox; and quantum theory posits an exchange of particles ("gravitations") as being responsible. There clearly are many different ways of looking at natural phenomena, and it is unrealistic to imagine that even after hundreds of years of research only one, major, unassailable theory would survive, without complications, without anomalies, without flaws. When even in the "hard" sciences fundamental problems continually re-emerge (as for instance that of the wave or corpuscular nature of light), it is not to be wondered at that psychology too has its eternal problems.

One of these is the body-mind relation. From one point of view this is often considered a philosophical problem, and no doubt philosophers have plenty to say about it. But clearly the problem has its psychological aspects; indeed, to many people it is the central psychological problem. It emerges under many different guises; its most recent manifestation comes in the debate about cognitive theories as opposed to physiological theories of behaviour and behavioural change. Cognitive theories almost as a matter of course assume some form of interactionist theory of the body-mind relation; usually they do so without realization of the enormous conceptual and philosophical problems which such a stance involves. Bandura's theory of "self-efficacy" is such a cognitive theory, and it assumes that in the causal chain which mediates behaviour and behavioural change, cognitive (mental) events act upon, and are acted upon by, physical (non-mental) events, such as physiological changes. How such interaction is possible is not discussed; it is assumed.

Non-cognitive (physiological) theories do not assume such interaction; in particular, they do not assume that cognitive (mental) events act upon physiological ones. Mental phenomena may be regarded as epiphenomena (perhaps the most widely accepted solution to the mind-body problem among scientists), or a theory of parallelism may be held; in either case the possibility of mental events influencing physical ones is denied. Materialism, too, of course has its difficulties; Pavlov's second signaling system, for instance, seems to imply quite a different type of activity to that emphasized in the actions of the primary signaling system. Here too, though, it is possible to assert that the fundamental reality is physical, and that the conscious ideas which seem introspectively to have a causal function in the chain of events linking stimuli to responses, are merely epiphenomena mirroring some underlying physical reality.

My own theory of neurotic disorders, their origin and their treatment, is fundamentally

materialistic in this sense (Eysenck, 1976, 1977). It asserts that neurotic behaviour arises through a process of Pavlovian conditioning, and can be modified through a process of Pavlovian reconditioning or extinction. The theory calls for many modifications of the old Watson—Mowrer theory; in particular, it includes an entirely new hypothesis concerning extinction. But the details are not important in this connection; what is important is that the theory presents an alternative to Bandura's mentalistic—cognitive view, and it is interesting to see whether Bandura adduces any facts which would be difficult to reconcile with, or explain in terms of, such a materialistic theory. My own view is that there is nothing in Bandura's theory that would necessitate any such addition of cognitive material to a fundamentally non-cognitive theory. Bandura recognizes that "*it is performance-based procedures that are proving to be most powerful for effecting psychological changes*" and that, "*as a consequence, successful performance is replacing symbolically based experiences as the principle (sic!) vehicle of change*" (Bandura, 1977, p. 191). The methods pioneered by Herzberg, Wolpe, and others have proved their worth, but their mode of operation surely suggests a physiological rather than a cognitive basis for their effectiveness.

What Bandura has done, or so it appears to me, is to translate the major features of my own theory of neurosis and behavioral change into mentalistic (cognitive) language. My theory postulates that previously neutral stimuli become conditioned to evoke CRs similar to UCRs associated with UCSs by simple pairing of CS and UCS (contiguity.) This, of course, is common ground. The crucial element of the new theory is that the presentation of the \overline{CS} (unreinforced CS), under carefully specified conditions, produces an enhancement (incubation) of the CR, thus producing in time a positive feedback cycle which is experienced as a neurotic breakdown. The experience of this positive feedback cycle may conveniently be called "efficacy expectations"; each repetition of the CS—CR cycle reinforces the expectation of anxiety, leading to avoidance and withdrawal, i.e. the "symptoms" of the neurosis. But in my theory these cognitive elements are merely epiphenomena; the causal element is the incubation of the conditioned response. By stressing the cognitive epiphenomena, to the exclusion of the truly causal conditioning events, Bandura has stood the causal sequence on its head.

Figure 1 shows the difference in diagrammatic form. Figure 1a shows the form of the theory presented in my original paper (Eysenck, 1976.) Figure 1b shows the same theoretical sequence with Bandura's concept of "failure expectations" interpolated in the causal sequence. Figure 1c shows Bandura's concept introduced as an epiphenomenological byproduct of the true causal sequence. It is this latter figure which, to my mind, represents reality; the introduction of mentalistic elements into the otherwise physiological chain, as in Fig. 1b, raises a host of philosophical and other problems which Bandura never acknowledges, let alone answers.

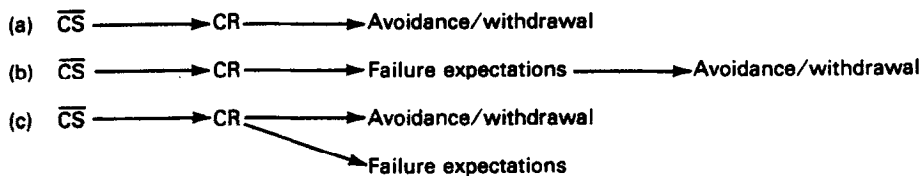


FIG. 1.

In a similar manner, we can diagram the course of behavioural change as in Fig. 2. The first part of the figure shows how the \overline{CS} , after deconditioning or reconditioning (Eysenck and Beech, 1971), gives rise to CR+ (relaxation and other positive CRs) as well as CR- (anxiety and other negative CRs), the ratio of the two determining the actual behavioural and other consequences of the exposure of \overline{CS} . Bandura's notions of mastery expectations and failure expectations have been incorporated, as he would do, as causal elements into this sequence; in other words, CR+ gives rise to mastery expectations, and through them

initiates approach behaviour, while CR- gives rise to failure expectations, and through them initiates avoidance/withdrawal behaviour. Figure 2b, on the other hand, gives my own view of the matter, with mastery and failure expectations playing a mere supporting role as epiphenomena of the underlying physiological change in autonomic and other sectors. Bandura never goes into detail as to how we could experimentally distinguish between these two ways of looking at the data (indeed, he fails to clarify the relation between his cognitive concepts and the non-cognitive concepts of earlier theorists). In the absence of good evidence in favour of the cognitive theory, Occam's razor would seem applicable here, cutting out from the causal chain cognitive elements which do not seem to play an essential part, and which are not necessary to the adequate working of the theory.

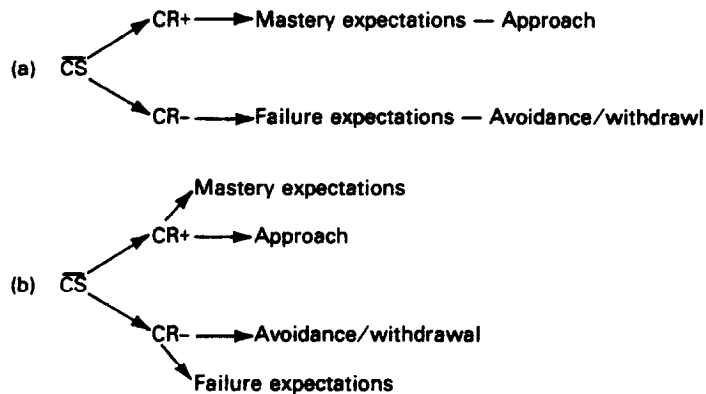


FIG. 2.

Bandura makes comments which suggest that expectations may be all-important. Thus he states (Bandura, 1977, p. 194) that “*given appropriate skills and adequate incentives . . . efficacy expectations are a major determinant of people's choice of activities, how much effort they will expend, and of how long they will sustain effort in dealing with stressful situations*”. But this statement lacks adequate documentation along experimental lines; it is merely programmatic. Substitute the statement that “*given adequate skills and adequate incentives . . . conditioned autonomic responses are a major determinant of people's efficacy expectations*”, and the whole statement can be re-worded leaving out entirely, or condemning to a secondary, epiphenomenal role, these expectations which figure so prominently in Bandura's original version. Bandura asserts what needs to be proved, and fails to realize that it is not sufficient to prove the existence of expectancy and self-efficacy elements in order to demonstrate their causal role in the sequence of events which bring about behaviour and behavioural change. His failure to deal with alternative, non-cognitive theories is perhaps the weakest aspect of his paper; it is impossible to see how he would have dealt with what is perhaps the central problem of his theoretical approach.

Bandura repeatedly makes assertions such as that “*with the ascendancy of cognitive views of behavior, the concept of expectancy is assuming an increasingly prominent place in contemporary psychological thought*” (Bandura, 1977, p. 204), or that, “*the theoretical framework presented in the present article is generalizable beyond the psychotherapy domain to other psychological phenomena involving behavioral choices and regulation of effort in activities that can have adverse effects*” (Bandura, 1977, p. 204). Both statements are true, but they merely illustrate one of the common evils of psychological theorizing, namely that it is subject to periodic changes which owe little to experimental demonstration of theoretical advances. Cognitive theorists reject non-cognitive theories for no good, experimental reason, and they embrace cognitive views in the absence of good, experimental support which would be adequate to persuade doubters. To adopt Bandura's point of view one

would need more clear-cut empirical support than the fact that many other authors are also advocating similar points of view.*

Bandura quotes two experiments to support his views, but both of them are tangential. In the first, he shows that "consistent with the social learning analysis of the sources of self-efficacy, experiences based on performance accomplishments produced higher, more generalized, and stronger efficacy expectations than did vicarious experience, which in turn exceeded those in the control condition" (Bandura, 1977, p. 205). This is insufficient, as similar predictions would be made by conditioning theory. Bandura also found that "the higher the level of perceived self-efficacy at the completion of treatment, the higher was the level of approach behavior" (Bandura, 1977, p. 206). Also, "in all conditions, the stronger the efficacy expectations, the higher was the likelihood that a particular task would be successfully completed" (Bandura, 1977, p. 207). All these consequences would be as well predicted from conditioning theory as from social learning theory, as shown in Fig. 2. The real issue, i.e. the causal relevance of expectancy, is not raised in these experiments.

Two facts would seem to support Bandura's case. He finds that "*if one predicts that those who performed maximally in treatment will likewise achieve terminal performances when assessed with similar tasks, the error rate is relatively low for the similar threat . . . but high for the dissimilar threat If, on the other hand, one predicts that those who express maximal expectations will perform maximally, the error rate is comparably low for both similar . . . and the dissimilar . . . threats.*" (Bandura, 1977, p. 207.) This indicates that performance has reached a ceiling, while expectancy ratings have not; this is a well-known psychometric effect which cannot be interpreted to favour Bandura's theory. The result as reported cannot be used to support Bandura's statement that "*these differential findings indicate that experienced mastery altered subjects' sense of personal efficacy rather than merely providing behavioral cues for judgments of self-efficacy.*" (Bandura, 1977, p. 208.)

The same criticism must be made of Bandura's other experiment, in which he finds that "*phobics whose anxiety reactions to visualized threats have been thoroughly extinguished emerge from the desensitization treatment with widely differing efficacy expectations*" (Bandura, 1977, p. 210). Performance, apparently, corresponds closely to level of self-efficacy — "*the higher the subjects' level of perceived self-efficacy at the end of treatment, the more approach behavior they subsequently performed in the posttest assessment*" (Bandura, 1977, p. 210.) Again, there is an obvious ceiling effect when all subjects have been "thoroughly extinguished", leaving other indicants, such as verbal ratings, as possible predictors. All this has nothing to do with the really crucial point at issue, namely the causal or epiphenomenal nature of these efficacy expectations. Regardless of their status in relation to this question, such expectations would be expected to show high correlations with outcome. (There may also be here an element of self-fulfilling prophecy!)

We must conclude that Bandura's re-evocation of level of aspiration theories, and their application in a rather novel manner to problems of clinical psychology, presents an interesting alternative to classical theories and their more recent modifications, but fails to present any evidence on the crucial question of the causal influence of cognitive elements in the whole sequence of events which lead to neurosis or to recovery. It is curious how older theories (imitation, level of aspiration) are periodically revived under other names (modeling, efficacy expectations), usually without reference to the experimental literature accumulated around these older theories, as if the new name provided a new lease of life, and a new sphere of application to these theories. It requires a much more purposeful experimental analysis of the phenomena in question to answer the many theoretical questions which arise to throw doubt on any "cognitive" analysis of neurotic behaviour, or behaviour change through performance-based behaviour. Bandura's paper begins with the

*It is often taken for granted that "social learning" theories, such as those using "modeling" procedures, are in some essential sense different from conditioning theories, and demand and provide alternative explanations of a "cognitive" kind. It must be very doubtful if this is really so. The Yale school used conditioning principles to explain learning through imitation, and no attempt has been made by cognitive psychologists to design crucial experiments to discriminate between their principles of explanation and those used by conditioning psychologists. It is easy to *assert* that one set of principles explains a particular set of explicanda better than another set, but proof can be very difficult. Assertions of this kind should be treated with great caution, particularly when such proof has not even been attempted.

paradox that modern theories of behaviour change tend to be cognitive in nature, while the methods which actually produce such behaviour changes are performance-based. His theory does not succeed in solving this paradox, the proper answer to which must surely be that cognitive theories stress epiphenomenal concepts, leaving out the truly causal elements in the chain of events which mediates changes in behaviour.

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