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## SELF-EFFICACY: A CAUSE OF DEBATE

RUSSELL M. F. HAWKINS

University of South Australia

**Summary** — In response to a paper (Hawkins, 1992) arguing that self-efficacy is a predictor but not a cause of behavior, Bandura (1995) has raised a series of counter arguments. None of these counter arguments seem sufficient to retreat from the claim that self-efficacy is not a true cause of behavior. The present paper reaffirms the position that self-efficacy is a useful concept when used as a descriptive metaphor. Examples from applied psychology are raised to justify this position. Discourse analysis is introduced as one approach which is able to circumvent the debate about the reality or nonreality of mental entities.

In 1992, I published a paper suggesting that self-efficacy was a predictor of behavior but had no claim to being a cause of behavior. Since authors naturally feel protective of their work, a spirited response from Bandura was perhaps to be expected and of course debate is important in order to make progress. There are some logical flaws in Bandura's account of my criticisms, and he has drawn inferences beyond my specific criticisms. While I do regard self-efficacy as "metaphorical" it does not follow that I believe that human thought has no effect on human motivation or action, as he claims. Nor do I regard mentalism as a "sin", nor do I assert that self-efficacy has no impact on learning. Self-efficacy is a construct, albeit a useful and intuitively appealing one. Rather than engaging in an "overzealous effort to refute self-efficacy theory" (Bandura, 1995, p. 187), my intention is to raise issues which could be used to modify rather than discard the theory.

At times, Bandura slips in a few "ad hominem" style quips in aid of his commentary. For example "Hawkins may fervently believe that his thoughts have no functional value . . ." (Bandura, 1995, p. 185). This is quite witty and I do not take offense, but as an aside let me say that this style may inhibit frank debate if it acts to intimidate potential critics.

It should be said that an anonymous reviewer of the first submission of the Hawkins (1992) paper advised the author to choose words carefully as "he [Bandura] often ridicules minor points in an effort to discredit the larger argument".

The content of Bandura's paper "Comments on the crusade against the causal efficacy of human thought" focuses on criticisms of the Hawkins (1992) paper. This is curious in that I made no claim that human thought was not causal; the more restricted claim was that self-efficacy was not causal. I do not argue with Bandura's comment that "A theory that denies that thoughts can affect motivation and action does not lend itself readily to the explanation of complex human behavior" (Bandura, 1995, p. 183). Other authors may well wish to argue about whether or not human thought is causal (see for example Lee, 1992, p. 259). There is a degree of mismatch between the title of Bandura's paper and the specific focus on the Hawkins (1992) paper. Had he really wanted to comment on the "crusade", it is surprising that other critical authors were not included in his commentary.

Lee (1992) has categorized cognition-based models of human social behavior as

veridical *explanations* of behavior, or, less grandly, as heuristically useful but explicitly fictional *descriptions* of

behavior, descriptions which serve a practical function in applied psychology but do not pretend to identify the underlying causes of behavior. (Lee, 1992, p. 257)

Lee then criticized cognitive–social models at both of these levels, suggesting that the models have “no explanatory value” and that even as descriptive models, they need further development in order to be more useful (Lee, 1992, p. 266).

I have made no claim to being a “devout behaviorist” (Bandura, 1995, p. 187). Although Bandura may not welcome me into the club, for practical purposes I see myself as a cognitive–behaviorist and routinely use the concept of “ego-strength” (analogous to self-efficacy) in clinical work (Hawkins, 1988), for example in the treatment of chronic pain (for a review of the shift towards the use of cognitive approaches in the treatment of chronic pain see Gamsa, 1994). My own position is not dismissive of the concept of self-efficacy. It has intuitive appeal and demonstrable utility as a descriptive term. In terms of “scoring points against self-efficacy theory” (Bandura, 1995, p. 182), I would be pleased to support the theory rather than criticize it, if it were not for the claim of causation.

Clinical psychology often uses hypothetical constructs which are explicitly acknowledged as metaphors (e.g., the Parent, Adult, Child concepts in Transactional Analysis). Sometimes the constructs are reified or taken rather too literally (some forms of Freudian psychotherapy, neurolinguistic programming, re-birthing therapy, past lives therapy). In the area of child sexual abuse, we are currently observing the problems associated with confusion about whether memories of abuse are “real” or constructed in the context of therapy and whether abuse, imagined or real, can be regarded as the cause of subsequent symptomatology. In applied psychology then, as well as in theory, the concept of causation is troublesome.

#### Self-Efficacy in Applied Psychology

A wide body of literature has demonstrated the association between success with a range of

clinical problems and self-efficacy. Dolce (1987) found that efficacy beliefs were associated with the level of functioning of chronic pain patients and their response to treatment. Biran and Wilson (1981) found self-efficacy to be an accurate predictor of task performance in a group of people who avoided either height, elevators or darkness. Chambless and Murray (1979) showed that self-efficacy related to the ability to avoid overeating and Weinburg, Hughes, Critelli, England and Jackson (1984) found that both pre-existing self-efficacy to avoid overeating (i.e., to lose weight) and efficacy manipulated through false feedback, affected the amount of weight loss. Avoidance of bulimic behavior was related to increases in self-efficacy in a study reported by Schneider, O’Leary and Agras (1987). Cognitive behavioral treatment was effective in raising perceived self regulatory efficacy and self-efficacy in turn “was associated with” (the word “caused” was not used) more favourable outcomes. Wilson, Rossiter, Kleifield and Lindholm (1986) further showed that self-efficacy measurements predicted response to cognitive–behavioral treatment for bulimia, including probability of relapse.

Avoidance of cigarette smoking has been intensively investigated from a self-efficacy perspective. A study by Devins and Edwards (1988) claimed to “go beyond earlier findings” principally because these earlier studies included methodological weaknesses which their study avoided. Using multiple regression analyses, Devins and Edwards (1988) found that perceived self-efficacy was the only significant predictor of reduced smoking at 1- and 3-month posttests. Stuart, Borland and McMurray (1994) have also shown that self-efficacy is related to the success of attempts to give up smoking.

Self-efficacy is related to adherence to diabetes treatment (Kavanagh, Gooley & Wilson, 1993) and to coping with gastrointestinal endoscopy (Gattuso, Litt & Fitzgerald, 1992). Sadri and Robertson (1993) have used meta analysis to show that self-efficacy is related to both work-related performance and choice and a large number of papers have explored the role of self-efficacy in career decision making (e.g., Vasil, 1992). Self-

efficacy has been used to predict the use of condoms (Brien, Thombs, Mahoney & Wallnau, 1994; Wulfert & Wan, 1993), and of other forms of contraception (Heinrich, 1993) and as a predictor of coping with abortion (Cozzarelli, 1993).

Self-efficacy has even been found to be predictive of university faculty research productivity (Vasil, 1992) and of psychology student course goals (Singer, Stacey & Lange, 1993).

These references to applications of self-efficacy theory are included to underline the point that the theory has utility when used to describe and predict behavior.

### Specific Responses to Criticisms

Bandura (1995) has presented examples of nonperformance-based manipulations of self-efficacy which have then resulted in performance which correlates with the induced self-efficacy beliefs. For example, he reported that when Cervone and Peake (1986) biased the way in which people arrived at self-efficacy judgements, these people then acted in ways consistent with their judgements. While these results are consistent with self-efficacy being a "cause" of behavior, the experiment can be explained in other ways. Explanations based both on suggestion effects (e.g., see the hypnosis literature) and on a need for consistency (e.g., consistency theories) would, for example, predict similar experimental results but without inferring that self-efficacy was the "cause".

Bandura has described Litt's (1988) study which involved providing subjects with bogus feedback regarding their pain tolerance in terms of comparative norms. In this study, bogus feedback affected reported self-efficacy and this in turn was followed by changes in actual performance on a pain tolerance task which were consistent with reported self-efficacy. Since subjects did not know that the feedback they received was bogus, the feedback, in the absence of any other contradictory information was their best way of understanding their performance level. That the

feedback would influence self-efficacy ratings is not surprising, and neither is the subjects' subsequent pain tolerance performance, since it (*and* the self-efficacy ratings) were predicated on prior performance (the bogus nature of the feedback does not matter).

Bandura is particularly impressed with the fact that when feedback to subjects involved suggesting that they had lost an earlier apparent superiority in term of pain tolerance, their reported self-efficacy also dropped and so did subsequent actual pain tolerance. This demonstrates that "perceived self-efficacy overrode past performance" as a predictor of subsequent performance (Bandura, 1995, p. 181). However, the cold pressor task is novel and it lacks evaluative cues other than those provided by the experimenter. In a sense the performance was whatever the feedback said it was. In this context, all that had happened was that the subjects most recent "poor" *performance* had affected both their most recent self-efficacy ratings and their subsequent performance. In the same context, Bandura says that "If perceived self-efficacy is an epiphenomenon of prior performance it should have remained high" [when the bogus feedback was supplied] (Bandura, 1995, p. 181). But why should it remain high? If the initial rating of self-efficacy was dependent on feedback from the first performance, then similarly the self-efficacy ratings after performance two should respond, in part, to feedback following performance two. To the subjects, the feedback was real, not bogus, and the experiment should be interpreted accordingly. The Litt study reinforces the value of feedback in predicting performance (Feather, 1968, Annett, 1969).

Lee (1992, p. 262) has indicated that while self-efficacy correlates well with behavior in the short term, factors other than self-efficacy may be better predictors of performance in the longer term (even apparently irrelevant variables such as the sex of the respondent).

Bandura says that I attribute changes in performance to extinction of anxiety and then criticizes me for using the term "anxiety," which he says behavior analysts would reject. This is a

tortured interpretation of my comments. I actually said that in the context of desensitization to a phobia, "what essentially matters is the subject's continued tolerance of the objects (i.e., his or her behavior)", (Hawkins, 1992, p. 253) and I added that the conventional explanation of the success is in terms of anxiety. But here anxiety is used as a convenient metaphorical concept. I am quite comfortable with using mentalistic states (such as anxiety or willingness, or even more difficult concepts such as love, lust, shame or patriotism) as a convenient shorthand. We know what we mean (more or less) when such words are used. It doesn't mean that we have to believe in the words as causes in anything but a heuristic sense.

Bandura's data (Bandura, 1995, Table 1) showing the superiority of self-efficacy to anxiety as a predictor of behavior are not surprising since somebody who is anxious about a task may still feel able to undertake that task and may perform well (consider the example of public speaking where you may feel anxious yet retain confidence in your ability to perform well). These data show the utility of self-efficacy but do not bear on the issue of causation.

Bandura argues the obvious case that human behavior is more complex than that of experimental animals and that self-efficacy can predict such complex human behaviors as career choice. But this is not at issue. I agree that self-efficacy may be able to predict complex human behavior. We disagree where Bandura wants to say that the person's career choice is determined by his or her self-efficacy. I would assert that the person's self-efficacy is an index, in this case of the various performances relevant to the career choices available. That index is modifiable, in the case of humans, not only by actual performances but also at least temporarily by such things as bogus feedback, mood, fatigue and the effects of drugs.

Bandura contends that, contrary to my assertion, "thoughts about personal efficacy are not a 'hypothetical construct'" (Bandura, 1995, p. 185). He then goes on to argue the phenomenological reality of thoughts, the

usefulness of "thought probes" as a research method and the value of studying phenomena not available for direct observation using the example of physics to show how such a process can be productive. But his criticism involves some legerdemain since I have not nominated "thoughts about personal efficacy", but self-efficacy *itself* as the hypothetical construct. I do not contest the phenomenological reality of thoughts (does anybody?) and I have no argument with the value of studying unobservable phenomena.

Although not central to the current debate, some comment must be made about Bandura's amusement at my comment that self-efficacy theory is a derivation of Rotter's social learning theory (Bandura, 1995, p. 186). I refer readers to Kirsch's (1986) paper from which this conclusion can be understood. In this paper, Kirsch has said "Self-efficacy is not a new concept; it is a new name for a construct that has been known to the community for some time" (Kirsch, 1986, p. 354). Kirsch also says that "It is also clear that self-efficacy theory can be deduced from Rotter's social learning theory" (p. 342). Feather's research program (e.g., Feather, 1995) has also long used the concept of expected success, which Kirsch suggests has now been renamed as "self-efficacy".

#### Broader Issues

Causation has long been a problematic in the behavioral sciences, as illustrated by decades of argument about whether attitudes cause behavior or whether behavior causes attitudes (Olson & Zanna, 1993). Cause is problematic in the self-efficacy literature as well. There is a sense in which the debate seems paradigm limited. Alternative approaches have been developed and may be productive. Discourse analysis is one theoretical model which has attempted to circumvent the debate about the reality or nonreality of mental entities. This approach contends that "much of our vocabulary of mentalistic terms has no 'inner' referent at all; instead of being merely descriptions of mental

states these words are themselves an autonomous part of particular social practices” (Potter & Wetherell, 1987, p. 179). Potter and Wetherell use the example of the term “understanding” to illustrate their point and the following long quotation is included to clarify their meaning:

Take the term “understanding” as an example. When first thinking about this word, we are probably tempted to view it as a description of some private or inner experience. However, when we look more closely at the way the this word is actually used this view seems less convincing. Ryle has pointed out that this term is often used to mark a claim to success, the sort of situation where one might say “I have been working at the problem and I think I understand it now”. However, merely having a moment of insight or a feeling of having cracked the problem is not sufficient. If the person with the feeling of comprehension tries to apply their ideas and finds they do not work, they will know that they were mistaken in the belief that they understood.

Furthermore, if *other* people assess the understanding gained, they may decide that the person only *thinks* they understand—they do not *really* understand. The general point is that although cognitive processes are clearly going on, and people without a brain certainly do not understand, this is not a sufficient condition for understanding. Understanding is assessed by *public criteria and practical tests*. The term understanding is properly used when these criteria can be, or have been, satisfied, not merely when people have a certain experience. Similar arguments have been used with a variety of other terms, such as “knowledge” (Harre, 1983), “memory” (Coulter, 1979a, 1983; Harre, 1983), “belief” (Coulter, 1979b), “intention” (Anscombe, 1957), “envy”, “anger” (Sabini & Silver, 1982) and “motives” (Mills, 1940; Peters, 1959; Sharrock & Watson, 1984). (Potter & Wetherall, 1987, p. 180.)

In adopting a discourse analysis perspective, Potter and Wetherall (1987) explicitly want to avoid “linguistic imperialism which denies all significance to cognitive processes” (p. 180), but they suggest that discourse analysis can avoid fruitless debates about the reality or nonreality of mental entities.

I regret any infelicities which have led Bandura to react so strongly to my previous paper. Bandura’s work must be given credit. He is the architect of an influential theory in modern psychology. This theory is valuable regardless of the argument about causation. Nonetheless, there is still room for debate about the “dichotomy between heuristic promise on the one hand, and conceptual propriety on the other” (Borger & Cioffi, 1970, p. vi).

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