WHAT ARE THE BENEFITS FROM A DEBATE ABOUT COGNITION, BEHAVIOR, AND CAUSALITY?

RUSSELL M. F. HAWKINS
University of South Australia, Australia

Summary — Some authors have argued that the debate about whether or not self-efficacy can be legitimately viewed as a cause of behavior is futile due to incommensurability between underlying philosophies. Nevertheless, the debate provides a valuable case study of comparative paradigms in psychology for teaching purposes. It also stimulates a secondary debate about whether clinical utility is a sufficient criterion in clinical decision making. Another consequence of the debate has been a reminder of the negative consequences of the focus on the individual in contemporary psychology. Copyright © 1996 Elsevier Science Ltd

The collection of papers in the recent special issue of this journal concerning cognition, behavior, and causality (Journal of Behavior Therapy and Experimental Psychiatry, Vol. 26, No. 3, 1995) can be divided into three groups. Some papers take one side or the other of the argument that cognitive models (Bandura's concept of self-efficacy in particular) are not able to meet the criteria for demonstrating causal links with behavior and attempt to substantiate their position through argument and/or evidence. Some papers take the position that the argument cannot be resolved due to fundamental philosophical differences between proponents and some also assert that the different perspectives are based in philosophy, but then argue the superiority of a particular philosophy.

The papers represent the various views clearly and persuasively; what then is to be gained from a further round of the debate? Certainly not any resolution of the conflict. However, the debate relates to broader issues in psychology and, in this context, teaching and clinical practice will be discussed.

It is interesting to substitute any mental concept other than self-efficacy into the current debate. I have previously mentioned the examples of love, lust, shame, and patriotism (Hawkins, 1995) and this list can be easily expanded. What is the status of these concepts? Many of them are phenomenologically self-evidently real and that is our quandary. Hayes and Wilson put it well when they said "the rejection of the causal efficacy of cognitions has nothing to do with a rejection of the importance of cognitions" (Hayes & Wilson, 1995, p. 242). From conscious experience we have a compelling understanding of categories of experience.

As soon as we attempt to elucidate the dimensions of these experiences to others or even to ourselves we run into difficulties. Poets and skillful authors sometimes manage to eff the ineffable but as scientists we tie ourselves up in knots.

Garcia (1995) appeals for a tolerance of multiple cognitions. This he illustrates by showing

Request for reprints should be addressed to Russell M. F. Hawkins, School of Psychology, University of South Australia, Magill, SA 5072, Australia.
that views of astronomy based on either a flat earth, earth as the center of the universe, or earth as merely a part of a much larger system can all be adaptive in different circumstances.

This notion of a frame of reference driving how one views cognitive models is a repetitive theme in the last round of debate. Although the points of difference between debate participants have not been resolved, the exchange of views does prompt comment on the implications of the impasse.

Teaching

While considering the issues involved, I have at times found myself thinking that the whole debate is irresolvable, perhaps even trivial and that we would be better off allocating time to more applied research than to such esoteric, theoretical nit-picking. At other times though, it has seemed that the issue is actually very important indeed and that views about causation are central to the whole world view taken by psychologists. These two views may be capitalized on in teaching. In our university there is a belief that psychology students should be encouraged or even required to take a subject of the type “Conceptual foundations in psychology” which would expose them to some history of psychology, some philosophy of science, and encourage them to see how current approaches to psychology have evolved and may be paradigm limited. The subject which offers such a curriculum was in 1995 not taught due to insufficient student demand. The consequence may well be that students have not been encouraged to confront the alternatives to status quo psychology, they may not necessarily see alternatives to dominant schools of thought, and they may well perpetuate a conservatism in psychology. The student reluctance represents a preference to study subjects which have immediate and obvious applications.

The present exchange of views may serve a role in providing a case study for learning and teaching in subjects such as “Conceptual foundations in psychology.” Lee has provided a provocative and critical analysis of contemporary psychology which is seen as too apolitical and conservative (Lee, 1995). Her paper points to a way of meeting student needs for something practical and useful. By pointing out her notion that such issues as poverty and violence are not well dealt with by the status quo, individualistic psychology students may be encouraged to take “conceptual foundations-type” subjects which offer a comparison of alternative frames of reference within psychology.

Some time ago Diespecker said:

The concept of consciousness still has not achieved respectability in psychology teaching programmes. In my experience the notion of consciousness is viewed with considerable suspicion in the psychological community. Paradoxically, many students are not only interested in studying consciousness—they have read some of the many currently popular paperbacks ... it is sad for me that so little of psychology can be experienced in teaching programmes and that so much of what we teach as psychology is profoundly boring for students. It seems to me that in teaching psychology we may unwittingly deflect our students from what it truly is. It is as if students know intuitively that Psychology implies the psyche, mind, consciousness, our magical powers of imagination, creativity, and intuition. Many are disappointed with what they regard as undue emphasis on behaviour, measurement, prediction, data, and experimentation. We encourage the view that experiment in psychology is all-important and that we can successfully control or exclude variables. We cannot realistically separate ourselves from what we study (Diespecker, 1978, p. 422).

A challenge in the teaching of psychology is to pay heed to Diespecker’s criticism while nonetheless avoiding “pop psychology” and retaining the critical, scientific and empirical approach. There is room to study subjective data. Fortunately, psychology offers a rich history in uncovering inadequate causal explanations. Classic lessons are available from Orne’s notion
of demand characteristics, Rosenthal's experimenter bias research, the Hawthorn studies, conformity research, attribution theory and so on. Awareness of this history, together with skills in experimental design and methodology (skills which psychology prides itself on and which help differentiate the discipline from other social sciences) should be sufficient to foster research into consciousness without too readily being deceived by inadequate explanations for patterns of behavior. Yet, the current debate illustrates the difficulty of using science to resolve questions about consciousness. Catania (1995), for example, views Bandura's position as a form of creationism in behavioral science.

While critics of cognitive models have been active in their criticisms, "crusade" is too strong a descriptor of the process. "The crusade is not against the causal efficacy of human thought, it is instead a crusade for the application of rigorous requirements for inferring causal variables of any kind" (Layng, 1995, p. 257).

In this spirit of trying to protect the legitimacy of subjective experience, Locke's admonition that "it is time we took consciousness seriously" (Locke, 1995, p. 272) fits well. Yet, his perspective is confusing since while he claims that "Concepts like the 'id' or 'instinct' which cannot be validated by introspection, and are usually just labels for observed behavior, are not objective psychological concepts and are not legitimate as explanations of human action" (p. 272), he somehow does not see self-efficacy as in this category at all. Somehow self-efficacy is "not hypothetical but real" (p. 272). In addressing the difficulties involved in the validity of subjective report, Layng described research examples which effectively showed that demand characteristics could account for subject responses.

The problem of the validity of verbal report is well known. Conflict between objective and subjective data is often illustrated in the hypnosis literature. Sheehan and Perry (1976) have, for example, described Hilgard's reliance on verbal report as both a strength and weakness of his theory of hypnosis (e.g., Hilgard, 1977). It is possible in hypnosis to produce an analgesia for a stimulus normally regarded as very painful. When this is done, the objective data such as blood pressure continue to be indicative of the pattern known when people are in pain. Yet, if the subject is asked for a verbal report, they will say they do not feel pain. Which is the more valid in this circumstance, the objective or the subjective data? Clinically, we accept the verbal report. If a person previously complaining of pain no longer reports pain after hypnotic intervention, that is sufficient. It does not really matter if the person's physiology is inconsistent. Of course, there is a whole separate debate about the validity of such denial of pain (e.g., see Barber, 1969) but instances such as a woman returning to have hypnotic analgesia for a subsequent birth following an earlier pain-free birth justify faith in verbal report in this instance.

Clinical Practice

Corrigan suggests that trying to determine whether efficient or formal models of causality are better should be put aside in favor of rapprochement. This is essentially a plea to recognize the utility of cognitive models. "We cannot ignore the utility of cognitive models merely to satisfy the demands of those who insist on such a narrow epistemology built on direct observation and efficient cause" (Corrigan, 1995, p. 213). Such a rapprochement is quite possible at an everyday practical level, but it also defers attempts at resolution of the fundamental conflict of views. In day to day clinical practice, I find cognitive psychology useful for my patients, but I nonetheless question the validity of its models.

In a refreshingly honest approach to one of their texts which describes applications of a clinical model, Grinder and Bandler (1981) explicitly disavow any pretense that their model must be
valid. The criterion they wish to emphasize is usefulness, not validity. So often in clinical psychology this issue re-emerges. If a person walks into our consulting room and reports a disturbing or curious dream which they believe is linked to their symptoms, is the better therapist the one who matches the client in their model of the world and incorporates discussion of the dream into therapy or the therapist who attempts to educate the patient about the lack of validity of dream analysis and persuades them instead to attend to other issues as dictated by the therapist's own brand of therapy? The pragmatic view is that the therapist should use whatever works best, but this does not satisfy the therapist who wishes to be scientific.

Spaulding (1995) not only brings a sense of humor to the debate, but provides an analysis which suggests that we would do well to think in terms of systems which are by their nature interactive and reciprocally causal. He reminds us that proximal causes to which our attention is drawn may lead us to neglect other aspects of a system which "may contribute importantly to the identified 'causal' process. Causality is in this sense a heuristic metaphor—an explanatory fiction, no more and no less intrinsically valid than many others, including 'superego' and 'reinforcement'" (Spaulding, 1995, p. 281). Yet, after this statement which seems to support the criticisms of self-efficacy theory, Spaulding later supports self-efficacy theory by suggesting that in that model, we are seeing "gradual and systematic movement towards" conditions sufficient to describe a model as having causal efficacy. Thus, this view sees less of a contradiction between cognitive and behavioral models than other authors have suggested. Spaulding accuses both Skinnerian and cognitive approaches of taking too linear a view of causality instead of recognizing the more complex and superordinate category of systematic causality. Spaulding sees value in Bandura's cognitive model because it "can inform a case-by-case, hypothetico-deductive approach to clinical work, wherein treatment is the independent variable and outcome is the dependent variable" (Spaulding, 1995, p. 282).

In an analogy with the perspective of Dougher (1995) who said that the scientific adequacy of cognitive models depends on whether you are seeking to control or just to predict behavior, cognitive therapy can be valuable if the evidence shows that it helps, even though the implicit causal mechanism may be illusory. A well known clinical lesson is that therapeutic success does not necessarily validate the conceptual model said to explain that success. As a simple example, Bowers has reviewed the treatment of warts by hypnosis. He noted that, while good results could be achieved by suggestions in hypnosis that the blood supply to the warts could be enhanced, thus increasing the availability of infection—fighting white corpuscles, equally good results could be achieved by suggestions of blood starvation to the warts or various other suggestions, thus the implicit rationale for the successes has "limited value as a scientific explanation" (Bowers, 1976, p. 144).

Clinical training in the scientist-practitioner model advocates a reliance on empirical data to guide such clinical decisions as choice of therapy. This model might find a focus on usefulness rather than validity a cavalier approach which should be condemned. In fact, the author has heard advocates of the scientist-practitioner model state that it is unethical to engage in a brand of therapy which has not been empirically demonstrated to be efficacious.

Yet, there is still a lack of clear evidence for clinicians about which models they should use. Torrey (1986) uses a quote from Alice in Wonderland to make this point: "Everybody has won and must have prizes." This is a succinct way of summarizing the following meta-analysis based observation:

Psychotherapy is beneficial, consistently so and in many different ways ... different types of psychotherapy (verbal or behavioral; psychodynamic, client-centred, or systematic desensitization) do not produce different degrees of benefit. No school of psychotherapy has a franchise
on therapeutic efficacy. Indeed, no school of psychotherapy can claim that research proves its effects on a particular problem or type of client are superior .... Those elements that unite different types of psychotherapy may prove to be more influential than those specific elements that distinguish them (Smith, Glass, & Miller, 1980, p. 201).

Torrey (1986), in a fascinating book called Witchdoctors and Psychiatry, has argued that the important ingredients to successful therapy are a shared world view, the personal qualities of the therapist, the expectations of the client and an emerging sense of mastery (self-efficacy by another name?). He says that witchdoctors and psychiatrists alike essentially do remarkably similar things and while there is no doubt about the beneficial results obtained, there is really very little science involved.

The techniques used by western psychiatrists (a term used generically to cover nearly all types of therapists) are, with few exceptions, on exactly the same scientific plane as the techniques used by witchdoctors. If one is magic, then so is the other. The only exceptions are some of the physical therapies, in particular drug therapy and shock therapies (Torrey, 1986, p. 11).

This point of view was picked up also by Hayes and Wilson (1995) when quoting Hollon and Beck (1986): “There is not, as yet, compelling evidence that cognitive therapy works, when it works, by virtue of changing beliefs and/or information processing.” Thus, for purposes of clinical work and for heuristic value in description, the cognitive position is valuable. There is demonstrable utility in such concepts as self-efficacy or similar “cognates of personal control” (Peterson & Stunkard, 1992) such as ego-strength (Hartland, 1971), expected success (e.g., Feather, 1968), locus of control (Rotter, 1992) and explanatory style (Seligman, 1992).

Yet for attempts at understanding it is not useful to be prejudiced too much against alternative perspectives and the criticisms of self-efficacy as a cause of behavior have not been resolved in spite of Corrigan’s curious statement that “formal models of causation need not be hindered by concerns about the direction of a relationship” (Corrigan, 1995, p. 211). Voltaire said those whose duty it is to worship the sun do not study the laws of thermodynamics and we need to have the ability to explore ideas without being treated as zealots.

Future Directions

A pluralist approach seems both inevitable and appropriate. Some clinicians will be content with the utility of self-efficacy and will continue happily to work with the concept. Some theoreticians, with different goals, will be concerned to clarify differences between utility and validity. Some will seek the promise of discourse analysis or Relational Frame Theory to circumvent the debate and others will find “rapprochement” by tolerating the inconsistencies between the different views debated. The consequences of our debate are practical as well as theoretical. Lee (1995) has reminded us that psychology is apolitical and has ignored “broader issues of inequality, poverty and violence” (although for a significant example of psychologists involving themselves in politics, note the Psychologists for the Prevention of War group which is part of the Australian Psychological Society) and she has argued that it is our attachment to dualist thinking with its consequent emphasis on individuals which has led to this inaction at the macro level.

References


