ON THE LIMITS OF RADICAL BEHAVIORISM:
A REPLY TO LEIGLAND’S REPLY

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Over the past 25 years, often in collaboration with Barry Schwartz, I have written extensively in criticism of radical behaviorism (RB), its variants and successors. My approach has been that of a philosopher of science. I have asked the same kinds of questions about RB as I ask about other research programs in the sciences, including (where applicable) the physical sciences. These questions include, for example, in the first place: What kind of science is RB? What are its historical antecedents? What are its methods, instruments, techniques, vocabularies and theoretical commitments? What are its achievements and limits? What can and cannot be expected from it as a research program and in practical application? How well supported empirically are its fundamental claims? Secondly: How does RB compare—in the scope and power of its applicability, theoretical depth, degree of evidential support, fertility—with other approaches to “scientific” psychology (e.g., cognitive psychology)? How does it interact with results arrived at in other approaches? Thirdly: What are the reasons to engage in RB rather than in (say) cognitive psychology and rather than to rely on modes of understanding, common in daily life, that are not derived from experiment and that generally do not attempt to display human action as lawful?

Leigland (1998) is responding to introductory remarks made about Skinner and RB in a review (Lacey, 1996), for which the supporting arguments and analyses are to be found in the larger body of my writings (see References). In this brief response, I think that the most useful thing I can do is to identify sharply some of the places where Leigland and I disagree (for at times he has...
LACEY intertwined my views with those of other critics of RB\(^1\), and to indicate where I have discussed my views in detail, thereby clearing some of the ground needed to make fruitful critical exchange possible. Above all, while space does not permit me to recapitulate my arguments in any detail, I hope to clarify the broadly empirical character of my argument for the limited explanatory power of RB.

**The Explanatory Power of Radical Behaviorism**

There are limits to the explanatory power of RB (Lacey & Schwartz, 1986, 1987; Schwartz & Lacey, 1982, 1988). Explanatory power is a standard criterion with which scientific theories are evaluated: *ceteris paribus* the theory (of a domain) that explains the greatest range of phenomena in the greatest detail is preferred (Lacey, 1986, 1997a). Schwartz and I have argued that there are behavioral phenomena (including those involving scientists engaged in their scientific work) that cannot be explained with the categories and principles that may be developed within the research program of RB. Our argument deploys the intentional scheme not only to characterize the bounds within which phenomena can be explained using the principles of RB, but also to explain human action outside of these bounds—so that, we conclude, RB illuminates human behavior in the social environments of daily life only where the boundary conditions of these environments instantiate the bounds.

Qua scientific research program, RB should be treated like any other research program. In particular, hypotheses about the applicability of behavior principles to any set of phenomena are subject to the same kind of (rational)

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\(^1\) Leigland and I, so far as I can tell, do not disagree on the following matters: 1) For the conduct of an experimental research program, it is not appropriate to use intentional language to describe phenomena involving animals in typical operant conditioning experiments. 2) Skinner’s program is not one of physicalist reductionism (Schwartz & Lacey, 1982). 3) Skinner aims to produce functional analyses, grounded in experimentally derived principles, of uses of ordinary intentional terms and not to eliminate these uses and to replace them by terms taken from RB analysis—Skinner’s “interpretations” of verbal behavior (Skinner, 1957) are conjectured functional analyses, not “translations” understood in terms of identity of truth conditions, as they are in various versions of “philosophical behaviorism” (Lacey, 1974). 4) All sciences must deploy a technical vocabulary—I prefer to say “theoretical” vocabulary—to achieve some of their specific ends. 5) “Rationality” does not designate “a special mental faculty.” 6) Skinner’s RB has a place for “internal events” of an organism. On this last point Leigland (his Endnote 1) is correct that I lapsed when I wrote that Skinner occasionally relaxed his general views by admitting a role to internal events. I discuss Skinner’s views on internal events elsewhere (Lacey, 1974, 1979a). There is, however, an incongruity in admitting them and characterizing reinforcement as Leigland does as a name “applied to observed relations between environment and behavior,” for within RB these events enter into contingencies of reinforcement.

I will not discuss these matters.
evaluation as are any theoretical proposals in any science or any empirically based beliefs, that is, evaluation that appeals to such criteria as empirical adequacy, predictive and explanatory power, capability to anticipate possibilities, consistency with other highly evaluated theories, fertility (Kuhn, 1977; Lacey, 1997a, 1997b). Note that I do not include “control” among the criteria—for the reasons, and a critique of the notion that the goal of psychology is “the prediction and control of behavior,” see Lacey (1979a). While I recognize that one’s holding a theory (whether of physics or of RB) has a causal history, the rational evaluation of a theory is not reducible to the causal history of its being held. This is a conclusion not a presupposition. The phenomenon of scientists making rational evaluations of theories, a commonplace in scientific practices, can be described in intentional idiom: Judgments are made, after engaging in critical dialogue and controversy, about how well a theory fits with the available empirical data and other accepted theories in view of the criteria of evaluation. There is no evidence that renders remotely plausible that functional analyses of the scientist’s verbal and experimental behavior in relation to environmental contingencies can describe this phenomenon; and any apparent plausibility it may have is parasitic upon making loose and inadequate paraphrases (“translations”) of intentional terms into RB idiom (not “interpretations,” functional analyses of their uses) (Lacey & Schwartz, 1986, 1987; Schwartz & Lacey, 1988).

The Empirical Character of the Argument

My argument is broadly empirical in character, where “empirical” is not identified with “experimental” or “scientific.” In clarifying this, I will make four points. First, the question of the applicability of principles consolidated in experimental research, both to applied phenomena and to phenomena generally in the realm of daily experience, is always open to empirical scrutiny regardless of the science under discussion (Lacey, 1984, 1986, 1999). Yes, it applies equally to Newton’s Laws as to principles of RB; “tired complaint” or not, it pertains to an essential matter of the evaluation and generation of theories. Of the great founders of modern science, Newton grasped this point

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2 I presuppose this—in the sense of “take to be uncontentious”—in my argument about the limits of applicability of the theories developed within RB. But Leigland’s remark, “Skinner’s view of science is a pragmatic view (rather than a ‘rationalistic’ one),” gives me pause. Is he suggesting that, because Skinner holds the aim of science to be prediction and control, the theories he produces are not subject to evaluation according to the same rational criteria as any other scientific theories?
most clearly, and it was central to his most famous achievement, the explanation of the motions of the planets (Lacey & Schwartz, 1986; Schwartz & Lacey, 1988). In order to test severely the scope of application of his laws of motion, Newton drew upon the empirical charting, provided by the astronomers, of the motions of the planets (a nonexperimental nonapplied phenomenon), summed up in the empirical regularities (Kepler’s laws) of planetary motion. This enabled him to demonstrate (Principia, Book III) that, in conjunction with a further law (gravitation) that he derived in the same process, the laws of motion could be extrapolated to explain the motions of the planets. That the extrapolation from experimentally established (and practically applied) principles to phenomena in general cannot be taken for granted is clear when one remembers that Newton misextrapolated his principles to the passage of light.

Secondly, it is difficult to chart empirically the spaces of ordinary human behavior, social relations, and their histories. Moreover, any relevant charting needs to be sensitive to potential variations with social context, institutional location, cultural tradition and historical moment. Providing a sketch of such a charting, Schwartz and I have argued that only where social settings have become “closed” under special sociohistorical conditions do we find clear exemplifications of approximations of behavior principles. As already mentioned, our argument deploys intentional categories which also apply outside the bounds of closed settings—and we can interpret behavior inside closed spaces as “degenerate” cases of intentional action. From the intentional perspective, then, we get greater explanatory power, including an explanation of the applicability of behavior principles being limited in the way we specify; but from the perspective of RB we get no corresponding or competing story about intentional explanations—hence our conclusion about the bounds of applicability of RB. The detail, of course, is critical in an argument of this type; it is provided in several writings (Lacey & Schwartz, 1986, 1987; Schwartz & Lacey, 1982; Schwartz, Schuldenfrei & Lacey, 1978).

Thirdly, predictions follow from Schwartz’s and my analysis: in particular that clear exemplifications of principles discovered in the research program of RB will not be found outside of the bounds we specify. Thus, developments of RB could refute our analysis—just as developments of Newtonian mechanics could have refuted the relativistic prediction that Newton’s laws would not be exemplified at speeds approaching that of light. Adherents of RB have great confidence that our predictions will be refuted. There is nothing in the general character of science that grounds that confidence. Neither, we have argued in
detail, do the successes of applied behavior analysis ground it (Schwartz & Lacey, 1982, ch. 8; 1988). What does ground it?

Fourthly, variants of our argument apply to all approaches to “scientific psychology,” that is, to all approaches aiming to display the lawfulness of behavior, with experiment as the central source of evidence (Lacey, 1979b; Lacey & Rachlin, 1978). Our argument is empirical but not “scientific” in this sense. It is part of a systematic, empirically-based inquiry that deploys intentional categories, and an array of social science and historical methods. While such inquiry does not aim to represent phenomena lawfully, it produces narratives often of considerable generality of scope—see, for example, our account of the formation and transformation of values (Lacey & Schwartz, 1996).

I referred (Lacey, 1996) to the explanatory force of explanations put forward in the intentional scheme as resting on the “presupposition that actions are performed for the sake of ends in the light of the agent’s beliefs about means,” and to this presupposition being the counterpart of the presupposition of scientific explanation that events are lawful. Any mode of explanation has presuppositions (or background ideals) about the form that explanations should take. Clearly metaphysical views (among other things) influence what one takes to be the explanatory ideal (Lacey, 1980, 1999)—in the present case,

3 I have not reviewed all the recent applied studies that Leigland references, and I acknowledge the burden to check out how well Schwartz’s and my analysis of earlier applied studies, and my criticisms of Rachlin’s (1995) account of self-control (Lacey, 1995), hold up in the light of them. That cannot be done in this brief response. Note that we (Lacey & Schwartz, 1986, 1987) do not, as Leigland implies, define closed spaces as those in which functional variables are controlled with more precision. Our conclusion is empirical not tautological.

4 Just as Schwartz and I argue that, under specific sociohistorical conditions, human beings can become largely as they are portrayed under RB, so Dreyfus suggests that, under different sociohistorical conditions, human beings may become largely as they are portrayed in cognitive science:

Man’s nature is indeed so malleable that it may be on the point of changing again. If the computer paradigm becomes so strong that people begin to think of themselves as digital devices on the model of work in artificial intelligence, then since . . . machines cannot be like human beings, human beings may become progressively like machines. . . . People have begun to think of themselves as objects able to fit into inflexible calculations of disembodied machines. Our risk is not the advent of superintelligent computers, but of subintelligent human beings (Dreyfus, 1992, p. 280).

5 For the arguments that deployment of the intentional scheme does not lend itself to the formulation of laws, see Davidson (1980) and especially Donagan (1987). Note that there is a usage of “scientific” in which Schwartz’s and my kind of inquiry counts as scientific. On this usage, but not the narrower one, Aristotle’s psychology is scientific. I mention this because Rachlin (1996) seems surprised that I contrast contemporary Aristotelian approaches with “scientific” ones.

6 Note that this sense of “presuppose” differs from my earlier usage in Note 2, “uncontentiously hold.” Ambiguity unfortunately marked the use of “presuppose” in Lacey (1996) and no doubt caused some misunderstanding of the thrust of my argument.
depending on whether one thinks of the nature of human beings in terms of agency (as I do), or one thinks, “A person is not an originating agent; he is a locus, a point at which many genetic and environmental variables come together in a joint effort” (Skinner, 1974, p. 168), one will be drawn respectively to the intentional or the lawful ideal. Either way metaphysics is involved. But metaphysics is not the last word. Explanatory ideals are subject to the test of their explanatory power, in the long run to an empirical constraint.

The point of the “dilemma,” which Leigland dismisses, comes in this context. To say that science is rational, I assume, implies that it is a practice in which theories are (should be!) accepted and rejected on the basis of how well they fare in the light of the available empirical data with respect to the appropriate criteria of evaluation. The very characterizing of science as rational (and, if it is not rational, what special claim does science make upon us?), then, involves intentional acts described in intentional idiom. Furthermore, all arguments for RB (or any other comprehensive approach to psychology) are formulated in the same idiom. As such, these acts and arguments cannot be formulated now in the language of RB (or any other framework that presupposes lawfulness). Suppose that RB were to develop, contrary to my predictions, and increasingly give accounts of more and more behaviors outside of closed settings. Then, in accord with the rationality of science, we should adopt the RB theory of these behaviors—and, if it extended to include the very behaviors of evaluating and accepting theories, we should accept it of them too. But, in applying a RB theory to this last act of acceptance, we would be accepting that the act was adequately accounted for by its causal history, and so it is not an act of evaluation—but that is itself making an evaluation. The point is that intentionality is ubiquitous and that whenever we appear to have got rid of it somewhere, it crops up again somewhere else. Neither Leigland nor Skinner wants to eliminate intentional terms from our verbal behavior, but they do want to eliminate intentionality from a role in the explanation of behavior. When one pushes the attempt, one confronts the dilemma.

Rachlin sees this argument as “arrogant” and (associating it with an argument of Danto) says, “They assume that their respective theories of the meanings of mental terms must be right and that anyone who uses these terms must automatically accept their respective theories” (Rachlin, 1996, p. 82). In fact, I assume only that the intentional story is the best one around, judged in the light of the usual criteria of evaluation, most notably explanatory power,
and that adherents of RB are unable to explain (in their own terms) their own behavior of evaluating and accepting their own theories; the dilemma shows that they have no way of articulating in their own terms, even speculatively, those acts that currently we describe as rational evaluation of theories.

Things might change! If they do, we will have to deal with the consequences. Meanwhile, no support for the expanded explanatory power of RB is gained by appealing to the “two vocabularies” argument. When dealing with the actions under discussion there are not two vocabularies, the intentional and the behavioral; there is only one. True, there is a vocabulary being developed in the program of RB and it is of recent origin, but it is sheer speculation (far distant from experiment, and inarticulate in the face of actions of evaluation and acceptance of theories) to assume that it can come to illuminate these actions; certainly, saying that “some of the nonverbal and verbal practices of sciences may be described in terms of verbal rules, social conventions, and the like” only intensifies the sheer speculative character of the expanded explanatory power of RB.

Of course, there is nothing wrong with speculating or with engaging in a research program hoping to ground a speculation—so long as one is aware of what one is doing, and provides reasons for doing it. My hunch is that Leigland’s reason is expressed in the following passage: “... continuing to ask the relevant question of ‘why’ eventually leads from such [intentional] terms to the biology, history, and context of activities of the individual; that is, to the factors of interest to behavioral science.” The reason, in short, is metaphysical.

Conclusion

As I read them, the defenders of RB typically hold (in one version or another) the following views: that the general canons of scientific practice somehow especially endorse the approach of RB, from the outset, prior to the empirical and theoretical unfolding of its research program; that its applied successes give testimony to its fine scientific credentials; and that the arguments of their critics, and not their own, spring from the grip of metaphysics. Leigland is no exception. If these views were directly addressed with sharp arguments, the debate about RB could be greatly enhanced.
REFERENCES

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