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INTENTIONAL EXPLANATIONS AND RADICAL BEHAVIORISM: A REPLY TO LACEY

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In a recent review of two books on contemporary behavioral psychology, Lacey (1996) began with a brief critical overview of "behaviorism" in general and "radical behaviorism" in particular. The purpose of this commentary is to respond to these two sections of Lacey's review, rather than to respond specifically to Lacey's reviews of the books by Staddon and by Rachlin. Lacey's (1996) comments regarding radical behaviorism were clearly intended to serve as brief summaries of more extensive arguments published elsewhere (e. g., Lacey & Schwartz, 1987), as noted in his review. The nature of the summary arguments presented by Lacey (1996) in his review and in his more extensive treatments (e. g., Lacey & Schwartz, 1987) have prompted the present summary comments in reply. It is hoped that this might constitute a small step in furthering increased communication between philosophers and psychologists.

Generally speaking, Lacey sees scientific psychology in general, and "behaviorism" in particular, as a productive, if seriously limited, enterprise. Lacey outlines several types of philosophical problems which are intended to illustrate these limitations, since scientific psychology (and thus behavioral psychology) is apparently incapable of dealing effectively with these problems in the sense of "explaining" them (whether philosophy has ever adequately solved or explained any of these problems or issues is not addressed in the review).

Of particular interest to this commentary is Skinner's radical behaviorism and the line of "intentionalist" criticisms which Lacey has sought to bring against it. The most significant problem in Lacey's attempts to do so is that Lacey shares with most philosophical critics of Skinner the practice of heaping a variety of unnecessary philosophical assumptions onto Skinner's position, assumptions Skinner himself had never adopted, and then holding him responsible for inadequate treatment of the "issues" derived therefrom. Skinner's radical behaviorism is a systematic position for the development of a particular version of

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psychological science, and as such, it does not engage issues of traditional philosophy on the traditional playing fields, so to speak, but rather using different terms, different criteria of explanation, different rules of engagement. In this sense, Skinner's work shares with the later work of Wittgenstein (e. g., Day, 1969) and the pragmatic philosophy of Richard Rorty (e. g., 1991; Leigland, in press), for example, the characteristic of attacking traditional philosophical problems from nontraditional angles—angles that are inescapably historical, cultural, and linguistic (rather than, say, foundational, metaphysical, or rationalistic).

The Intentionalist Criticism

Lacey's "intentional criticism" of Skinner's radical behaviorism may be the most frequently employed criticism of Skinner's work by philosophers (e. g., Dennett, 1978; Flanagan, 1991; cf. Baum & Heath, 1992; Leigland, 1996b). The alleged problem concerns the widespread use and understandability of the "intentional scheme" (or, talk involving intentional idioms) by "human agents" (or, people) when explaining behavior. The criticism normally takes the following form: that is, that Skinner's science forbids the use of intentional idioms as part of the vocabulary of the science, yet an understanding of the former is required and necessary if the latter is to be understood. One version of the criticism has been summarized by Flanagan (1991) in the following way:

> The true behavioral laws Skinner comes up with in situations such as [the operant experimental laboratory] make sense precisely because there are true mentalistic laws which underlie them. That the animal pecks or paws at rate *x* in the presence of stimulus *s* on schedule *r makes sense* because we know that any organism at 80 percent of normal weight is hungry and desires food. (Flanagan, 1991, pp. 96-97; emphasis added)

Lacey's argument is similar to Flanagan's in that both propose that intentional talk involving wants and beliefs in the everyday explanation of behavior must be reckoned with by any version of scientific psychology, since such intentional talk must be taken as a *presupposition* which provides the "grounding" of any scientific explanation, and without which the scientific explanation could not be understood. A problem confronting any version of scientific psychology, then, is how to explain, replace, or reduce such intentional explanations within the systematic framework of the science. Lacey summarizes the problem in the following way:

> . . . we cannot talk intelligibly about the practices engaged in within our research program—at least at present—except with the categories grounded in [the supposition involving intentional terms and concepts]. [The intentionalist] claim comes from the fact that all of us, in the course of learning our first language, routinely pick up and use without difficulty the descriptive and explanatory capabilities that presuppose [intentional categories]. (Lacey, 1996, p. 63)

In addressing questions posed by philosophers, it is worth emphasizing that radical behaviorism is a perspective directed toward the development of a certain kind of psychological science. Thus the ways in which radical behaviorists would approach traditional philosophical questions would generally be viewed as psychologistic and interpretive. Nevertheless, it is the case that any comprehensive psychological science must at some point engage any and all of the traditional philosophical domains, addressing any and all of the questions arising from metaphysics, ontology, ethics, spirituality, or any other domain of inquiry or experience.

To the extent that any such questions engage issues of human language, culture, and history, the interests of any comprehensive psychological science will be engaged as well. This is not to say that the scientific analyses will in some sense "answer" the questions within the verbal framework of the question itself (any more than they will find sufficient answer in philosophy), but such analyses may have something to say about the framework, so to speak. Speaking metaphorically, such analyses might help to clarify the functions and rules of the language games involved in such questions, and the senses in which such questions are regarded as important in the context of the cultures in which they occur.

Intentional Idioms and Verbal Behavior

In assessing any possible relevance of Lacey's "intentionalist criticism" to Skinner's radical behaviorism, it is of critical importance to understand Skinner's views of science and of verbal behavior. Regarding the latter, it is instructive to begin with a brief examination of Skinner's "translations" from ordinary language into the technical vocabulary of contingencies of reinforcement (see also Leigland, 1996b). Such "translations" have occurred frequently in Skinner's writings (e. g., Skinner, 1953, 1957, 1964, 1969), and have been a frequent target of critics (e. g., Koch, 1976; Malcolm, 1964; cf. replies by Day, 1992 and 1977, respectively; also Leigland, 1996b). Representative examples may be seen in the following:

> Frustration is rather a different condition, which includes a tendency, often characteristic of a failure to be reinforced, to attack the system. . . . The

> expression "frustrated expectations" refers specifically to a condition produced by the termination of accustomed reinforcement. (Skinner, 1974, p. 58)

> Most reinforcements occur intermittently, and the schedules on which they are programmed generate conditions which are described with a wide range of terms. The so-called ratio schedules supply many good examples. When the ratio of responses to reinforcements is favorable, the behavior is commonly attributed to (1) diligence, industry, or ambition, (2) determination, stubbornness, staying power, or perseverance (continuing to respond over long periods of time without results), (3) excitement or enthusiasm, or (4) dedication or compulsion. (Skinner, 1974, p. 59)

> *It is of no advantage to say* that [occupational] therapy helps the patient by giving him a "sense of achievement" or improves his "morale," builds up his "interest," or removes or prevents "discouragement." Such terms as these merely add to the growing population of explanatory fictions. One who readily engages in a given activity is not showing an interest, he is showing the effect of reinforcement. We do not give a man a sense of achievement, we reinforce a particular action. To become discouraged is simply to fail to respond because reinforcement is not forthcoming. (Skinner, 1953, p. 72, emphasis added)

The standard criticism of such "translations" is that they constitute an attempt to reduce the mentalistic talk found in ordinary language to some sort of physicalistic foundation. This is not the case, however, since the technical vocabulary of behavior analytic science is itself not reducible to the language of physics (i.e., movement through space over time; for discussions see, e. g., Catania, 1998; Day, 1977, 1992; Leigland, 1993, 1996b). The technical terms of behavior analysis (such as "reinforcement," "discriminative stimulus function," etc.) may be viewed as abstractions (or abstract tacts; Skinner, 1957) in which names are applied to *observed relations* between environment and behavior. No "physical foundation" may be found in such cases, as there is no way to turn the talk about such abstracted relations into talk about "merely physical" movements through space over time; the "physical" and the "behavior analytic" vocabularies may be viewed as different ways of verbally interacting with the world depending upon context (for an analogous argument, see Rorty's, 1991, "Non-reductive Physicalism"; Leigland, in press).

Rather than physicalistic reduction, it could be said that Skinner's "translations" involve *pragmatic reformulation*. That is, the move to "translate" is not to say that the ordinary language mentalist/intentional terms are somehow "not meaningful," or that they are not useful in certain contexts (e. g., see Skinner, 1957, on literary vs. scientific verbal behavior); nor is it to say that the technical scientific vocabulary is "what is really happening," or that it is in some sense "closer to reality" than ordinary language. The move to "translate" is not to say

that the technical scientific vocabulary is the "underlying truth" to which the natural language may be reduced, or is the foundation upon which the "meaning" of the natural language statement rests. From the perspective of radical behaviorism, both types of statements are "meaningful," but may be said to serve different *functions* depending upon context. This is the sense in which Skinner (1953), in the third quote listed above, spoke of "advantage" in comparing the ordinary language terms with the technical scientific vocabulary.

But advantage for what? The advantage is seen by those who, like Skinner, "demarcate science in a merely Baconian way" (to borrow a phrase used in a different context by Rorty, 1991, p. 60). That is, from the perspective of radical behaviorism, science is not to be distinguished from other human activities on the basis of a special "rational" characteristic, or because science is metaphysically "privileged" with a special means of glimpsing the true reality which underlies "mere appearances." Rather science, as a kind of human activity, is to be distinguished by a certain kind of practicality or usefulness—a kind usually associated with goals concerning prediction and control (e. g., Skinner, 1953, 1957). As scientific fields have progressed, of course, ordinary language, as meaningful and useful as it is in ordinary discourse (including those cases which Lacey would regard as "explanation"), has given way to an emerging technical scientific vocabulary which enables effective action with respect to the subject matter of the field.

To say that there are different vocabularies under a given state of affairs (e. g., the technical and the lay vocabularies, or the physiological/technical and the behavior-analytic/technical) is not to say that the vocabularies are fully translatable or "equivalent" in the sense of complete interchangeability or substitutivity under all conditions, since the different vocabularies serve different functions (cf. Rorty, 1991; e. g., the chemist is not likely to say, "Please pass the sodium chloride," to her grandmother at Thanksgiving dinner). Thus it is not the case that one vocabulary (say, the lay vocabulary) is likely to be "eliminated" from use by the development of an alternative vocabulary (say, the technical vocabulary of contemporary chemistry, where "replacements" are available). It is also important to note that neither vocabulary is viewed as the "foundation" for the other in any ontological or epistemological sense; rather, they are simply "different ways of talking about the same or similar things," to state the matter informally.

Intentional versus Scientific Explanation

Contact may be made with Lacey's "intentional criticism," then, in the following way. First, we may say that there are different ways of speaking about

human actions; we have the vocabulary of everyday language and the emerging technical vocabulary of behavior analysis as examples. The former has a long and evolutionary/etymological history in human culture, and serves the purposes associated with ordinary discourse. The pervasiveness of this vocabulary, to paraphrase Lacey (1996), "comes from the fact that all of us, in the course of learning our *first* language, routinely pick up and use without difficulty the descriptive and explanatory capabilities" (p. 63, emphasis added) that involve intentional idioms. It is a monumental and entirely unnecessary assumption, however, to move from this "fact" to the contention that this sort of temporal and cultural priority must carry with it all of the metaphysical heavy-lifting described by Lacey.

The fact that we learn to speak about behavior in a particular way early in life and in broad social contexts may "mean" nothing more than just that. To say that this way of speaking constitutes a "scheme" which involves a "presupposition" of "rational connections" is to add a great many verbal "connections" to the original statement indeed, but it doesn't constitute a problem for a scientific technical vocabulary, unless the scientist were to contend that the original vocabulary would or should be eliminated by the scientific one. This is not, however, what is being advocated by Skinner and other radical behaviorists.

Rather, the point that is being made is that ordinary language is not sufficient to enable effective action in certain contexts. In the chemistry laboratory, one who is armed only with traditional distinctions, descriptions, and modes of "explanation" will be incapable of producing more than a mess, while the contemporary chemist, armed with a special history of training and the verbal repertoire which makes it possible, is in the position of producing materials and events which would certainly be regarded as miracles by the medieval alchemist. Similarly, the sort of explanation described by Lacey is fine for ordinary discourse ("He did that because he believed that you wanted it done that way, and he wanted to surprise you," etc.), but the fact is that in many fields of human application as well the human laboratory, a specialized training history and its accompanying specialized vocabulary is likely to be necessary if one is to replace the making of a mess with effective action.

To take what may be the simplest possible example (many more complex examples are possible), it is clear that the "intentional presupposition" described by Lacey is relevant to "pigeon agents" as well as to "human agents." That is, in the explanation of the actions of a pigeon in an operant chamber, the language of intentional categories is applied quite easily by human observers. Students in the undergraduate laboratory course find it difficult to describe the behavior of the

pigeon (or rat) in anything but intentional idioms, at least at first. The laboratory situation has even been developed into a procedure for studying the conditions of observed environment-behavior interaction which are most likely to evoke "mentalistic" terms in the verbal behavior of observers (e. g., Leigland, 1989, 1996b). The fact that pigeons may be viewed by philosophers, for example, as "agents" to which Lacey's "intentional presupposition" may be applied may be seen in the above quote by Flanagan (1991).

In the ordinary language of intentional terms, it is easy to apply Lacey's (1996) "practical syllogism" to the pigeon, where it is clearly meaningful to "explain" the actions of the pigeon with Lacey's formula (see pp. 62-63) in the following way: "If the pigeon wants food, and (s)he believes that pecking the key will contribute to bringing about food, then ceteris paribus (s)he pecks the key." Such a demonstration of rationality in pigeons may be supported by other kinds of commonly-occurring statements which include intentional categories; for example, "The pigeon pecks when the key is red and not when it's green because it knows that red means food," "The pigeon pecks the light before the key because it (falsely) believes that such a peck to the light is a necessary part of the sequence that must occur before it will get food," and so on.

In a sense, to take the behavior analytic perspective is to ask the questions of "why" and "how" in the context of the intentional idioms. For example, "*Why* does the pigeon want food?" "*Why* does the pigeon believe that pecking the key will bring about food?" "*How* does it come to know that red means food?" and even, "*What does it mean to say* that the red light 'means' food to the pigeon?" and so on. The factors, variables, and contingencies which are the subject matter of behavior analytic science (of which the concept of "reinforcement" is only one point on a centerless web of contextual/historical interaction) not only provide answers to such questions (provisionally, as is the case in all science), but, because the analysis stays at the same "level" of the phenomena under investigation, it provides particularly useful answers as well.

To take another sort of example, in a recent study on verbal behavior (Leigland, 1996a), it is clear that for one of the participants the question could be asked as to why she talked about the weather for the entire session, when there were no requirements and no instructions as to the topic. Why this particular topic, and why did she talk about it and related themes for the entire session? The simple, everyday sort of explanation would be something like, ". . . because she falsely believed that this particular topic had been designated for discussion for that session." In terms of our ordinary language practices, this was certainly the case, and such an account might even be enough to satisfy the curiosity of some

observers (perhaps even some philosophers). Nevertheless, a description of the contingencies operating before and during the session provide a much more powerful explanation—one that includes, again to speak in everyday terms for present purposes, how such a belief originated, and how it was maintained, and it does so in terms which allow for the arrangement of the conditions (in this case, nonverbal conditions) which produce the phenomena. Where the critic might say that one only gets more intentional terms through such a process, the behavior analyst would reply that continuing to ask the relevant question of "why" eventually leads from such terms to the biology, history, and context of the activities of the individual; that is, to the factors of interest to behavioral science.

A more extensive ordinary-language account could be given of the experimental findings described above with a more extensive narrative, just as extensive narratives could probably be constructed to describe some of the phenomena which may be described by Newton's laws of motion, but the advantage of the scientific/technical account, whether couched in the verbal conventions of a technical vocabulary or in more quantitative forms, is in the efficient abstraction of observed regularities and their application to new situations, contexts, or instances of the phenomena of interest by way of basic research, applied research, interpretation, and so on. Certainly there is no reason to regard the ordinary language terms that occur in a narrative to be serving as some sort of metaphysical *grounding* for the technical terms.

In the analysis of behavior, ordinary language "intentional" terms may be useful for the purposes of description, particularly in complex areas that wait upon an effective scientific analysis (note that Skinner's writings frequently employ such terms as descriptions; e. g., Skinner, 1974; for a discussion of the etymology of "mentalistic" language, see Skinner, 1989b), but they are not considered as useful for scientific explanation. A similar point has been made in the following discussion by Day and Moore (1995) concerning dispositional terms:

> Specifically, how does radical behaviorism deal with dispositional analyses of "mental" terms? Suffice it to say that for radical behaviorism, any particular *description* of behavior will make use of whatever *descriptive* vocabulary is occasioned by the behavior being described, given the idiosyncrasies of the circumstances under which the description is made. The supposedly "mental" implications of some descriptors do not implicate mental properties of the behaving organism to which the descriptors directly or indirectly refer. Instead, the descriptors are regarded simply as instances of verbal behavior, influenced by the particulars of the verbal environment shaping that person's verbal repertoire. Dispositional terms are therefore perfectly reasonable *descriptors* to employ. . . . The major problem concerns trying to generate a causal explanation, rather than a description. (pp. 11-13; emphasis in original)

Skinner's famous criticisms of "mentalism" are pragmatic in character, rather than "metaphysical." Radical behaviorists have no "in principle" argument against the use of intentional idioms, dispositional terms, and the like. Further, Skinner had written extensively and at length regarding the inclusive role of private experience in a science of behavior.[1] Skinner's criticisms of "mentalism" were aimed at a particular type of explanatory practice, rather than particular classes of terms per se. The issue is illustrated in the following quotes:

> We may quarrel with any analysis which appeals to . . . an inner determiner of action, but the facts which have been represented with such devices cannot be ignored. (Skinner, 1953, p. 284)
>
> We are interested in finding terms, not to take traditional places, but to deal with a traditional subject matter. (Skinner, 1957, p. 115)
>
> No entity or process which has had any useful or explanatory force is to be rejected on the ground that it is subjective or mental. The data which have made it important must, however, be studied and formulated in effective ways. The assignment is well within the scope of an experimental analysis of behavior, which thus offers a promising alternative to a commitment to pure description on the one hand and an appeal to mentalistic theories on the other. (Skinner, 1964, p. 96)

Returning to the example of the behavioral laboratory, intentional expressions are every bit as meaningful in such contexts as if a visitor to a chemistry lab were to remark, "Oh, I see; if you mix the gray powder with the white liquid and heat it a bit, you get that amazing effect!" The "intentional category" statements above regarding the pigeon presumably constitute examples of the "explanatory force" of such statements in Lacey's (1996) review, and they are indeed perfectly understandable, and useful enough as we as we attempt to "explain" or "explain away" the actions of people in everyday verbal discourse. But as comfortable and common as such accounts are, they are unlikely to be of much help in *arranging or producing* any of the complex psychological phenomena seen in the pigeon in the standard undergraduate operant laboratory, let alone in the human lab (e. g., see the recent and rapidly-growing area of research on equivalence phenomena; e. g., Sidman, 1994; Hayes & Wilson, 1993). The arrangement or production of such phenomena requires, as it does in chemistry, the advantage of a technical vocabulary and the special history of training that goes with it.

The central issues may be summarized in the following way. Lacey (e. g., 1996) contends that the explanatory power of radical behaviorism (by this Lacey presumably means the explanations of "behavior" [see also Note 1] provided by the field of behavior analysis; e. g., Catania, 1998; Martin & Pear, 1996) rests

upon a "presupposition" concerning a "rational" connection between wants/beliefs and actions. The "presupposition" may be seen in the fact that the "principles" of behavior analysis are not intelligible without traditional "intentional categories" such as those involving wants and beliefs. Thus, in order to demonstrate appropriate scientific explanatory power, the radical behaviorist must be able to account for the occurrence of intentional terms/categories with principles that do not themselves use intentional terms/categories.

Is it possible that there is another way to interpret this problem? Following the issues described in this section, such a reinterpretation may be summarized in the following way. First, different vocabularies arising from different classes of environment-behavior interaction may develop and evolve such that they are under the influence of common (or at least overlapping) phenomena. The fact that scientific fields such as physics, chemistry, and biology characteristically develop a specialized scientific vocabulary shows that the distinctions, extensions, and categories found in ordinary language do not *suffice* for the purposes of science[2] (e. g., Skinner, 1957; cf. Rorty, 1991). Some of the distinctions, extensions, and categories found in ordinary language may be *necessary* for the purposes of effective communication in such fields, however, as these fields constitute a subset of a broader verbal/linguistic community, and the situation is the same for the field of behavior analysis. The fact that (a) one set of verbal practices and terms evolved before another, with (b) the latter practices and terms entailing different goals (again, speaking in everyday terms), and where (c) there is incomplete translatability from the first language to the more recently-developed technical language, and with (d) the former verbal practices learned first by individuals in the culture, where (e) this first language is easily, readily, and commonly used when compared to the specific, precise, and broadly-applicable (given the goals and practices of science) terms and associated practices of a technical scientific vocabulary, may have little relevance to the sorts of "philosophical" verbal relations which Lacey seeks to impose.

Lacey's (e. g., 1996) entire set of arguments against radical behaviorism depends critically upon his "presupposition" concerning the "rational grounding" of behavior-analytic technical terms in everyday intentional terms. Historical priority along with extensiveness and ease of use of the everyday over the technical may mean that the only associated problems of any consequence to be engaged are those of a verbal or linguistic, as opposed to a philosophical, "rational," or "metaphysical" character. If ordinary-language intentional terms are to be endowed with the metaphor of "grounding" at all, it is as well conceived as a cultural or linguistic grounding, since it is with these everyday terms that a

speaker in the English-speaking community must be familiar before any other technical vocabulary, behavior analytic or otherwise, may be acquired.

Is there any way to show unequivocally that a mundane, verbal/linguistic interpretation of Lacey's problems is "the way it really is," as opposed to the multilayered, metaphysical/rational one assumed by Lacey? Probably not; the rules of the respective language games are tightly wound—there are no conditions under which technical talk may not be said, in some sense, to involve a "presumption" of, or "the use of," or to be "grounded in," intentional terms or categories. My purpose here is merely to illustrate that an alternative interpretation of such problems is possible; one which turns such problems into one more class of "psychological" issues to be addressed when the instruments and methods of behavioral science are able.

Closed versus Open Settings

Lacey (e. g., 1996; Lacey & Schwartz, 1987) has argued that even when a case for productive explanation in radical behaviorism can be made, it must be limited to "closed systems"; that is, essentially, settings in which certain variables are controlled for such purposes as demonstration, study, or solving a particular problem. Lacey's concept of a "closed system" is remarkably similar to the general concept of the scientific laboratory, in which a phenomenon of interest is studied in a simplified context for the purposes of seeing certain kinds of interactions more clearly (cf. Skinner, 1956).

Lacey's argument is a specific version of the tired complaint about the limited scope and artificiality of the laboratory. Naturally, the question of the generality of observed regularities from the laboratory context to the context outside is a legitimate one (e. g., Sidman, 1960, 1989; Skinner, 1953, 1971), and certainly it is the case that as one moves from the laboratory to less controlled settings, the application of the abstracted regularities formulated in the laboratory becomes more difficult. Despite the broad judgments put forward by Lacey (1996; Lacey & Schwartz, 1987), however, this places no more limitations on behavior analysis than upon genetics, chemistry, or particle physics.

Although Lacey (1996; Lacey & Schwartz, 1987) had been attempting to formulate an argument that targeted the problem of "closed" versus "open" systems or settings in behavior analysis specifically, the fact is that the argument may be applied equally well to *any* field in which experimental findings obtained in the laboratory are said to apply to contexts outside the laboratory. For example, Lacey and Schwartz (1987) have the following to say about their argument:

> The argument . . . suggests that the exemplification of behavior principles should become increasingly discernible as the setting becomes increasingly closed. Demonstrating that behaviorism has explanatory success in closed settings does not suffice to show that its explanatory principles are comprehensive, if not all settings are closed. (Lacey & Schwartz, 1987, p. 170)

In looking at Lacey and Schwartz's (1987) description of the characteristics of a "closed" setting, it is obvious that a setting becomes increasingly "closed" as more of the functional variables are brought under experimental control, or as those variables thus controlled are controlled with more precision (whether in the laboratory or in an applied research setting). Thus, one might as well say that genetic principles are more discernible as one imposes the constraints of laboratory controls for the purposes of experimentation, and similar statements could be made about the controlled observations and experimental research of Galileo, Sherrington, and Millikan, to name just a few, and Lacey's "discernibility" argument would apply as well to Newton's laws of motion as to "behavior principles." Of course, there *are* limitations on the generality of laboratory findings to contexts which are "open" or less subject to experimental control (for a recent discussion of one such limitation in physics, see Coveney & Highfield, 1995), but it is the custom of science to address such issues through empirical means, rather than simply dismissing the generality of experimental findings through the dubious practices of so-called rational argument.

While it cannot be reviewed here (a very brief sampling follows), a large and rapidly growing body of basic (e. g., Catania, 1998; Sidman, 1994; Steele & Hayes, 1991), applied (Johnson & Layng, 1992, 1994; Martin & Pear, 1996; Sundberg, San Juan, Dawdy, & Arguelles, 1990), and clinical research (e. g., Hayes & Wilson, 1993, 1994; Kohlenberg & Tsai, 1991) have shown very clearly that the contingencies and regularities which have been investigated in the laboratory do in fact extend to human domains, and further that the knowledge can be brought to effective and significant use in a wide variety of applications. Further, the sorts of advances which have been gained from behavior analytic science are most unlikely to have occurred using the sorts of everyday, simple-minded "explanations" which are advocated as sufficient by Lacey (1996; Lacey & Schwartz, 1987). Anyone having doubts on this point should read carefully some the recent and remarkable results from the area of educational technology (specifically, in the teaching of reading, mathematics, and language arts; e. g., Johnson & Layng, 1992, 1994; and if possible, visit one of the schools!).

Lacey's "Dilemma" and Skinner's "Thesis"

An additional issue raised by Lacey (1996) concerns a "dilemma" for science which arises out of the "intentional scheme." Essentially, the alleged problem comes from the notion that the "presupposition" involving intentional terms and talk is itself not derived from scientific practice, and yet it constitutes the foundation for the rationality of science. How is science to deal with such a problem? If science were to formulate a theory which somehow eliminated the "intentional scheme" (presumably by showing that the behavior of scientists is "lawful"), this would, according to Lacey (1996) provide "evidence that wants and beliefs do not exist" (p. 64), and worse still, such a theory would "undermine the rationality of science, and so there would be no reason to believe the new theory" (p. 64). In the face of such a crisis, science would presumably find itself unraveling into a confused, irrational, chaotic state.

While it is true that there are those who hold scientific practices to be "paradigmatic of rationality" and that scientific explanations are "rationally grounded" (Lacey, 1996, p. 63), there are also those in science and philosophy who do not share such assumptions, depending upon what the statements are taken to mean. The "rationality" of science may be taken to mean that some of the nonverbal and verbal practices of sciences may be described in terms of verbal rules, social conventions, and the like. It is more likely, however, that Lacey is speaking of "rationality" as a special mental faculty which is set apart from the crudeness of mere human activity in contact and interaction with the world. If such a special mental faculty is not what Lacey has meant by "rational" then it would be helpful if the usage of the term could be clarified, for if by "rational" Lacey (1996; Lacey & Schwartz, 1987) means some variation on a merely verbal/linguistic theme, then his "dilemma" evaporates as a verbal contrivance without any special "philosophical" interest.

As noted above, and as even a superficial reading of Skinner's work shows (e. g., 1931, 1945, 1953, 1956, 1957, etc.; see also Chiesa, 1994; Leigland, 1992; Smith, 1986), Skinner's view of science is a pragmatic view (rather than a "rationalistic" view). Further, it goes without saying that Skinner's antimentalism would hardly permit the sort of rationalism which is required for the contrivance of Lacey's "dilemma."

The supposed dilemma is invoked again under Lacey's (1996) discussion of Skinner's "thesis" regarding radical behaviorism. In Skinner's (1957) book *Verbal Behavior*, for example, Lacey takes Skinner to be saying, on the one hand, that intentional talk should be eliminated, while on the other hand Skinner is clearly trying to change the readers *beliefs* (which is to say, "beliefs") about verbal

behavior. But here again the contrivance of Lacey's "dilemma" depends critically upon views and assumptions which are not part of the verbal context under examination.

To state the matter briefly, the issue for Skinner and other radical behaviorists is not the language game of metaphysics, but rather the issue of which vocabulary will be most effective given certain goals. Skinner (1957), of course, was speaking of a special kind of science of behavior, and the point is that where the interests of science are engaged (e. g., the prediction and control of verbal behavior), a developing and evolving scientific/technical vocabulary will be most useful or effective. The question is certainly not whether "beliefs exist" for example, since from the standpoint of a comprehensive science of human functioning, such matters are construed as language problems (or problems of verbal behavior) which, in this case, depend critically upon the usages of terms such as "exist" (cf. Day, 1969; Leigland, 1996b; Wittgenstein, 1953). Similarly, while the term "belief" is certainly meaningful and useful enough in ordinary discourse, it is not the case that to "use the term," is to necessarily engage the metaphysical talk which is of interest to some philosophers (cf. Rorty, 1991). As noted above, the question is whether the language game of "beliefs" and related terms will be sufficient to enable effective action in the practical domains of basic and applied science.

History has shown, and continues to show, that ordinary language is not enough when faced with such goals, tasks, and contexts. What has been necessary is the development of a different way of interacting with the phenomena of interest, and the different way of speaking which has facilitated such interactions.

NOTES

1. Lacey (1996) describes Skinner's treatment of private events in the following way: "At times (e. g., Skinner, 1945) Skinner relaxed the thesis. . ." (p. 77), where the "thesis" was that "all of the behaviors that we would be inclined to call *intentional* [etc.]. . .are lawfully related to environmental contingencies" (p. 64). Ignoring for the moment the possibility that Lacey was using the term "lawfully" in a way that was substantially different from Skinner's use of similar terms (cf. Smith, 1986), there are at least two ways in which Lacey's statements are misleading. First, Skinner's inclusion of occurrent, private events into the domain of a science of behavior was described on virtually every occasion he had to describe what he meant by the term, "radical behaviorism" (beginning with his first use of the term in the 1945 paper; e. g., Skinner, 1945, 1953, 1957, 1964, 1969, 1974, 1989a; see also, e. g., Day, 1977, 1980, 1983; Moore, 1980, 1990), and while Skinner did indeed speak of the issue of private events "at times," the times were frequent and consistent with one another over a substantial period of time. Second, Skinner's position on private events is hardly a case of "relaxing" his "thesis," but rather is simply a part of the working assumptions which constitute the scientific system. That is, private events are

construed as fully incorporated within the context of the interaction between "environment" and "behavior"; part of the world context is contained within our bodies, and Skinner clearly included *all* of the functional activities of the organism or person, whether public or private, social or nonsocial, verbal or nonverbal, to be included within the language game of "behavior." To state the matter crudely, private events and experiences are assumed to have no special "metaphysical" status, and they are not regarded as "causes" of behavior, but rather such events are to be included as part of the "doings" of the person; or in other words, they *are* "behavior" (or are included under the term) in the pragmatic sense that they are *in need of* explanation. From this scientific view, the explanations are to be found in the study of regularities between such functional activities and the biology, history, and context of the activities.

2. Throughout this paper, ordinary language terms (such as *purposes* of science) are used freely for purposes (again!) of ease of communication of the present issues. In some cases of such terms, technical translations could be provided; for others, an interpretation would have to suffice until an adequate scientific analysis could be made. In any case, it need not be assumed that such issues of language and scientific progress imply *metaphysical* significance.

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