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INTENTIONAL BEHAVIORISM REVISITED

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ABSTRACT: The central fact in the delineation of radical behaviorism is its conceptual avoidance of propositional content. This eschewal of the intentional stance sets it apart not only from cognitivism but from other neo-behaviorisms. Indeed, the defining characteristic of radical behaviorism is not that it avoids mediating processes per se but that it sets out to account for behavior without recourse to propositional attitudes. Based, rather, on the contextual stance, it provides definitions of contingency-shaped, rule-governed verbal and private behaviors which are non-intentional. However, while the account provided by radical behaviorism fulfills the pragmatic criteria of prediction and control of its subject matter, it has problems of explanation that stem from the failure of radical behaviorist interpretation to address the personal level of analysis, to provide for the continuity of behavior, and to show how its accounts can be delimited in the face of causal equifinality. This leaves gaps in its explanation that radical behaviorists choose either to ignore or to fill with the very intentional locutions that they formally abhor but which I argue are essential. As a result, many psychologists who style themselves radical behaviorists have already moved beyond radical behaviorism as a philosophy of science. They have done so primarily as a result of adopting the language of intentional psychology in order to explain behavior; where they have not done this, they have resorted to the unscientific assumption that somewhere there is a learning history that explains their data. While it is indeed the case that a learning history precedes all operant behavior, the explanatory gap that is revealed by the search for this unobtainable information reveals the inescapability of intentionality. Hence, psychologists, including radical behaviorists, are right to employ it. The challenge is to understand why this is so and to celebrate rather than denigrate the resulting extension of behavioral science. In this response, I first raise and seek to answer two questions: what is radical behaviorism, and what is intentional explanation? I go on to discuss the incompleteness of radical behaviorism. There follows a summary of the

AUTHOR'S NOTE: I am grateful to the commentators for giving their time and intellectual effort and to the editor, Jack Marr, for his patient, informed, and judicious encouragement. I cannot do full justice to all the points the commentators raise, some of which already fill the annals of philosophical inquiry. It is debate that is important, and I thank them for engaging in this while hoping that I have also made some small contribution to it. I have tried not to repeat material from "Intentional Behaviorism," and where I have covered commentators' points elsewhere in greater detail than I can here I have indicated this in preference to using current space. Otherwise unattributed page references are to the commentators' responses. If I have not explicitly referred to **Killeen** in the text, it is only because we seem to agree and disagree simultaneously on every single point. Apart from that, I would say that his wife sounds much like mine, as does the postal "system" he has to contend with. Please address all correspondence, *preferably by email*, to Gordon Foxall, Cardiff University, Aberconway Building, Colum Drive, Cardiff, CF10 3EU, Wales, UK; E-mail: foxall@cf.ac.uk.

argument with respect to intentional behaviorism and super-personal cognitive psychology. And, finally, I bring the discussion back to Daniel Dennett, the philosopher who initiated so much of it.
Key words: radical behaviorism, intentional psychology, philosophy of science, explanation

What is Radical Behaviorism?

Will the Real Radical Behaviorist Please Stand Up?

A difficulty that arises in discussing radical behaviorism is that its adherents claim that it exists in more than one form and these forms are incommensurable; characterizing and critiquing radical behaviorism is therefore difficult since the ground is always shifting beneath one's feet. In taking Skinner's version as the definitive one in "Intentional Behaviorism," I was reflecting his origination of the term, his definition of radical behaviorism as opposed to other behaviorisms in terms of its inclusion of private events (Skinner, 1945), his sustained work in refining and extending the paradigm, and the near-universal agreement, at least until comparatively recently, that his was a central voice of radical behaviorism. A scientific worldview must, of course, be more than the lengthened shadow of one person, and even one person's views may change with the contingencies, but it was convenient to make this assumption and to contrast other scientific schools (for instance, the teleological behaviorism of Rachlin, 1994, and the theoretical behaviorism of Staddon, 1997, 2001a, 2001b) as deviations from or extensions of it. In addition, many writers, adherents and critics, have made Skinner their reference point for radical behaviorist thought, and many continue to do so. It is also confusing to find some authors interpreting Skinner by arguing that his intentions in writing this or that were such and so. Skinner is known as a meticulous intellectual craftsman, and it seems absurd not to take him at face value when he is so clear. In spite of **Baum**'s pointing out that Skinner himself caused problems for radical behaviorism and that his version cannot therefore be taken as definitive, and despite Skinner's disarming avowal that he did not write as "*the* behaviorist" (e.g., Skinner, 1974), I intend still to retain this convention if only for ease of exposition. Skinner's inconsistencies thereby become fuel for the critique of radical behaviorism rather than privileged deviations from the truth.

Using Skinner—as far as is possible given the vicissitudes of a long intellectual life—as a template for radical behaviorism has the advantage that we can identify and measure deviations from this standard. If perpetrators of the deviations claim to be radical behaviorists themselves, they must acknowledge en masse the diversity of views which go under this label. Non-behaviorists have a right to know the full range of views that are to be included under the radical behaviorist banner.

There is, for instance, a noticeable difference between radical behaviorists who are willing to wait for someone else to discover and fix their problems and those who engage actively now in intellectual problem finding and solving. Skinner's much-repeated willingness to see behaviorism as setting the agenda for the physiologist and leaving the problems to that other science exemplifies this

well. **Baum** is similarly willing to trust in action at a distance until something better comes along. **Hocutt** is also waiting for physiology. Others, however, see no problem with taking the quest to understand (as opposed to merely predict and control) behavior out of the realm of patient waiting and into that of active intellectual inquiry. Rachlin's (1994) teleological behaviorism and Staddon's (2001a) theoretical behaviorism come into this category; the study of equivalence and the construction of relational frame theory (Hayes, Barnes-Holmes, & Roche, 2001) do too. I omit some of these thinkers for want of space, but I would mention Malott's (1989; see also Malott & Garcia, 1991) use of private events in the explanation of rule-governed behavior and in finding a role for rules in the explanation of temporal gaps in our knowledge of delayed reinforcement. Lowe's work (e.g., 1983) is similar in suggesting that resort to private events is inevitable if verbal behavior is to be explained adequately. Several points arise: first, often these more adventurous/impatient thinkers are going beyond radical behaviorism as Skinner presented it, even though they remain within an extensional mode of explanation; second, they are dealing with a different set of problems from those raised by laboratory-based experimental studies—they are working in the realm of theory and interpretation; third, some of them nevertheless claim to be radical behaviorists—without clarifying how their systems vary from Skinner's and without acknowledging important deviations from his thought[1]; fourth, their explanations have wider implications in that they have strayed out of a radical behaviorist frame of reference by using language (inescapably) that belongs to another order of explanation: intentionality. I am merely seeking to go a little further in pointing out that the kind of solution offered by Lowe and Malott is (a) acceptable and (b) entails intentional explanation. This seems a logical outcome of what they have been doing already, not a grand departure from the point at which they have arrived.

The Essential Difference

Although each appears in a variety of guises, cognitive and behaviorist approaches to the explanation of behavior differ from one another in one major respect. The essential difference, which is linguistic rather than ontological, is illustrated by Mele's (1992, p. 17) pointing out that "Oedipus intentionally married Jocasta, and Jocasta was his mother; but he did not intentionally marry his mother: he married his mother quite unwittingly." It is not possible to render this sentence in extensional terms without adding information (though it can be rendered in alternative intentional terms, e.g., what Oedipus wanted, thought, believed, etc.). It is not possible, therefore, to convey the meaning that the events mentioned in this sentence had for the participants in extensional terms. This is what the intentional opacity of the sentences that occur in Juarrero's (1999) examples of an unintended

[1] Rachlin (1994, 2000), for instance, takes pains to explain *his* differences with the Skinnerian system, but this is not true of all writers on behavior theory.

wedding and an unintended divorce, which I cited in "Intentional Behaviorism," imports.

While intentional explanation is inevitably an underlying component of the philosophical basis of cognitivism, the distinguishing characteristic of radical behaviorism, by contrast, is its avoidance of intentional idioms as explanatory devices (Smith, 1994). They thus represent incommensurable theories of behavior, with no apparent hope of being organized into an overarching framework of conceptualization and analysis. In "Intentional Behaviorism" I argue that each of these contending paradigms, the one based on the intentional stance, the other on the contextual stance (Foxall, 1999), is in need of the other if it is to approximate a more nearly complete explanation of behavior. In making a case for this proposition I introduce a third framework of analysis, intentional behaviorism, to which both behaviorism and cognitivism are integral contributors.

There appears to be some confusion over this distinction among radical behaviorists. While **Hocutt** appears to agree with my linguistic approach, **Baum** does not seem to accept the linguistic difference. **Branch** argues that radical behaviorism is not strictly extensional, asks what intentional language *is*, but is confident nonetheless that Skinner can cope. **Burgos** asks what intentional inexistence, which is crucial to understanding the essential difference, is, while **Moore** asks what intentionality is. Let us take each of these in turn.

Baum contends that "verbal behavior has nothing to do with reference or content in any conventional sense. Intentional utterances are no more opaque than others" (p. 59). The point about the difference between intentional and extensional sentences is not that we have a problem in explaining people's use of the former but simply to point out that behavior analysis is extensional, and that means it avoids intentional idioms or strays into another kind of explanation. The purpose of intentional behaviorism is to clarify what is going on in the verbal behavior of the *investigator*, and since radical behaviorism rests a priori on the use of extensional language, we must do more than simply analyze the verbal behavior of its adherents when they deviate from such usage: we must evaluate this adoption of an alternative mode of expression and determine its consequences for the nature of radical behaviorism and its subject matter.

Branch's argument that radical behaviorism is not strictly extensional (p. 62) rests upon its inclusion of theoretical terms such as "operant." This betrays a confusion between theoretical terms, which are extensive in radical behaviorist writings, and intentional terms, which are a special kind of linguistic device that have important implications for explanation. Hence, a science may contain (surely, *must* contain) theoretical terms whilst retaining its extensional mode of explanation. I cover this point in *Context and Cognition* (Foxall, 2004), which notes that radical behaviorism is indeed theoretical in practice but that this is something different from being intentional. It is the difference between intentional and extensional sentences that is crucial here, though it is something with which Skinner's analysis of verbal behavior can cope, according to **Branch** (p. 61): "A functional analysis proceeds in terms of the behavior that follows from a sentence rather than the form (structure) of the sentence." *But this is not my point:* the point

is that one cannot translate one sentence into another without adding meaning. They belong to different universes of discourse and thereby imply different approaches to explanation. The point is that when *we, as scientists,* use intentional language we are using a means of explanation other than radical behaviorism. Skinner more than once appealed for an analysis of the behavior of the scientist: why scientists employ intentional language even when their avowed intent is to produce an analysis based on extensional sentences is one aspect of the required study.

By extensional language (**Moore**) I mean sentences that are referentially transparent, that conform to the normal usages of science, that contain no intentional terms, that allow substitution of identicals. Extensional language is a means devised by Quine (1960) to express statements that met the truth value of science. The nature of extensional language is described most succinctly and accurately by Quine (1960, p. 151): "All failures of extensionality are failures of substitutability of identity." His argument is that terms which have the same extension can be substituted for one another in extensional sentences *salva veritate*. Such substitutability of identity is not possible *salva veritate* in intensional sentences. In the latter, the terms have different intensions or meanings in the individual's mental/subjective/private life[2]. Hence, although intentionality is a linguistic phenomenon, use of it makes assumptions about the individual's mentality/subjectivity/privacy. It does not inhere entirely in the realm of abstract analysis but makes assumptions about the nature of human verbal behavior.

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[2] Quine is further explicated by Davis (2003, p. 91): ". . .Quine's idea of formal semantics is of what is called a fully extensional language, or one that restricts meaning and truth to set-theoretic relations between names and predicates that have spatiotemporal objects and abstract entities respectively as their extensions (Quine 1960 [see especially p. 151]). The extension of a name is the particular object denoted by that name [e.g. a particular person]; the extension of a predicate is the set of objects having the property represented by that predicate [e.g. happiness]. An extensionalist language is one in which any expression in a sentence can be replaced by another that has the same extension without change in the truth value of the sentence. The advantage of such a language for science is that it preserves the truth value of sentences across the use of different expressions for the same things. Expressions with the same extensions may be exchanged *salva veritate*. However, another thing that an extensionalist language eliminates is all language expressing intentional life, that is, beliefs, desires, fears, and so on. When a sentence expresses what a person believes or desires, Quine made use of what were called propositional attitudes [Russell, 1912], logical expressions such as 'believes that _____' or 'desires that _____,' where the blank is filled in by the relevant belief or desire. The problem with these expressions, termed intensional expressions, is that they are vague and referentially opaque, so that the replacement of what appears in the blank by expressions with the same extension often changes the truth value of the sentence in which they appear. It may be true that Smith believes that the evening star has risen, but not that Smith believes Venus has risen, though 'evening star'\ and 'Venus' have the same extension." The problem is that these two expressions nevertheless have different *intensions* or meanings and are not therefore substitutable if the truth value of the sentence about Smith is to be preserved.

The use of intentional language raises another problem for scientific expression as it is generally understood. It is that intentional idioms "can only be understood in terms of one another" (Davis, 2003, p. 91). Echoing Chisholm (1957), Quine (1960, p. 221) points out that "there is no breaking out of the intentional vocabulary by expressing its members in other terms." Now, there is an obvious way out of this problem—that taken by Quine, which involves just abandoning intensional language and using exclusively the formal semantics that permits the substitutability of identity as the lingua franca of scientific discourse. This is essentially what methodological behaviorism and Rachlin's (1994) teleological behaviorism, divergent as they are in other respects, attempt.

Quine (1960) draws attention to these linguistic phenomena and also the assertion by Brentano (1874), demonstrated by Chisholm (1957), among others, that intensional language cannot be reduced to extensional without the addition of information. Quine's conclusion, because his search is for a formal semantics in which to frame scientific expression, is to urge abandonment of intensional language in favor of the advantages of a fully extensional locution which maintains a truth value appropriate to scientific discourse. Intensional sentences, he would argue, resist such formal analysis. However, since behaviorists are ultimately unable to express their science without resort to intensional language, we cannot endorse this conclusion. While Quine's restriction befits a physical science, it cannot be applied to a psychological science, the language of which must include intentionality.

The Incompleteness of Radical Behaviorism

Radical behaviorism's *explanation* of behavior is deficient in three ways[3] that are particularly evident in its attempt to interpret complex behavior, that which is not amenable to direct experimental analysis, by reference to behavioral principles established through the study of that which is. I do not mean by this that radical behaviorism fails to predict behavior or to assist its influence or control. I do not, therefore, deny that it identifies environmental events that are the independent variables of which behavior is a function. Indeed, I have no criticism of radical behaviorism in terms of its own pragmatic success criterion: that is, the prediction (often plausible postdiction) and control (influence) of behavior which remain open to empirical scrutiny. Here it undoubtedly succeeds, not only in the closed settings of the operant laboratory and therapeutic community, but in the open settings presented by the economics of everyday life (Lacey & Schwartz, 1987;

[3] I am here using the term "explanation" in a different sense from that which I believe belongs to radical behaviorism. The radical behaviorist, as I maintain in the text, is satisfied that behavior has been explained when the environmental conditions that permit its prediction and control have been identified. I am arguing that even when we can predict and control there may still be something left to explain in the sense of making the factors that account for the continuity of behavior and its appropriate radical behaviorist interpretation explicit and intelligible.

Schwartz & Lacey, 1982, 1988). Nor am I unaware that for many behavior analysts this is sufficient to constitute an explanation of behavior, as it is in the Machian positivism that guided Skinner's early construction of this paradigm and from which he never departed. In other words, I am not criticizing the capacity of radical behaviorism to succeed on its own terms. I *am* arguing that on those terms radical behaviorism cannot account for (a) behavior at the personal level (as opposed to accounting for behavior–environment relationships), (b) the continuity of behavior over time and space, and (c) the delimitation of interpretations of behaviors. The picture is complicated by dint of the insistence of radical behaviorists that these putative problems can be overcome by means of private events, verbal behavior, rule-governance, relational frames, and other devices. I argue that none of these, in fact, provides the necessary conceptual framework for a science of behavior that succeeds in accounting for the three requirements in which I find it deficient.

Hence, radical behaviorism as it stands is not in need of change. That it continue its present course is as important to the program I advocate as it is to its own (i.e., that it continue to develop as a science of behavior that elucidates behavior–environment relationships). It is a means of predicting and influencing behavior, one of several psychologies that attempt these tasks, and it is necessary that it continue to provide a limiting statement of how behavior may be determined by its consequences. But it is also imperative that it be challenged by a theoretical approach that identifies its deficiencies in the realm of explanation into providing a robust experimental program charged with showing that such empirical work can provide a full explanation of behavior. I *do* argue, however, that explanation that deals with the personal level of explanation, that accounts for continuity, and that is delimited requires that subtle use be made of the intentional psychology that underlies cognitivism. This takes nothing away from radical behaviorism; it adds what is necessary to explain and interpret rather than simply to predict and control.

This section is concerned with the three reasons why radical behaviorists require intentional language: to account for the continuity of behavior, to address the personal level of explanation, and to delimit their interpretations of complex behavior. "Intentional Behaviorism" argued for each of these from the point of view of an investigator trying to make sense of some aspects of his or her data. It is also clear that radical behaviorists *cannot* and *do not* avoid intentionality in practice and thereby slip into a mode of explanation that is alien to that presumed by radical behaviorism.

Behavioral Continuity

Three reactions are apparent to the argument that behavior analysis cannot account for behavioral continuity. **Hocutt** argues that intentionality offers no explanation either; **Baum** argues for acceptance on faith of action-at-a-distance; and **Branch**, **Hocutt**, and **Baum** acknowledge that radical behaviorism cannot explain continuity now, but that physiology will, at some time, come to their aid.

"Intentionality does not explain, either." No doubt there is no final, comprehensive explanation of any phenomenon—but more important than

searching for one anyway is to recognize the nature of the explanation we are using—that perhaps we are forced to use. So, indeed, intentionality does not explain in any final sense. I have argued that radical behaviorism, while adequate for the prediction and control of behavior, breaks down at the point of accounting for continuity, the personal level, and the conduct of an interpretation. **Tonneau** believes that I exaggerate even this adequacy and that I should re-emphasize, in response, that behavior analysis performs these tasks better the more closed the setting in which behavior takes place. The more open the setting, the greater the need for interpretation rather than experimentation; hence the *raison d'etre* for my research program (Foxall, 1995, 1996). I have also argued that at this point we need to adopt intentional language (hence intentional explanation), that many radical behaviorists do just that, and that in the process the extensional philosophy on which the radical behaviorist project rests has, to say the least, been modified. Recognition that this is what we are doing—indeed, that it is what we must do—does not of itself engender a final explanation; it does, however, lead to a greater appreciation of the nature of our explanation and, pragmatists though we may be, of our subject matter.

The inability of an extensional account of behavior–environment relations to explain behavioral continuity is expressed strongly by Taylor (1964). Taylor (p. 125ff) argues that extensional accounts which deal only in stimuli and responses can lead to prediction and some degree of control but, in view of their inability to account for the continuity of relationships among their dependent and independent variables, they cannot explain the observed behavior. It is inevitable, therefore, that an intentional explanation will be required for this higher level of behavioral complexity. Hence, in the case of a rat that is rewarded for jumping toward a white card, behavior may not be reinforced if the rat is not "paying attention" to what is happening:

> Hence on the cognitive view it matters what the rat is doing, that is, what action he is performing, and thus what intentional description the action has for him., whether 'jumping right' or 'jumping to white', whereas on the S–R view, the response is not an action, the intentional description is irrelevant, and it matters only what description the card actually bears to which the rat jumped. (p. 125)

The rat's selective attention and selective perception must be considered in order to account for the continuity of behavior since the particular feature and positioning of the card might be the stimulus to which it was responding, whatever the definition of the stimulus accepted by the experimenter. Taylor is clearly assuming that the required selective perception has ontological significance which inheres within the animal; there is no need to make this assumption within the intentional behaviorism framework. The latter recognizes only that our explanation of the behavior is incomplete without the acknowledgement that the description of its continuity transcends extensional description and relies on the use of intentional terms. Similar problems of explaining continuity and the inevitability of an intentional explanation arise also in the cases of behavioral discrimination and generalization and in molar analyses of behavioral sequences.

If intentionality adds nothing to explanation, we must wonder why behaviorists use it so freely. **Lacey** notes this reliance of behaviorism on the intentionalistic, seeing its explanatory capacity as universal and arguing that science is impossible without it. Behaviorists use the intentional scheme all the time, he says, not just in popular writings but in their scientific analyses and writings (and science could not proceed in any other way). This is more than a heuristic overlay on super-personal extensional science: it is the science itself.

Whatever behaviorists insist is necessary, it is what they do (what they have to do) that is so revealing of the explanatory mode inherent in behavioral science that attempts to deal with the three problems of broader explanation to which I have drawn attention. **Baum** offers as a definition of explanation that an event has been explained when we feel comfortable with it (I am perfectly comfortable with the idea that the sun goes round the earth—there it is, every day, doing just that!), and he argues that behavior consists of "*whatever events we can talk about with our invented terms"* (Baum, 2005, p. 33). But he is clear that those invented terms must be extensional, as he says on p. 59: ". . .in a science of behavior, everyday mentalistic terms like *believe, expect,* and *intend* either must be avoided or carefully redefined." I argue that Baum, like other exponents of radical behaviorism, uses all of these, or synonyms, and that they cannot be "redefined" without altering meaning.

First, it appears that we cannot speak about behavioral events without resort to intentional terms. **Baum**'s introduction to behaviorism (Baum, 2005), for instance, is replete with intentional language offered in explanation of behavior. For example, having said that behaviorism "favors definitions of activities that include [reasons for performing them]" (p. 33), Baum talks of a person who "is running a race *in order to* qualify for the Olympics" (emphasis added) and concludes that "running a race in order to qualify for the Olympics is a behavioral event." This is the kind of intentional language that Skinner went out of his way to avoid, presumably on the understanding that its use violated the primary canon of radical behaviorism that the description of behavior proceed strictly in extensional terms. He writes in *Contingencies of Reinforcement*, for instance, "We say that spiders spin webs in order to catch flies and that men set nets in order to catch fish. The 'order' is temporal" (1969, p. 193). Skinner (1971, p. 18) is also scrupulously careful to avoid intentional language in pointing out that "Operant behavior is behavior that *operates* on the environment to produce consequences." He also notes that it is the experimenter rather than the subject who "associates" stimuli and that he does so nowhere else but in the experimental space. But let us return to "in order to." Some case might be made for this denoting a temporal relationship, and perhaps such linguistic dexterity could amount to what Baum refers to as careful redefinition—but it is not possible to make the case that one is alluding to a temporal relationship when one is using this terminology to elucidate the reasons someone does something. Explaining behavior in these terms amounts to more than simply parsing a subject's verbal behavior functionally after she has said "I did this for the reason that. . ." Rather, the *investigator* is offering, as explanation, the fact (or opinion) that she behaved in such a way that her behavior was intended

to produce certain consequences. In fact, the investigator is proposing that the reason lies at the very heart of the definition of the behavioral episode. This is intentional language and it is intentional explanation.

I will not labor this point, but a few more examples will give some substance to my claim, and **Lacey**'s, that radical behaviorists cannot do without intentional expression (see also Lacey, 1974, 1979). Baum (2005, p. 26) speaks of a potter's apprentice *learning about* his or her new profession; otherwise, the apprentice would have no way "to be sure" what to do in order to fulfill the requirements of the job. Similarly, he notes that a driving instructor is likely to *explain* concepts to a learner so that the latter will *know* what to do in certain, as yet unencountered, future circumstances. This latter example goes under the heading "Conceptual Economy," which is presumably the outcome of behaviorists using intentional terms and explanation in contrast to the phoniness of others' uses thereof. Science, Baum tells us, proceeds in similar fashion, devising concepts that enable one to tell others "what to expect if such-and-such happens—to predict on the basis of past experience. . . .When scientists make up terms like 'oxygen,' 'satellite,' or 'gene,' the one word tells a whole story of expectations and predictions. These concepts allow us to talk about such expectations and predictions economically, without having to go through long explanations over and over again" (2005, pp. 26-27). In other words, the scientist's invention of extensional terms is justified on the basis of their offering intentional explanations.

Baum is, of course, not alone in this. In his technical exposition of teleological behaviorism, *Behavior and Mind,* Rachlin (1994) refers to *information,* in the form of CSs and discriminative stimuli, that *signals* respondent or operant contingencies. Although he defines the process as operant or respondent conditioning, the terminology, hence the explanation, is distinctly intentional. Moreover, the book speaks of verbal reports as being of representations and decisions, which are operations that can be described only in intentional terms. And Moore, in Chapter 10 of his *Conceptual Foundations of Radical Behaviorism* (2008), which is on "Private Events," makes frequent allusion to intentional verbal behavior. His question "How does the speaker know which words to apply to the internal events, so that a listener knows what the speaker is talking about?" (p. 219) must stand as a paradigm example of such inevitability. None of these intentional terms is translatable *salva veritate* into the extensional language of radical behaviorism.

"Action at a distance must be assumed on faith." Action at a distance would be constructive if it led behavior analysts to work out how to speak meaningfully of learning histories in extensional terms, for instance by specifying what characteristics an account of a learning history would need to have in order to contribute to the prediction and control of behavior. Or, if it is knowledge of physiology that must ultimately bear this burden, specifying how our understanding of learning history can lead us to identify the relevant neuroscientific findings when they come along (incidentally, it is surely a contradiction for behavior analysts to say that their science of behavior is self-contained and then to have to appeal to another science to answer some of their

fundamental questions). I do not think that behavior analysts are doing this or are even aware that it needs to be done. Constructing plausible (but only imagined) learning histories to fill the gap of distant explanation will never lead to knowledge that can be tested, knowledge for which there will ever be a persuasive warrant of assertibility. The intentional approach to filling this gap is not based on plausible conjectures but another form of linguistic expression that is appropriate to the subject matter and question at hand. Figuring out how verbal behavior might be employed in this task would be a first step, one for which I have proposed a method elsewhere (Foxall, 1995, 1996, 2005, 2007a, 2007b).

Action at a distance was ultimately substantiated in the case of post-Newtonian physics because it implied that an extensional solution would, at some time, emerge. But whenever behavior analysts try to deal with the problem of behavioral continuity, for instance, they are found to employ non-extensional language, an admission (if they would own up to it) that they can solve the problem only by adopting a novel mode of explanation—as do Rachlin, Baum, and Lowe, among others. This is not action at a distance as Newton would have understood it!

In any case, although action at a distance is viewed askance by many modern physicists, there is an important difference between Newton's situation and that of the psychologist. While physics deals *only* in extensional sentences and any solution to a physical problem must therefore eventually present itself in extensional terms, psychology has the option of employing intentional language *if there seems to be a way in which an extensionally-presentable solution can be found.* Of course, the use of intentionality may prove to be temporary; it may or may not itself contribute to the identification of a solution that can be portrayed in extensional language: or this is the source of a real dilemma for psychologists which they ought, as scientists, to face up to rather than resort to faith.

Baum appeals to Newtonian action at a distance as a legitimate element of scientific practice, and here he is in good company, for Popper wrote that

> I am inclined to think that scientific discovery is impossible without faith in ideas which are of a purely speculative kind, and sometimes even quite hazy: a faith which is completely unwarranted from the point of view of science, and which, to that extent, is 'metaphysical'. (1972, p. 38)

Loasby, writing in the context of behavioral economics, makes clear what the status of such thinking is:

> There is no better example of such unwarranted faith than Newton's reluctant acceptance of the clearly absurd notion of force acting at a distance as the essential explanation. . .of gravity: such occult planetary influences seemed far more akin to astrology than to mechanics. (1976, pp. 27-28)

Loasby pinpoints what even Newton thought of this practice; we may argue about whether it is necessary to progress in science—the physicist Ziman (2008) thinks so; the behavior analyst Malott (1989) does not. But first let us be clear

what the practice is: closer to the occult and astrology than science. And, second, let us be clear that it is far from what I am doing in advocating intentional behaviorism. I am making no ontological claims about intentionality; I am merely pointing out that some theories entail different locutions from others and that when we use those locutions we are necessarily using the means of theoretical reasoning that derive from them.

The correlation-based law of effect (Baum, 1973) also involves a temporal disconnection between dependent and independent variables and, since no empirically available mechanism is suggested by the theory's author for bridging this gap, we are again confronted with action at a distance (the fact that even a strong correlation link may complicate a functional relationship by entailing extended temporal gaps between the relevant variables, coupled with the ruling out of contiguity, throws the theory into a strong reliance on action at a distance). This is odd since there is a legitimate mechanism, in the form of private events, which is both the essential feature of radical behaviorism (Skinner, 1945) and apparently fully endorsed by Baum (2005). Of course, private events are not (yet?) available for experimental analysis, which may account for Baum's reluctance to enlist them in this cause—but this seems shortsighted. It is not part of radical behaviorism to deny that the covert behavior to which we refer as decision-making actually occurs (Skinner, 1969); if we are ever to identify physiological mechanisms that can account for this (as Baum must be arguing, since the reliance on action at a distance is temporary) we need to know what is required of the decision-making system—otherwise we shall miss the physiological connections when they emerge. Malott and Garcia (1991) propose a well-grounded behavioral–analytic theory for how private events can fill the explanatory gap here. Why ignore it? Why believe that such analysis involves spooky, ghost-like entities?

What it often entails, of course, is the necessity of using intentional language, for while it is the case that behaviors tend to have learning histories, they are not always empirically available and hence cannot enter into a scientific analysis:

> As every psychologist knows, keeping track of the history of reinforcing contingencies of even a laboratory animal is difficult; working out what has happened to a human years before, when no records were kept, is just impossible, and all talk of doing so is fanciful. All talk of a 'proper behavioral analysis' is just *deus ex machina*. (McLaren, 2007, p. 29)

"Physiology will somehow save behaviorism."[4] One of the targets of action at a distance is the putative capacity of the physiology of the future to solve the problems of continuity. **Branch** is content to wait for physiology to come to the rescue of behavior analysis in the context of demonstrating why behavioral continuity occurs, while **Hocutt** says that there is strong empirical support that "states and acts of mind are conditions of, or processes in, the brain" (p. 89). He offers no justification for this appeal to identity theory, itself a minefield of

[4] Please see my *Context and Cognition: The Interpretation of Complex Behavior* (Foxall, 2004) for an account of this theme.

philosophical analysis and conjecture. **Baum** is of similar opinion with respect to the eventual efficacy of physiology. The question is not whether physiology has given us, or will give us, answers for behavioral continuity, but whether it *can* do so. It seems it cannot do so in the context of explaining subjective (private) behavior; can it do so otherwise (see McGinn, 1989, 1991; Strawson, 1994)?

The radical behaviorist position on reductionism is actually ambiguous. Delprato and Midgley (1992) point out that Skinner (1947, p. 31; 1974, p. 215; 1975, p. 42) embraced the possibility that physiologists would eventually produce a biological basis to which the phenomena of behavior analysis would be reducible. However, he also claimed that behavior analysis was a field in its own right, that behavior was an independent subject matter (1938, p. 433; 1961, p. 64; 1975, pp. 42-44). Perhaps Skinner's contention that operant psychologists were setting an agenda for physiological research (e.g., 1974, p. 215) was a means of reconciling these ideas, acknowledging that biology would one day substantiate behaviorism, keeping behavior analysts free to pursue their own science at a higher level of analysis (Delprato & Midgley, 1992; Foxall, 1996).

The Personal Level

There are two issues here. First, the contention that first- and third-personal sentences do not attract similar methodologies of verification in radical behaviorism; and, second, the view that dealing in the first-personal level through the medium of intentional language involves using a sub-personal ontology to explain the behavior of the intact organism causally. Baum seems clear on this. Discussing differences and similarities between Skinner's behaviorism and Rachlin's, Baum (2005, p. 57) says "Although in Skinner's view the event being reported on is private, both Rachlin and Skinner would agree that reporting on one's private activities *is the same as* reporting on one's public activities (emphasis added)." **Branch**, however, is confused by the assertion that behaviorists assume that a first-personal sentence has a content and verification equivalent to those of the corresponding third-personal sentence, especially in Malcolm's (1977) critique of Skinner's approach to statements like "I am about to go home." Skinner would say that verbal behavior of this kind was under the control of variables that have been present when one has gone home. In similar vein, **Baum** argues that the distinction between first- and third-personal statements is irrelevant for behavior analysis. First-personal verbal behavior is just behavior to be explained. To the behavior analyst none of these utterances *refers* to anything. None is uncaused. Behavior analysis explains it in terms of the history of context in which it occurs. He admits that the problem here comes not just from Dennett and Malcolm but from Skinner himself: Skinner was using the word "observe" casually.

I agree with Baum that first-personal sentences *can* and *should* be explained in line with radical behaviorist procedure. Schnaitter (1999), for instance, has shown that it is feasible to parse such statements functionally. But the fact that they can be analyzed in this way does not overcome the problem raised by Malcolm. The commentators do not treat this problem other than by assertion of what they

see as the radical behaviorist position. However, a deeper criticism of the intentional critique of radical behaviorist treatments of verbal behavior of this sort, to which both **Baum** and **Branch** allude, has been well put by Day (1992), who contends that the statement of a functional relationship does not have "content"; it is simply the verbal behavior of the scientist. It is under environmental control and therefore a subject for study by behavior analysts. Day accuses Malcolm of confusing (1) the problem of how to analyze verbal behavior that describes human behavior with (2) the problem of how to test the content of such descriptions. I should like to address what might be behind the statements of some of the commentators by dealing in greater detail with Day's detailed analysis, according to which the description of the verbal behavior of the man who moves papers on his desk as a statement that "he is doing the things he has done in the past which have eventuated in his findings his glasses" (Skinner, 1953, pp. 89-90) is to be construed as part of a scientific analysis of this aspect of human functioning, one that proceeds in strictly extensional terms. It is separate from a test of the content of such a statement. Now, I think that Day is confusing the first- and third-personal standpoints here. Skinner (1953, pp. 89-90) may well be talking about the analysis of verbal behavior when he talks (in a third-personal way) about the man whose glasses are not apparent to him, but it is Skinner himself who introduces the question of how this seems to the man himself (from the first-personal perspective). If he says, on being asked what he is doing, "I am looking for my glasses" (a use of intentional language), this is not a further description of his behavior but of the variables of which his behavior is a function. It is equivalent, Skinner says, to "I have lost my glasses," "I shall stop what I am doing when I find my glasses," or "When I have done this in the past, I have found my glasses." But these are not equivalent. "I am looking for my glasses" is first-personal, subjective; it is something he knows he is doing without reference to a learning history. We can say that the fact that he is looking for his glasses is "something *he knows*" without scrutinizing or making reference to his past or future behaviors. It is knowledge that is open only to the spectacle-less man to have and, should he choose, to analyze. The translations are different; they are publicly available and publicly checkable references to three-term contingencies. Malcolm's point is wholly different from Day's: It is not to point out that a behavioral analysis would require an examination of the behavior by reference to its environmental correlates; it is to argue that the man does not need to say, to himself at least, that he is enacting behavior that has culminated in his finding his glasses in the past. There is a level of understanding of his behavior, expressed in terms of what the man *knows* without external reference, that cannot be expressed in language other than the intentional.

The "translations" suggested by Skinner are all framed in extensional language: "I am looking for my glasses" falls into another category of expression and belongs to another category of explanation to that on which radical behaviorism necessarily depends as a distinct philosophy of psychology. It refers to a level of experience that cannot be validated through public verification. First-personal utterances of this kind cannot be verified in themselves, whereas

extensional sentences, which are essentially third-personal, are (in principle at least) verifiable. Such verification is not at issue here; it is the irrelevance of verification to the searcher's knowing and the impossibility of its translation into the language of extensional behavioral science that matter. We could conceivably check that on previous occasions (and certainly on future occasions) such behavior has led to the man's coming across his glasses, but we cannot check what he is looking for in the sense of verifying his subjective experience; his expression of this behavior in intentional language is a statement of a subjective, personal, private happening. If this is a dispositional concept as defined by Ryle (1949), we could, at a first-personal level, establish its truth value by ascertaining what the person does under the circumstances; some behaviors would be typical of "someone looking for his glasses" and some atypical. But, of course, this is not what the person himself would need to consider to know whether the sentence was true.

This first-personal knowing is something different from a "private event" as the term is usually used in behavior analysis. Although it is conceivably contingent on a learning history, it is not a conclusion that the man arrives at through scrutiny thereof. We cannot even translate his utterance into "He said that he was looking for his spectacles" because he may not know that glasses are sometimes so-called. We are dealing with different kinds of sentence: they are not equivalent because the extensional statement always adds something that is not in the original, intentional sentence.

Day seems to be arguing that Skinner is saying that some behaviors are accompanied by feelings of purpose or intention and that this is all that purpose and intention amount to; they are not causative. But this is not what Malcolm is addressing. He is pointing out the non-equivalence of the two forms of sentence and what they mean to the experience of the man looking for his glasses. Moreover, it is not true to say, as Skinner does (1953, p. 90), that the man's statement "I am looking for my glasses" is "not a further description of his behavior but of the variables of which it is a function." On the contrary, and this is actually Malcolm's key point, it cannot be at all a scientific statement that needs verification; rather, it is an intentional statement arising from personal experience that is not open to scientific scrutiny in the same way. This *is* a unique description of his behavior, one that can come from only one source, one that is neither up for public scrutiny nor in need of self-scrutiny; it is what the person intrinsically *knows* and can act upon. This does not mean it is autonomous—only that knowing such a thing arises through different processes from those involved in establishing the external contingencies. Malcolm's point is independent of the difference between the problem of how to analyze verbal behavior that describes human behavior and the problem of how to test the content of such descriptions. He is simply saying that there is no way to understand the behavior of the man who says "I am looking for my glasses" except in terms of intentionality; such knowing is inherently intentional, and the sentence in which the man expresses it is equally intentional. An extensional behavioral science cannot cope with this.

Now, there is one important area of radical behaviorist interpretation to which these conclusions are highly relevant. The tendency of human subjects in operant experiments to show behavior that is insensitive to changing reinforcement schedules has been explained by some researchers by reference to those subjects' capacity to self-tact, to produce rules that describe the contingencies as they perceive them, and to act upon those rules: "Their behavior is not under the control of the experimental variables but. . .controlled by self-produced cues, which vary from subject to subject" (Lowe, Harzem, & Hughes, 1978). Naturally, such formulations are not amenable to direct experimental analysis, so this explanation rests upon an interpretation, albeit one which (as far as the researchers are concerned) must conform to the principles of behavioral control that may be extracted from other experimental analyses (Lowe, 1979, 1983, for instance, goes to considerable lengths to argue this).

However, it is difficult to see how such an interpretation can actually proceed in other than intentional terms. Many private events—thoughts, certainly, and some feelings—are intentional, always about something other than themselves. This is true from the first-personal subjective view, involving the qualia resulting from what Russell (1912) called "knowledge by acquaintance" and from the first-personal or third-personal objective viewpoint of propositional attitudes ("knowledge by description"). Qualia are intentional, i.e., about something other than themselves. We think about something, feel something, perhaps our own bodies, as Skinner (1974) proposes. All of the other behaviors normally referred to as mental or cognitive—perceiving, remembering, hoping, fearing, and so on—all share the aboutness that is the essence of the philosopher's definition of intentionality. Moreover, verbal expressions containing propositional attitudes are also inevitably about something other than themselves; such expressions refer or intend. Note that we are speaking here about differences between types of sentence, not between physical and mental realities (Chisholm, 1957; Dennett, 1969).

If we are going to incorporate them in our explanations in order to account for schedule-insensitivity, we are adopting a mode of explanation that goes beyond that of the radical behaviorist, which must confine its explanations to extensional language. But if we accept that the subjective level of experience is real—a proposition that Skinner certainly seemed, much of the time, to embrace—we have to have to face up to the fact that any attempt at expressing these experiences in language, be it by the person whose experience it is or by the observer, must employ the language of intentionality. The resulting sentences differ from those on which extensional behavioral science is built; they do not admit the substitution of coextensives, they permit the phenomenon of intentional inexistence, and they are not reducible to extensional sentences—but this does not prevent their being conceptualized as discriminative stimuli and thereby taking their place in an extensional behavioral science. So, while we have raised the possibility that behaviorists are using the language of an antithetical mode of explanation without realizing it, we have not shown that behaviorism must adopt intentional explanation in its quest to predict and control. However, there is one area where it

is impossible to avoid intentional language and thus intentional explanation: our description of the self-rule formulation process cannot but be expressed in terms of the individual's comparing, planning, determining, and so on. At this point the radical behaviorist has switched from a purely extensional description of behavior to one based on intentional idioms. Nor can these be avoided by an attempt to translate the intentional sentences into extensional accounts.

This is irrelevant to teleological behaviorism, for instance, in which explanation takes the form of constructing the consequences of behavior as its final consequences, and to which efficient causation is immaterial. But it is apposite in any consideration of the use of private events which follows the more usual Skinnerian line. Moreover, as consideration of the need to delimit radical behaviorist interpretation entails, teleological behaviorism itself stands in need of intentionality, albeit for quite different reasons. **Rachlin** claims, in addition, that I am taking an internalist approach, a claim that has certain ontological implications that do not apply to intentional behaviorism. In fact, my invocation of intentionality is without ontological implication, but perhaps it is necessary to state it again because **Baum** (p. 59) argues that failure to recognize that behavior consists of natural events leads me to suppose (erroneously) that complex behavior requires that the organism have the capacity to process information and select responses. Behavior analysis is not about the person or pigeon any more than mechanics is about the earth or moon. The aim is to understand behavior, which does not require using stopgaps like intentional terms (imagined learning histories are the real stopgaps, of course, because they proceed in extensional terms and do not even recognize that it is essential to change our explanatory mode when dealing with these areas that extensional behavioral science cannot cope with; as the discussion of action at a distance shows, such extensionally-framed stopgaps are inadmissible in scientific discourse).

I do not (contra **Rachlin**) insist on an internalist stance. The intentional terms ascribed in intentional behaviorism result entirely from an attempt to overcome radical behaviorism's problem of legitimately applying theoretical terms of an intentional nature. Intentionality is ascribed only to the person, not to sub-personal entities. My warrant for attributing intentional terms (why beliefs and desires?) is that we think in terms of these factors at the subjective level. It is therefore natural to apply them in heterophenomenology. How else can heterophenomenology proceed? From where else can it derive its warrant?

Nevertheless, on "internalism" it can be said (again contra Rachlin) that:

> It often happens that the best theory which accounts for observed phenomena and makes predictions about unobserved but observable phenomena makes use of a good deal of theoretical apparatus for which our only evidence is inferential. An analogy may be helpful in seeing this point. Imagine typing things into the keyboard of a computer, observing the computer's responses, and trying to formulate hypotheses about how the machine will respond to various future stimuli. Conceivably we could do this without appealing to any hypotheses about how the machine is programmed, so that our theory simply took the form of correlations between inputs and outputs. But it seems quite clear that it will

> be far more useful to hypothesize about the machine's (internal, not directly observable) program, using hypotheses about the program together with information about inputs to formulate predictions about the machine's output. Now we may not be quite like computers, but presumably the principles which govern our behavior are at least as complex as those that govern a computer, so we may reasonably expect that formulating hypotheses about our own internal states and processes will turn out to be the most effective way of explaining and predicting our behavior. At the very least, it seems clear that it would be a mistake to rule out *a priori* any theory which made use of such hypotheses.[5]

Tonneau (pp. 141-142) argues that I might conflate linguistic and empirical arguments in defense of my research program and that I might have overstated the dependence of information-processing theories on concepts of belief and desire. Tonneau claims, further, that even if Chisholm's argument is correct it does not show that behavior analysis lacks explanatory completeness. Tonneau is right that natural selection cannot be said to be explanatorily incomplete because theological language does not translate into biological. But if a scientific community adopts a priori the assumption that it can provide a complete explanation of chosen phenomena by adopting one linguistic (i.e., theoretical) mode and outlawing others, there is no reason why it should not exhibit inadequacy if its practitioners are subsequently unable to fulfill their aim without resorting to a proscribed locutionary style. Hence, my argument is more subtle than that which Tonneau supposes: it is that when behavior analysts do use intentional language they are adopting a mode of explanation that goes beyond that allowed within the philosophical terms of radical behaviorism. This is an argument with which my critics seem, on the whole, unwilling to engage, although their accounts of behavior resort often to intentionality.

The Delineation of Behaviorist Interpretation[6]

In the realm of delimiting behavioral interpretation, **Tonneau**'s answer is to forget teleological behaviorism and concentrate on the search for "temporally extended causal antecedents instead of future consequences" (p. 145). It is because of the impossibility of discovering these histories that an intentional stance is required.

Rachlin's teleological behaviorism has this recurring problem, as the following quotation from Mele (1992, p. 4) shows:

> Anscombe asks us to consider a man who simultaneously and with the same bodily motions 'moves his arm, operates the pump, replenishes the water supply, poisons the inhabitants' [1957, p. 45]. Is he performing four actions, she asks, or one? Her answer, with which Davidson (1963) concurs, is a single action

[5] Curtis Brown: Behaviorism: Skinner and Dennett: http://www.trinity.edu/cbrown/mind/behaviorism.html

[6] Please see my *Context and Cognition: The Interpretation of Complex Behavior* (Foxall, 2004) and Foxall (2007a, 2007b) for accounts of this theme.

> described in four different ways—one action under four different descriptions. Goldman disagrees: 'An act-token is the exemplifying of a property by an agent at a time,' and 'two act-tokens are identical if and only if they involve the same agent, the same property, and the same time' (1970, p. 10).

Moving one's arm and operating the pump are not identical properties; they are therefore distinct actions. Thus, Anscombe has described four distinct actions. The same point can be made with respect to the timing of the consequences of an action, which is how Rachlin defines a response. The timing of the consequence of poisoning the inhabitants is not coterminous with that of replenishing the water supply: they are different behaviors. Now, if they are different behaviors, what are the implications for teleological behaviorism?

Intentional Behaviorism and Super-Personal Cognitive Psychology

My argument (which, prose notwithstanding, **Tonneau**, **Burgos**, and **Lacey** not only grasp clearly but reproduce accurately) is linguistic and a-ontological at least in a third-personal scientific sense (**Baum** misunderstands; I am not making any ontological claims about what is going on in the organism with respect to storing etc.). The argument is facilitated by, but not dependent upon, the apparent subjective experience of thinking and feeling and by the apparent universality of first-personal accounts. For it is on the basis of what we take, but cannot prove, to be our thoughts and emotions that we can label the apparent experience of others and try to use it to understand and predict their behavior as well as our own. Folk psychology takes the beliefs and desires so arrived at as causative of behavior, but intentional behaviorism, from its a-ontological stance on these matters, sees them as placeholders rather than elements in a causal theory. Beliefs and desires may have an ontological status, but it is not something that can figure in a third-personal scientific account that relies on interpersonal agreement on observation. But intentional behaviorism's interest in thoughts and desires is only as components in a linguistic portrayal of behavioral explanation that acknowledges the facility offered by apparent subjective experience without reifying it. It simply affords one possible understanding of how we come to employ sentences that incorporate intentional terms, and it is from the differences between sentences that do this and those that do not that its theoretical contribution derives. For the use of one or other linguistic mode entails acceptance of one or other of two incommensurable modes of explanation, only one of which is permissible in radical behaviorism. There is no need to use nouns such as belief and desire here, which have ontological implications. It is sufficient to employ verbs such as believes and desires—as many radical behaviorists advocate—but these are the very locutions that incur intentionality.

Radical behaviorism is a philosophy of psychology that claims to proceed exclusively in extensional terms, employing sentences that permit the substitution of co-designatives. Such referentially transparent locutions are customarily accepted as the very stuff of scientific discourse. Intentional explanation, by

contrast, deals in sentences that contain terms characterized by their "aboutness" or reference to entities other than themselves. The terms in question may refer to items that do not exist (Brentano's "intentional inexistence"). If the sentences in which they occur are to maintain their truth value, they cannot permit the substitution of co-designatives; that is, they are "referentially opaque." Radical behaviorism as practiced by Skinner resolutely avoids such terms and such sentences, at least in its scientific, as opposed to popular, accounts of its subject matter, behavior.

To move from an extensional to an intentional account of behavior, or to include in the former terms and usages that properly belong to the latter, is to shift from one mode of explanation to another, antithetical, mode. While radical behaviorism succeeds in its aims of demonstrating the predictability and control of behavior in experimental spaces and other relatively closed settings (none of which need extend beyond extensional discourse), other scientific objectives such as accounting for the continuity of behavior can be fulfilled only through the use of an intentional vocabulary. To the extent that radical behaviorists have crossed this linguistic line, they are employing intentional explanation.

It follows from what I have said about the ontological status of intentional terms that I am not, by their employment, delving into anything that is going on within the organism. My justification of the usage of intentional terms relies solely on the identification of evolutionarily-consistent neural functions as advocated by Dennett (1969), but even here I am not keen to attribute causal competence to sub-personal events. Many radical behaviorists' interest in neurophysiology is more geared to such a strategy of causality than is intentional behaviorism, for these radical behaviorists have pinned their hopes on a yet-to-be-discovered neural basis of behavioral causation. Ultimately, their reliance on the internal, "beneath the hood" working of the machine that behaves may prove the greater.

Explanation

Radical behaviorism is limited, deficient, and incomplete (**Moore**) because the extensional language to which it ought to be confined does not serve it well beyond the confines of the operant laboratory where the behavior of interest and the independent variables of which it is a function can be unambiguously observed. It is impossible, for instance, to say anything other than the rudimentary about private events in the absence of intentional idioms. This is important because we need to explain behavior that cannot be accounted for in terms of the contingencies of reinforcement. If people's behavior is not sensitive to the schedules in operation we cannot explain it by reference to the contingencies. As Lowe and other agree, we must turn to private events. In this case we have to use intentionality.

Echoing Skinner, **Moore** says that radical behaviorism is "is the very field of what is traditionally identified as voluntary, purposive, or intentional behavior" (p. 115). He claims, moreover, that the inclusion of intentionality "as a future cause" into extensional accounts is "manifestly troublesome" (p. 115). Again in a passage reminiscent of Skinner, Moore agues that to use intentional language means a

move towards an explanation that is cast in different language from that in which the original observation is described (pp. 115-116; see also Moore, 2008). But if an explanation in those observation terms is impossible, we may have to seek a different kind of explanation if we are to have one at all. Again, Skinner's realization that human behavior must be explained in terms of rule-governance as well as contingency shaping is relevant here. Adhering solely to the language of contingency shaping simply because it uses the same terms as the original observation would surely have been restrictive. By also incorporating the language of rule-governance explanations, which included private behavior that must necessarily be described in terms that refer to the private rather than the public sphere of observation and which involve both the phenomenology of intentional experience and the expression of behaviors in intentional terms, Skinner was, implicitly at least, acknowledging that there is no observation language separate from theoretical considerations (Zuriff, 1985, presents the behaviorist view, from which I am clearly departing here, in detail). **Moore** claims that the generic treatment of behavior in radical behaviorism overcomes the need to employ intensional language (p. 116); it is the operant class that is strengthened. But this is purely a description, not an explanation of generalization. It is sufficient for prediction and control, but not for a full understanding. It does not explain why generalization occurs. My use of intentional terms makes no ontological demands; it is not concerned with the internal structure or functioning of the organism even though there is private, subjective information. However, much of Moore's argument up to page 120 seems to overlook this[7].

Branch wonders what a radical behaviorist explanation is. I argue that according to Skinner and other radical behaviorists (e.g., Blackman, 1980, 1983) it consists in the identification of the environmental stimuli that control behavior; according to Branch, it is when the learning history has been uncovered. That is the history that established the discriminative control. *This is not actually at odds with what I am saying.* My essential point is that the radical behaviorist explanation points exclusively to things that exist in the environment, not to intentional entities. Branch and I actually have no need to disagree about this; we are saying the same thing about the controlling factors of behavior, though I would argue it is more than the learning history, it is the intersection of the learning history and the current setting variables—discriminative stimuli, motivating operations, and what they imply for reinforcement.

In asking what kind of explanation intentionality offers, the question arises whether intentions can be considered *causal*. The claim that reasons cause behavior is as widespread in philosophy of mind as to be almost axiomatic. Even your opening the door to the refrigerator is "brought about by the bodily feelings,

[7] I am not, incidentally, deliberately ignoring **Moore**'s appeal to Smith's (1994) wider considerations of theory—I simply do not have space to address them. I have chosen, therefore, to concentrate on what I see as Smith's more relevant thesis to the effect that radical behaviorism is inescapably extensional and seeks resolutely to eschew intentionality.

beliefs, desires and intentions which you had in the minutes leading up to the action. It is clear enough that. . .experiencing and attitudinizing are responsible for the eventual door-grasping behaviour" (Guttenplan, 1994, p. 80). This is the apparently unassailable starting point of behavioral analysis, even though the mechanics of the mind-to-body sequence are not understood. Assert the causal relationship as they may, philosophers cannot agree "in what way. . .that amalgam of attitudes and experiences which could be called your 'mental condition' [is] responsible for your subsequent action. . ." (p. 80), yet it seems absurd to doubt that it is. Dretske (1989, p. 1) writes:

> If beliefs and desires are not causally relevant to behavior, I, for one, fail to see why it would be worth having them. If reasons aren't causes, one of the chief—indeed (for certain people) the *only*—motive for including them in one's inventory of the mind, vanishes. They are no longer capable of doing the job—actually getting us to *do* what they justify us in doing—that was their primary excuse for existing. (emphasis original)

The claim is made by philosophers of mind who bring to bear a variety of ways of understanding the terms involved and the relationships among them, but the assertion of mental causation is near-ubiquitous:

> All mental events are causally related to physical events. For example, beliefs and desires cause agents to act, and actions cause changes in the physical world. Events in the physical world often cause us to alter our beliefs, intentions and desires. (Davidson, 1994, p. 231)

> To explain an action is to give its causes. Its causes are psychological states. (Searle, 1983, p. 67)

The essence of a cause is that without it nothing follows: absent the cause, absent the effect. In addition, we may accept that a cause is a definite event with traceable loci in time and space and that it precedes its effect, but what is crucial is that "It is such that if it hadn't happened then neither would the effect" (Guttenplan, 1994, p. 81). It is frequently asserted that this is a criterion that applies to mental constructs as they are philosophically conceived by dint of mental states' preceding their effect and being indispensable to it—but your having thought about a bacon sandwich before making for the fridge could only be considered a cause of your action on the flimsiest of evidence. You were also thinking about numerous other things, none of which occurred and none of which can therefore have been mentally engendered. Moreover, even if thinking about food is a necessary prerequisite of opening the refrigerator, this account of mind-into-action hardly constitutes a causal explanation since other factors, nonmental as well as mental, such as a learning history with respect to fixing bacon sandwiches, exert causal pressure. (Thorny problem of how your mental operations could have been as causally relevant as we have assumed if, on reaching the

kitchen, you remembered that your roommate had finished off the bacon for breakfast).

The only means by which the indispensability of an antecedent to a subsequent effect can be established in science (so that the antecedent can assume the status of a cause) is experimental replication. Some sciences such as astronomy and evolutionary biology, in which experimentation is infeasible, also rely for their interpretations on inductive reasoning from systematic observation and/or limited experimentation. These are the methods by which radical behaviorists have sought to construct a wholly extensional behavioral science which I will define for now as a scientific account of behavior that eschews intentional locution, assuming the same stance toward its subject matter as does, say, neuroscience or radiochemistry. Radical behaviorism employs as its central explanatory device the "three-term contingency," which comprises an antecedent stimulus in the presence of which an organism has learned to discriminate its behavior as a result of previous trials having resulted in reinforcing consequences (those that increase the likelihood of a repetition of the operation or punishment (consequences that decrease probability of repetition). The three-term contingency comprises, therefore, a discriminative stimulus, a response, and a reinforcing or punishing consequence, and the relationships among them. Moreover, the elements of this explanatory device are defined functionally rather than by reference to intentionality (Smith, 1994). Descriptions of contingent behavior do not take propositions as their object; rather, their object is relationships between an organism's behavior, its environmental consequences, and the elements that set the occasion for those contingent consequences. So behavior analysis strives not to attribute propositional content to any of the elements of the three-term contingency[8].

The sole means we have of demonstrating causality scientifically is the experimental method employed in the context of an extensional science—but when we apply method to the analysis of human behavior the results impinge on the question of the mental causation of behavior. They therefore require careful interpretation. Skinner, who originally was willing to ascribe causation only to public events (retaining the status of responses rather than that of stimuli for private events), came to accept that thoughts and feelings could be discriminative stimuli, "non-initiating" causes which rely for their causal influence on their association with publicly available reinforcers (cf. Skinner 1945, 1988). If we

[8] However, in the case of changing contingencies it may be difficult to establish even this basic level of causal attribution without resort to intentionality in order to account for the continuity of behavior. For example, when a red light that has functioned as an S^D for food, for a food-deprived pigeon, does not predict food any longer, is it an S^D or is it an S^Δ? It seems that at that moment it is (functions as) an S^D for the pigeon but it is an S^Δ for the experimenter. This is surely a case of the experimenter's ascribing an intentional idiom to the pigeon. Would not radical behaviorists explain the pigeon's high-rate pecking as a function of previous reinforcement produced in the presence of the red light? Is this not the same as saying that "from the pigeon's perspective it is still an S^D"? (I am grateful to Dr. Jorge Oliveira-Castro for this illustration which reinforces the points made by Taylor, 1964, to which I referred above.)

accept that individuals have the capacity to modify publicly-provided rules which then serve as discriminative stimuli or motivating operations, we are forced to acknowledge that private verbal behavior may assume a causal role for public behavior. When we attribute to others the verbal rules that they have apparently formulated and followed and which manifest in their insensitivity to scheduled contingencies, we cannot avoid using intentional language that suggests that "He believes that *p*" or "She understand that *p*." But this is not to attribute causality to intentionality. It is merely to say that when an individual's actual rule-formulation coincides with the intentions we attribute to them, their behavior will be predictable in behavior analytic terms. The causes of the behavior are to be found in the contingencies, though the questions (1) whether the contingencies can therefore be modified by the person's rule-making, and (2) just how non-initiating private stimuli are, remain to be answered. But there is an element of the *explanation* of such behavior that involves the ascription of intentionality.

Intentional Behaviorism

Intentional behaviorism is a philosophy of psychology that derives from and extends Dennett's (1969) attempted resolution of the problem of accommodating intentionality within a materialist framework of conceptualization and analysis. It retains Dennett's argument that the mental inheres in the necessity of describing some behavioral phenomena in intentional language, the language of propositional attitudes, which exhibits referential opacity and which is not reducible to the referentially transparent sentences that are usually employed in the natural sciences. The ascription of intentionality is appropriate at the personal level of explanation. The problem arises of using intentional idioms in a disciplined way that both avoids the tendency to proliferate mentalistic language in order to account (usually in a circular fashion) for whatever behavior is observed, and links the use of intentional language with physical reality. Dennett (1969) proposes that intentional content be added, as a further level of heuristic interpretation and in an evolutionarily consistent manner, to the theories and findings of extensional neuroscience, itself a sub-personal level of explanation. When **Burgos** (p. 68) thus confesses himself unsure exactly what I mean by "layer of interpretation" and "another story," it is to Dennett's original argument that I must refer him (though the extended treatment I have given it in Foxall, 2004, might also be useful). The overriding point is to emphasize that this is not an explanation or even description on the same level, ontologically and methodologically, as that proposed by the extensional sciences of neurobiology and behavioral science.

Intentional behaviorist interpretation is thus straightforward: since intentional theory assumes that the structures and events they seek to explain are, having evolved through natural selection, appropriate to their purpose, an important link in this ascription is provided by hypotheses drawn from the natural selection not only of species but, as we have seen, of brains and the nervous system—a system which, through evolution, has the capacity to produce appropriate efferent responses to the afferent stimulation it encounters. It clearly has the ability to

discriminate among the repertoire of efferent responses it might conceivably make. Its ability so to discriminate and respond to the stimulus characteristics of its complex environment means that it must be "capable of interpreting its peripheral stimulation," to engender inner states or events that co-occur with the phenomena that arise in its perceptual field. In order for us to be justified in calling the process intelligent, something must be added to this afferent analysis—the capacity to associate the outcomes of the afferent analysis with structures on the efferent portion of the brain.

For instance, in order to detect the presence of a substance *as food*, an organism must have the capacity not only to detect the substance but thereafter to stop seeking and start eating; without this capacity to associate afferent stimulation and efferent response, the organism could not be said to have detected the presence of the substance *as* that of food. Dennett uses this point to criticize behaviorists for having no answer to the question how the organism selects the appropriate response. There is a need to invest the animal which has discriminated a stimulus with the capacity to "know" what its appropriate response should be. (In fact, behaviorists have ducked this problem by designating it a part of the physiologist's assignment and drawing the conclusion that the behavioral scientists need be concerned with it no longer. The conventional behaviorist wisdom over the kind of cognitive ascription to which Dennett refers is that it amounts to no more than "premature physiology.")

The content attributed to an organism on the basis of its neural state, event, or structure relies on its stimulation *and* the appropriate efferent effects to which it gives rise, and in order to delineate these it is necessary to transcend the extensional description of stimulus and response[9]. It is necessary to relate the content to the environmental conditions as perceived by the organism's sense organs in order that it can be given reference to the real-world phenomena that produced the stimulation. And it is equally important to specify what the organism "does with" the event or state so produced in order to determine what that event or state "means to" the organism. An aversive stimulus has not only to be identified along with the neural changes it engenders to signify that it means danger to the animal; in addition, the animal has to respond appropriately to the stimulus, for example, by moving away. Failure on its part to do so would mean that we were not justified in ascribing such content to the physiological processes occurring as a result of the stimulation. If we are to designate the animal's activities as "intelligent decision-making" then this behavioral link must be apparent. Only

[9] I am careful in my wording here. The possibility of attributing content to the neural states and events themselves is proposed by Dennett, but it is clear from "Intentional Behaviorism" that I question the attribution of intentionality at the sub-personal level, and from "Intentional Behaviorism" and the present paper that I feel the contextual stance to be adequate and appropriate for the explanation of the behavior of non-human animals. Since I would reserve intentional explanation for organisms which can discriminate extensional and intentional language (i.e., to humans), my invocation of intentional explanation can logically apply only to *Homo sapiens*.

events in the brain that appear appropriately linked in this way can be ascribed content, described in intentional idioms.

How are the intentional ascription and the extensional descriptions related, then? (The answer is clearly relevant to **Burgos**'s query on p. 68, which I have just mentioned.) This ascribed content is not an additional characteristic of the event, state, or structure to which it is allocated, some intrinsic part of it discovered within it, as its extensionally-characterized features are discovered by the physiologist. They are a matter of *additional interpretation*. The features of neural systems, extensionally characterized in terms of physiology or physics, are describable and predictable in those terms without intentional ascription, which makes reference to meaning or content. Such a scientific story, consisting in an account of behavior confined to talk of the structure and functions of neural cells and so on, is entirely extensional in character. But such an extensional story could not, according to Dennett, provide us with an understanding of *what the organism is doing*. Only an intentional account can accomplish this, "but it is not a story about features of the world *in addition to* features of the extensional story; it just describes what happens in a different way" (Dennett, 1969, p. 78). Such an extensional theory would be confined to the description/explanation of the *motions* of the organism rather than of its *actions*.

Increasingly, Dennett's justification for the inclusion of intentionality into accounts of behavior has become the facilitation of prediction of the behavior of intentional systems, and since he argues that behaviorism has failed in its attempts to predict and explain behavior, he is left with little else than the intentional stance[10] as a predictive and partly explanatory device. However, it is clear that behaviorism has achieved considerable predictive capacities, at least in the relatively closed setting of the operant chamber. Intentional behaviorism argues that it is less as an aid to prognostication that behaviorism stands in need of an intentionalistic overlay of interpretation, more as a route to explanatory completeness. Without resorting to intentional ascription, behaviorism—especially in its interpretive mode—cannot account for behavior at the personal level of explanation or for behavioral continuity, nor can it show how its interpretations can be feasibly delimited in the face of the equifinality of behavioral consequences.

Intentional behaviorism is essentially Rachlin's position in applying teleological behaviorism to the behavior of addicts. Although he attempts scrupulously to confine himself to extensional language, Rachlin employs intentional terms in explaining behavior change (the converse of continuity). Having described a sequence of addictive behavior as a molar pattern of operant response, he can account for a change in the pattern only by alluding to a "decision" the addict has made to alter his behavior. This arises from his treatment of the breaking of patterns in the process or self-control (Rachlin, 1995). His

[10] The intentional stance proposes that the behavior of systems such as people and computers can be predicted from the desires, beliefs, and other intentional idioms that can be rationally attributed to them.

explanation of behavior cannot proceed without the ascription to the individual of intentionality or even cognitive processing. Hence, on the first occasion of one's ceasing the pattern of overeating—in other words, the next time one eats—there is no pattern of reduced/healthy/responsible eating. The initial lone act must be accompanied by the intentionally construed procedure of changing one's attitude or intention, or the attribution of cognitive processing with respect to one's future, novel behavior. The point is well put by Kane (1995) in his response to Rachlin's (1995) exposition of self-control. Kane (1995, pp. 131-132) argues that the word "pattern" is ambiguous, referring to either (1) a customary form of behavior, or (2) an internal plan or intention to act in a customary way. Rachlin thinks he is talking exclusively about (1), not an internal state but an overt sequence of acts. Kane believes any theory of self-control must include both (1) and (2). A person who has habitually drunk four beers every night may, on sight of his midriff, determine to reduce this to two. After two he is tempted to drink a third but goes home instead. According to Rachlin, exercising self-control is continuing a pattern that is costly to interrupt. The man's exercise of self-control on the first day after his resolution must involve a pattern-as-internal-cognitive-plan, for at that point there is no actual pattern-as-overt-behavior to continue through the exercise of self-control. The only overt pattern in force on the day-after-resolution is the four-beer-a-day pattern, and it is this that must be interrupted by the exercise of self-control, not continued: "It seems that Rachlin must make a concession to cognitive theorists on this point or else find some behavioral substitutes for internal plans newly formed by resolutions or choices" (Kane, 1995, p. 132). I do not agree that teleological behaviorism is *de facto* cognitive, but I would argue that, by embracing intentional locutions in handling its subject matter and employing molar patterns of contingent behavior to infer mental events and processes, Rachlin's system of explanation is closely akin to what I have termed "intentional behaviorism."

Super-Personal Cognitive Psychology[11]

An important difference between intentional behaviorism and super-personal cognitive psychology is that whereas the former identifies placeholders that indicate where an intentional explanation is required, super-personal cognitive psychology identifies the kind of decision processes that would be consistent with observed choice behavior. Skinner acknowledges that such activity surely exists. What super-personal cognitive psychology is doing is specifying the kinds of cognitive operations that would be necessary in order to affect the observed behavior. This is important because it shows what would be consistent with currently known physiology as well as with molar patterns of behavior, but also because it gives a clue to the physiological processes that would be consistent with such decision making and choice. It is the a-ontological character of intentional behaviorism (**Burgos,** p. 67) that motivates the super-personal cognitive

[11] Please see Foxall (2007a, 2007b, 2008), especially the last two, which propose more detailed sequences for the conduct of super-personal cognitive psychology.

psychology program, which is concerned with ascertaining the ontological status if any of cognitive elements. It may prove to be the case that super-personal cognitive psychology is a competence rather than a performance theory (I think there is some utility in maintaining this distinction, though I recognize the concerns of Staddon, 2001a at least insofar as it allows a critical approach to the epistemological status of cognitive theories). Because this stage of my research program is continuing, I have not sought to give an ultimate description of the shape of super-personal cognitive psychology. Elsewhere, however, I have suggested how it might proceed, step by step, to investigate the logic and justification for the attribution of cognitive processes to the explanation of behavior. Since a full exposition of super-personal cognitive psychology is therefore unmanageable in the current context, I hope that these suggested frameworks might satisfy, for now, **Hocutt**'s entirely understandable request for more detail (see Foxall, 2007a, 2007b, 2008; Foxall & Oliveira-Castro, in press).

I share his urgency. For, when **Hocut**t says that he is content to wait for the physiology, the worry is that without super-personal cognitive psychology or something similar he would not know where to look for it nor recognize it once it arrived! That is, in the absence of some idea of the cognitive behavior that the physiology is expected to explain, there is no way of knowing what physiology is relevant to the problem, and the search for such physiological knowledge would have to proceed in the absence of an appropriate behavioral searchlight. It is hard to see how the behaviorist can set the assignment of the physiologist (one of Skinner's well-rehearsed attempts at agenda-setting for other scientists) without the guidelines of a behavioral specification—or, at least, approximation—of the kind of sub-personal processes that the physiologist should be looking for or which the behaviorist will ultimately have to recognize as having resulted from the physiologist's efforts[12].

But in all this talk of intentionality and cognition it is important to recognize that I am not, contra **Baum** (p. 57), saying that there cannot be a science of behavior: indeed it is essential to my scheme that there be one. The ability to predict and control behavior is necessary for the establishment of molar patterns of behavior that are essential to intentional behaviorism. **Tonneau** finds this capacity of behavior analysis to predict and control less convincing than I do (but then I argue that it is principally in closed settings, notably the operant laboratory, that it is best able to predict and control, and the whole point of my research program is to see how far radical behaviorism extends beyond the closed setting). He uses my own words to support this. The conclusion I would draw, however, is that behavior analysis is less than totally successful for the very reasons that it cannot explain behavioral continuity and generalization (e.g., as in imitation, from which—in Bandura's words—I take the example of not being able to identify each element of the three-term contingency; and from considerations arising from memory which

[12] Intentional behaviorism also has the advantage of allowing us to identify the kind of explanation being generated by social psychology (e.g., the multiattribute attitude models). Radical behaviorists ought to be interested in this kind of comparative psychology.

also is a problem of explaining continuity). His criticism of interpretation as based on private events and rule-governed behavior is one I actually endorse and which *I argue below* (see also Foxall & Oliveira-Castro, in press) strengthens the call for intentional behaviorism. Nevertheless, I would not go as far as Tonneau in claiming that "behavior analysis as it stands is not *generally* successful in terms of prediction and control" (p. 140, my italics). In the relatively closed settings described by Lacey and Schwartz (1987), which I have adapted for the analysis of economic behavior (Foxall, 1990/2004), behavior analysis has a good success rate. I agree, however, with the necessity of behavior analysis achieving greater conceptual sophistication, and I have tried to outline in my paper a framework in which the required debate might progress.

Tonneau says, intriguingly, that using intentional idioms does not lead to talking the talk and stopping worrying (p. 145). This, I believe, is why we need to go on to ask whether a performance theory is feasible and what form it might take. This means facing the question: What does it mean for intentionality to "*explain*" behavior (albeit partially) when intentions are assumed not to be causal?

Intentionality can only be invoked once behavior has been causally accounted for by extensional (biological and behavioral) sciences. While the causes so identified suffice to explain molecular instances of behavior, molar sequences of behavior involve temporal and/or spatial distances between response and reinforcer that cannot be bridged by trying to explain the behavior in extensional terminology. Rather, it requires intentional expression. We have to deal with behavioral continuity, the personal level of explanation, and the limits of behavioral interpretation by invoking intentional language, speaking in terms of what the individual "knows," "believes," "desires," and so on. The mode of explanation has thereby changed, but given that we are making a linguistic rather than an ontological departure from extensional science, what does this change signify for the nature of our explanation?

It may simply be the case that molecular and molar behavior have to be spoken of in these separate ways, that the performance theory that is available to account for the former by means of measurable variables that can be functionally related must yield to a competence theory incorporating terms which cannot function at the same level of precision but which are nevertheless the appropriate coinage in which the necessary transactions must be calibrated. Before reaching this conclusion, however, we should consider whether an acceptable performance theory can be formulated that overcomes the temporal and spatial disjunction of dependent and independent variables. Dennett (1981) proposes sub-personal cognitive psychology as the vehicle through which such causation must be sought, while I have suggested that super-personal cognitive psychology, which explicitly recognizes the role of behavior–environment relationships as well as that of afferent–efferent neural linkages, be investigated. If the elements of sub- or super-personal cognitive psychology fail to provide the basis of a causative theory which can adequately supplement the extensional sciences that are inadequate to explain the three imperatives of intentionality, this would support the conclusion that molecular and molar behaviors require distinct modes of explanation.

In order to incorporate intentional or cognitive terms into a *causal* theory of behavior it is vital to demonstrate the indispensability of the entities they represent to the enactment of behavior. This might be done by including them as variables in an experimental analysis or, if appropriate, a survey-based statistical analysis. If they cannot participate in such a procedure *directly* (i.e., not via biological or behavioral proxy variables) they must be accorded the status of *abstracta* rather than *illata*. Their explanatory significance as noncausal participants in theory is not inferior to that of causal elements in an extensional science: it is complementary but of a different kind. Both must then be counted essential to explanation, but the abstracta that are the intentional component of the theory are licensed, shaped, and defined by the variables that compose the extensional components. If the intentional terms of intentional behaviorism prove to be genuine placeholders (i.e., having an essential but temporary status) then the positions they hold will eventually be taken by the illata of the performance theories that supersede them. But if the sub- and super-personal cognitive psychologies that eventuate from further inquiry themselves prove to be competence theories, then the terms that comprise both intentional and cognitive theories will have proven to have similar explanatory significance vis-à-vis those of extensional variables, albeit at different levels of explanation. The evidence of such investigation, at least in the sphere of economic psychology, suggests that this is the case. Intentionality and cognition are not available to participate directly in experimental or other empirical research; they can be approached only indirectly through verbal behavioral proxy variables that, in any case, fail to correlate highly with behavior unless situational correspondence between the measures is high (for review of the relevant literature of cognitive social psychology and the implications of the results for behavior theory see Foxall, 1997, 2004, 2005, 2007a, 2007b).

Explanation Again

In delineating what I understand by explanation, I want to draw a distinction between (1) what can be experimentally demonstrated in terms of causal or (better) functional relations between a dependent variable and an independent variable and which is the domain of extensional science, and (2) what also needs to be said by a science but which is not amenable to experimental rigors (the whole subject of private events enters into this second realm, which is why Skinner categorized their analysis as "interpretive" rather than "scientific," though it is not really obvious what this distinction meant to him). I have used prediction to denote the business of the former and explanation that of the latter. This is clearly not all there is to say about the nature of explanation, but it is a practice that is widespread. However, Dennett takes care to say that the adoption of the intentional stance provides prediction and "partial explanation" of behavior, and this is a practice I shall endeavor to adopt.

Pragmatism and Realism. Contrasting pragmatism and realism—as, for instance, Baum (1994) does in allocating the latter to radical behaviorism and the former to methodological behaviorism—may be simplistic (Lattal & Laipple,

2003). Lattal & Laipple (2003 pp. 56-57) point out that Peirce was both the father of American pragmatism and a realist; James also combined both philosophies. Marr (2003a, 2003b) argues that the two are not necessarily incompatible. Both Skinner and Mach incorporated aspects of realism in their philosophies (see also Bridgman, 1927). Moreover, as Zuriff (1985) and Smith (1986) point out, many of those whom Baum casts as methodological behaviorist realists were also pragmatists. Moreover, if pragmatism is taken to exclude all varieties of realism, then we cannot even say that behavior itself exists.

Burgos proposes that pragmatism is incompatible with how I conceive intentionality and that this is as a result of my adopting the linguistic view of intentionality (p. 68). I see what he means—using intentional idioms means that one is using concepts that may not aid prediction and control—but I am a pragmatist in a wider sense than this, an intellectual pragmatist, interested in the use of intentionality to explain rather than to predict and control. In fact, the methodological approach I am advocating, one which is concerned primarily with the implications of how we talk about behavior rather than with the alleged realities that cause it, is surely redolent of a more pragmatic approach than most work in the experimental analysis of behavior.

Instrumentalism and Real Patterns. Tonneau raises the question of whether centers of gravity differ from intentions in terms of their imputed realism, and he casts me as an instrumentalist. These are complex issues, and I do not expect to do justice to them here any more than philosophers who have taken immense amounts of time and brain power to consider them have been able to. Dennett, to whom the issues are particularly germane, has struggled throughout his career to overcome the insensitivities inherent in the labels instrumentalism and realism. The issues are of central significance to radical behaviorist explanation because if terms such as "operant" and "reinforcement" are considered theoretical, then there is a degree of instrumentalism in Skinner's approach to prediction and that assumed by so many of his followers. Instrumentalism, like pragmatism and realism, comes in many varieties, each with its philosophical justifications and imperfections. I will take it in the sense in which it is frequently used in social sciences such as economics to refer to the inclusion of entities that are not directly empirically available in predictions of behavior. The rest of the approach therefore hangs on the accuracy or usefulness of the prediction. This is, for instance, the sense in which Friedman (1953) is an instrumentalist. This does not appear to apply to my position since I am adamant that prediction and control inhere in the spheres of extensional sciences such as neurobiology and behavior analysis. If this use of intentional terms as a means of explaining behavior is instrumentalism, then it is something that is rife throughout science and should not attract special interest. Moreover, the charge of instrumentalism is not to be leveled against intentional behaviorism for the reason that intentionality derives ultimately from subjective experience, from what Skinner would refer to as the private events observable only by the person who has them. It is not ascribed simply in order to predict. I have sensations (which are intentional because they refer) and I think in terms of

propositional attitudes. I can, therefore, ascribe these to others on the basis of their overt behavior.

Since intentionality is a linguistic phenomenon, it can make a difference only to entities capable of appreciating its linguistic subtleties. The application of intentional explanation is confined, therefore, to humans, since they alone have the linguistic capacity for the differences between extensional and intensional sentences to impinge on their thinking and overt behavior. This exerts a significant restriction on the way in which intentional explanation can be used. Contra Dennett it rejects the notion that nonhuman animals and inanimates can be the subjects of intentionality; contra Searle it means that all intentionality is original intentionality and that derived intentionality is a myth. An intentional system is, therefore, not any entity which can be predicted using the intentional stance (as Dennett maintains): it is one which is capable of making first-hand (conscious, first-personal) use of that stance. Other kinds of entity have other stances; much of the chess-playing computer's behavior can be easily approached from the design stance without recourse to intentionality. Animals' behavior can be predicted via the contextual stance. This is a far more realist basis for any notion of real patterns than Dennett's.

Formally, within the intentional behaviorism framework, we ascribe intentionality *on the basis of patterns of molar behavior and, if it is available, neurological data on afferent–efferent linkages.* Hence, *intentionality* is ascribed carefully on the basis of behavioral criteria and neurophysiological evidence, both of which are required to be shown to be evolutionarily consistent. These considerations combine to make my instrumentality, if it exists at all, very weak—far weaker than Dennett's early apparent instrumentalism, for instance—and he has been at pains to establish the weakness or even nonexistence of his[13].

On Dennett

The person whose work occasions the strongest stimulus for reconsideration on my part is Daniel Dennett. I argue in "Intentional Behaviorism" that Dennett has retracted from a categorical distinction between the personal and sub-personal levels of explanation, a view shared by others who have closely examined his *Content and Consciousness* and compared it with later developments of his philosophy (Dennett, 1969; see Elton, 2003; Hornsby, 2000). The issue revolves around the appropriateness of the ascription of intentionality at levels other than the personal: the "categorical distinction" to which I drew attention precludes this.

[13] I refer readers to the literature on real patterns for enlightenment on Dennett's attempts to escape the instrumentalism label and to establish the kind of realism that might be attributed to the theoretical entities he employs. First and foremost is his paper "Real Patterns" (Dennett, 1991; reprinted in Dennett, 1998). Considerable general philosophical commentary has been generated by this paper, some of which can be found in Brook & Ross (2002), Dahlbom (1993), Elton (2003), and *Philosophical Topics* (1994). For a particular, critical application of real patterns in microeconomic explanation, see Ross (2000, 2005), and in the philosophy of physics see Ladyman and Ross (2007).

However, Dennett's book reveals that, in spite of his pathbreaking (*pace* Wittgenstein, 1953, and Ryle, 1949) denotation of personal and sub-personal levels, manifesting in his relating them respectively to the intentional and extensional modes of explanation (and we shall see his current defense of this position) he left the door open to intentional ascription at the sub-personal, particularly the neuronal, level (see Dennett, 1969, esp. pp. 90-96).

I still think that the distinction is imperative, that mixing levels and intentionality invites a category mistake and that Dennett's writings are both equivocal and confused on this matter. This conclusion is borne out by what he has written in response to Bennett and Hacker's (2003; Bennett, 2007) argument that the intentional stance, when assumed in regard to the sub-personal, rests on the "mereological fallacy"[14]. Dennett's reply indicates both the equivocation and the confusion. On the one hand, he is at pains to point out that in *Content and Consciousness* he advocated the personal/sub-personal distinction being made in such a way as to maintain their separateness with respect to the appropriateness of ascribing intentionality. On the other hand, he makes a plea for the as-if use of intentional language to redescribe what is already covered by the extensional language of neuroscience. If this is the basis of his additional heuristic overlay then I have no objection, but it is essential to point out that such reinterpretation belongs to another level of discourse and explanation than the extensional—that it belongs, in fact, at the personal level.

Dennett and Mereology

Hocutt claims to have a different reading of Dennett from mine. Dennett (1969), he says, justifies attributions of intentionality, despite their unscientific character, on two grounds: first, they can facilitate the prediction of behavior; second, they can guide research into its underlying causes. They provide a "heuristic overlay." Beyond this early statement of philosophical position which formed the basis of his DPhil, in *Brainstorms* (Dennett, 1978) the author develops the intentional stance as a guide to behavioral prediction.

Hocutt argues that, with Dennett, he maintains that to go beyond behavior analysis one must turn to brain science (p. 77). I agree wholeheartedly; the question is how we do so. He claims, moreover, that I do not understand Dennett's position as he (Hocutt) does (p. 82). I cannot see the conflict here. Using intentionality to improve our understanding of behavior and seeing the intentional stance as a heuristic overlay are perfectly compatible. How, he asks, does providing an account in terms of intentionality provide an explanation? In exactly the same way that applying the intentional stance does so. Dennett claims repeatedly that the intentional stance provides both a prediction and an explanation of behavior. I would add that it may be an interim explanation, but that it is nearer a final explanation that behavior analysis can provide in terms of identifying

[14] For a critique of Bennett and Hacker and their use of the mereological fallacy, see Searle (2007).

stimulus and response contingencies. *Using intentionalistic terminology does explain because it allows us to add content to the actions of the organism: it learns such and such, believes such and such, etc. This is a kind of explanation, though it is, of course, corrigible* (see pp. 83-84). This seems to be entirely consistent with Dennett.

I do not accept that pointing out behavior analysis's inadequacies of explanation and arguing that intentionality can supply an answer (a) differs from what Dennett is doing in *Content and Consciousness* and (b) bears any resemblance to informing Newton that his physics needs divine intervention (p. 84). I do not follow his argument that while intentionality is "indispensable" it offers no scientific advance on behavior analysis (p. 84). **Hocutt** seems inconsistent here, first disagreeing with me in strong terms then putting my points in his language—and from this concluding that perhaps I am not wrong! Yet, through it all, I see what I am doing as exactly consistent with Dennett's (1969) seminal approach. Hence, I do not go "in the opposite direction to Dennett" (p. 86). I never abandon the sub-personal level and Dennett's brilliant insight of his first book (though I fear that Dennett himself de-emphasizes it in his later writings). In moving from the personal to the super-personal level, I am actually reinforcing the importance of the sub-personal but finding new ways of using it—that is, via behavior as a criterion of the intentional and appealing to a wider range of evolutionary arguments. Hocutt says (p. 89) that to find the intentionality that is ascribed to states of mind we must look to the objects that cause them and the behavior they elicit. This is precisely what I am doing, why I am expanding on Dennett's project, though I would not argue that mentality is a cause of behavior. Hocutt rambles and is not very clear, but he does offer a defense of Dennett on the mereological fallacy, arguing that Dennett denies, in fact, that brains actually have beliefs and desires (p. 86). Now, it is true that Dennett's earliest statement of the intentional stance led to accusations of instrumentalism, something he was content to let stand for some time but which he eventually repudiated by means of his theory of real patterns which ascribes at least a kind of reality to such mental events. In view of this, and since **Tonneau** associates me with the intentional stance (whereas much of my paper was addressed to explaining how I differ from Dennett on this matter) I will elaborate here in light of Dennett's more recent response to Hacker and Bennett at the 2005 meeting of the American Philosophical Association (APA).

Neuroscience and Philosophy

I agree with Dennett on key matters, and I believe that his method of appealing to evolutionarily consistent afferent–efferent links as a basis for the ascription of intentionality is revolutionary and brilliant. On other matters, such as confining intentional language to the personal level, I disagree. My position on this is essentially that of Bennett and Hacker (2003, 2007), whom I quote now to make sure there is no ambiguity:

> Empirical questions about the nervous system are the province of neuroscience . . . .By contrast, conceptual questions (concerning, for example, the concepts of mind or memory, thought or imagination), the description of the logical relations between concepts (such as between the concepts of perception and sensation, or the concepts of consciousness and self-consciousness) and the examination of the structural relations between distinct conceptual fields (such as between the psychological and the neural, or the mental and the behavioural) are the proper province of philosophy. (Bennett & Hacker, 2007, p. 4)

I would add that behavioral science is akin to neuroscience in this regard; it is the study of the empirical questions that arise with respect to the environmental consequences of behavior and the rate of emission of that and similar behavior. Mental language belongs at the philosophical level, not at the level of extensional sciences such as neuroscience or behavioral science.

Now, Dennett's position is abundantly clear from his response to Bennett and Hacker in the APA debate (Dennett, 2007). The defense Dennett first falls back on here is that his original (1969) distinction between the personal and sub-personal levels of explanation is precisely the distinction that Bennett and Hacker are treating as fundamental. I have no quarrel with this; indeed, I referred to it in "Intentional Behaviorism" as the "categorical distinction," and my argument against Dennett focused on his deviation from this position as he introduced sub-personal cognitive psychology, which required the ascription of intentionality at levels other than the personal. Actually, the position is not quite as clear as that: my rereading of *Content and Consciousness* indicates that even there Dennett made some statements that could be considered at one with his later, more liberal pattern of ascription. But it is that later pattern to which Bennett and Hacker take exception: the ascription to brains of an intentionality that can properly be the property only of minds or persons. Dennett's second line of defense, however, certainly muddies the waters:

> It is an empirical fact, and a surprising one, that our brains—more particularly, *parts* of our brains—engage in processes that are *strikingly like* guessing, deciding, believing, jumping to conclusions, etc. And it is *enough* like these personal level behaviors to warrant stretching ordinary usage to cover it. If you don't study the excellent scientific work that this adoption of the intentional stance has accomplished, you'll think it's just crazy to talk this way. It isn't. (Dennett, 2007, p. 86, emphasis in original)

Dennett is, of course, perfectly aware of what is being said by Bennett and Hacker and is not in denial of the usefulness of the categorical distinction, but he believes there are grounds for deviating from it, nonetheless, in what he would characterize as a limited way:

> We don't attribute *fully fledged* belief (or decision or desire—or pain, heaven knows) to the brain parts—that *would be a fallacy*. No, we attribute an attenuated sort of belief and desire to these parts, belief and desire stripped of many of their everyday connotations (about responsibility and comprehension,

> for instance). Just as a young child can *sort of* believe that her daddy is a doctor (without full comprehension of what a daddy or a doctor is), so a robot—or some part of a person's brain—can *sort of* believe that there is an open door a few feet ahead, or that something is amiss over there to the right, and so forth. (2007, pp. 87-88, emphasis in original)

Whatever their take on Dennett, I cannot see radical behaviorists talking in this way. It is reasonable, of course, in informal discussion to simplify by using everyday language if it communicates more directly—but it is not unduly Gradgrindian to argue that much confusion is avoided in scientific discourse by the maintenance of Dennett's original meanings for the personal and sub-personal *levels of explanation*. Bennett and Hacker (2007) respond by pointing out two things. First, Dennett's use of intentional terms is not scientific; second, he does not answer their point by referring to the personal and sub-personal levels. Only the first is relevant here[15]. These authors proceed to deliver a scathing assessment of Dennett's insistence that brain parts "sort-of believe" etc., but their main point is that there is no ontological basis to even this level of thinking. Clusters of neurons patently cannot be shown empirically to think or decide.

From the point of view of intentional behaviorism, the important fact is that intentionality cannot be the subject of an experimental analysis, and it cannot, therefore, be part of the extensional sciences, which deal with sub-personal and super-personal levels of explanation and on which it relies for its explanatory ascription at the personal level. This, I think, is in line with Dennett's original distinction which, for the most part, is the message of *Content and Cognition.* From Bennett and Hacker's critique and, especially, from Dennett's reply to them, I think I have read him correctly.

Conclusion

Once we have accepted, as several commentators do, that the analysis of behavior involves theoretical terms (e.g., operant, reinforcement, automatic reinforcement, private autoclitic effects) we have, *pace* **Baum,** left behind the notion that our science is only about behavior. Intentional behaviorism recognizes that some theoretical terms carry the linguistic properties associated with intentionality, and that if we use them we are employing not just something extra-behavioral but of a different explanatory significance from the extensional mode that usually characterizes radical behaviorism.

The fact that some terms in our science are inferential should not deter us. Skinner (1988) accepts that talk of the private events of others involves inference,

[15] Bennett and Hacker point out that their objection to Dennett's use of intentional terms does not revolve around his failure to keep the personal and sub-personal levels distinct but on his distinction between mechanical and non-mechanical processes (see Bennett & Hacker, 2007, pp. 132-133). This is an interesting point, but it is not germane to the present argument.

and physics uses inferential terms in much the way that psychology does. Moreover,

> . . .many scientific terms from our very best science are. . .irreducibly inferential. For example, we infer the existence of electrons because of the tracks they leave in cloud chambers, and because of the explanatory richness this inference adds to our overall ability to understand and predict the nature of physical reality. The fact that mental terms are theoretical terms in psychology [we can as well say "The fact that private events are theoretical terms in behavior analysis. . ."] provides no more epistemological warrant, in and of itself, for excluding them from psychology than the theoretical status of terms like 'electron' and 'gluon' provides warrant for excluding them from physics . . . .Overall, the process by which a novice comes to learn how to use the mental term 'pain' from his parents and playmates is not so different from the way a physics student learns how to use the theoretical term 'electron' from his physics professor. In both cases learning the correct usage admits of degree, but learning the correct usage is entirely possible. (Flanagan, 1991, pp. 102-103)

The concern for radical behaviorists is not whether their philosophy of psychology ought to embrace theoretical terms—they are already integral to its operation (Foxall, 2004)—but how such terms should be legitimately ascribed. Moreover, radical behaviorism must take account of the special nature of those theoretical terms that are intentional in orientation (and that its practitioners are apt to employ) insofar as they imply a different mode of behavioral explanation from the extensional, which characterizes their philosophy.

The point of intentional behaviorism is to specify how intentional content can be responsibly and legitimately ascribed in order to explain the behavior of humans. It needs to do this since extensional behavioral science cannot explain (at its level that means predict) aspects of complex behavior such as its continuity. It justifies such ascription on the basis of afferent–efferent neural links and molar patterns of behavior. It is not interested in either of these qua causes of behavior, though it accepts them as such; it is interested only in how they permit the behavior to be interpreted in evolutionary terms, permitting intentional language to be used of it. Hence, its contribution to explanation lies in its showing how behavior is linked with evolutionary logic and findings. In the case of intentional behaviorism, this is specifically with respect to natural selection. Intentional behaviorism is a recognition that intentionality must be employed in the explanation of complex human behavior such as consumption; it attempts to show that this attribution of intentional content can be done responsibly on the basis of the findings of extensional science and the application of evolutionary logic. Intentional behaviorism is a competence theory of behavior, that is, one which indicates the kinds of construct which it would be necessary to invoke in order to explain satisfactorily by providing an interpretation of behavior that is consistent with scientific findings in the spheres of neurophysiology and behavior analysis. The case may eventuate that the intentional terms employed at this level, *abstracta*, are simply placeholders for more precise intentional terms which can enter into

scientific theories, *illata*, and which may transform the theory of behavior into a full-blown extensional account which can be tested using the methods of standard scientific procedure. That is, it may be a performance theory.

Super-personal cognitive psychology is the attempt to evaluate cognitive psychology as such a performance theory (i.e., to determine whether its components are in fact *illata* or remain at the level of [albeit sophisticated] *abstracta*). A suitable candidate for an evolutionary basis for this level, with which the account must be consistent, may prove to be evolutionary psychology, which is invoked in addition to natural selection. Super-personal cognitive psychology moves on from the intentional terms used at the previous level (which include the verbs that enter into propositional attitude statements such as *intend*, *prefer, desire, believe*) to cognitive terms which specify operations at the higher level. The task is to validate (or invalidate) the use of such terms on the basis of neuro-physiologically-based evidence that is consistent with natural selection (here the recent work on neuroeconomics becomes especially pertinent) and molar patterns of operant choice that are consistent with evolutionary psychology. This phase of the research program, currently in progress, will indicate the status of cognitive psychology as a performance theory which can act as an extensional science in its own right by virtue of its incorporation of variables that can enter into scientific theories and give rise to empirical testing, or as a higher-level competence theory whose principal contribution will be the more sophisticated interpretation of observed patterns of choice.

Super-personal cognitive psychology is thus a means of facing up to the ontological questions that arise from intentional behaviorism. It is reasonable to say that intentional behaviorism is a-ontological, that it refers to no more than a level of analysis or explanation that consists in linguistic usages, that its intentional terms are placeholders. A spirit of inquiry makes it inevitable, however, that other questions be asked: placeholders for what? What relationship does intentionality have, if any, with the neural substrates of intending, with molar patterns of operant behavior, with cognition? Does intentionality have an ontological status of itself? Or will it transpire that a cognitive theory of behavior will occupy the same epistemological position as intentional behaviorism—in which theoretical terms are used because the linguistic form in which they inhere is necessary in order to explain behavior rather than because they name an identifiable physical reality? The cognitive theory of operant behavior may prove yet to be a competence theory rather than a performance theory, but it is useful to know the answer if only to set radical behaviorism itself into sharper focus. And, should both intentional and cognitive terms prove to be placeholders for a physiological explanation after all, then at least with the answers to questions such as these we shall be in a stronger position to recognize and appreciate it when it comes along.

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