

BEHAVIORISM AND CHISHOLM'S CHALLENGE

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ABSTRACT: Foxall's intentional behaviorism is supposed to provide explanation and understanding where radical behaviorism provides only prediction and control. Foxall does identify empirical and conceptual issues with the operant reinforcement framework, but he underestimates the extent of its flaws and partly misidentifies their nature. His intentional behaviorism suffers from conceptual difficulties, and its adherence to a form of instrumentalism may actually make it harder to understand intentional phenomena.

Key words: intentionality, behaviorism, environment, causation

Foxall's intentional behaviorism (2007) is offered as a solution to the troubles of the behavior analysts who are "struggling to accommodate the thinking of other psychologists, usually cognitive in orientation" (p. 2). One way to alleviate our troubles is to incorporate a modicum of cognitive principles in behavior analysis (e.g., Killeen, 1984). Intentional behaviorism shares the same ecumenical spirit, but combines it with complex philosophical themes that make it more difficult to evaluate. Foxall is careful to point out that he is not merely conjoining intentional and behavioral principles in an additive effort to combine "the best of each" (p. 46). In intentional behaviorism, incommensurable types of statements form different levels of a single explanatory hierarchy.

As I understand it, the proposed hierarchy is as follows. Foxall assumes that behavior analysis, under the guidance of Skinner's "radical behaviorism," generally succeeds in terms of prediction and control (p. 1). Its exclusion of intentional idioms, however, implies that behavior analysis remains explanatorily incomplete; only through the intentional stance can behavior analysts transcend the limitations of their discipline and attain understanding. In this search for understanding, the extensional regularities of behavior analysis serve as input to the "intentional stance" (Dennett, 1987), that is, the interpretation of behavior in terms of beliefs and desires. Like Dennett's, Foxall's intentional behaviorism is purely instrumentalist. It says nothing of what may happen inside the person, and Foxall emphasizes that he is concerned with the methodology of inquiry rather than ontological questions (p. 9). The attribution of propositional attitudes to persons, however, permits models of the underlying information processing to be formulated. These models, in turn, play a heuristic role with respect to neuroscience and ultimately allow behavioral and neural data to be connected to evolutionary accounts in terms of natural selection.

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Foxall's intentional behaviorism integrates multiple levels of explanation and does not seem to detract from anything. It "takes nothing away" (p. 51). It almost looks like the best of all possible worlds. Foxall's paper is broad, provocative, and ambitious, and he is right that the issue of intentionality deserves more discussion among behavior analysts. I have doubts, however, about the coherence of his philosophical framework and the soundness of his arguments—and when I look at behavior analysis, cognitive psychology, and philosophical work on intentionality, I see a pattern of strength and weaknesses that differs sharply from what Foxall assumes.

Behavior Analysis and Operant Behaviorism

The sort of behavior analysis that Foxall endorses derives from Skinner's radical behaviorism. Its conceptual and empirical ingredients include "pragmatism" as opposed to "realism" (p. 6), the description of correlations between environment and behavior (p. 14), the prediction and control of operant responding through n-term contingencies of reinforcement (p. 6), and the idea that behavior is determined by the "reinforcing and punishing consequences of similar behavior previously enacted in settings similar to that currently encountered" (p. 9).

Foxall asserts that such operant explanations are generally sufficient in terms of prediction and control, and he criticizes Dennett for saying otherwise in the absence of empirical evidence (p. 47). But the evidence is easy to come by, and, curiously, Foxall seems aware of it (pp. 13-15). In his own words, "it is not always possible to detect each element of the three-term contingency when behavior is learned or performed" (p. 13). Aside from the examples of response acquisition that Foxall discusses, consider sensory preconditioning and Pavlovian devaluation effects in animals (e.g., Dickinson, 1980) or the free-recall phenomena that serve as benchmarks for cognitive models of human memory (e.g., Raaijmakers & Shiffrin, 1981). Thirty years ago there was no prediction or control of these facts in operant terms. As far as I know, today there still isn't any. That operant "interpretation" (p. 47) might be offered instead is irrelevant to the scientific standing of behavior analysis; science is not judged on its capacity to interpret—otherwise we would all be Freudians. Besides, the interpretations to which Foxall alludes typically rely on unhelpful concepts (e.g., "private events") or ad hoc categories (e.g., "rule-governed behavior") applied to performance in the absence of any coherent specification of the relevant behavioral processes.

Contrary to what Foxall asserts (p. 1), therefore, it is clear on empirical grounds that behavior analysis as it stands is not generally successful in terms of prediction and control. The problem here is one of scope (Kantor, 1970). Operant theory is generally successful, sometimes strikingly so (e.g., McDowell & Kessel, 1979), when dealing with reinforced responding, but it fails to address numerous cases of behavior change through exposure to environmental sequences and in the absence of operant responses and reinforcers. If progress is to be made, such facts should be dealt with scientifically and, presumably, some prominent assumptions

of radical behaviorism should be modified. At the very least, behavior analysts should study non-operant processes of response acquisition (from simple cases to complex ones) in greater detail, examine how temporally extended sequences of stimuli affect current behavior (e.g., Himeline, 1984), and develop adequate conceptual treatments of memory (Wilcox & Katz, 1981). Above all, behavior analysis must grow beyond the current operant framework (Malone, 1975, 1978); otherwise it will forever face the depressing bill of fare that Dennett (1981) offered to a science of behavior: either flawed accounts in terms of reinforcement and discriminative stimuli, or the intentional stance (Foxall chooses both).

Empirical and methodological changes may bring in philosophical adjustments as well (Laudan, 1984). An aspect of behavior analysis that makes it particularly vulnerable to criticism, I think, is its official reliance on what Foxall calls "Machian positivism" (p. 6). Although this commentary is not the place to defend any specific alternative, for the time being I want to suggest that a realist view that takes causation as a metaphysical primitive better suits science in general, and behavior analysis in particular, than neo-Humean approaches to causation in terms of observable regularities (for supporting discussion see Corry, 2006, Forster, 1988, and Irzik, 1996).

An advantage of adopting causal realism is that it helps diffuse the referential opacity of intentional idioms that Foxall mentions (p. 5), and that seems to make them uniquely mysterious. For causal descriptions do exhibit analogous forms of referential opacity, as a simple example adapted from Dretske and Enç (1984) will show. At Tom's party only two beverages are available, gin and orange juice. Tom drinks a quart of gin and becomes intoxicated. Now, it is not gin being the only the clear beverage available at the party that causes Tom's intoxication. It is gin being gin, an alcoholic drink. Yet gin is the only clear beverage available at the party (compare: "Scott is Scott" and "Scott is the author of *Waverley*"). The implication is that coreferential substitution can fail whenever a single object has two distinct properties with distinct causal roles. This is not mysterious at all if one takes a realist stance both toward properties (Armstrong, 1978) and toward causation.

From Philosophy to Cognitive Psychology

Another starting point of Foxall's analysis is that an intentional framework in which beliefs and desires assume a central role "provides the foundation of cognitive psychology" (p. 2). This may have led Foxall to conflate arguments of quite different types (linguistic and empirical) in the defense of his research program and to overstate the dependence of information-processing theories on the concepts of belief and desire.

First consider the linguistic thesis that intentional idioms cannot be translated in non-intentional terms. In analytic philosophy this thesis derives largely from the attacks of Chisholm (1957) and Geach (1957) on philosophical behaviorism and from Reichenbach's (1947, 1951) failed attempts to explain the concept of perception in extensional terms. It is important to understand why the attempts failed. As Geach (1957) argued, a mental state has implications for behavior only

when acting in concert with *other* mental states. When trying to translate a statement about belief in behavioral terms, for example, one finds oneself forced to mention the person's desires or other beliefs (e.g., Chisholm, 1951, 1952). As a result, mental idioms cannot be eliminated and the purported translation fails. In Quine's words, "there is no breaking out of the intentional vocabulary by explaining its members in other terms" (1960, p. 220).

Yet, even if this linguistic thesis is true it does not demonstrate that behavior analysis is flawed in the sense of being explanatorily incomplete. Arguing otherwise would be like claiming the theory of evolution to be explanatorily incomplete because theological *language* cannot be fully translated in biological terms. In order to make his case Foxall must document actual explanatory limitations of behavior analysis. A possible argument along these lines is to suggest that correlations are not explanatory (p. 14) and that scientific explanation means more than prediction and control (p. 51). This line of criticism has much force against the Machian positivism to which behavior analysts often subscribe (e.g., Chiesa, 1994), but I see no reason to concede the point from my own realist perspective on causation. In my view, an account of behavior in terms of its environmental causes is as fully explanatory as any other scientific account.

As to the difficulties that Foxall mentions in relation to response acquisition (pp. 13-15), they point to empirical limits of the operant framework (e.g., Skinner, 1969). In these cases, Foxall is correct that operant reinforcement concepts provide no explanation—but, of course, they provide no prediction and control either. The cognitive theories that filled this explanatory vacuum over the last forty years (e.g., Bindra, 1972) were advanced largely in response to empirical deficiencies of the operant model, not as an "inevitable linguistic turn" (p. 17), and the extent to which such theories rely on the intentional stance is debatable. Conditioned responses such as freezing in rats do not seem sensitive to desires at all, and the "expectancies" to which Foxall alludes can just as well be seen as memory traces (e.g., Rescorla, 1974). The behavior of animals in such situations seems rather purposeless (Sheffield, 1965) and hardly fits the optimality requirements for applying Dennett's (1987) intentional stance.

More generally, although some influential models of memory may be said to represent propositional content (e.g., Anderson, 1976), others do not (e.g., Wagner, 1981). In information-processing theories of skill acquisition, for instance, the representations of previous responses seem to qualify neither as beliefs nor as desires, and the "aboutness" of such representations is minimal. If it is true, as Foxall emphasizes (p. 50), that radical behaviorism proceeds without recourse to propositional attitudes such as believing that *p*, then we have to admit the existence of cognitivist explanations that make no appeal to propositional attitudes either.

Five Arguments for the Intentional Stance

Aside from stressing the heuristic value of cognitive concepts such as that of expectancy, Foxall gives specific arguments in favor of intentional behaviorism and against extensional behavior analysis. His first criticism, related to what he

calls the “personal level” (p. 9), is divided into two parts: the theory of human action and the nature of consciousness.

The Wedding Example

It is widely conceded that the performance of an action such as signing a check or buying bread cannot be reduced to mere bodily movements. The case of the couple who unexpectedly found themselves married after going through the movements of a Jewish wedding (Juarrero, 1999, p. 53) shows that an action cannot be reduced to behavior plus the social context either. The point here is not that people's actual intentions are irrelevant to the social results of behavior—which at best would be an argument *against* the intentional stance—rather, the point is that people's actual intentions cannot be determined unambiguously from their current behavior plus its actual and future consequences (Juarrero, 1999).

This may well count as an argument against Rachlin's (1992) teleological behaviorism, and more generally any account of behavior that is correlational, non-causal, and future-oriented. But the example of the unintentional wedding does not refute a causal realist approach that takes behavior to be the effect of temporally extended sequences of stimuli (e.g., Tonneau, 1990, 2004). The historical variables behind the behavior of a person whose purpose is to get married presumably are quite different from those that cause people to mimic ritual movements as a joke.

Furthermore, it is not clear that Foxall's proposal fares any better than Rachlin's when dealing with the case of the unintentional wedding. For the issue here is how to delineate a person's actual intentions (if any)—and on Foxall's account there are none, since his intentional behaviorism is as instrumentalist as Dennett's. Perhaps Foxall will reply that one's purpose while performing a given action is the (unique?) purpose that results from applying the intentional stance to oneself. In any event, Juarrero (1999, pp. 54, 58) suggests that intentions cannot be individuated without taking into account consciousness and its causal role with respect to behavior.

Consciousness Unexplained

Foxall's second argument focuses on personal phenomenology and heavily relies on Malcolm's (1977) Wittgensteinian critique of Skinner. Although I agree, from a very different point of view, that Skinner's conception is flawed, the arguments that Foxall takes up do not seem entirely convincing. Malcolm's point is that I need to observe how people behave in order to identify their intentions (anger, happiness, etc.), whereas in stating *my* intentions (anger, happiness, etc.) I do not observe anything in particular.

The point is well taken, but it may arise from rather mundane restrictions on the concept of observation. There are all sorts of commonsense physical things that I may observe in others and not in me. A punch in my nose can make me cry, for example, but it would be very strange of me to claim that I *observed* the punch (although I could be said to observe how brutally Albert punched Boris). Neither could I observe the last ten years of my life or my trip to Oaxaca, for example, yet

the latter is a public event of a philosophically unproblematic sort. If this line of reply is correct, then Malcolm's (1977) distinction between first- and third-person perspectives has few substantive implications.

This being said, deep and controversial issues remain about the nature of consciousness and its relation to physics (e.g., Manzotti, 2006). Look at the colors around you, for example. What are they? Physical theories make no mention of color (they mention wavelength and reflectance at best), yet colors surely exist since they are accessible to consciousness (Chalmers, 1996). Invoking the brain as a *deus ex machina* leads us nowhere since the brain is itself a physical object and remains, according to physics, as colorless as our surroundings. Invoking the intentional stance or the "personal level" (p. 10) fares no better. The fundamental issue of consciousness remains unsolved, with or without intentional behaviorism.

Behavioral Continuity

Foxall's third argument involves what he calls the continuity of behavior (pp. 13-16). Some of the problems that he mentions in this respect are empirical and have been discussed above. It is implausible that the philosophical fate of intentionality would hang on the effects of intermittent reinforcement (p. 15) or on the number of elements of the three-term contingency that are absent during response acquisition. As to explaining how novel stimuli can "select the appropriate response" (p. 25), I doubt that this is harder than explaining how novel stimuli can select the appropriate belief. In any case, Foxall points out that behavioral explanations involve, or seem to involve, time gaps between cause and effect (Rosen, 1975), and he suggests that intentional behaviorism can fill the gaps.

The argument fails, however, because the time gap issue is an *ontological* worry about causal connectedness (what fills the gap between the initial cause and the final effect?) and ontological worries cannot be answered in an instrumentalist framework such as intentional behaviorism (p. 37). Consider an analogous situation in classical physics. In physics it is common to fill spatial and temporal gaps between cause and effect by postulating fields that mediate the observed interactions and to argue for the reality of these fields precisely on the ground that they eliminate action at a distance (Lange, 2002, pp. 39-40). Other physicists disagree and assert that the "electric field," for example, is merely a calculational device—but the physicists who argue along these lines cannot insist that action at a distance is a serious issue. In short, anyone may refuse to raise an ontological question, but those who do raise it should give an ontological answer. With respect to the temporal gaps between environment and behavior, the trust in an eventual physiological explanation is far from superstitious.

Delimiting Behavior

In his fifth criticism, Foxall extends Juarrero's (1999, pp. 53-54) argument to Rachlin's (1992, 1994) teleological behaviorism. Foxall correctly points out that human action typically involves an indefinite number of nested consequences, but that not all of them may be cited as the final causes of current behavior. In order to

identify the person's belief and desire, co-occurrent consequences are not enough and the intentional stance is needed (p. 19).

This difficulty with Rachlin's teleological behaviorism is well known (e.g., Tonneau, 1995) and stems from its reliance on part-whole relations to the exclusion of causality. As far as behavior analysis is concerned, however, the problem has a simple solution: forget Rachlin's teleological behaviorism. Retain the focus on temporal molarity, but look at temporally extended causal antecedents instead of future consequences. The advantages of a realist, non-Humean construal of causation are once again evident in this context. The two workers who state "I intend to place one more brick" and "I intend to construct a cathedral" (p. 20) have vastly different causal histories behind them. The workers (who must be adults, not newborns) react to different cross-sections of a single, temporally extended environment (Holt, 1914).

Science and Intentionality

Our talk of beliefs and desires has been with us for a long time, and probably for good reasons (Sharpe, 1991), yet its relation to science remains controversial. To those of us who worry about what propositional attitudes *are*, instrumentalism offers a comforting response: use the talk and stop worrying. Like Dennett (1987, p. 72), Foxall argues that psychologists should be no more embarrassed by intentional talk than physicists are embarrassed to mention centers of mass (p. 46).

Beliefs and desires (as Foxall views them) and centers of mass are deeply disanalogous, however. Physicists do not shun the concept of center of mass for "some more remote physical explanation" (p. 46) because they already *have* the physical explanation. Furthermore, the concept of center of mass, far from being irreducible, *has an explicit definition in classical mechanics* (in terms of a particular integral). It is this definition, together with the axioms of the explanatory theory, that guarantees the success of center-of-mass approximations (Taylor, 2005, p. 88)—exactly what Foxall's claims of irreducibility (pp. 2, 10) preclude in the case of propositional attitudes and behavior analysis.

If the physicist's lack of embarrassment is to be "an example to the psychologist" (p. 46), therefore, propositional attitudes had better reduce to behavioral terms. And indeed, like a few others (e.g., Boyer, 1984, 1985; Robinson, 1985, 1988) I suspect that there may be a way for behavior analysts to meet Chisholm's (1957) challenge. Our task is a difficult one, but some truisms about beliefs and desires point to what an adequate account should be like. One cannot seek the fountain of youth, for example, if one knows nothing about fountains and nothing about youth, and one cannot believe anything about Hobbits unless told that Hobbits are small human-like creatures with hairy feet. No ordinary object has all the properties being described (small, human-like, with hairy feet), but we are certainly familiar with these properties. We have encountered them one by one while interacting with things past. "Intentional objects" may well be *sequences* of such features scattered over disjoint portions of space-time (cf. Maloney, 1984). As to the interaction of belief and desire, which

proved so troublesome for earlier behavioral accounts, it may consist in the interaction of some environmental sequences (“beliefs”) with others (“desires”) so that behavioral control propagates from shared to unshared components (from “ends” to “means”).

Whether the proposal I have just sketched can be brought to fruition is entirely unknown. A molar viewpoint that acknowledges temporally extended patterns and a realist perspective on causation are two prerequisites of the proposed reduction. Although space limitations prevent me from arguing in their favor, I suspect that they are needed to explain human and nonhuman behavior, regardless of the fate of intentional idioms. But beliefs and desires are too important to be left dangling, isolated from the rest of behavior analysis by time-honored claims of irreducibility. Foxall has the advantage of arguing from positions that seem well established, whereas on the positive side I can only gesture—but I find enough difficulties in his intentional stance to recommend that behavior analysts again rise to Chisholm’s challenge.

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