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On Professor Rychlak's Concerns

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Professor Rychlak has thoughtfully responded to my target article that addressed private events and theoretical terms in psychology. I agree with his objections to the mechanistic approaches that dominate contemporary psychology. However, I disagree that Skinner's radical behaviorism is one of those mechanistic approaches. Moreover, the alternative he advocates is heavily influenced by mentalistic traditions. According to the perspective presented here, the value of his approach derives from the extent to which it may be reinterpreted within a comprehensive behavioral framework.

Professor Rychlak has generously read my target paper (Moore, 1992, this issue) and offered a thoughtful reaction to it. I believe he expresses the following three concerns: (1) that most forms of contemporary psychology adopt a mechanistic view of behavior (Rychlak, 1992, p. 348); (2) that Skinner's radical behaviorism is just as mechanistic as mediational neobehaviorism and cognitive information-processing psychology (Rychlak, 1992, p. 348); and (3) that a nonmechanistic account is needed which accommodates formal and, especially, final causation (Rychlak, 1992, p. 354). I would now like to take this opportunity to respond to Professor Rychlak's concerns.

Concern #1: Most Forms of Contemporary Psychology Adopt a Mechanistic View of Behavior

The radical behaviorism I understand agrees wholeheartedly with Professor Rychlak's first concern. Most forms of contemporary psychology are mechanistic. Although not a contemporary figure, Clark Hull was a mediational neobehaviorist of tremendous intellectual significance who markedly influenced both the style and substance of American psychology. He was explicitly

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committed to mechanistic explanations of behavior. The cognitive psychology identified in the target article unselfconsciously perpetuates the same mechanistic tradition as it seeks to investigate what it calls the cognitive or mental mechanisms underlying behavior (Leahey, 1991, p. 329).

In any event, an equally important issue here is the notion of linear antecedent causation with which the mechanistic view is often associated. As noted in the present target paper, the S–O–R explanatory framework in psychological science is all too common in psychology. According to this orientation, one starts with independent variables, postulates mediating intervening variables or hypothetical constructs if necessary—and they usually are—and then ends with dependent variables. The problem is that this orientation carries with it a sometimes not very-well hidden commitment to linear antecedent causation in characterizing the process by which scientists become knowledgeable. Often defenders of contemporary practices will deny that the event itself takes place according to mechanistic principles. Indeed, debate has raged for years whether the mediating theoretical terms actually exist (i.e., a realist view of theories), or are just inferred constructs that aid predictions, with no position being taken on their actual existence (i.e., an instrumentalist view of theories).

However, the problem from my perspective is why do contemporary theorists of whatever intellectual persuasion believe that they become knowledgeable by embracing a linear chain of independent–mediating–dependent variables? What does that belief entail about themselves and the way they become knowledgeable? In fact, when Professor Rychlak endorses the position that "constructs . . . have the power to cause the behavior of correct predicting, controlling, explaining, and understanding" (Rychlak, 1992, p. 354), I believe he accepts the linear antecedent chain account of how he becomes knowledgeable, as well as the very mechanistic views to which he is otherwise opposed.

Concern #2: Skinner's Radical Behaviorism is Just as Mechanistic as Mediational Neobehaviorism and Cognitive Information-Processing Psychology

On this point Professor Rychlak and I agree in some ways, but disagree in others. As suggested in the discussion of his first concern, I agree that mediational neobehaviorism and cognitive information-processing psychology are equivalently mechanistic. However, I disagree that Skinner's radical behaviorism is mechanistic.

Skinner's notion of operant behavior was the outgrowth of his rejection of S–R mechanistic analyses during the 1930s. The three-term contingency of discriminative stimulus, response, and reinforcer is distinctly anti-mechanistic. Papers are available elsewhere that differentiate radical behaviorism from mediational neobehaviorism as well as from mentalistic approaches such as

cognitive information-processing psychology, so old wine need not be rebottled (Day, 1976; Moore, 1981, 1990; Skinner, 1990).

In the target paper I suggested two broad classes of causes: organismic and environmental. I loosely compared these two classes with Aristotelian material and efficient causes. Professor Rychlak correctly suggests that Aristotelian causation also included two other types of causes: formal and final. Rychlak then points out that from his perspective, restricting an analysis to material and efficient causes is mechanistic. He feels radical behaviorism is not alone in this regard, because most contemporary sciences follow the same practice, and are comparably mechanistic.

Aristotle's doctrine of causes is actually quite a complex doctrine, and my "loose" comparison admittedly cries out for tightening. For example, Aristotle's causes were not always regarded as isolated and independent. In fact, very special relationships connected pairs of causes, such as material–formal and final–efficient, perhaps even to the point of coincidence.

In any event, suppose the contingency is regarded as the efficient cause. Given this supposition, the case can be made that the discriminative stimulus is the formal cause, and the reinforcer is the final cause. The sentient organism remains the material cause. To make such a case requires additional assumptions, of course, some of which may not be acceptable to all readers. The idea is not simply to accept Aristotle's classic doctrine without modification, but rather to consider the abstract sense in which Aristotle's doctrine may be relevant, even though Aristotle's doctrine includes much that is not presently useful. Such a consideration may require readers to focus on refined aspects of Aristotle's causes, rather than accept each cause in its entirety. Nevertheless, the notion of operant behavior as a function of discriminative stimuli and consequences seems a meaningful framework for nonmechanistic analysis.

Concern #3: That a Nonmechanistic Account is Needed Which Accommodates Formal and, Especially, Final Causation

Here again Professor Rychlak and I agree in some ways, but disagree in others. I agree that a nonmechanistic account is needed. I disagree that Professor Rychlak's teleological approach will prove to be valuable, by virtue of its mentalism.

The extent to which any account of behavior should accommodate formal and final causes depends on the interpretation of those causes, as indicated above. Note that an important section in Skinner (1974) titled "Purpose and Intention" begins as follows:

> Possibly no charge is more often leveled against behaviorism or a science of behavior than that it cannot deal with purpose or intention. A stimulus–response formula has no answer, but operant behavior is the very field of purpose and intention. (p. 56)

Skinner's radical behaviorism explicitly engages the class of behavior that is called purposive. It is no wonder that Professor Rychlak can report that Skinner said he would not be bothered in the least if someone claimed that he [Skinner] had capitalized on teleology in his theory (Rychlak, 1992, p. 351). What radical behaviorism does reject is that there is any pre-existing "mental" entity called a "purpose" that pushes events toward some inexorable resolution. Indeed, to so conceive of purposiveness is reminiscent of the "billiard-ball" mechanistic causation that Professor Rychlak otherwise rejects.

Terms such as agency, intentionality, and intensionality have been in the forefront of critical discussions of theoretical positions in psychology for over 25 years (e.g., Boden, 1970; Mischel, 1976; Ryan, 1970; Taylor, 1964). The agent is presumed to contribute something to the event in question that cannot be specified in extensionalistic "physical–thing" terms. Rather, the meaning of the response from the agent's point of view needs to be incorporated into the analysis. As Rychlak (1976, p. 218) notes, agents do not merely respond to antecedent pushes called stimuli. Rather, they precedently array events for the sake of which they will sequaciously behave. Intentionality refers to the belief or desired end that is the object of the behavior. Intensionality is usually taken to refer to the connotation of the behavior in question. Thus, a teleologist holds an analysis must appeal to the agent's conception or belief of what the behavior will accomplish, and what other means are available to achieve this same end.

To be sure, mechanistic explanations do not readily accommodate such issues, perhaps because of some overriding commitment to methodology instead of theory, or perhaps because of limited conceptions of material and efficient causation. What does radical behaviorism say about them? To talk of agents is simply to talk of the behaving organism. After all, the organism supplies its own presence to the behavioral event. Without the organism, no behavior takes place. The organism then stands as a locus where a wide range of different variables—some inside the skin, some outside, some physiological, some environmental—come together to result in a unique achievement: a response (Skinner, 1974, p. 172). The organism is not viewed as merely passive or merely reactive, but as an important contributor to the event under consideration.

In what ways do persons contribute to behavioral events? Persons contribute in several ways. First, persons contribute their genetic endowment, which is unique, unless identical twins are involved. Second, persons contribute a history of interaction with the environment, which is unquestioningly unique. Third, persons might contribute private events, such as thoughts, ideas, perceptions, conceptions, images, precedent arrays of events, or what have you. These private events are often the stimulus consequences of their own precurrent behavior, but the private events have the same effect

as if they originated from another source. The private events do not originate from another source, of course. In fact, whether private events play a role at all may not be apparent to anyone else. Nevertheless, an adequate science of behavior must be able to account for the efficacy of private events without appealing to powers and forces from somewhere else, at some other level of observation, which must be described in different terms, and measured, if at all, in different dimensions (Skinner, 1950).

Intentionality is the ineliminable mark of the mental, according to Brentano. Mental phenomena have an object, or are directed toward something. Purposes are said to possess intentionality, by virtue of their being directed toward some goal object. The trouble is that various ontological commitments to mentalism have meant that a lot of mischievous and deceptive preconceptions are included when we talk about intentions. What is called the intention of a given instance of the most significant kind of behavior, operant behavior, is the reinforcer. Operant behavior is not mechanically elicited. Rather, it is a direct function of its consequences. The expression of the contingency as the functional relation between operant behavior and reinforcement puts the matter in good order.

Intensionality is probably one of the most controversial topics in philosophical psychology (e.g., see discussion in Zuriff, 1985, pp. 42–44). As noted earlier, mentalistic approaches often proclaim their superiority by arguing that a psychological explanation must take into account the meaning of a response to the agent, i.e., intensionality. To be sure, anyone's observations, whether radical behaviorist or humanist, do sometimes suggest that the behaving organism is simply substituting one response for another that has the same "meaning." However, these facts may be readily accommodated by the notion of classes of stimuli and behavior involved in the contingencies of reinforcement. Importantly, responses (and stimuli, for that matter) are not independent entities, but rather are members of collections or sets of responses. Thus, the experimental operation called reinforcement strengthens a class of responses having the same effect, not simply a singular response.

Genetic factors selected over the lifetime of a given species no doubt determine the membership of some classes of stimuli and responses. Interaction with the surrounding environment during the lifetime of the individual organism determines the members of other classes. No "mental" powers need be involved in the analysis, and no intensional or mental attributes need be assigned to the behavior that results.

Professor Rychlak further calls for a psychology that includes an introspective appreciation of inner phenomena. Radical behaviorism agrees with him. In fact, radical behaviorism feels that *it* is just such a psychology. To cite Skinner (1974, pp. 14–15), radical behaviorism restores introspection. Radical behaviorism even provides a nonmentalistic account of how people

can introspectively label or otherwise respond with respect to their introspected private events. To be sure, radical behaviorism holds that what is felt or introspectively observed are not initiating causes of behavior in some nonphysical world of consciousness. Rather, the objects of introspection are such things as the conditions of one's own body. Regarding the internal conditions that are the objects of introspective reports as causes is incomplete because at the very least the antecedent circumstances responsible for those internal conditions need to be specified.

Professor Rychlak then goes on to suggest that it is a *proven empirical fact* that persons are critically involved in behavior change, presumably in ways not recognizable by mechanistic views (Rychlak, 1992, p. 353). In support, he cites Brewer's (1974) provocative claim that "There is no convincing evidence for operant or classical conditioning in adult humans" (see also the excellent review of the data in Rychlak, 1976, pp. 255–271).

However, there is a host of other data that bear on this question. For example, consider the following three experiments investigating awareness as a necessary factor in human operant conditioning. The first two were performed by Hefferline and his associates. In the first, Hefferline and Keenan (1963) selected an invisibly small thumb twitch, measured in microvolts of muscle contraction, as the operant response. Whenever a response in a designated range was detected via electromyographic amplification, a count was added to a visual display. The counts were worth money at the end of the experiment. Under these conditions, responses in the designated range increased in frequency. When extinction was in effect, responses decreased in frequency. Thus, the procedure succeeded in differentiating thumb muscle contractions in the range correlated with reinforcement for each subject. Importantly, however, *no subject was able to report that the thumb twitching produced the reinforcers.*

In the second experiment, Hefferline and Perera (1963) used the thumb twitch preparation in a slightly different way. The experimenters arranged for subjects to earn money if a visible, right-hand button push followed an invisible, left-hand thumb twitch within 2 sec. Their data showed that subjects were able to respond appropriately under this contingency, i.e., where the right-hand button press served as a report of the occurrence of the invisible left-hand thumb twitch. Again, however, *subjects could not describe the contingency in effect.*

The third experiment was performed by Rosenfeld and Baer (1970). It deals with a more complex situation than "muscle twitches." The study involved a deception. The subjects were two undergraduate college students, who were told they were going to serve as "experimenters." They were further told that their task was to reinforce fluent speech in a supposed subject with whom they spoke via an intercom. The supposed subject was to say

nouns, one at a time, on request by the "experimenter." The "experimenter" would then provide points as reinforcers for fluent pronunciation.

Actually, the words on the intercom came from a multi-track tape recording. One track of the tape recording contained fluently spoken nouns, and the other disfluently spoken nouns. The playing of a fluently spoken noun, and the attendant opportunity to award points, was set up to serve as the reinforcer for the "experimenter." If the "experimenter's" request to continue with the next noun was in a specified form, a noun from the fluent track was played, meaning the "experimenter" could award the point. Requests in any other form produced a noun from the disfluent track.

The results showed that the opportunity to award points functioned well as a reinforcer, and repeated conditioning of specific forms of the "experimenter's" requests to continue was accomplished. Again, however, *the "experimenters" were unable to describe changes in their own behavior, or the contingencies applied.*

The three studies cited above suggest that human behavior is indeed susceptible to operant conditioning, and that awareness is not necessary for behavioral change. Of course, one can question whether one has correctly measured "awareness," but at that point the question becomes intractable.

Consider next a recent study by Critchfield and Perone (1990) on the more general relation between human operant conditioning and awareness. In this study, responding was investigated in a procedure where monetary reinforcement depended on both speed and accuracy of the response. Importantly, the subject was sometimes asked in probes to indicate whether the last response had met criterion for reinforcement. The researchers found the reports were inaccurate as much as 44% of the time (Critchfield and Perone, 1990, p. 338). This figure is surprisingly high, given an environment that was relatively free from distractions, a task (button-pushing) that was relatively simple, and extensive exposure to the task. The point is that subjects are not necessarily "aware" of their behavior to the extent many accounts suggest (cf. Brewer, 1974).

A study by Hayes, Rosenfarb, Wulfert, Munt, Korn, and Zettle (1985) provides additional insight into the contribution of subjects to their own behavior in a "self-reinforcement" setting. In this study, college students who wanted help preparing for the Graduate Record Examination served as subjects. In the initial stages of the study, the subjects read a passage from a commercially available self-help study book. The subjects were then presented with a set of six multiple choice questions on the passage. No subject was able to answer more than three questions correctly.

Subjects were then divided into a control group and four experimental groups. The control group was left on its own to study further, and told they could eat experimenter-provided consummables (such as M & Ms, raisins, or

peanuts) whenever they wanted. The first experimental group was a private goal-setting group. These subjects were instructed to set a goal (an increase in number of correct answers on subsequent passages), write it on a slip of paper, and put it "anonymously" into a box. A deception was in place, whereby the experimenters could reconstruct which subjects had set which goals, but so far as the subjects knew, no one else was aware of their goals. The second experimental group was a private goal-setting group with "reinforcement." These subjects were given the same instructions as the first experimental group, but were instructed to use the consummables as reinforcement. The third experimental group was a public goal-setting group. These subjects were given the same instructions as the first experimental group, but the experimenter looked at the slip and said "So your goal for the number of correct answers is _____" when the subject placed the slip of paper in the box. Thus, the goal for these subjects was public: someone other than themselves—namely, the experimenter—knew of their goals. The fourth experimental group was a public goal-setting group with reinforcement. These subjects were given the same instructions as the third experimental group. The experimenter read their goals from their slips of paper, and the subjects were instructed to use the consummables as reinforcement.

The significance of this research stems from the predictions that would be expected on the basis of teleological, organism-based accounts of behavior. According to many such accounts, the critical factor causing the effectiveness of such procedures as used in Hayes et al. (1985) is the agent's conception of the discrepancy between self-standards and self-evaluation of performance related to those standards. On this view, all four experimental groups should have shown comparable improvement, because all four experimental groups explicitly committed themselves to a goal, and were able to evaluate their subsequent performance with respect to that goal.

The actual results were that only the last two experimental groups showed any improvement. The private goal-setting of the first two experimental groups did not work. These results suggest that the public nature of the goal-setting was the critical element in the effectiveness of the procedure. Any change in behavior was related to the potential for socially-based consequences, rather than self-evaluation. (The presence of consummables in this study is simply an incidental part of the design, and no reinforcing significance was expected from them.) The results further suggest that so-called self-reinforcement procedures work by setting a socially available standard against which subsequent performance can be evaluated. Hence, *the agents' autonomous conception of their own performance is not ordinarily a causal factor in the determination of behavior* (for additional conceptual analyses of "self-reinforcement," see Catania, 1975, 1976).

To be sure, there are many cases in the literature where subjects' behavior does seem to change only if they are "aware" of experimental procedures

(e.g., Spielberger and DeNike, 1966). Let us stipulate that all these cases are free from methodological problems, etc. How might these cases be explained? In these cases, the presence of a self-report presumably makes the circumstances very different, and the subject is brought into contact with unique aspects of the procedure that do produce the change in behavior. The change can be explained by a change in discriminative stimulation. The "self-report" or the "awareness" (in whatever material form one stipulates) is simply a new discriminative stimulus that is added to the situation, producing a different form of behavior.

A related issue, of course, concerns the effect of instructions in experimental situations. For example, Kaufman, Baron, and Kopp (1966) showed that if subjects' responses were actually reinforced according to a variable–interval schedule, but they were instructed that a different response–reinforcer relation was in effect, their behavior conformed to the instructions rather than to the actual response–reinforcer relation. These results are often taken to suggest that the cognitive/mental beliefs generated by the instructions are what are important, rather than the actual operant contingencies.

There is no question that the subjects were more influenced by the instructions than the actual response–reinforcer relations. However, such effects may be readily explained in terms of the relatively great strength exercised by verbal discriminative stimuli in our culture. The subjects had presumably lived 21 years or whatever in a culture where they had been repeatedly encouraged to heed verbal instructions. Experiments like Kaufman, Baron, and Kopp (1966) are important because they show the verbal discriminative stimuli of the instructions were stronger than the response–reinforcer relations of the variable–interval schedule when the two are placed in competition, at least for the relatively brief period of the experiment.

Summary and Conclusions

On the occasion of being recognized as "Humanist of the Year," B. F. Skinner delivered an address titled "Humanism and Behaviorism" in Tolman Hall on the UC–Berkeley campus in May 1972 (Skinner, 1978, pp. 48–66). I happened to be in the audience at the time. As I recall, his remarks as well as his selection were controversial because humanism was turning more and more toward the individual as an initiator of action, and radical behaviorism disputed precisely that orientation. The issue was one of interpretation. If humanism means that individual repertoires must be strengthened so that we can all lead free, happy, and productive lives, and indeed survive without over-populating, polluting, or destroying our world with our modern society, then of course radical behaviorism is a humanism, and Skinner was a preeminent humanist.

Professor Rychlak voices important concerns, but those concerns are more appropriately directed toward mediational neobehaviorism and cognitive information-processing psychology than radical behaviorism. To be sure, radical behaviorism is wary of the mentalism in Professor Rychlak's approach. Nevertheless, the facts that make his statements important deserve to be studied and formulated in effective ways.

References

Boden, M. (1970). Intentionality and physical systems. *Philosophy of Science, 37*, 200–214.

Brewer, W.F. (1974). There is no convincing evidence for operant or classical conditioning in adult humans. In W.B. Weimer and D.S. Palermo (Eds.), *Cognition and the symbolic processes* (pp. 1–42). Hillsdale, New Jersey: Erlbaum.

Catania, A.C. (1975). The myth of self-reinforcement. *Behaviorism, 3*, 192–199.

Catania, A.C. (1976). Self-reinforcement revisited. *Behaviorism, 4*, 157–162.

Critchfield, T., and Perone, M. (1990). Verbal self-reports of delayed matching to sample by humans. *Journal of the Experimental Analysis of Behavior, 53*, 321–344.

Day, W.F. (1976). Contemporary behaviorism and the concept of intention. In W.J. Arnold (Ed.), *Nebraska symposium on motivation, 1975: Conceptual foundations of psychology* (pp. 65–131). Lincoln, Nebraska: University of Nebraska Press.

Hayes, S.C., Rosenfarb, I., Wulfert, E., Munt, E.D., Korn, Z., and Zettle, R.D. (1985). Self-reinforcement effects: An artifact of social standard setting? *Journal of the Applied Analysis of Behavior, 18*, 201–214.

Hefferline, R., and Keenan, B. (1963). Amplitude-induction gradient of a small-scale (covert) operant. *Journal of the Experimental Analysis of Behavior, 6*, 307–315.

Hefferline, R., and Perera, T.B. (1963). Proprioceptive discrimination of a covert operant without its observation by the subject. *Science, 139*, 834–835.

Kaufman, A., Baron, A., and Kopp, R. (1966). Some effects of instructions on human operant behavior. *Psychonomic Monograph Supplements, 1*, 243–250.

Leahey, T.H. (1991). *A history of modern psychology*. Englewood Cliffs, New Jersey: Prentice–Hall.

Mischel, T. (1976). Psychological explanations and their vicissitudes. In W. J. Arnold (Ed.), *Nebraska symposium on motivation, 1975: Conceptual foundations of psychology* (pp. 133–204). Lincoln, Nebraska: University of Nebraska Press.

Moore, J. (1981). On mentalism, methodological behaviorism, and radical behaviorism. *Behaviorism, 9*, 55–77.

Moore, J. (1990). On mentalism, privacy, and behaviorism. *Journal of Mind and Behavior, 11*, 19–36.

Moore, J. (1992). On private events and theoretical terms. *Journal of Mind and Behavior, 13*, 329–346.

Rosenfeld, H., and Baer, D. (1970). Unbiased and unnoticed verbal conditioning: The double agent robot procedure. *Journal of the Experimental Analysis of Behavior, 14*, 99–107.

Ryan, T.A. (1970). *Intentional behavior*. New York: Ronald Press.

Rychlak, J. (1976). Psychological science as a humanist views it. In W.J. Arnold (Ed.), *Nebraska symposium on motivation, 1975: Conceptual foundations of psychology* (pp. 205–279). Lincoln, Nebraska: University of Nebraska Press.

Rychlak, J. (1992). A teleologist's reactions to "On Private Events and Theoretical Terms." *Journal of Mind and Behavior, 13*, 347–358.

Skinner, B.F. (1950). Are theories of learning necessary? *Psychological Review, 57*, 193–216.

Skinner, B.F. (1974). *About behaviorism*. New York: Knopf.

Skinner, B.F. (1978). *Reflections on behaviorism and society*. Englewood Cliffs, New Jersey: Prentice–Hall.

Skinner, B.F. (1990). Can psychology be a science of mind? *American Psychologist, 45*, 1206–1210.

Smith, L.D. (1986). *Behaviorism and logical positivism*. Stanford, California: Stanford University Press.
Spielberger, C., and DeNike, L. (1966). Descriptive behaviorism versus cognitive theory in verbal operant conditioning. *Psychological Review, 73*, 306–326.
Taylor, C. (1964). *The explanation of behavior*. New York: Humanities Press.
Zuriff, G.E. (1985). *Behaviorism: A conceptual reconstruction*. New York: Columbia University Press.