The Insufficiency of Mechanism and Importance of Teleology

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The replies to our original symposium papers are thoughtful and scholarly, but also diverse and complex. I empathize with most readers as they attempt to draw conclusions from this abstract and intricate discussion. It is with this in mind that I wish to cast off the "chaff" of our discussion—much of it my own—and examine more carefully what I see as the remaining "grains" of special importance.

To this end, I will attempt to characterize major points of agreement and disagreement among the articles, symposiasts and responders alike. It is usually risky to do this in a discussion of this sort, because so many authors have qualified their positions so much—and I, of course, have a position of my own (itself qualified). Still, an attempt at characterization seems worthwhile, given the significance of the issue and the varied backgrounds, purposes, and sheer number of authors involved.

I also have an ulterior motive for examining the "grains" of the discussion in this manner: I believe that the major point of agreement among these varied authors would be surprising to many cognitivists and others who rely on cognitive theorizing for educational strategies and clinical interventions. I believe that the full recognition of this agreement and its implications leads to a disaffection from the style of theorizing that now pervades cognitive psychology.

But what is the alternative to the present style of theorizing? This issue must go hand-in-hand with any critique, and unfortunately there is little agreement among the authors on the answer to this question. I will contend that whatever the alternative is, it involves a teleology of one sort or another. To illustrate this, I reply to Westerman's excellent description of his alternative to show the need of teleology in his *assumptive* framework. I emphasize "assumptive" because mechanistic assumptions, even if disguised as a teleology, are insufficient.

An Agreement and Its Implications

Let us begin with an interesting point of agreement. All the authors of this issue, despite their other differences, seem to agree that much of cognitive psychology is not significantly different from behaviorism in its use of mechanistic explanations. Cognitive psychology has extended these explanations into the "black box," certainly not a trivial difference from behaviorism, as Chaplin and Martindale note. However, the authors seem to agree that the explanations themselves, as based on "efficient causation" and "mechanism," do not qualitatively differ from one another.

Not all of the authors agree that *all* of cognitive psychology can be so characterized. Martindale feels that this is less true of some recent cognitive psychologists, and Chaplin asserts that this applies only to "experimental cognitive psychologists." Their issue, of course, is the important one of definition, that is, what *is* a cognitive psychologist. Let us dispense with this by accepting these qualifications to our discussion. But even granting these, a large portion of cognitive psychology remains.

With respect to this portion, the authors appear to agree with Martindale that "cognitive psychology is not really a new paradigm but merely a disguised version of behaviorism." For instance, Westerman and Muscari would seem to agree with Westcott when he states that cognitive psychology "is no different, ideologically, from behaviorism." And Tyler appears to agree with the symposiasts that "cognitive research is as mechanistic in its basic assumptions as behaviorism was." Indeed, Chaplin and Leahey seem to feel that this point is not controversial at all. Chaplin considers the point "not very troubling to many experimental cognitive psychologists," and Leahey feels that "accusing cognitive psychologists of being mechanistic is like accusing Mikhail Gorbachev of being a Communist."

The obvious implication of this consensus is that cognitive psychology is liable for some of the same criticisms as traditional behaviorism. Cognitive psychology modified its subject matter (to the mind) and updated its mechanistic explanations (to the computer), but left its essential assumptions of human nature intact. Note that this is precisely the opposite of Martindale's contention. He feels that the problem with cognitive psychology is its topics rather than its theory. Once cognitive psychology turns its sites on other topics, he contends, its utility will be revealed. But unless I seriously misunderstand the consensus of these authors, the issue is not one of topic. Cognitive psychology has long dealt with the qualities of cognition. The issue is rather one of explanation, namely the cognitivists' propensity to explain such qualities in exclusively mechanistic terms.

If this is true, it has enormous ramifications for those who employ cognitive theory in teaching strategies or therapy interventions. Some professionals left or never joined the behaviorist fold because of its "determinism" and "mechanistic" view of human nature. They (mistakenly) thought that cognitive psychology offered a more humanistic view of human nature (Slife and Barnard, in press).

Many felt cognitive psychology had given the person back some *intentional* control of thoughts, feelings, and behaviors. Yet, if I perceive the authors' agreement correctly, no transfer of control has occurred in cognitive theory. Just as behaviorism placed the control of behavioral change with the environment, cognitive psychology likewise relies on the environment for the ultimate control of mental change.

The Meaning and Insufficiency of Mechanism

The more subtle implications of this consensus involve the meaning of mechanism itself. What does it mean that cognitive psychology explains mind with mechanistic metaphors? As Martindale (1981) observes in his book, mechanistic metaphors have been the dominant metaphors for mind and brain for many years. The steam age brought into fashion hydraulic models, and early in this century the telephone switchboard was the primary model. Even before such "modern" notions of machines, the catapult was considered to compare favorably with our mental processes (Searle, 1985). Now, of course, we have the most sophisticated machine of all—the computer. This machine has so permeated our thinking that it is constantly reflected in our language, as we ask for "input" or "feedback" and consider people "programmed."

Few contemporary theorists, of course, would view the catapult as a good model for the qualities of mind under discussion here—agency, self-awareness, intentionality—but many apparently claim that the computer is a good model. To make this claim, however, modern mechanists must demonstrate that the computer is qualitatively different from the catapult or other mechanisms that are not good models. Westcott builds an interesting case for the computer not being ideologically different from other machines. In this sense, the advent of the computer may have saved the mechanistic metaphor from destruction. And, as Dreyfus (1979) has observed, it is the so-called "new developments" in computers that have continued to keep cognitivists from facing the many promises that modern mechanism has left unfulfilled.

The crucial issue is whether the computer (or any mechanism) has qualities that permit it to *operate* as the human mind does. Please note that this differs greatly from the criterion of Martindale who asks whether the computer can merely *mimic* our mental qualities. The issue, as it is framed here, involves the *nature* of minds and computers. Are their natures alike, and if so, in what respect? The issue is not whether something can "put on a good show" of being like something else. My son can sound and act very much like our car. But should I presume that because my son can mimic the car, the car operates like my son? Surely, the criterion of mimicry is not how this issue should be evaluated.

Obviously, the nature of the computer is similar to our minds in some ways, but the question is, is it similar in fundamental ways? "Fundamental" is often in the eye of the beholder, but the mental qualities under discussion here include

self-awareness, purposiveness, and agency. My original paper was an attempt to demonstrate the insufficiency of cognitive psychological examinations of these qualities. Permit me to cut through my own "chaff" and summarize those here.

(1) *Self-awareness:* Feedback loops do not *know* that they *are* feeding back. Because all cybernetic and computational mechanisms are based upon feedback loops, they cannot become aware of their own sphere of operation, regardless of the complexity of the feedback operations.

(2) *Purposiveness:* Like any inanimate object, mechanisms can be *assigned* purposes (e.g., a statistical analysis), but a mechanism cannot be *purposive,* because this would imply that it could intentionally decide its own purpose (e.g., that it prefers *not* to do the analysis).

(3) *Agency:* This issue concerns whether the computer can decide for itself, apart from the purposes or directives given it by its manufacturers and programmers. Computers *seem* to do this at times, but this is always because the user is unaware of a higher-order program (i.e., purpose assigned by the programmer) directing the activity.

Metatheoretical Issues: Explaining or Explaining Away?

In spite of these apparent inadequacies, Leahey and Martindale feel that it is cognitive psychology's "goal to explain, not accept" (Leahey) telic constructs like purpose and agency. This is an interesting characterization of cognitive psychology, and helps us see that the issues here are metatheoretical as well as theoretical. The assumption of these men is quite clear: to "explain" means to use mechanistic metaphors. That is, the entity under study is not fully and completely understood until it is explained with mechanistic metaphors (cf. Slife, in press). This implies that not only the theory is mechanistic, but the metatheoretical criteria for the sufficiency of theory is also mechanistic.

My question to them is probably best put hypothetically: What if the entity under study is *not* mechanistic? Perhaps they would hold that no such entities exist—but what if such entities did exist? Would these authors' criterion for sufficiency of explanation allow them to see these entities? Would they be able to explain them? What alternatives to mechanism are available to them for seeing and understanding these phenomena? As Kuhn (1970) taught us long ago, unless we can conceptualize the *possibility* of an entity, we will never see it in our data.

This is often the purpose of metatheoretical frameworks such as the "Four Causes" of Aristotle (Ross, 1937)—to have a broad enough framework that most possibilities are considered. Mechanism is not *the* explanation in this framework, but merely one possibility—"efficient causation." However, the hold of mechanism on Western scientists and culture is intense, and the tendency is to view efficient causation as the most basic or complete explanation (Slife, 1981). Hence, authors such as Martindale and Leahey attempt to explain final causation in

terms of efficient causation (or teleology in terms of mechanism). Such attempts are more explaining *away* rather than explaining, and the original purpose of the four causal framework is undermined.

An example of this is when a mechanistic theorist tries to conceptualize a final causal construct, such as the possibility of humans having free will. Please observe that the issue is not whether humans actually *have* free will, but whether the theorists can conceptualize the *possibility* of humans having free will. To do so requires an understanding of how free will could conceivably come about and be manifested. Such theorizing might begin with the notion that humans would be able to freely choose, but with a mechanistic framework, theorizing inevitably gets stuck on the question of what determines the particular choice. To say that the person determines the choice or that the choice is arbitrary is begging the question for mechanistic, efficient causal theorists. They, of course, are interested in the prior (efficient) cause of the particular choice (Slife, 1981).

The mechanist is essentially asking here "what determines the free will." However, the framing of the issue in this manner does not recognize even the possibility of free will, because they assume that something must determine the "free" will. The problem is the same for any final causal construct, including purposiveness, agency, and self-awareness (cf. Slife, in press). The point is that free will and final causation are themselves foundational assumptions. They cannot be "explained," as Leahey and Martindale wish, without making them impossible to even conceptualize. They must instead be accepted for the alternatives to mechanism and efficient causation that they are, and "tried on for size" with respect to the data of cognitive psychology.

Teleology as the Foundation of the Alternative

Has final causation been "tried on for size?" Has it been used as an alternative framework in cognitive psychology and perhaps even cast aside in favor of mechanism's greater utility? I can find no evidence of this. As I show elsewhere (Slife, in press), leading cognitive texts make no mention of telic assumptions. Yet, some psychologists like Chaplin, feel that teleology is alive and well, and we "have more friends out [t]here than [we] seem to think." Although I appreciate this sentiment, I question its validity. As each of our original papers show, cognitive psychology is replete with researchers and theorists who use telic-sounding language, but do not use telic assumptions.

The result is the sort of mixed metaphors and "incoherence" of language discussed by Westcott. For instance, many naive readers of cognitive research interpret words such as "goal-directed" to indicate a telic framework, when often the meaning is closer to Martindale's description. For him, goal-directed means the "telic machine" is directed to a goal specified by the programmer in a programming statement. When the human mind is described as goal-directed, however, the naive reader may presume that the mind directs *itself* through its

own selection of goals. Such "mix-ups" occur frequently in cognitive psychology, but can be avoided by differentiating efficient and final causal explanations more explicitly.

Thus far I have dealt very little with teleology *qua* teleology. Its "alternativeness" has been used more as a means of revealing the insufficiencies of cognitive theorizing. It has not been developed extensively in its own right for two reasons. First, the context of the original symposium papers was "questions put to cognitive psychology by teleologists." This context lends itself to a critical rather than a developmental stance. Second, although all of the symposiasts have developed their respective ideas, cognitive psychology has not been especially desirous of aiding us in this endeavor. My hope is that recognition of the insufficiencies of the present cognitive paradigm will facilitate this development.

Unfortunately, this relative lack of development prompts responses like those of Westerman and Muscari in my view. Both men seem very supportive and sympathetic to, as Muscari puts it, the "major premise of these papers." Yet both have important questions and correctives that could and should be addressed as teleology and the dialectic are further developed. Westerman, of course, has more than mere questions and correctives. He contends that he is offering an "alternative viewpoint" to teleology. Space limitations prohibit a thorough reply on my own part, but I accept much of his criticism of my own contribution. My only defense is that I was attempting to "play the game on their turf" with the notion of metacognition. To do so is to accept some common ground in the bargain, and be liable to criticisms of that ground. In this sense, his critique of "uninvolved subjectivity" is an important response.

Our main disagreement has to do with the assertion that his position is a fundamentally different point of view. As my preceding remarks imply, I consider his analysis to be a welcome corrective in light of the symposiasts' focus, rather than a different view. Indeed, Westerman's article is to me an excellent application of telic assumptions among other compatible assumptions, and not a *competing* view at all. I do not consider teleology to be bounded by "uninvolved subjectivism." This is grossly underestimating its level of abstraction in theorizing, and its necessity in the type of thinking that Westerman exemplifies.

Westerman sees my approach as reductive, focusing on the subject's "side" of the subject-object relationship (and false dichotomy). However, a focus on part of the whole is permitted as long as the meaning of the part (i.e., its relation to the whole) is taken into account. In other words, the issue is level of analysis. Westerman chooses to stand back and focus on the "social interaction," whereas I choose here to focus on part of that interaction — cognition. He feels that my perspective suffers from too little emphasis on the subject's involvement in the "world of shared practices," and I acknowledge this. However, I feel his perspective suffers from an incomplete analysis of cognition.

My contention is that without a final causal framework for cognition, the "subject" in Westerman's "involved subjectivity" is lost, and only the "objects"

remain. For Westerman to explain his constructs, he must make clear his principles of explanation. Efficient causation would disallow his description of goals and conditional freedom, whereas final causation is quite compatible with these constructs. He obviously does not have to employ the "four causes" per se, but he cannot avoid this issue, particularly if he wishes to sell his explanation in the cognitive marketplace. As I have described, this is a marketplace in which all explanations are currently measured against mechanistic standards. Westerman's elegant theorizing will likely be viewed as a variation on the themes of established theorists who discuss social interaction, such as Mischel (e.g., 1973). I do not believe that Westerman views his position as simply a variant of Mischel, but without an explicit telic framework for cognition, he runs the risk of this type of misinterpretation.

Conclusion

An interesting agreement among the authors of this journal issue has emerged. The consensus is that a large portion of cognitive psychology is, in some important ways, a disguised behaviorism. The cognitivists have changed psychology's focus to the mind and updated its mechanistic metaphor to the computer, but left behaviorism's assumptions of human nature essentially intact. Is this updated version of mechanism capable of explaining fundamental properties of cognition? If those properties include such aspects of mentation as self-awareness, intentionality, and agency, the answer appears to be "no." The reason that cognitivists continue to rely on mechanistic explanations is largely *meta*theoretical. As long as their criterion for the completeness of theory is efficient causal, alternatives to mechanism will never be considered theoretically or tested empirically. The authors of this journal issue disagreed about what the most viable alternative conception would be. However, without some clarity at the metatheoretical level, such discussions of alternatives will not be heard. The value of teleology is that it draws attention to metatheoretical issues, while simultaneously showing a clear, though perhaps underdeveloped, path to a theoretical alternative.

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