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A Mote in the Mind's Eye

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I should perhaps be gratified by the generally positive responses from the commentators to my little essay on mechanism and contextualism (Marr, 1993), but that would be as inappropriate as it would be irrelevant. My primary response has been one of surprise and perplexity that the issues raised in discussions and presentations of this topic have occasioned so much interest, excitement, and controversy. The heavily attended and lively "debate" on mechanism and contextualism organized by Steve Hayes at the recent ABA meeting in Chicago attested not only to the intense interest in the topic but also to the vast conceptual distances between perspectives. Regrettably, much of the discussion came to, using an old expression, fighting over the shadow of an ass.

ESSE QUAM VIDERI

Part of the problem is that raised by Schull and Lawrence, namely, a disadvantage to the exposition and support of a mechanistic view is that one must necessarily incorporate the inherent and often subtle complexities of what it means to exercise that approach. Morris had already demonstrated in his very thoughtful and scholarly analysis of definitions of "mechanism" (1993) that there is no single useful definition as it is applied to behavior analysis. Nagel (1979a) spends some 50 dense pages attempting to provide a logical analysis of Newton's three laws of motion and, with that, a conceptual foundation to mechanics in physics. I quote from the last paragraph of his essay:

> . . . no brief and simple answer can be given to the question: What is the logical status of the Newtonian axioms of motion? It is quite certain that the axioms are not a priori truths to which there are no logical alternatives; and it is equally clear that none of them is an inductive generalization that has been obtained by extrapolating interrelations of traits found to hold in observed cases. But beyond these negative characterizations of the axioms, a reasonably satisfactory answer to the question requires reference to the place the axioms occupy in some particular codification of the theory of mechanics, and to the uses to which the axioms are put in various special contexts. (p. 202)

So, even without venturing into the nitty-gritty of "doing mechanics" but staying on the lofty plane of "conceptual mechanics," the complexities are immense, and we never step out of context. My principal point was that, as Schull and Lawrence recognize, it is in the doing that one comes to understand not only the power of the perspective but also its difficulties and limitations. A comparable situation exists between the traditional formal presentations of what constitutes a science and the actual practice of science itself.

Of course, a similar argument about conceptual versus functional approaches might be made by those who call themselves "contextualists." The problem here is that they seem largely content with a formal, conceptual analysis of something called contextualism without an explicit empirical program to demonstrate its effectiveness as an approach, set in contrast to what they call mechanism. Meanwhile, those whose work fits well within a mechanistic framework continue to explore areas ranging from Pavlovian conditioning to behavioral pharmacology to stimulus control to motivation to problem solving to memory, incorporating manipulations of context as an element inherent in their investigations and formulations. In that context, what does it matter what Pepper or anyone else might say about what our Weltanschauung *ought* to be?

Putting forth anything like a coherent mechanistic view to an audience of be-

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havior analysts, and to psychologists in general, is made all the more difficult because of their often dismal training, graduate and undergraduate alike, in basic science and mathematics. Behavior analysts traditionally have been suspicious, if not outright contemptuous, of mathematical treatments of behavior. Worse, many students pursue behavioral science *because* they can avoid the "hard" sciences and mathematics. The deep irony is that behavior analysis, almost uniquely, has revealed a level of orderliness in behavior that cries out for mathematical treatment. Now, some 40% of papers in the *Journal of the Experimental Analysis of Behavior* reflect a mathematical approach to their topic. But the number of contributors to this work remains woefully small. If the majority of our students (and their instructors!) remain ignorant of these developments, not only are they fated to fall by the wayside, but the training of new contributors will be in jeopardy.

STAY, ILLUSION!

Staddon provides a wonderful critique of the whole contextualism affair, and critics of "mechanism" as opposed to "contextualism" would do well to study his work as a model of the power, the beauty, and the elegance of an essentially mechanistic and analytic approach to behavioral phenomena. I would take issue, however, with his assessment of naive realism as an essential dogma of the scientific enterprise.

"Faith" as Staddon uses the term is unlike "faith" in the theological sense. Theological faith is not based upon evidence. If one demanded evidence, then you would question his or her faith! "I believe *because* it is absurd," said one believer. A more cynical view was expressed by Mark Twain: "Faith is believing what you know ain't so." This is in contrast to scientific "faith" ("working assumption" sounds better) in a reasonably orderly and discoverable external world, invoked in an effort to reveal the nature of Nature. The origins of the reality assumption are arguable and obscure, but surely "successful working" in the form of useful prediction and control of phenomena must have played (and continues to play) a role in shaping and maintaining this perspective. As for when comparisons should be made to assess successful working, the same question applies to the belief in a single, external reality. Just when do we have a real grasp of what that is? What criteria other than successful working would one apply? Toward the end of the 19th century, there were eminent physicists who believed all the basic questions in physics had been resolved; only the details needed working out. Were they in for a surprise! I argue that pragmatism is both the proximal and fundamental source of scientific progress. Science itself emerged from successful working in solving everyday practical problems.

Certainly, many scientists have professed and vigorously defended a realist view (Einstein is the most notable example). Nevertheless, there are many who have not. The same situation exists in mathematics, where many mathematicians believe in a curious "reality" of mathematical objects and relations. They see themselves as explorers and discoverers in this Platonic world. Clearly, however, history shows that progress in either science or mathematics has not, and does not, depend on such beliefs, or, indeed, on any particular ontological stance.

REDUCING AGENTS

I am sympathetic to Blackman's cogent remarks on the apparently peculiar status of behavioral explanations immersed in a sea of reductionist pharmacology. I, too, have wrestled with the roles of reductionism and causality in behavioral pharmacology (Marr, 1990). The issues are complex even within a purely behavioral account, but context is an essential element in any significant reductionist scheme, at any level. For example, although each of the fields of behavior analysis, pharmacology, physiology, and biochemistry have their own turf (i.e., terminology, methods, modes of analysis and explanation, etc.), there inevitably

arise questions of the interrelations among such fields. We can view any biological system in terms of a hierarchy of processes whose description is founded upon a set of interpretations of phenomena at one level in terms of another level, a *heterogeneous reduction* in Nagel's terms (Nagel, 1979a, 1979b). Each level of the hierarchy provides the boundary conditions for interpreting the phenomena at the subadjacent level. Such constraints are not inherent in the laws or features of the subadjacent level. Another way of saying this is that boundary conditions are not part of the laws; they provide the context for interpretation of the laws. To quote myself (Marr, 1990):

> . . . in order to achieve such a [heterogeneous] reduction, laws would have to contain terms or derived relations that provided bridges to, or a communality with, statements of behavioral principles. This, in turn, is dependent upon some *theory* of the role of the reducing processes in reference to behavior, not to some putative *inherent properties* of the constituents of the reducing system. What one is actually talking about here is the verbal behavior of the scientist, and not the "revealed nature" of physiological constituents. In the most successful of reductions—classical thermodynamics to kinetic theory of gases—*statements* about temperature were reduced to *statements* about the average kinetic energy of gas molecules. There are no inherent properties of molecules that would allow the observation that temperature is identical with the average kinetic energy of the molecules. So-called "emergent properties" then depend on theoretical systems adequately developed at the relevant levels which include laws to bridge one level to another. (p. 9)

Thus, reduction does not eliminate or replace, say, behavior, by something "below" it; rather, behavior and behavioral laws provide the context to understand the levels below. Reduction, then, is always a two-way street. Imagine claiming to understand the nervous system, including its pharmacology, without understanding the behavior of the organism.

Blackman's frustration, which is shared by many behavior analysts, is in getting these other reductive fields to pay proper attention and respect to a behavioral science, especially when those fields have an interest in behavioral phenomena. I do not think that an "acceptance of a role for contextualistic explanations" (p. 238) is the key. I have tried to argue that context plays an essential role in every science. Part of the difficulty, I think, is that the barrier of ignorance between behavior and physiology is much higher than that between physiology and any other science in the reductive hierarchy. As evidence, just look at the training necessary to produce a productive researcher in physiology or pharmacology, compared with that of the typical behavior analyst. The working scientific and quantitative repertoire is at least an order of magnitude greater in those fields, with the good reason that there exists an almost continuous nexus of content, laws, concepts, reductive relations, and so forth within and between pharmacology, physiology, biochemistry, molecular structure, physical chemistry, and on and on. Compared with all that, behavior analysis looks like a lonely island indeed. We should train more capable and adventurous explorers.

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