

HOW SHALL WE ACCOUNT FOR VARIANCE?

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ABSTRACT: Field and Hineline have shown how pervasive and insidious is the tendency to make dispositional attributions, even among those who criticize the practice, and they identify a bias for models of contiguous causation as one reason for this tendency. They argue that order can be found at multiple scales of analysis and that in some cases a translation to a model of contiguous causation is impossible. I suggest that pragmatic considerations are sufficient to justify a particular scale of analysis and observe that behavioral principles are fundamentally extended in time. However, I argue that accounting for variance is the goal of science; when events at one level are indeed mediated by those at another, more of the variance can be accounted for by considering both, and there is no principled reason for considering only one.

Key words: Accounting for variance, mediation, scales of analysis, molar vs. molecular accounts

The Field & Hineline paper (2008) is a remarkable document. The authors follow the implications of the assumption of contiguous causation wherever it may lead; like a hound dog on a scent, they range widely across the landscape of intellectual discourse, and in so doing they have produced a provocative document of exceptional scholarship that will surely serve as a benchmark for all future discussions of the topic. A putative effect of the assumption is to encourage dispositional attributions, and one important contribution of the paper is in showing how pervasively and insidiously such attributions seep into the discourse even of those whose goal is to show the “error” in the practice. Such attributions are not wrong in principle, but they are incomplete at best and almost invariably vacuous or circular.

A second point of the paper, and a deeper one, is that behavior can be a function of temporally extended patterns of events, that order emerges at multiple scales of analysis, and that an exclusive commitment to contiguous causation in nature is an error. This is a difficult topic, and I shall devote this commentary to it.

There can be no dispute that order emerges at multiple scales of analysis, and there are often sound reasons for preferring a relatively macroscopic level of analysis. The authors make this point clearly and cogently. Moreover, they are careful to point out that they have no principled preference for temporally extended units, nor do they object to appeals to physiological events or verbal mediation when they can be demonstrated independently. As they say, “*Focusing upon one scale does not deny the importance or validity of the others*” (p. 45, emphasis in

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original). Their principal target is the widespread practice of inventing hypothetical surrogates for patterns of events to serve as immediate causes of the phenomena of interest, when an objective account is there for the taking at a different level of analysis. With all this I find myself entirely in sympathy. However, as one with an avowed taste for relatively molecular accounts, I find myself urging the consideration of several additional points. Whether these can find an easy home in the conceptual scheme outlined in the target article, I am unsure.

First, to confirm the extent of our common ground, let me observe that the very conception of an independent science of behavior is an implicit endorsement of the thesis of Field and Himeline. Skinner (1931) defined a reflex as the correlation of a stimulus and a response, explicitly omitting unobserved physiological mediators. In a broader vein, he emphasized that a science of behavior could proceed independently of the science of neurophysiology, that its facts and principles did not need validation from the latter discipline (Skinner, 1938). Even the finest-grained of behavioral accounts is “molar” in the sense that relevant variables are separated in time and space.

Moreover, Skinner argued that units of analysis in science should be defined empirically, not *a priori* (Skinner, 1935/1999). As far as I can tell, this policy is unique in the field of psychology, and it fits squarely within the framework adopted in the target article. Specifically, he argued that an experimenter should modify his definitions of independent and dependent variables until the relationship between them reaches maximal orderliness. When put into practice, Skinner found something surprising:

As a matter of fact, when we have reached the point at which orderly. . . changes appear, we cannot go beyond it with further [precision in the definitions of the terms of our analysis] without destroying this desired result. In the example of the lever, we may obtain smooth curves by restricting up to a certain point only; if we further limit the response by excluding all examples except those of one given kind (pressing with a certain muscle group, for example), we destroy our curves by eliminating many instances contributing to them. The set of properties which gives us “pressing the lever” is uniquely determined; specifying either fewer or more will destroy the consistency of the result obtained. (p. 516)

Thus, Skinner argued that in practice the relationship between behavior and its controlling variables is most orderly when terms are defined with a bit of slack in them, not when they are defined with maximal precision¹. I see these remarks about the conceptual foundations of behavior analysis as entirely compatible with the discussion of Field and Himeline’s paper—but Skinner was making a pragmatic argument, not a principled argument for generic terms; he assumed that behavioral phenomena could be explained at other levels:

I am not overlooking the advance that is made in the unification of knowledge when terms at one level of analysis are defined (‘explained’) at a lower level.

¹ See Palmer & Donahoe (1992) for a discussion of the relevance of this point to the status of behavior analysis as a selectionist science.

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Eventually a synthesis of the laws of behavior and of the nervous system may be achieved, although the reduction to lower terms will not, of course, stop at the level of neurology. (1938, p. 428)

I take it that this view of levels of analysis is not shared by Field and Hine, for they are clear that at least some analyses cannot be reduced to lower-level terms. The structural members of bridges may be arranged in triangular patterns, but triangles are not found at the level of the molecules of which those members are composed. Moreover, the authors object to the assumption that relationships at one level of analysis must be mediated by those at a finer level; physics adopted a model of action at a distance, so there is no reason to object to action at a temporal distance. As I understand the target article, the authors would argue that Skinner's vision of "the unification of knowledge" rests on unjustified assumptions about the hierarchical nature of our interpretations of the world.

I am not persuaded by these arguments. It may be that triangles cannot be found in molecules of steel, but the explanatory task at hand is not to explain triangles but to explain the strength of bridges, and one can do so without appealing to triangles. The resonance interpretation of the tides in the Bay of Fundy may not be possible in terms of molecules of water, but it is not clear that one cannot explain the tides without appealing to resonance. The terms *triangle* and *resonance* are analytical terms inherent in a particular level of analysis, but they are not intrinsic to the phenomena to be explained. As for the analogy between action at a physical distance and action at a temporal distance, it is not exact. So far as we know, there is no mediating ether in space. In contrast, there really is a nervous system, and it really does mediate the relationship between environmental events and behavior.

The primary task of science is to account for variance in phenomena of interest. If a relatively molar principle is adequate to the task, as is often demonstrably the case, one can proceed without consideration of mediating events. Moreover, one can often explain substantially more of the variance in the data at a relatively molar level, for the relevant variables may be more accessible at that level. One can demonstrate orderly relationships between weights on a seesaw, and there is little point in speculating about the mediating role of supposed molecular bonds in the board. One can observe orderly relationships between behavior and its controlling variables without concern for the presumed mediation by the physiological substrate.

The tensile strength of a board can be neglected because it does not vary appreciably over the course of our observations, but if the board becomes riddled with termites, or if we replace it with a band of thin spring steel, the tensile strength can no longer be neglected. Likewise, the regularity of functional relationships in behavior permits us to ignore the role of the nervous system, so long as that role is essentially constant, but if, over the course of our behavioral observations, a variable affects the action of the nervous system—say the rise and fall in the action of a drug—our behavioral observations will cease to be orderly, whereas an analysis at the level of neurophysiology will continue to be so. More

generally, when a relationship at one level is indeed mediated by events at another level, we may not need to appeal to both levels under many conditions, but it is indeed an advance if we are able to integrate the analyses, for we will ultimately be able to account for more of the variance in the phenomena under consideration by a consideration of mediating events. With regard to gravitational attraction at a distance, it appears that none of the variance in functional relationships can be explained by a medium, but that is not the case in behavior.

Relevant mediating events need not be physiological. Behavior is often the joint product of an external environment and other behavior of the organism, behavior which may escape notice. It is often appropriate to call such other behavior “mediating.” If one is asked to find the next largest prime from 83, the target response is “mediated” by a set of counting and factoring responses which may occur with great speed and below the threshold of observation by another. In this case, a consideration of mediating behavior seems to be required for an adequate account of the performance.

Much human behavior is multiply determined. For example, what we say at any given time may be influenced by any combination of physical objects in our field of view, by motivational variables, by the presence of an audience and the properties of that audience, by what someone has just said, by the orientation of our receptors, by ambient textual stimuli, by our own recent behavior, and so on. If all of these variables indeed affect behavior, then accounting for variance in behavior will require a consideration of all of them. Temporally extended accounts necessarily treat such fine-grained events as either constant or irrelevant; if they were not, we would be unable to “look through” a temporal pattern. The farther we move away from idealized laboratory preparations, the more complex the web of controlling variables is likely to be and the less likely it is that they will remain constant over the course of an extended temporal window.

The authors’ principal objection is to appeals to hypothetical mediating events whose only purpose is to bridge temporal gaps in the analysis in order to preserve a notion of contiguous causation, just as reflex physiologists invented a “conceptual nervous system” for the same purpose in Skinner’s day. On this score I have no argument, but I would simply add this codicil: If there are (real) mediating events in the temporal gaps of one’s analysis, they are likely to account for some of the variance in question and should be brought into the analysis whenever it is practical to do so.

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