

Behavior theory: A contradiction in terms?

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Skinner's major thesis in "Methods" is that theory is not needed in the study of behavior, where "theory" refers to "any explanation of an observed fact which appeals to events taking place somewhere else, at some other level of observation, described in different terms, and measured, if at all, in different dimensions." To flesh this out, he discusses in some detail what he means both by the study of behavior and by theory in this domain.

The basic datum of behavior is rate of responding. To be sure, this is not the only thing one can observe about behavior, but it is the one that, to date, has provided the most striking regularities. "Once in possession of an effective datum, however, we may feel little need for any theoretical construct" such as strength of bond or excitatory potential. "It is no accident that rate of responding is successful as a datum because it is particularly appropriate to the fundamental task of a science of behavior. If we are to predict behavior (and possibly to control it), we must deal with *probability of response*." Of course, probability is not an observable in the same sense as is rate. Moreover, "rate of responding is not a 'measure' of probability, but it is the only appropriate datum in a formulation in these terms." If I understand this correctly, Skinner is contrasting, on the one hand, the time series of discrete events that he observes – key presses, bar pushes, and the like, which he reports as a cumulative record – with, on the other hand, an underlying mechanism of what he calls response probability, which itself is not observable. (In statistics, such a probabilistic rate function, which has none of the usual properties of probability as such, goes by the name of hazard or intensity function, not response probability, which is used for static probability structures.) Many view the unobservable intensity function as a theoretical construct, designed to relate discrete events that can be observed to the hypothetical concept of an underlying, continuous disposition to respond. Apparently, theory at this level is acceptable, and the time series of behavioral events and the underlying intensity function are viewed as being measured on the same dimension.

Yet when it comes to choice, Skinner seems to feel that theory, at what seems to me a comparable level, is unacceptable. For example, we find:

An effort may be made to define [choosing] solely in terms of behavior, but in traditional practice [it] refer[s] to processes in another dimensional system. . . . The appeal to theory is encouraged by the fact that choosing . . . is not a particular piece of behavior. It is not a response or an act with specified topography.

Neither, of course, is response rate or "response probability." I fail to grasp why a stochastic intensity function is acceptable but a choice probability is not; they seem cut from the same cloth, the one having to do with a process that unfolds in time and the other with the behavior at a prescribed instant when the subject is required to make a choice.

Skinner's major objections, however, are reserved for modeling of a more substantive character. He divides models into three classes. The first consists of purely behavioral ones that formulate empirical generalizations in terms of behavioral and

experimental observables. Because these observables are all viewed as arising from the same dimensional system – exactly what this means is never specified clearly – such theory is acceptable, and Skinner believes it will arise when an adequate amount of orderly behavioral data are in hand. And, indeed, in the past few years, long after this material first appeared, such modeling has become a prominent development in operant research (e.g. Prelec 1982). To my knowledge, Skinner has not commented publicly on such models.

Skinner's second and third categories both involve reductive models that attempt to account for behavior in terms other than behavioral and experimental variables. In the first, the attempt is to reduce behavior to physiological observations. Here he says there are two systems of dimensions, not one, and one tries to account for the details of the behavior in terms of measurable brain activity. At the present time such modeling is almost always carried out by postulating hypothetical mechanisms that are assumed to operate on physiological variables we can currently observe and measure. For this reason, these models are not greatly different from the second class, which consists of those that attempt to reduce behavior to hypothetical mental processes. It is the hypothetical mechanisms invoked in both types that draw Skinner's fire, leading to constructions that he says are worthless or worse. The central objection is: "When we attribute behavior to a neural or mental event, real or conceptual, we are likely to forget that we still have the task of accounting for the neural or mental event."

When one reads this, what comes to mind? One reading, suggested to me by R. J. Herrnstein, has to do with studies that demonstrate a correlation between a behavioral and a physiological event, and the reader is asked to accept, without a carefully worked out argument as to how, that the latter event explains the former. Often in these cases one neither understands how the physiological event arises nor what, if any, causal relation it has to the behavior. A second reading, suggested to me by S. M. Kosslyn, is that even if an explanatory theory is provided, one must still account, in behavioral terms, for the origins of that particular physiological state. To me, a third reading seems more natural. It involves something like a homunculus, located somewhere in the brain, who manages to "read" the output of the retina, to "hear" the resonances of the basilar membrane, to have drives, and the like. If that is Skinner's meaning, then few will disagree with the objection. But that really is not very typical of the modeling of the past few decades. Usually the mechanisms discussed are far more mechanistic and their properties are specified in some detail. For example, a matrix system of memory, such as the one developed by Anderson (1973), which is a simple model of distributed memory, will be justified by finding a physiological system that is functionally such a matrix. It is not a question of accounting for it so much as finding its physiological embodiment.

An analogy, which seems to me to be close, is the theoretical development in biology of the (originally hypothetical) construct of the gene and its ultimate detailed investigation at a level of observation far different from those to be explained. It is not obvious to me why such theory in biology (and there are numerous similar examples in other sciences) has proven useful – nay, essential – and yet analogous constructions will necessarily fail in the study of behavior. To be sure, there is not yet an example of a highly successful, reductive behavior theory, else there would be no issue to debate. Nonetheless, I fail to see the ways in which behavior is so inherently different from other scientific questions that one can be certain, as Skinner seems to be, that modeling in terms of constructs at a different level of discourse is a sterile activity. It is difficult to imagine that research in genetics would have been better off had it not been driven by a theory of the (then) hypothetical gene, just as I find it hard to believe that current high-energy physics is being greatly distracted by theories having to do with complex, hypothetical particles. Saying this does not preclude the existence of suc-

cessful theory that is not reductive in character – relativity theory is a case in point. It does, however, lead me to question the wisdom of asserting – with little detailed argument as to why in this particular field and in sharp contrast to other fields – the harm necessarily follows from the study of the consequences of hypothetical mechanisms that relate concepts at two different observational levels.