

the appropriate verbal repertoire. But we will try again, responding directly to some of the points in Hayes's comment.

1. If in our book we ignore the importance of verbal behavior in human action, it is because behavior theory virtually completely ignores it. Yes, we know that Skinner wrote a book about it almost thirty years ago and that the journal *Behaviorism* is littered with articles about it, but where are the data? Translation, reinterpretation, and plausibility arguments wear a bit thin over thirty years.

2. Hayes maintains that our view of behaviorism is neither contemporary nor accurate. We claim that it is, on the basis of the body of experimentally derived principles that are reported in any behaviorist text. And, unlike Hayes's view of behaviorism, it is neither incoherent nor empty.

3. It would indeed be entirely appropriate to show that our fear that behaviorism "attempts to universalize relations of domination" is based on a misunderstanding of behaviorism. Unfortunately, Hayes did no such thing. Further, it should be noted that our argument here is *not* philosophy masquerading as data. It is an empirical argument, though based on sociohistorical rather than experimental considerations.

4. We do not misunderstand the role of a worldview in contrast to a specific theoretical proposition. What troubles us is the apparent rule of thumb that claims get elevated to the status of worldview when they are challenged empirically.

5. Finally, from Hayes's attempt to clarify what really is distinctive about behaviorism, we conclude that it is the emphasis on control. The problem with the analysis typically used by cognitive psychologists (behaviorism's archenemies), is not that it involves internal events, for cognitive psychologists surely think "memory," "syntactic rules," and the like are "real," "physical," and "theoretically manipulable." But as Hayes correctly notes, they cannot be manipulated directly—only indirectly, via the manipulation of environmental events. So out they go, onto the garbage heap of useless theoretical distractions to join "force," "charge," "electron," and other impediments to intellectual progress.

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#### Dated assumptions and misunderstandings

The problem here is that the authors misunderstand the philosophical base of radical behaviorism and thus cannot appreciate the critical role that conceptual and empirical analyses of verbal behavior plays in it, nor how it deals with private events, scientific observations, or causality. For example, obviously nonmanipulable events are not "thrown out"—They are just not regarded as initiating causes. To the cognitivist, memory (as a thing) causes behavior. To the behaviorist, remembering (as behavior) is ultimately caused by manipulable events; like all behavior, it cannot itself be the initiating cause of the same individual's behavior. You may not like this view, but if you are writing a review of behaviorism you should at least understand it.

I wonder if the misunderstanding is the result of careful study of Skinner or vice versa. Schwartz recently wrote: "Fifteen years [ago as] an undergraduate I read *Science and Human Behavior* [and] found the book appalling. I was unwilling to give up my belief in human freedom . . . so I decided to study the Skinnerian program . . . so that I would be able to criticize it" (*Swarthmore College Bulletin*, March 1981, p. 12). The text seems based on dated assumptions and misunderstandings. With all due respect, Schwartz may have explained why.

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#### On the limits of induction: Tinkering only helps so much

[Re: *Quantitative Analyses of Behavior: Matching and Maximizing Accounts. Quantitative Analyses of Behavior Series, Vol. 2* edited by Michael L. Commons, Richard J. Herrnstein, and Howard Rachlin. Reviewed by Michael Davison: *CP*, 1984, 29, 24–25. Previous OTOH exchange: *CP*, 1984, 29, 678–680.]

Davison's charming reply reveals surprising agreement with my objections to the matching theory (Herrnstein, 1970; Timberlake, 1982), but he still holds out hope that some variant of the generalized matching law (GML) of Baum (1974) will provide an ultimate account of schedule-responding. I do not share his optimism. Compared to the matching theory, the GML has fewer assumptions, but it lacks the ability to

deal with responding on simple schedules and shares the absence of an empirical or conceptual relation to the local effects of reward. Compared to the simple matching law, the GML fits a wider range of choice data, but at the cost of adding two fitting parameters that show some difficulties in interpretation (e.g. Davison, 1982; Davison & Hogsden, 1984).

The fundamental defense of the GML appears to be that it fits or can be altered to fit most current choice data (even data that it does not handle conceptually). Unfortunately, both history and logic show that the ability to fit data does not entail a correct theory (e.g. Ptolemy). Tinkering with the terms of an equation that is neither uniquely nor precisely related to a basic set of tested assumptions may show improvements in variance accounted for, but ultimate success depends on how closely the conceptual basis of the equation reflects underlying causal processes. Induction works best when all of the important determinants of a phenomenon are present and conceptualized. There is some doubt in my mind that the terms of any matching-based equation map cleanly to the important determinants of responding under schedule constraint. To return to our automobile analogy, if the car is not suited to the track, new accessories and fancy spare parts will not help it qualify for the main event.

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