The Dark Side of Skinnerian Epistemology

One of the larger tragedies in psychology's intellectual history is its recent repudiation of the behaviorist program for our discipline. As I have put it elsewhere,

> we need ... a resurrection of behaviorism. Not of specific mid-century behavior theories [whose simplicities] are clearly obsolete. And certainly not of the largely mythological positivistic behaviorism that proscribed theories of the inner organism as idle fancy. The behaviorist ideal which takes seriously the old-fashioned tried-and-true scientific distinction between evidence and hypothesis, which seeks to shape our models of psychonomic mechanism by tough-minded inference from sceptically hardened data on which mentalistic interpretations have not been imposed at the outset, *that* is the doctrine whose revival to counterbalance current cognitive science's runaway aprioricism has become urgent. (Rozeboom, forthcoming)

What has gone wrong? Essentially, it is that behaviorism became prevailingly viewed as a perverse, stultifying suppression of concern for what goes on within us. It takes little attentive reading of the neobehaviorist classics, notably Hull and Tohnan, to perceive that image's malign inaccuracy. But it does fairly characterize behaviorism's radical splinter for which Skinner has been the latter-day spokesman. And because moral outrage is both emotionally gratifying and a convenient substitute for tight thinking, extremist views are what outsiders love to hate. It is ironic that the same Skinner who has so powerfully enriched the technology of behaviorist research should also have contributed so much to its demise as an active intellectual force.

The issue here—the scope and practical methodology of human knowledge—could scarcely be larger. We can surely agree that the main task of any empirical science is to work out credible conclusions about its chosen topic by plausible inference from firm evidence. And let us not dispute that psychology's most reliable evidence is behavioral. But then we must ask, What can be inferred from such data, and how? One might suspect that to be a question of considerable depth and intricacy, on which responsible opinion should be accompanied by some thoughtfully articulate theory of knowledge acquisition. But Skinner has never voiced more than intuitive fiats on this matter, nor has he shown much interest in probating these in the court of debate on the detailed praxis of data interpretation. His aversion to the licentiousness of hypothetico-deductive reasoning is indeed amply justifiable by arguments I have developed elsewhere (Rozeboom, 1970, 1972, 1982). But although it is important to expose textbook hypothetico-deduc-tivism

for the epistemic fraud it is, Skinner offers no principles of practical inference to replace this, only loose positivistic slogans that would be intellectually impossible to live by even were it not foolish to try.

Skinner urges that we eschew attempts to explain "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." But *why* should we abstain from this? Because events of nonobserva-tional kinds do not exist at all (ontological positivism)? Because we cannot meaningfully conceive of what we cannot observe (semantic positivism)? Because observational data can never confer high credibility upon assertions containing nonobserva-tional constructs (epistemological positivism)? You don't believe any of those things, and neither on pain of incoherence can Skinner: His response probabilities and even momentary response rates are prime examples of conjectured causes of overt behavior that we never observe directly but only infer from past and present performance. (Skinner will retort that these are "measured in the same dimensions" as observed responding, but that is just not so.) The operative problem of scientific inference is not *whether* we should try for conclusions about the hidden sources of overt events, but by what patterns of reasoning in what real-life circumstances this becomes epistemically feasible.

What Skinner and his opposition made up of the many philosophers and an occasional scientist who extol hypothetico-deductive theorizing as the quintessence of scientific method have alike failed to appreciate is that there exist determinate forms of *explanatory induction* by which in practice we discover and progressively refine our understanding of hidden causes. These are patterns of inferential disclosure which, at levels of confidence often approaching the force of commonsense perception, transform observed local regularities into inductive conclusions about how these are due to underlying source factors of which the local data parameters are diagnostic. There is far more to say about such inductions than what is covered in my previous accounts (Rozeboom, 1961, 1966, 1972), but here I can only note once again that their most primitively compelling version is the logic by which we acquire dispositional concepts. Skinner has made plain his disdain for the explanatory value of the latter (e.g. "the term [viscosity] is useful in referring to a characteristic of a fluid, but it is nevertheless a mistake to say that a fluid flows slowly because it is viscous or possesses a high viscosity. A state or quality inferred from the behavior of a fluid begins to be taken as a cause" (Skinner, 1974, p. 161); see also Skinner (1952, pp. 202ff.). But even disregarding my own realist arguments for the causal efficacy of dispositions (Rozeboom, 1973, 1984), there is a large technical literature (see, e.g., Tuomela, 1978) to attest how ingenuous is Skinner's understanding in this matter. And dispositions are merely the bottom rung of hidden mechanisms to which iteration of explanatory induction gives us epistemic access.

Because explanatory induction is data driven, its practice strongly endorses Skinner's call to search out empirical regularities, the cleaner the better, described in terms from which all problematic theoretical presumptions have been expunged. But Skinner's refusal to see the explanatory import of these also blinds him to the more intricate behavioral regularities that manifest central states deeper than surface dispositions. As a major case in point, I give you conditioned generalization (Rozeboom, 1958), which is the empirical underlay of the "what is learned?" controversies that so greatly exercised mid-century mainstream behavior theory, and for which Skinnerian behavior principles have made no provision.

Suppose that organism o's rate of operant R has been intermittently reinforced to high strength by a stimulus S^r which has become secondarily rewarding for o through its discriminative cuing of primary reward. (Say, R is bar pressing which occasionally produces a tone that signals delivery of a food pellet.) If, in the absence of R-doing, S^r's reinforcement value for o is now reconditioned from positive to negative (say, the bar is removed and o is repeatedly presented with the tone followed by shock instead of food), to what extent does this reconditioning of S^r suppress o's responding when R's availability to o is renewed on a straight extinction schedule that no longer yields S^r? That is, once R has been established by its production of reward S^r, does subsequent altering of S^r's reinforcement value correspondingly modify the strength of R prior to new contingencies of S^r upon R? Or does the curve of R-extinction begin instead at the level (adjusted for complicating factors such as aversive conditioning of the background stimuli) to which R was terminally reinforced by S^r, as Skinner would have it? Commonsensically, it seems evident that if o learns to expect S^r from doing R, then o's R-emissions should fall off abruptly if S^r is switched for o from attractive to aversive. And conversely, although response shifts so induced can have many explanations other than mentalistic ideation, they *are* strong evidence for the involvement in o's postconditioning R-output of *some* internal mediator, whatever its nature, that is functionally rather like a cognitive representation of S^r.

Hard evidence for conditioned generalization, which has many varieties beyond the one just described, is still meager at the infrahuman level. (For me, loss of innocence was discovery from my early research on this paradigm that rats and pigeons just don't seem to think like people do.) But it is clearly demonstrable in human learning (Rozeboom, 1967)— which is to say that this phenomenon is strongly local at least across species and probably even more so with variation in the parameters of training and testing. How local degrees of conditioned generalization covary with other simple or data-structurally complex observables remains a seminal issue for behavioral research that seeks to chart the subtler contours of organismic adaptability to change. But Skinnerians find it difficult to acknowledge such higher-level regularities, not because these are any less obser vational than the basic reinforcement phenomena that operant conditioning research has

worked out in such instructive local detail, but because they are incompatible with the simplistic overgenerality in which operant reinforcement *theory* has been orthodoxly formulated.

To summarize: Soft theoretical speculations, if astutely analyzed, can guide us to the discovery of complex empirical phenomena whose theory-free descriptions instruct us by explanatory induction about the central mechanisms behind overt behavior even though, in all likelihood, this confirms only fragments of the theories instigating the inquiry and may well cast doubt on their remainders. The inevitable practical consequence of Skinner's doctrinaire insensitivity to this interplay between theory and data is inflation of local regularities into sweepingly rigid laws that prejudge many significant operational questions about the management of behavior. In short, Skinnerian psychology, too, remains largely vacuous, despite its enormous power in those special circumstances to which its generalities legitimately apply. For it has worded its findings to claim a universality vastly beyond the scope supported by their data base, and thereby implies closure for complex empirical issues that in fact remain fascinatingly unresolved.

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