

"Expectancy": Sleight-of-Hand Mentalism, Not Mechanism or Process

Jacob L. Gewirtz
*Department of Psychology
Florida International University*

Martha Peláez-Nogueras
*Department of Pediatrics
University of Miami Medical School*

Jeffrey Fagen (1993, this issue) has misunderstood our main point about reinforcement as a central principle of behavior change (Gewirtz & Peláez-Nogueras, 1992, p. 1414) and the issue of description versus explanation in the use of the objective reinforcement term or of a subjective term such as *contingency expectancy*. He argued that reinforcement cannot explain some findings of his and his colleagues' infant research and that the higher order mechanism of expectancy is the major process that can account for those results. We will raise no critical questions about the complex methodology of the two experiments to which Fagen alludes, nor about the fact that those experiments may involve more the influence of extinction-following-reinforcement and discriminative-stimulus constancy versus change than reinforcement itself. However, issues of parsimony will prompt us to question the necessity of moving beyond basic reinforcement and derivative principles for the fool's gold of intrapsychic concepts such as the *violation of contingency expectancy* to explain the infant behavior changes resulting from operant-learning procedures. This questioning is especially warranted in the absence of implementation of independent operations of any sort by Fagen (and his associates) to attain leverage on the violation-of-contingency-expectancy assumption central to his case.

We did not claim in our article (Gewirtz & Peláez-Nogueras, 1992) that all behavior is learned through operant reinforcement contingencies. Obviously, numerous learning processes exist that can change infant behavior (e.g., habituation, respondent conditioning). Our attempt was to identify key processes of infant learning and behavioral development. Throughout the article, we used the term *reinforcement* to describe the relations between infant responses and (often-reciprocal) environmental contingencies and contextual determinants of behavior (that include organismic factors).

It can be constructive at this point to reexamine what reinforcement is and what it is not. Reinforcement is a central operant principle in behavior analysis. It is descriptive, not explanatory. But reinforcement is

not the sole principle of behavior analysis that Fagen's (1993) critique mistakes it to be. Reinforcement simply organizes descriptively the straightforward effects of the provision of environmental contingencies that increase systematically the rate, amplitude, duration, or some other attribute measure of the response. Even so, in operant learning not every behavior change need result from reinforcement. Diverse other behavior-analysis processes that result from, or are independent of, the reinforcement process can influence behavior change. Among others, these processes include stimulus or response generalization, conditioned reinforcers and punishers, establishing operations or contextual determinants, and response adaptations to diverse intermittent reinforcement schedules as well as to the extinction following the withdrawal of those schedules.

The research-outcome pattern that Fagen (1993) reports is interesting, regardless of the theoretical approach one may prefer. In contrast to the higher mean, final-baseline-session responding of the AAAA and AAA groups, the decline in responding of the ABCA and ABA groups in the presence of the familiar mobile prompted a description-cum-explanation by Fagen. His cognitive interpretation (actually more an assertion about, than an interpretation of, his finding) is that the behavior discrepancy "resulted from the violation of their acquired reinforcement expectancy which consisted of a learned 'rule' regarding whether an event will occur in a stable or changing form" (p. 1154).

Fagen's (1993) protopostulate appears to be that, for their proper "explanation," such results necessarily require the use of mentalistic "expectancy" terms. Without the inclusion in the reported studies of operations that could provide independent leverage on his violation-of-expectancies assumption, he assumes also that use of such expectancy terms somehow provides an explanation of the results he reported that is superior to an explanation that might be provided by the principle of reinforcement. The absence of independent operations regarding expectancies and their violation, or even of preliminary speculation as to which operations might be used to attain leverage upon the expectancy-violation notion, is remarkable and certainly weakens Fagen's conceptual stance. Thus, Fagen concludes that the reinforcement concept could not explain the research outcome he reports, and he was prompted to postulate that a "higher order mechanism of expectancy" could explain the outcome. But his postulate was generated from the outcome itself! This practice does not give credence to his conceptual stance either. The discrepant

findings that Fagen reports could be seen plausibly as resulting from some nonreinforcement operation, perhaps similar to the way adaptive-response patterns are generated by intermittent reinforcement schedules.

In explanation, behavior analysis avoids the use of so-called *psychological structures* or *intrapsychic mentalisms*, given that its goal is to explain behavior as a function of environmental factors of the present and the past in biological context. Behavior analysis considers that the surplus meaning in such mentalistic terms makes their use unparsimonious, not to mention gratuitous. Thus, it is not necessary to mentalize or subjectivize in order to provide the appearance of a "complete" account of behavior outcomes. As Skinner (1989) has noted,

As more and more of the variables of which behavior is a function are identified and their role analyzed, less remains to be explained in mentalistic ways. There are proportionate gains in the application of the analysis. It has always been difficult to do very much with feelings and states of mind because of their inaccessibility. The environmental variables are often within reach. (pp. 73-74)

As indicated, a research arena that bears some similarity to the research outcome reported by Fagen (1993) stands as an exemplar of the behavior-analytic approach: Response-pattern adaptation to intermittent reinforcement schedules or to subsequent extinction conditions are routinely described objectively without the use of subjective terms such as *expectancies* or *expectancy violations*. The descriptions are adequate in themselves and can be related functionally to antecedent and concurrent stimulus factors. The functional relations provide their meaning. The issue becomes one of semantic preference, insofar as subjectivists and those who use mental structures prefer, and behavior analysts consider it gratuitous, if not fatuous, to use mentalistic terms to describe or explain the very same outcome-behavior sets that are assumed to result in a straightforward manner from environmental factors.

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