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Learned Expectancies Are Not Adequate Scientific Explanations

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Fagen (1993, this issue) asserts that the laws and principles of behavior theory are insufficient to explain the operant conditioning of infant behavior and it is therefore acceptable to infer cognitive explanatory mechanisms—in particular, the concept of expectancy. Fagen's comments raise two related issues. The first is whether the laws and principles of behavior analysis are sufficient to explain infant learning, and the second concerns the necessity of inferring cognitive events as explanations of behavior.

In paraphrasing Bolles (1972), Fagen (1993) writes, "when learning occurs, an organism acquires a direct appreciation of the new environmental contingencies" (p. 1153). Operant conditioning is, thus, not simply the effect(s) of environmental events on behavior but rather the learning of expectancies. Behavior analysts describe operant conditioning more parsimoniously. Operant conditioning is said to occur when some behavioral class produces stimulus consequences that alter the frequency of responses comprising that behavioral class under similar circumstances.

There are important differences between behavior-analytic and cognitive accounts of operant conditioning. The subject matter of behavior analysis is the functional relationship between environmental and behavioral variables. This delineation of subject matter establishes the independent and dependent variables of the science—environmental and behavioral events—both of

which are observable and, hence, directly testable. The subject matter of cognitive psychology—the presumed relation between cognitive events and behavior—does not meet the same scientific criteria. Moreover, the existence of cognitive operations must be inferred from the very behavior they are said to explain. Consider the concept of expectancy. Citing the findings of a study by Fagen, Morrongiello, Rovee-Collier, and Gekoski (1984), Fagen (1993) attributes decreases in infants' foot kicks to "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). However, the only observable evidence for the expectancy or the rule is the decrease in foot kicks relative to previous sessions; in other words, if there were no decrease in foot kicks, no expectancy or rule would be inferred. Fagen does not attribute the initial acquisition of foot kicking to the development of the expectancy but rather to the reinforcement principles he later dismisses. But if he were to do this, behavior analysts would still point out the logical inconsistency in the reasoning; namely, if no foot kicks were observed, then no expectancy would be inferred. On the contrary, the evidence for reinforcement as an explanatory concept does not have to be inferred solely from the behavior; we can actually observe and manipulate different values of the consequence and can relate them directly to changes in behavior.

Fagen (1993) suggests that because the concept of reinforcement is "incapable of explaining many findings of infant researchers . . . who rely on operant conditioning as their paradigm of choice. . . a higher-order mechanism that describes the relations between reinforcing events is needed" (p. 1154). It is true that a simple reinforcement analysis might not sufficiently explain some behavior; however, most behavior analysts long ago moved well beyond any kind of simplistic view of reinforcement and have been talking about and experimenting with higher order behavior–environment relations, without inferring unobservable internal processes (e.g., Sidman, 1986). Although most human behavior is controlled by complex stimulus events, we need not invent unobservable explanatory mechanisms. Complex stimulus control of behavior can be accounted for strictly in terms of known behavioral units (e.g., Thompson & Zeiler, 1986). For example, the finding by Fagen and his colleagues (e.g., Fagen, Rovee, & Kaplan, 1976) that the behavior of infants in the conjugate reinforcement procedure is a direct function of the quantitative aspects of the discriminative stimuli comprising the mobiles, as well as other similar "memory"

phenomena, can be parsimoniously explained according to known behavioral principles without inferring unobservable events (Schlinger, 1992).

It is somewhat disconcerting that infant behavior researchers who conduct otherwise valuable research using operant conditioning find it necessary to infer unobservable and, hence, untestable mechanisms to explain the effects of the conditioning. This is a problem because one of the hallmarks of science is that hypothesized explanations must potentially be subject to direct testing. Moreover, any proposed explanations must be plausible; that is, they must point to real events. The principles of behavior theory meet these criteria, and also they are congruent with other behavioral sciences, such as neurophysiology and evolutionary biology (Donahoe, Burgos, & Palmer, 1993). For example, reinforcement, as a principle of ontogenetic selection, follows from the phylogenetic principle of natural selection. Reinforcement represents a plausible and adequate mechanism for the selection of behavior in the lifetime of an organism. All things being equal, organisms whose behavior is sensitive to consequences are in a better position to survive.

The concept of expectancy, on the other hand, is not plausible. In postulating such an explanatory concept, we are forced to ask how it works. To answer this we would have to define expectancy independently from the behavior it is said to reflect. What are the dimensions of an expectancy? How does expectancy translate into biology of the organism? Finally, how do we know what the organism expects? For example, in the Fagen et al. (1984) study, why, after only one session of reinforced foot kicks with Mobile A, should foot kicks increase during the very first exposure to Mobile B, a novel mobile? Isn't it just as likely that the infants would "expect" the same mobile as a novel one? And why should foot kicks increase if they expected the same mobile? Following from expectancy theory we could argue either way depending on the outcome. Also, it is difficult to believe that infants in the ABC or ABCD groups began to expect a different mobile every day after only two exposures, each on a different day. Why wouldn't expectancy theory predict an ABAB arrangement? Expectancy theory, like other cognitive theories, can be modified to explain the data whatever the outcome. Although such flexibility seems to be a strength, it reveals the weakness of expectancy theory as a scientific explanation. The success with which a theory tests its predictions depends in part on how precise the predictions are—in other words, whether they specify clear independent and dependent variables and the relationships between

them. Moreover, "if there is room for reinterpretation and modification of the predictions after the data are collected, any result can be explained by the theory" (Mazur, 1990, p.7). These and other questions raise problems for expectancy theory and, indeed, for most cognitive theories. Expectancy theory is simply not an adequate scientific explanation of behavior.

The principles of reinforcement, broadly defined, are usually enough to explain infant learning and behavior, and when they appear not to be, what is called for is an experimental analysis of the behavior-environment relations, not a retreat into the unobservable, untestable, inferred world of cognition.

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The author is indebted to David C. Palmer for his invaluable contribution to this reply.

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

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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