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On Terms

When We Speak of Integrating . . .

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Early in his career, B. F. Skinner departed from stimulus-response (S-R) psychology and, with it, the legacy of mediating, proximal causation. By 1945, with his critique of classical operationism, the separation from S-R theory was nearly complete (Flanagan, 1980); however, profound theoretical differences between the two positions had become evident by the mid-1930s. For example, in his early exposition of the reflex, Skinner (1931, 1935) laid the groundwork for his unique approach to the study of behavior. He regarded the reflex as a relationship—a correlation between a *class* of environmental events and a *class* of behavior. This represented a significant point of departure from S-R approaches, whose preoccupation with specific instances of a reflex would ultimately, Skinner believed, require a taxonomic sorting of unique reflex units (the so-called "botanizing" of reflexes). Such a formal, or topographical, inventory would demand continual revision, a practice that Skinner viewed as both tedious and conceptually misguided.

Soon thereafter, Skinner (1938) articulated the three-term operant contingency in a manner analogous to that of the reflex (see Catania, 1973), defining an operant class as a generic collection of responses that may vary widely in form, but that retains its integrity through a common effect on the environment. Although this functional analysis circumvented certain problems, it carried with it problems of its own. For instance, how were classes to be defined? How large should a particular class be? Skinner opted for an empirical specification of the response unit, selecting small samples of behavior and studying them intensively, seeking "the natural lines of fracture along which behavior and environment actually break" (Skinner, 1935, p. 40). These lines of fracture are revealed as orderly, systematic relations between behavior and its environment. The temporal or spatial extent of any particular response class depends entirely on the level at which orderly relations are apparent. This means that specifying a functional unit of behavior is an empirical, not an a priori theoretical, matter. Thus, behavior analysis focuses upon order at any level; it is not restricted to, nor is it committed to, the identification of contiguous associations, such as S-R bonds, or to any other preconceived theoretical unit. As Schwartz (1986) aptly notes:

> One of the virtues of the operant framework, often missed by critics who confuse it with stimulus-response connectionism, has been that within a rather wide range, it allows the animal to tell the experimenter how behavior is to be partitioned, rather than the reverse. (p. 293)

With traditional methods, such as mazes and puzzle-boxes, the environment, and thus behavior, was arbitrarily carved into discrete temporal units, whose character was relatively unaffected by separation in time. By contrast, Skinner's free-operant methodology permitted behavior to be seen as it was arrayed over time, in a continuous interplay with the environment. With a few notable exceptions, however (e.g., Findley's, 1962, and Kelleher's, 1966, work on complex operants), reinforcement was generally viewed as the strengthening of responses with which it was contiguous (Ferster & Skinner, 1957). Before long, behavioral

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techniques were in wide use in applied settings, and the important role of immediate reinforcement in the shaping of new repertoires was made quite apparent. In addition, basic researchers tended to focus on fine-grained analyses of behavior, gleaning important details from cumulative records, interresponse time distributions, and the like. Thus, small-scale time relationships came to pervade behavior analyses, despite the fact that, in principle, Skinner's position was not so limited.

More recently, specifying an effective scale of analysis has emerged as a core issue within behavior analysis, as researchers have begun to re-address the problem of defining behavioral units (see Harzem & Zeiler, 1981; Thompson & Zeiler, 1986). The past two decades have seen this basic issue parsed in a number of different ways, usually assuming some form of a molar-molecular debate. Disagreement centers on the time frame defined by the "natural lines of fracture" delineating the domain over which behavior and its consequences are said to be interrelated. Proponents of a molecular view maintain the more traditional stance, arguing for control of behavior by its immediate consequences. Conditioned reinforcers are relied upon to bridge apparent gaps between behavior and ultimate consequences (Anger, 1963; Dinsmoor, 1977). By contrast, advocates of a molar position favor a larger time frame, where behavior and reinforcement are viewed within an extended temporal context (e.g., Baum, 1973, 1981; Rachlin, 1978; Rachlin, Battalio, Kagel, & Green, 1981). Thus, whereas a molecular account may focus on the local organization of behavior, a molar account usually emphasizes global relations between widely dispersed aggregates of behavior and clusters of correlated consequences. According to a molecular view, such large-scale relationships are illusory; they are seen as simple artifacts of averaging over many adjacently-related events. Fundamental processes are said to exist on the local level (Mazur, 1981; Shimp, 1966; Silberberg, Hamilton, Ziriax, & Casey, 1978; Staddon, Hinson, & Kram, 1981; Vaughan & Miller, 1984).

The intellectual tension between the molar and molecular positions continues to escalate, as perusal of virtually any recent issue of the *Journal of the Experimental Analysis of Behavior* attests. The disagreement surfaces in discussions within numerous areas, including choice, self-control, foraging, aversive control, conditioned reinforcement, economic analyses, basic schedule analyses, and stimulus control. The definition of behavioral units also has important implications for applied work, as behavior analysts address complex social problems and issues of instructional and cultural design (Malagodi, 1986). Although such theoretical disagreements are normally resolved empirically, the theoretical battle lines surrounding the molar-molecular issue seem to cut deeper than data. Indeed, the data are sometimes overshadowed by formidable mathematical models. Even when the data are taken on their own terms, however, they are equivocal—different procedures seem to favor units of differing size.

To those accustomed to interpreting behavior in terms of contiguous events, an enlarged unit of behavior-environment interaction may pose a serious challenge. For instance, how does a nonmediational account characterize action over time, without relying upon mediating events to bridge temporal gaps? A potential answer to this question can be discerned through examination of the term, "integration." In recent years, this term has gained currency when theorists are concerned with events distributed over time, but some confusion and controversy has been generated over its proper use. A major source of confusion concerns matters of agency—"who or what is *doing* the integrating?" For example, Dinsmoor (1977), in dismissing the molar principle of shock-frequency reduction as a potential basis for negative reinforcement, asserts: "The animal must sort out samples of time, assigning some to the denominator of the fraction and rejecting others . . ." (p. 90). Such a characterization may conjure up images of internal wheels and gears, where a process like integration is said to be carried out. But "integration" can also, as Hineline (1984) points out, be understood in the sense of integral calculus, expressed quantitatively as follows:

$$B = \int_{t1}^{t2} f(E)\,\mathrm{dt}.$$

where B represents a class of behavior and E represents changes in the environment from time 1 (t_1) to time 2 (t_2). As Hineline (1984) states:

> Evaluating the integral for different values of t_1 and t_2 can reveal the range of times over which the particular class of behavior is sensitive. Experimentally, this translates into procedures that identify specific values of t_1 and t_2 between which the specified events affect the behavior in question, and outside of which the behavior is unaffected. (p. 506)

A molecular theorist may still not see the unitary character of the events arrayed between t_1 and t_2. To make it more tangible, consider an analogy proposed by Morris, Higgins, and Bickel (1982), who have likened the analytic search for functional units of behavior to the fine adjustments one makes when bringing microscopic phenomena into focus. The intricate, detailed structure of some phenomena favor small-scale examination, while the crucial features of other phenomena are only discerned with analyses that bring larger patterns into focus. Only by continuously adjusting our analytic scale may we hope to illuminate the natural regularities between behavior and the environment.

Focusing on relationships at one level does not mean that relationships at other levels have become invalid or have ceased to exist; they are simply different aspects of the same ongoing activity. Like phenomena we observe under a microscope, behavior may reveal order on several distinct levels simultaneously. For example, individual key pecks, whether emitted by a pigeon in an experimental chamber or by a human at a typewriter, may be controlled by immediate events in the local environment, and thus may be efficiently described at that level. The pattern that emerges from these individual pecks, however, may be part of a larger functional unit. Thus, just as behavior analysis allows orderly relations to be specified on any level of activity, it also allows this order to exist concurrently on multiple levels. This, as Branch (1977) has pointed out, is commonly misunderstood by critics, such as Shimp (1976, 1984), who assume that behavior analysis is committed to a rigid, linear organization. With respect to the integration analysis proposed here, the task becomes one of systematically defining the boundaries of each response unit, as well as assessing the extent of their interaction (e.g., Bacotti, 1976).

As an analytic tool, integration, as interpreted here, has much to offer. First, it maintains a commitment to the environment. In its quantitative form, the time values above and below the integral sign, which define the limits of integration, are defined with respect to classes of environmental events and classes of behavior; integration need not refer to inner or conceptual "hardware." Second, using empirical rather than theoretical criteria as a guide, integration clearly embodies Skinner's functional, "sliding" scale of analysis. Third, by defining action within a continuous time scale, integration underscores the complementarity between molar and molecular scales of analysis. The language of integration may better enable our descriptions to capture the dynamic character of behavior-environment interaction on the many levels upon which it may unfold.

Despite these virtues, the ultimate fate of integration, like all other scientific terms, rests with its usefulness in dealing effectively with the subject matter. The issue is therefore one of pragmatics, not one of logic (see Skinner, 1945). Whether the methods and language of integration are subsumed within behavior-analytic practices depends on the extent to which such efforts clarify the effective unit of behavior-environment interaction—the "natural lines of fracture" within the ongoing behavioral stream. If this were to happen, an empirical specification of the response unit would replace the overworked notion of contiguous causation. It may also change the nature of the experimental questions we ask, and in the process, perhaps make irrelevant the distinction between molar and molecular accounts.

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