BEYOND THE MOMENT: COMPLEX BEHAVIOR IN TEMPORALLY EXTENDED ENVIRONMENTS

DOUGLAS P. FIELD

TEMPLE UNIVERSITY

Donahoe, Palmer, and Burgos have provided a valuable contribution toward an account that can accommodate order at both the neurological and behavioral levels. Among the attractive aspects of their account are a neural network model that appears to be remarkably effective at simulating behavior–environment interactions at the level of single responses; an emphasis upon the importance of keeping all behavior–environment relations (operant and nonoperant) within our interpretive window; and an excellent discussion of the subtle range of discriminative functions that the contiguous antecedent environment can acquire. In patterning an explanatory account in accord with the kinds of events that are known to occur at the neurological level, they have raised a number of controversial issues for behavior analysts. The present comments will focus primarily on the issue of scales of analysis and the manner in which the relation between the antecedent environment and behavior is construed by Donahoe et al.

Noting that reinforcement strengthens "the environmental control of responding" (p. 193), Donahoe et al. propose that behavior can best be understood as guided by the environment contiguous to it. *Environment,* as they use the term here, refers explicitly and exclusively to current environmental stimulation, the momentary antecedent context of the behavior in question:

> When input units are stimulated by the simulated occurrence of environmental stimuli, the interior units to which those input units are connected are probabilistically activated in the following moment. If a reinforcing signal is present at that moment, then connections are strengthened between input units and all recently activated interior units to which they are connected. p. 203)

It is this aspect of their account that prompted Shull (1995b) to remark on the similarities between their account and traditional S-R theories.

Although it is likely that the immediately antecedent environment often exerts control, it seems unnecessarily constraining to limit the instantiating functions of the environment only to that moment. As Skinner noted,

> The analysis of behavior is not an act of arbitrary subdividing, and we cannot define the concepts of stimulus and response quite as simply as "parts of behavior and environment" without taking account of the natural lines of fracture along which behavior and environment actually break. (1935/1959, p. 347)

To subdivide the environment, as Donahoe et al. appear to do, into successive moments of stimulation acting upon individual responses precludes an analysis based upon "natural lines of fracture" that may reveal the importance of larger aspects of the environment and larger patterns of behavior.

Although Donahoe et al. have provided a plausible model of simple responses on a momentary level, the generality of that model in accounting for larger units of behavior and more temporally extended aspects of the environment remains unclear, at least to this reader. Their neural network simulation, as they presently describe it, appears to be locked into a particular molecular time scale. The account has yet to capture what some consider to be an important aspect of behavior-analytic accounts, that being the ability to handle multiple scales of behavioral process with a remarkably small set of defining principles. The same principles that account for a red light evoking a lever press can account for temporally extended behavior–environment relations such as engaging in arguments or running marathons.

Behavioral Patterns and Larger Units of Behavior

One of Skinner's most important contributions to the development of a science of

---

I thank Leslie Cohen for her helpful comments on the manuscript.

Address correspondence and reprint requests to Douglas P. Field, Department of Psychology, Weiss Hall, Temple University, Philadelphia, Pennsylvania 19122.

behavior was his conception of rate as a fundamental dimension of behavior (Hineline, 1990; Skinner, 1938, 1956; also see Shull, 1995b). Building upon that innovation, as behavior analysts have done, intrinsically involves analyses extended in time, for one cannot derive rate from isolated single responses. At any particular moment, a rate may be present even while its constituent events are not visible. It follows that the instant, the setting, the situation, and other spatiotemporally contiguous elements attendant to a specific response are not *necessarily* to be given privileged status.

In support of their position on the control exerted by the contiguous environment, Donahoe et al. cite Skinner's (1976) lament of the decline of the cumulative record, but they fail to convey that aspect of the cumulative record which he found most compelling:

> The additional information to be found in a simple cumulative record, where for the first time [the observer] can estimate rate of responding accurately, compare different rates, and follow the accelerations which are now obvious. (Skinner, 1969, p. 9)

That is, if sufficiently magnified, a cumulative record does provide information regarding moment-to-moment stimulation as it relates to moment-to-moment responding, but, more importantly, viewed at lesser magnification it allows one to observe changes in rate across time as they relate to changes in the environment. After devising the cumulative record, Skinner could observe the development of patterns of responding under the control of relevant, not necessarily contiguous, aspects of the environment. Consider, for example, the scallop pattern of responding that is acquired and maintained on a fixed-interval schedule of reinforcement. That pattern is most appropriately described as a pattern of behavior that is under control of a temporally extended aspect of the environment (the relation between periodically available food deliveries) rather than as responses evoked by momentary stimulus situations.

The Temporally Extended Environment

For Donahoe et al., responding is necessarily evoked by the environment of the prior moment, and, although the precise temporal dimensions of the moment are not specified, their descriptions suggest that it is brief. Thus, although they have acknowledged the contribution of the temporally extended environment to behavioral selection (pp. 203–204), they have not yet incorporated its relevant instantiating functions. Those functions include evocative (e.g., discriminative) and alterative (e.g., conditional and motivative) relations that account for behavioral occurrences (Glenn & Field, 1994). Time is a critical dimension of the environment, present even in very punctate events. The importance of its inclusion becomes apparent when one considers the extent to which relations between temporally dispersed features of the extended environment can be functionally related to behavior, considered here in terms of (a) duration, (b) rate, and (c) more complex relations.

Duration. Temporal discrimination tasks with pigeons (e.g., Fetterman, Dreyfus, & Stubbs, 1989) and avoidance conditioning studies with rats (e.g., Mellitz, Hineline, Whitehouse, & Laurence, 1983) have demonstrated the behavioral relevance of duration. Mellitz et al. found that when offered a choice between two otherwise equal shock-postponement procedures that were concurrently available, rats preferred the alternative in which a session-shortening contingency was also operative. They found that reductions in the duration of the avoidance session came to control choice even though the local (moment-to-moment) differences between alternatives were explicitly arranged to be indistinguishable.

Rate. Just as rate of occurrence is a fundamental dimension of responding, it is also a fundamental dimension of events that acquire function with respect to behavior. Herrnstein and Hineline (1966) demonstrated that a reduction in the overall rate of electric shock was sufficient to generate and maintain avoidance responding. Environmental rate also plays a key role in adjunctive behavior. For instance, the rate of food presentations is the primary variable that controls polydipsia in rats. When food deliveries occur at a particular frequency (the operative frequency varies across subjects, between 30 s and 180 s), a rat will consume much more than its normal daily water intake (Falk, 1966; Hineline, 1981; Wetherington, 1979, 1982).

In these two examples, it is the relation between environmental events (food deliveries or shocks distributed over time), rather than any specific momentary instance of either, that controls responding.

More complex relations. Beyond duration and rate, there are other invariant relational aspects of an extended environment (Gibson, 1966, 1979; Michaels & Carello, 1981; see also Fetterman, Stubbs, & MacEwen, 1992; Glenn & Field, 1994, Footnote 2). For instance, Shull (1995a) proposes that contingencies and contingent relationships, as discriminable relations, are ''*molar* features of the environment rather than something that one can point to as occurring at this or that moment in time'' (p. 145). He continues with a further elaboration:

> Contingencies are molar in the same sense that rhythm is a molar property of music. One cannot detect rhythm by considering only the individual notes because the term *rhythm* refers to the *pattern* of relationships among the individual notes. The pattern (i.e., the rhythm) is just as much a physical property of the music as the individual notes are; but the property that we call rhythm is, of its essence, molar in the sense of being extended over time and relational. (p. 145)

Any comprehensive account of the environmental control of behavior, including that of complex behavior, must eventually include control evidenced by such temporally extended aspects of the environment.

Order at Multiple Scales of Process

In focusing exclusively on the ''momentary relations between environmental and behavioral events'' (p. 201), Donahoe et al. take an implicitly reductionist stance in which it must be assumed that ultimately a particular molecular level can account for order at all levels. A potential embarrassment to any reductionist argument is that the particular scale proposed as molecular can itself be further reduced: There will always be more molecular levels of analysis. It is easy to see how an analysis of process at the level of the single input unit or a single neuron might occlude much relevant order. I suggest, then, that further elaborations of the model incorporate greater flexibility in transition between scales. To respond with sensitivity to order at multiple scales, one must recognize that although process at more molar scales cannot violate regularities at molecular levels, the order found at any given level may not necessarily be more predictive than, or anticipate organization at, other levels. Indeed, neural network models may prove to be particularly well suited to studying both molecular and molar processes that proceed simultaneously at the neurological level (e.g., Wright & Liley, 1996).

The neural network simulation set forth by Donahoe et al. provides a valuable contribution to the development of a selectionist account that is compatible with phenomena found at both the neurological and the behavioral levels. A neural network interpretation that accommodates order at multiple, simultaneous, or overlapping scales of process will provide an even more powerful explanatory model. Such a model, elaborated to incorporate spatiotemporally dispersed components of behavior and environment, would retain what we have come to see as a critical element of behavior-analytic interpretation:

> When order is not apparent at a molar level, a more molecular analysis may be necessary (cf. Moore, 1982). Conversely, if one fails to find an immediate stimulus that controls a response, perhaps the response is only an element of a larger functional unit which is controlled by currently operating variables not immediately attendant to that element. (Morris, Higgins, & Bickel, 1982, pp. 119–120)

REFERENCES

Falk, J. L. (1966). The motivational properties of schedule induced polydipsia. *Journal of the Experimental Analysis of Behavior, 9,* 19–25.

Fetterman, J. G., Dreyfus, L. R., & Stubbs, D. A. (1989). Discrimination of duration ratios. *Journal of Experimental Psychology: Animal Behavior Processes, 15,* 253–263.

Fetterman, J. G., Stubbs, D. A., & MacEwen, D. (1992). The perception of the extended stimulus. In W. K. Honig & J. G. Fetterman (Eds.), *Cognitive aspects of stimulus control* (pp. 1–20). Hillsdale, NJ: Erlbaum.

Gibson, J. J. (1966). *The senses considered as perceptual systems.* Boston: Houghton Mifflin.

Gibson, J. J. (1979). *The ecological approach to visual perception.* Hillsdale, NJ: Erlbaum.

Glenn, S. S., & Field, D. P. (1994). Functions of the environment in behavioral evolution. *The Behavior Analyst, 17,* 241–259.

Herrnstein, R. J., & Hineline, P. N. (1966). Negative reinforcement as shock-frequency reduction. *Journal of the Experimental Analysis of Behavior, 9,* 421–430.

Hineline, P. N. (1981). Constraints, competing behavior, and the principle of resonance. In C. M. Bradshaw, E. Szabadi, & C. F. Lowe (Eds.), *Quantification of steady-state operant behavior* (pp. 153–164). The Hague: Elsevier/North Holland Biomedical Press.

Hineline, P. N. (1990). The origins of environment-based psychological theory. *Journal of the Experimental Analysis of Behavior, 53,* 305–320.

Mellitz, M., Hineline, P. N., Whitehouse, W. G., & Laurence, M. T. (1983). Duration-reduction of avoidance sessions as negative reinforcement. *Journal of the Experimental Analysis of Behavior, 40,* 57–67.

Michaels, C. F., & Carello, C. (1981). *Direct perception.* Englewood Cliffs, NJ: Prentice Hall.

Moore, J. (1982). Choice and multiple reinforcers. *Journal of the Experimental Analysis of Behavior, 37,* 115–122.

Morris, E. K., Higgins, S. T., & Bickel, W. K. (1982). Comments of cognitive science in the experimental analysis of behavior. *The Behavior Analyst, 5,* 109–125.

Shull, R. L. (1995a). Contingencies, the causal texture of the environment, and behavioral dispositions. *Mexican Journal of Behavior Analysis, 21,* 143–161.

Shull, R. L. (1995b). Interpreting cognitive phenomena: Review of Donahoe and Palmer's *Learning and Complex Behavior. Journal of the Experimental Analysis of Behavior, 63,* 347–358.

Skinner, B. F. (1938). *The behavior of organisms: An experimental analysis.* New York: Appleton-Century-Crofts.

Skinner, B. F. (1956). A case history in scientific method. *American Psychologist, 11,* 221–233.

Skinner, B. F. (1959). The generic nature of the concepts of stimulus and response. *Cumulative record.* New York: Appleton-Century-Crofts. (Reprinted from *Journal of General Psychology, 12,* 40–65, 1935)

Skinner, B. F. (1969). The role of the environment. In B. F. Skinner (Ed.), *Contingencies of reinforcement: A theoretical analysis* (pp. 3–28). New York: Appleton-Century-Crofts.

Skinner, B. F. (1976). Farewell, my lovely! *Journal of the Experimental Analysis of Behavior, 25,* 218.

Wetherington, C. L. (1979). Schedule-induced drinking: Rate of food delivery and Herrnstein's equation. *Journal of the Experimental Analysis of Behavior, 32,* 323–333.

Wetherington, C. L. (1982). Is adjunctive behavior a third class of behavior? *Neuroscience & Biobehavioral Reviews, 6,* 329–350.

Wright, J. J., & Liley, D. T. J. (1996). Dynamics of the brain at global and microscopic scales: Neural networks and the EEG. *Behavioral and Brain Sciences, 19,* 285–320.

IN TODAY'S CLIMATE, A FORECAST FOR CHANGE: A COMMENTARY ON DONAHOE, PALMER, AND BURGOS

GREGORY GALBICKA

WALTER REED ARMY INSTITUTE OF RESEARCH

Donahoe, Palmer, and Burgos' essay raises several interesting questions concerning the future of the analysis of behavior, independent of whether neural networks ultimately turn out to be the potent biobehavioral models the authors suggest. The real difficulty that thwarts the authors' attempted convergence of behavioral systems appears not to be the conceptual nature of the S-R issue, but rather procedural and measurement differences that have evolved following the divergence of operant and classical learning traditions.

Skinner's conception of the operant never denied antecedent controlling stimuli, but only ones that were reliably observable. Rather than postulate their existence as a matter of first principles, Skinner ignored them and concentrated instead on the reliable relation at the other end of the behavior–environment interaction, the R-S relation.

> An event may occur without any observed antecedent event and still be dealt with adequately in a descriptive science. I do not mean that there are no originating forces in spon-

---

Correspondence concerning this article should be addressed to Gregory Galbicka, Division of Neurosciences, Walter Reed Army Institute of Research, Washington, DC 20307-5100 (E-mail: dr. gregory galbicka@wrsmtp-ccmail.army.mil).

> taneous behavior but simply that they are not located in the environment. We are not in a position to see them, and we have no need to. This kind of behavior might be said to be *emitted* by the organism. (Skinner, 1937, p. 20)

Skinner would probably replace "environment" with "publicly observable environment" today, but the emphasis in this sentence is the phrase "we have no need to." Skinner was marshaling the call for the analysis of reliable relations between behavior and environmental events other than antecedent stimulus relations, ones that prior to that point had been ignored in the models of the time. "The attempt to force behavior into the simple stimulus–response formula has delayed the adequate treatment of that large part of behavior which cannot be shown to be under the control of eliciting stimuli" (Skinner, 1938, p. 20). Again, the reference is not to behavior that *is not* under the control of eliciting stimuli, but rather to behavior that *cannot be shown* to be under the control of such stimuli. Rather than wait for some future time when (as in Donahoe & Palmer's, 1994, neural networks or other processes) eliciting stimuli could be implicated, Skinner adopted the utilitarian strategy of working with an altogether different set of correlations, those between responses and their consequences. Although he abandoned it later, he may have been closer to the mark in *The Behavior of Organisms* when he maintained that both classes be considered reflexes, differing only with respect to the reliable correlation (S-R or R-S) that defined the specific unit.

> The kind of behavior that is correlated with specific eliciting stimuli may be called *respondent* behavior and a given correlation *a respondent.* The term is intended to carry the sense of a relation to a prior event. Such behavior as is not under this kind of control I shall call *operant* and any specific example *an operant.* The term refers to a posterior event, to be noted shortly. The term reflex will be used to include both respondent and operant even though in its original meaning it applied to respondents only. A single term for both is convenient because both are topographical units of behavior and because an operant may and usually does acquire a relation to prior stimulation. In general, the notion of a reflex is to be emptied of any connotation of the active "push" of the stimulus. The terms refer here to correlated entities, and to nothing more. (1938, pp. 20–21)

Operants and respondents were different perspectives on the analysis of behavior, not mutually exclusive classes of events. When the most reliable correlation was between an antecedent stimulus and a response, the reflex was a respondent; when the reliable correlation was between a response and consequence, the reflex was an operant. Conceptually, then, Donahoe and Palmer (1994) converge on the system of behavior originally presented in *The Behavior of Organisms* by providing for control of behavioral units via either antecedent or consequent stimuli through a unitary mechanism.

Greater difficulties arise, I believe, from the fact that the procedures and methods that were subsequently developed to study operant behavior are not well suited to the development of a science of behavioral dynamics like that promulgated by Donahoe and Palmer (1994), as I and others have argued elsewhere (e.g., Galbicka, 1992, in press; Galbicka, Kautz, & Jagers, 1993). They also differ from those associated with the respondent learning tradition, where, with its emphasis on discrete presentations, response characteristics such as magnitude or latency, or aggregates such as response probability (i.e., responses per trial) are far more common dependent measures. The partitioned nature of respondent conditioning procedures also predisposes them to iterative, trial-by-trial dynamic learning models and processes (e.g., Rescorla & Wagner, 1972). The operant conditioning tradition, lacking any defining response cycle, abandoned the traditional measures and adopted response rate in their stead. This was not sufficient in and of itself to forge a bifurcation, however, because Skinner's use of rate was far different from that predominating today in the analysis of behavior. Donahoe et al. correctly emphasize that "Skinner was resolutely committed to a moment-to-moment account at the behavioral level of analysis" (p. 200). In a recent chapter, I argue this same point in considerable detail (Galbicka, in press). For Skinner, rate's primarily value was as a means of visualizing behavior *change,* as depicted in the cumulative record, not as a dipstick into the "reflex reserve" from whence to measure overall response output. Only when the use of rate was combined with the adoption of steady-state methodology did the analysis of behavior abandon behavioral dynamics for quantitatively more comfortable descriptive models of asymptotic behavior. Analyses of local patterns of responding, the conditioning and extinction curves so common in *The Behavior of Organisms,* became relatively rare. In their place, large aggregates of behavior relatively void of local structure and large aggregates of consequences delivered at unpredictable points in time predominate, and models of the ratios of these values across samples of thousands of responses rule the day (e.g., Davison & McCarthy, 1988).

For many, the quantification of behavior that has accompanied the adoption of this perspective signals the maturation of a science of behavior, and advocating the development of models like those proposed by Donahoe and Palmer (1994) might be considered a step back into the murky dawn from whence we came. But as Donahoe et al. note, ''conditioning processes are instantiated in moment-to-moment relations between events'' (p. 201), and regularities at more global levels of analysis are not invalidated by the development of a local model. They cite several examples in which local reinforcement contingencies are demonstrated to override more molar relations. Two additions to this list that I find particularly relevant involve studies on shock-maintained behavior, and ones employing Platt's percentile reinforcement schedules. I and others have demonstrated that the effectiveness of local relations can be so powerful as to confound the apparent function of stimuli at the more molar level, as when differential IRT punishment contingencies generate reinforcement-like effects in procedures in which lever pressing is maintained by contingent presentation of electric shock (e.g., Galbicka & Platt, 1984; Lawrence, Hineline, & Bersh, 1994). Stimulus function is not altered under these procedures (i.e., shock does not get transformed into a positive reinforcer; see Pitts & Malagodi, 1991, for a particularly elegant analysis); rather, the behavioral unit that gets differentiated is diametrically opposed to pressing, such that the functional effect appears to be reversed (i.e., by suppressing long IRTs through punishment, lever-press rates increase). Confusion in this case stems from treating all responses as identical members of the same aggregate (lever pressing) when, in fact, there is a differential relation (i.e., a contingency) that is sufficient to shape local patterns of IRTs. The fact that this relation is also present under fixed- and variable-interval schedules of reinforcement makes the simple identification of reinforcers and punishers impossible under such procedures.

Shock-maintained behavior is an extreme example of how local contingencies can override molar relations. Platt's percentile schedules (cf. Galbicka, 1988; Platt, 1973), which were developed to control molar reinforcement contingencies while independently manipulating more local ones, provide a second realm of research indicating that molar relations are not outcomes independent of the local contingencies comprising those relations. These data, in addition to those cited by Donahoe et al., prompt development of molecular models of behavioral processes, of behavioral dynamics, like those proposed by Donahoe and Palmer (1994). As the authors indicate, however, any such models must have as one solution at equilibrium, the molar relations, such as matching, readily observed under standard concurrent scheduling arrangements. As I have argued in the past,

> A complete model of behavior must ultimately be able to account for behavior change that is produced both by changes in overall reinforcement rates and in more local relations like the one programmed by percentile schedules. Perhaps it is time to change strategies and attempt to model the local dynamics of responding as they are related to local reinforcement characteristics, while keeping as a linchpin of any such model the requirement that it track the behavioral effects of changing aggregate reinforcement parameters as well. (Galbicka et al., 1993, p. 182)

Behavior analysis has until very recently avoided developing a ''mechanics of the animate,'' to borrow Killeen's (1992) phrase, satisfying itself instead with descriptive analyses of steady-state performance. Attempts to do otherwise have met with resistance because, being new, they are necessarily incomplete, but also because they require a radically different view of the subject matter and a reexamination of response rate's value as a dependent variable. Reaction to the model proposed by Donahoe and Palmer (1994) is no less subject to controversy but no less desirable a course of action.

It may be helpful to consider a metaphor I used in discussing many of these same issues recently (Galbicka, in press). I noted that molar models stand in relation to behavior dynamics as "climate" does to "weather." Climate is an information aggregate predictive for large aggregates of time, but of little use either for dealing with today's forecast or for ferreting out the factors responsible for that climate. It is a summary of observations already made, not a mechanism capable of addressing day-to-day changes in the climate, which is termed *weather.* The weather is to be understood (predicted, if not controlled) from analysis of local changes in various meteorological factors (e.g., jet stream, wind and sea currents, air pressure changes, etc.). Weather can only be viewed as being responsible for the climate, not a product of it. In a similar fashion, models of behavior must at some point indicate not merely the aggregate value that responding will achieve (as current steady-state models do), but also the dynamic that allows it to attain that level.

To date, behavior analysis has functioned primarily as climatology. We have implemented schedules of reinforcement and described the resulting rates and patterns of responding, paying relatively less heed to the factors generating that behavioral climate. Donahoe et al., along with many others, wish to begin forecasting behavior change with considerably more precision than the simple ordinal relations that current definitions of reinforcement and punishment allow. Although quantification at this level must necessarily be cruder than quantitative models of steady-state performance (much like forecasting must be more variable than describing the climate), it is a necessary step in securing what is by rights the ultimate domain of operant and respondent conditioning: the analysis of variables that change behavior.

REFERENCES

Davison, M., & McCarthy, D. (1988). *The matching law: A research review.* Hillsdale, NJ: Erlbaum.

Donahoe, J. W., & Palmer, D. C. (1994). *Learning and complex behavior.* Boston: Allyn & Bacon.

Galbicka, G. (1988). Differentiating *The Behavior of Organisms. Journal of the Experimental Analysis of Behavior, 50,* 343–354.

Galbicka, G. (1992). The dynamics of behavior (Editorial). *Journal of the Experimental Analysis of Behavior, 57,* 243–248.

Galbicka, G. (in press). Rate and the analysis of behavior: Time to move forward? In L. J. Hayes & P. M. Ghezzi (Eds.), *Investigations in behavioral epistemology.* Reno, NV: Context Press.

Galbicka, G., Kautz, M. A., & Jagers, T. (1993). Response acquisition under targeted percentile schedules: A continuing quandary for molar models of operant behavior. *Journal of the Experimental Analysis of Behavior, 60,* 171–184.

Galbicka, G., & Platt, J. R. (1984). Interresponse-time punishment: A basis for shock-maintained behavior. *Journal of the Experimental Analysis of Behavior, 41,* 291–308.

Killeen, P. R. (1992). Mechanics of the animate. *Journal of the Experimental Analysis of Behavior, 57,* 429–463.

Lawrence, M. T., Hineline, P. N., & Bersh, P. J. (1994). The puzzle of responding maintained by response-contingent shock. *Journal of the Experimental Analysis of Behavior, 61,* 135–153.

Pitts, R. C., & Malagodi, E. F. (1991). Preference for less frequent shock under fixed-interval schedules of electric-shock presentation. *Journal of the Experimental Analysis of Behavior, 56,* 21–32.

Platt, J. R. (1973). Percentile reinforcement: Paradigms for experimental analysis of response shaping. In G. H. Bower (Ed.), *The psychology of learning and motivation: Vol. 7. Advances in theory and research* (pp. 271–296). New York: Academic Press.

Rescorla, R. A., & Wagner, A. R. (1972). A theory of Pavlovian conditioning: Variations in the effectiveness of reinforcement and nonreinforcement. In A. H. Black & W. F. Prokasy (Eds.), *Classical conditioning: II. Current research and theory* (pp. 64–99). New York: Appleton-Century-Crofts.

Skinner, B. F. (1938). *The behavior of organisms.* New York: Appleton-Century-Crofts.