FURTHER THOUGHTS ON BEHAVIOR REGULATION

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Because of time constraints due to the mutual desire of the editors and myself finally to publish the previous paper, I was not able to consider carefully all the remaining reservations expressed by the reviewers. However, there are several topics on which I felt additional comments might be useful. I will divide my comments into the following areas: primary instigation and deviation instigation, expression characteristics, molar and response-pattern levels of analysis, and reinforcement theory.

PRIMARY AND DEVIATION
INSTIGATION

There remains an important confusion about response instigation that should be addressed here. Two types of instigation are present in most contingency sessions; I will call them primary instigation and deviation instigation. Primary instigation determines the expression of behavior patterns in free baseline and represents the effects of physiological states and external stimuli on behavior. It is primary instigation that is held constant through the procedures of fixing the time of day, the deprivation regime, and the physical circumstances of the apparatus. Deviation instigation is produced when a schedule linkage constrains the expression of the primary instigation for an activity. Because of the schedule linkage, the deviation instigation produced by unexpressed (or overexpressed) primary instigation for one response is typically expressed through the other response. It is the resul-

tant "balancing" of expression of the deviation instigation for the instrumental and contingent responses that determines the changes from baseline expression under the contingency. Thus, in the present view a schedule has a dual role. In conjunction with the primary instigation it establishes the conditions for changes in behavior, and it provides the linkage for the expression of those conditions in behavior.

Most researchers in operant psychology (including Premack, 1959, 1965, in his early phase) appear to have confused something akin to primary instigation with deviation instigation. For most, the reinforcing ability of a particular event (response) is determined by some quality of that event, equivalent here to some aspect of primary instigation. However, this view is not correct. The critical determinant of response *change* under a schedule is the deviation instigation. To be sure if the same schedule is always used, differences in deviation instigation will be determined exclusively by differences in primary instigation. This is why Premack's (1959, 1965) probability-differential hypothesis appears to work, and why lawful drive effects can be shown in operant conditioning. However, if schedules differ, the deviation instigation cannot be predicted from primary instigation alone, and the probability-differential hypothesis fails to predict differences in the effects of various schedules.

One of the most startling illustrations of the distinction between primary and deviation instigation is provided by schedules that produce no deviation instigation because the ratio of instrumental to contingent responding specified by the schedule is the same as their ratio in baseline. Thus, repeated juxtapositions of, say, wheel running and drinking produce no increase in baseline responding despite the fact that considerable

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primary instigation exists and is expressed (Premack, 1965; Timberlake, 1980).

EXPRESSION CHARACTERISTICS

Mathematical modeling of the contingency situation (e.g., Hanson & Timberlake, 1983) makes clear that equal amounts of deviation instigation do not have the same effect on different instrumental responses, or on different combinations of instrumental and contingent responses. Such variation in expression of deviation instigation certainly can be attributed to expression characteristics such as elasticity, resistance, and flexibility. Thus, the examination of such expression characteristics can contribute much to the analysis of schedule effects, but these concepts are not sufficient to identify reinforcing circumstances *a priori*, or to determine systematic changes in expression characteristics over schedule values.

MOLAR AND RESPONSE—PATTERN LEVELS OF ANALYSIS

Contrary to the suggestion of several reviewers, I do not think the identification of molar behavioral set-points is particularly difficult at a pragmatic level. In typical laboratory paradigms, experimenters usually link only two responses in a contingency, and take care to eliminate or minimize stimuli that produce other responses. Thus, the importance of set-points related to instrumental, contingent, and, perhaps, background responding can be specified in advance. With more complex situations, more set-points may become relevant, but we always know that the responses linked by the schedule are critical.

However, this *a priori* certainty does not yet extend to the importance of characteristic patterns of responding. Several reviewers (and myself) were concerned about the proliferation of set-points apparently implied by the argument that local patterns of responding may reflect specific instigation. But even here I think there is potential order in the seeming chaos. There are two keys to the

systematic analysis of response-pattern set-points. The first is the empirical approach of specifically challenging particular patterns of responding using appropriate contingencies. The second is the conceptual approach of building an adequate hierarchical control model of behavior that relates molar and molecular characteristics of responding over time (e.g., Powers, 1978; Sibly & McFarland, 1974). Such a model should clarify the organization of multiple effects of constraints imposed at different levels, and the relative importance of each characteristic.

In contrast to other reviewers, Mazur was willing to accept the logic of response-pattern set-points, but not that of molar set-points. From the application of control-theory models to other biological phenomena, I expect both levels of control to be present in learned performance. That Mazur (1982) was able to simulate both pattern effects and total responding using the same model does not require that one level or the other be superfluous. In fact, Mazur's model, like most alleged molecular models, is not really a complete model of local effects in time, but of average local effects (response patterns) over some temporal or response-based window. It is thus in a sense a molar model that focuses on patterns rather than totals, instead of a true molecular (point) model of responding.

I agree with Mazur that many researchers now have the technology to delve into patterns of responding, and that these patterns are important. However, I think we need to change our style of modeling before we can progress much further. Among the problems to be solved are that of identifying the time interval over which a response alternative affects current choices to respond (Rachlin, 1982) and that of conceptualizing the organization of behavior (e.g., Gallistel, 1980). Recent evidence suggests that allocation of responding is insensitive to alternatives separated in time by an hour or more (Timberlake, 1984), a finding that has considerable ramifications for the distinction between open and closed economies (Hursh, 1978). On the other hand, there are also data that

suggest that animals can monitor intake over longer time periods such as seasons and years. Clearly we are dealing with complex organisms subject to more than the laws of exercise and effect.

REINFORCEMENT THEORY

Most of the reviewers of my paper adopted a skeptical "wait and see" attitude toward the approach outlined in the previous paper. For many this caution was based on the seemingly reasonable stance that reinforcement theory, if pressed, can handle the present phenomena just as it has handled many previous difficulties. I lack this confidence. The focus of reinforcement theory, though clear, is too restricted, dealing primarily with the manipulations of the experimenter and the close temporal juxtaposition of individual responses and reinforcers (Timberlake, 1983c). In contrast, learning has to do with evolution, ecology, and the regulation of internal and external environments, phenomena not reducible to a single principle of response strengthening.

I think we have pushed reinforcement-based explanations beyond their appropriate limits, beyond what is justified by the data or our understanding of the underlying processes. Reinforcement explanations simply do not consider a sufficient number of the determinants of behavior. We long ago reached a point where speculative explanations based on response strengthening may actually hinder rather than help our analysis of what is occurring (Moore & Stuttard, 1979; Timberlake, 1983a). Attempting to eliminate or hold constant other determinants of behavior to focus on response-reinforcer conjunctions is not the answer. I think the time has come to follow the Brelands' (1961, 1966) lead in considering at least a portion of the botanical jungles of response organization, regulation, and stimulus sensitivities (Gallistel, 1980; Timberlake, 1983b).

It is not that reinforcement theory is wrong—just that it is incomplete and overextended. Principles of reinforcement can be used to control behavior, but they

work in part because we ask the animal to do things for which it was designed, and in circumstances created to encourage it. In my laboratory we have a law termed variously Timberlake's second law, or the Law of the Operant Chamber. According to this law, modifications of standard operant chambers and procedures nearly always decrease their efficiency in producing learned behavior. The point is that operant apparatus and procedures were developed and refined by inventive, careful, and sensitive people. Thus, instead of being arbitrary, the apparatus and procedures have been shaped around the animal and its behavior. Discovering the reasons for the success of these designs will be an important step in discovering the nature of the organism and the role of learning in its ecology and evolution.

The behavior-regulation approach should serve some function in these tasks. It calls attention to a different set of causal agents and sets a larger framework for learning than does the reinforcement approach. The behavior-regulation approach was developed to model the determinants of asymptotic responding, not the acquisition of responses. Thus, it identifies reinforcing circumstances *a priori* and readily predicts outcomes that contradict the transsituational law of effect (Meehl, 1950). Unfortunately, despite the theoretical and empirical importance of this capability, it is rarely discussed in texts on learning or behavior modification.

Further, in the form of several mathematical models the behavior-regulation approach accurately predicts instrumental performance on simple schedules and in choice situations. It invites consideration of aspects of responding not adequately dealt with, such as the time period over which the distribution of responding is integrated, the role of the instrumental response, the response-constraining aspects of the contingency, and the levels of organization of responding. Although far from perfect, the behavior-regulation approach for the present is an heuristic way of organizing the phenomena of learned performance. Several of the reviewers of this paper use at least com-

ponents of the approach in their own work.

Finally, I would like to thank the reviewers of the several versions of the previous paper for their time and comments. I especially thank the "non-expert" reviewers in this area of theorizing (Case, Shull, and Branch) and the editors for their willingness to see the paper published even though they did not necessarily agree with important aspects of it. Too often experts in this area have been unwilling to see alternative views in print because of important flaws perceived from their point of view. Along with Fantino, I hope we are past our most insular period. What animals actually do under a schedule is fascinating. The part we have characterized is only a small part of the story. In our search for a single unifying principle, we have frequently closed our eyes to the complexities of behavior, focusing on one or another set of determinants as critical. Although comparison of reinforcement and behavior regulation as alternative explanations is inevitable as part of the process of clarifying our thinking, in the final analysis the important question is how the several determinants of learned performance can be put together in a coherent theory that does justice to the phenomena. As my deceased colleague, Frank Restle, assured me, "There is a lot of ditch to be dug, and words won't do it."

REFERENCES

- Breland, K., & Breland, M. (1961). The misbehavior of organisms. *American Psychologist*, *16*, 681-684.
- Breland, K., & Breland, M. (1966). *Animal behavior*. New York: Macmillan.
- Gallistel, C. R. (1980). *The organization of action: A new synthesis*. Hillsdale, NJ: Erlbaum.
- Hanson, S. J., & Timberlake, W. (1983). Regulation during challenge: A general model of learned performance under schedule constraint. *Psychological Review*, *90*, 261-282.
- Hursh, S. R. (1978). The economics of daily consumption controlling food- and water-reinforced responding. *Journal of the Experimental Analysis of Behavior*, *29*, 475-491.
- Mazur, J. E. (1982). A molecular approach to ratio schedule performance. In M. L. Commons, R. J. Herrnstein, & H. Rachlin (Eds.), *Quantitative analyses of behavior: Vol. 2. Matching and maximizing accounts* (pp. 79-110). Cambridge, MA: Ballinger.
- Meehl, P. E. (1950). On the circularity of the law of effect. *Psychological Bulletin*, *47*, 52-75.
- Moore, B. R., & Stuttard, S. (1979). Dr. Guthrie and *Felis domesticus* or: Tripping over the cat. *Science*, *205*, 1031-1033.
- Powers, W. T. (1978). Quantitative analysis of purposive systems: Some spadework at the foundations of scientific psychology. *Psychological Review*, *85*, 417-435.
- Premack, D. (1959). Toward empirical behavior laws: I. Positive reinforcement. *Psychological Review*, *66*, 219-233.
- Premack, D. (1965). Reinforcement theory. In D. Levine (Ed.), *Nebraska Symposium on Motivation: 1965* (Vol. 13, pp. 123-180). Lincoln: University of Nebraska Press.
- Rachlin, H. (1982). Absolute and relative consumption space. In D. J. Bernstein (Ed.), *Nebraska symposium on motivation, 1981: Vol. 29. Response structure and organization* (pp. 129-167). Lincoln: University of Nebraska Press.
- Sibly, R. M., & McFarland, D. J. (1974). A state-space approach to motivation. In D. J. McFarland (Ed.), *Motivational control systems analysis* (pp. 213-250). New York: Academic Press.
- Timberlake, W. (1980). A molar equilibrium theory of learned performance. In G. H. Bower (Ed.), *Psychology of learning and motivation* (Vol. 14, pp. 1-58). New York: Academic Press.
- Timberlake, W. (1983a). Appetitive structure and straight alley running. In R. L. Mellgren (Ed.), *Animal cognition and behavior* (pp. 165-222). Amsterdam: North-Holland.
- Timberlake, W. (1983b). The functional organization of appetitive behavior: Behavior systems and learning. In M. D. Zeiler & P. Harzem (Eds.), *Advances in analysis of behavior: Vol. 3. Biological factors in learning* (pp. 177-221). Chichester: Wiley.
- Timberlake, W. (1983c). Rats' responses to a moving object related to food or water: A behavior-systems analysis. *Animal Learning & Behavior*, *11*, 309-320.
- Timberlake, W. (1984). A temporal limit on the effect of future food on current performance in an analogue of foraging and welfare. *Journal of the Experimental Analysis of Behavior*, *41*, 117-124.