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REVIEWERS' COMMENTS ON TIMBERLAKE'S BEHAVIOR REGULATION AND LEARNED PERFORMANCE

The preceding paper by William Timberlake on behavioral regulation and set-points has been through several cycles of review and resubmission. After the paper was accepted, the reviewers were asked to reconsider and revise their original reviews in ways that would clarify the issues that Timberlake had addressed in the final version. Six such reviews follow, in an order corresponding to their original chronology. Our intention in publishing them is to present efficiently and constructively some alternative views of the interesting and important problems that Timberlake addresses.

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Former Editor and Action Editor on the manuscript

P. N. Hineline,
Editor

[The following is an extract from one of the original reviews of this article. The revised article addresses many of the original criticisms and this commentary represents the unanswered residual. It is offered here for the reader's consideration as a constructive counterpoint. I hope that this commentary may make a contribution to future thought in this area in the same way that the original review may have contributed in some small measure to the improvements contained in the present version. I retain great respect for the author and his thoughtful analysis contained in this important acticle.]

In this article William Timberlake attempts to expand on regulatory theory of performance by including consideration of multiple set-points. He illustrates these concepts by reference to two sets of data. The author has given birth to several new concepts; some are healthy but others could benefit from further intellectual nurture. I am in general agreement with the regulatory approach. My most fundamental concern centers on the idea of set-points.

A set-point conveys the idea of an ideal condition, most valued amount, or "normal" level. This notion makes most sense when there are only a few cardinal set-points that dictate adjustments in other aspects of performance when confronted with environmental constraints. For example, food and water intake might be two set-points that "drive" response rate, meal frequency, and meal size. Timberlake notes that certain of these other measures are more modifiable than others, so again he postulates secondary set-points for these. I find this form of analysis to have several shortcomings. First, I find it difficult to adhere to a set-point theory when even the cardinal measures (i.e., daily intakes) are not defended at all costs. Economically speaking, none of these measures is perfectly inelastic; they do vary in elasticity. It is unclear from the criteria for a set-point whether elastic demand that accompanies decreases in instrumental performance in the face of increasing constraint (e.g., increasing fixed-ratio requirement) would imply a set-point or not. For example, brain stimulation will reinforce instrumental performance to a high degree under conditions of minimal contraint but shows a highly elastic (steeply sloping) demand curve and decreasing response-rate function under increasing constraint. By contrast, the opportunity to eat food pellets will not maintain nearly as high a rate of performance under minimal constraint but will show a highly inelastic (minimally sloping) demand curve and increasing respone-rate function under increasing constraint (Hursh & Natelson, 1981). It seems more reasonable to think in terms of a continuum of elasticity or resistance to change rather than in terms of discrete categories of molar and response-pattern set-points, particularly because the baseline level of performance is a poor predictor of the elasticity of demand under increasing constraint (see Hursh, 1980). The usual way of plotting demand curves in log-log coordinates allows comparison of slopes of change without concern for differences in initial level.

A second related problem has to do with the mathematical imprecision of the terms *resistance* and *flexibility* as used by the author. How does one precisely compare the resistance of two measures that differ in flexibility? For example, Rat 1 in Figure 3 shows a more flexible (i.e., higher maximum) bar-press rate than meal size, but it is difficult to judge which one is more resistant (i.e., reaches its final level more rapidly). Visual inspection can be deceptive in such a case. In contrast, Nevin has more precisely defined resistance to change in discussions of response strength and behavioral momentum as the slope of the function (Nevin, Mandell, & Atak, 1983). The economic-demand-curve approach offers a similar but more inclusive analysis because it explicitly allows for changes in elasticity (nonlinear demand curves in log-log coordinates) with increases in constraint. Elasticity is usually the slope of the demand curve within a restricted range or the derivative of the function at a point (see "mixed elasticity," Hursh, 1980). A separate concept refers to the level of demand, which is simply the placement of the curve in relation to the origin.

Third, the measurement of set-points clearly suggests that set-points reflect an underlying "instigation." Accurate measurement of the set-point requires great care to maintain a constant level of this "instigation" throughout the experiment. This kind of object language when referring to relational processes can lead to an unintended search for the entity named and, worse, explanation of changes in performance by reference to changes in the "thing." This notion could imply for some that variations in elasticity or resistance to change for the same activity in different experiments point to uncontrolled levels of "instigation." It would seem more fruitful to look for contextual (i.e., environmental) differences such as differences in substitutable alternatives or differences in the overall economic context (i.e., open vs. closed). Furthermore, it is not clear to me whether differences in "instigation" should lead to differences in the baseline level (set-point), differences in resistance to change, or differences in maximum change "flexibility." That may be an empirical question but because "instigation" is not directly measured, it seems less susceptible to empirical analysis.

Finally, I find a set-point analysis arbitrary in that it takes a measure of intake under some ideal condition (e.g., no contingency) as intrinsically more valuable or perfect than intake under more constrained (less ideal) conditions. This approach seems doomed from the start because most species evolved under conditions of constraint; I find it implausible that they would, nevertheless, be structured to approach some imaginary set-point attainable only under zero constraint. What makes one set of conditions or combination of performances more "ideal" than any other? I am reminded of data showing that rats live longer when somewhat food deprived than when allowed to eat ad lib. Most of us accept the notion that laboratory animals overeat when given free access to food in a confined cage. The point here is that one must be careful when taking some initial value as the ideal and explaining everything else in relation to that value. One can make any situation seem ideal when examining deviations as percentage of baseline, but that picture is really artificial. Much of that problem is avoided by concepts such as elasticity of demand or Nevin's response strength because the referent is the slope of a function rather than some ideal point on the function. The author is cognizant of this limitation of the "free-baseline" measure of set-points; I see it as a fundamental problem with the entire set-point approach.

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To borrow from Timberlake, this comment began as a modest attempt, one of my first manuscripts for JEAB as a guest reviewer. The sections of that version of his paper which I judged to merit publication have been retained, and I am therefore satisfied that I probably made a sound favorable recommendation. However, other sections have been expanded (rather than shortened or deleted) and it now seems that there is as much to criticize about the paper as to applaud.

On the positive side, Timberlake makes several cogent arguments in rebuttal to Allison's (1981a, 1981b) critiques of optimal behavior-regulation theory in the section, "Behavioral set-points in optimal regulation." His point that deprivation (instigation) was probably not held constant as the ratio requirement was varied in the Collier experiments seems pertinent enough, but one might add that manipulation of the ratio was also apparently confounded with time in the experiment, further precluding meaningful conclusions on simple methodological grounds. Also, the points Timberlake raised concerning the concurrent-ratio data are convincing arguments that these data do not (and perhaps never even possibly could) clearly contradict the minimum-deviation hypothesis. Finally, Timberlake's treatment of the issue of molar versus molecular set-points seems to be cutting new ground in proposing that both types are important in determining behavior. Although this is an interesting idea that deserves further investigation, I fear his conjecture and/or the assumptions that underly it may not be testable. Timberlake did not make it clear how one substantiates as an explanatory device the existence of all the relevant set-points (and their relative importance) in a way that avoids circularity. Still, I think the section, "Response-pattern set-points and molar regulation." is intriguing and will make a significant contribution to the theoretical discussion of the molar/molecular set-point problem.

On the negative side, the paper may leave some misapprehensions and disagreements unresolved. Among these are the relation of behavior-regulation theory to reinforcement theory, the evaluation of the advantages and limitations of behavior-regulation theory, and conceptual differences within the basic regulatory approach.

Timberlake seems to hold three different views of the relationship between behavior-regulation and reinforcement theories. Behavior-regulation theory is alternately viewed as an opposing alternative, a descendent or refined reformulation, or an independent but compatible (even complementary) hypothesis to reinforcement theory. When viewed as an opposing alternative, Timberlakes's interpretation of reinforcement theory is not generally convincing. For example, he suggests that extra-experimental manipulation of access to food is the analog in reinforcement theory to the idea of constrained instigation as a predictor of the reinforcing or punishing effects of eating. However, I would suggest that reinforcement theory's assumption of cross-situational generality of the reinforcing effectiveness of a stimulus (or response, from Premack's perspective), modulated by reinforcement contingencies, provides similar predictive generality. In the same way as in behavior-regulation theory. this assumption allows prediction of reinforcement and punishment effects independent of extra-experimental operations so long as these conditions remain unchanged between estimation/prediction sessions and test sessions.

The discussion of the advantages and limitations of behavior-regulation theory is also confusing. The point about the regulatory approach being an a priori theory does not seem to be an advantage over other views, but rather merely a prerequisite to serious consideration. Indeed,if his claim is correct, one wonders why behavior-regulation theory is unable to specify all relevant set-points a priori. Perhaps the most confusing aspect of the discussion of advantages involves the claims of uniquely predicting bitonic functions between ratio requirement and response rate. There seems to be an incompatibility between the ability to predict bitonic functions for responding and the assumption that the definition of a set-point requires a direct function between responding and the challenge to expression of instigation. Can one have it both ways and still argue that the theory is well formulated?

Timberlake's discussion of conceptual differences within the basic regulatory approach could be clearer. I fail to see the problem with Premack's formulation, although Timberlake suggests that it misleadingly emphasizes response deprivation over response strengthening. Also, the dispute over the appropriate baseline, single response or paired response, seems only to point up weaknesses in the case for behavior regulation. It was not made clear whether differences in the type of baseline were critical to the accuracy of potentially discriminating predictions.

My final point, alluded to earlier, is that behavior-regulation theory may not yet be well formulated. The theory assumes that the notion of instigation for a response is critical for understanding learning and performance but cannot now predict what effects should occur from varying it. Even if behavior-regulation theory is well formulated within the boundaries currently set for it in predicting behavior, I predict it will not gain in acceptance until it more closely competes with the much more substantial set of applications addressed by more conventional views. Such a change will depend upon better specification of the regulation hypothesis.

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In my review of an earlier version of this manuscript, I was critical of Timberlake's discussion of both response-pattern set-points and molar set-points.This draft, however, satisfies many of my objections to his postulation of response-pattern set-points. On the other hand, I continue to be skeptical about the usefulness of molar behavioral set-points as explanatory concepts. I will discuss these two types of set-points in turn.

I originally thought that the postulation of a set-point for duration of drinking bouts in the Marwine and Collier (1979) experiment was superfluous. Now, however, it seems that Timberlake's use of the term *set-point* carries only three implications: (1) For each subject there is some bout duration of intermediate size that is preferred over either longer or shorter durations. A subject's preference presumably declines monotonically with increasing deviations from this bout duration. (2) As a consequence, a subject is motivated to minimize deviations from this preferred bout size. As Timberlake puts it, "the disparity between expression and instigation becomes a driving force for learned performance." (3) Because drinking-bout duration is not the only factor motivating a subject's behavior, there is no reason to expect that this bout duration will remain unchanged in all conditions where this is physically possible. For instance, it is not surprising that bout duration increased with increasing ratio size, because the advantages of maintaining the preferred bout duration had to be weighed against the disadvantages of more bar presses being required for each period of access to water. I find each of these assumptions reasonable, and if the concept of a response-pattern set-point involves no other assumptions than these, I have no objection to it.

In contrast, I have some serious doubts about the usefulness of molar behavioral set-points and of molar regulatory theories in general. All of the molar regulatory theories Timberlake cites share the assumption that there is something special about the total durations or amounts of behavior in a (multiple) baseline situation, and that subjects (1) are capable of measuring these total durations with a fair degree of accuracy, (2) do in fact go to the trouble of measuring these totals in both baseline and contingency sessions, and (3) are motivated to preserve these durations as closely as possible in a constrained situation. I find all three of these assumptions implausible. Data on animal timing (e.g., Gibbon, 1977) and counting (e.g., Hobson & Newman, 1981) suggest that standard deviations increase at least as rapidly as the means of the events to be counted or timed. These data challenge the first assumption, for they suggest that subjects' estimates of molar events will be quite inaccurate. The abundant data on the effects of delayed reinforcement (e.g., Ainslie, 1975) show that animals are motivationally very short-sighted, and these data run counter to the second and third assumptions.

To make my objections to molar regulatory theories more clear, imagine an hour-long baseline session in which a rat drinks water for 600 s, presses a lever for 10 s, and spends the remaining 2990 s performing other types of behavior. In a subsequent contingency session in which 20 s of drinking is allowed for every 50 lever presses, the rat spends 400 s pressing the lever and 500 s drinking. According to molar regulatory theories, the baseline results represent the most preferred distribution for this subject, and its behavior in the contingency session represents the best possible compromise under the circumstances. Molar regulatory theories assume, in effect, that the animal has as its goal, "accumulate 400 s of lever pressing, 500 s of drinking, and 2700 s of other behavior," because this is the best possible compromise in the contingency condition. In short, molar regulatory theories assume that this long-term goal is a direct determinant of the subject's behavior in the contingency session.

I suggest that the typical animal subject has no such long-term goal. Indeed, if we must speak of goals at all (and the concept of a set-point seems intimately related to the concept of a goal), then I maintain that the subject has no constant set of goals in an experiment of this kind. To use Timberlake's terminology, the "conditions of instigation" vary tremendously from the beginning of the session to the end, both in baseline and contingency conditions. At the beginning of the baseline session, the rat's motivation to drink is high, and much of its time is spent drinking. Toward the end of the session, the now-satiated rat spends little time drinking. At the beginning of the contingency session, the rat's motivation to press the lever is higher because this behavior makes drinking possible, but it is not high enough to allow as much drinking as occurred early in the baseline session. Because there is less consumption of water, the subject's motivation to drink (and consequently its motivation to press the lever) does not decline as quickly as in the baseline session.

In a recent chapter, I presented evidence consistent with this scenario (Mazur, 1982, Figure 3-11), and I argued that it is not long-term goals but the moment-to-moment "values" of the different types of behavior that govern an animal's time allocation in such situations. I have made these points for a number of years (Mazur, 1979, 1982), however, and I do not expect the advocates of molar regulatory theories to accept them now. All I ask is that they begin to collect and report information on within-session trends and moment-to-moment patterns of behavior in their experiments. All too often, the data for each activity such as drinking or lever pressing are reduced to a single number for an entire session, or for several sessions (as in Timberlake's Figures 2 and 3). When experiments are controlled by computer, it is not difficult to collect much more detailed information. The reporting of such small-scale behavior patterns would be extremely valuable to those of us who believe that the total durations or amounts of behavior across an entire session tell relatively little about variables that control an animal's behavior.

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This paper serves a valuable function in providing a clear statement of behavior-regulation theory. Timberlake points out how the use of behavioral set-points provides the possibility of more precise specifications of "learned performance in behavior." The strongest section of the manuscript is its assessment of the Allison (1981) paper. Readers of the earlier paper will find Timberlake's response cogent, persuasive, and deserving of publication.

I am not sanguine about the value of this paper for understanding behavior, nor of its ultimate impact. In an era in which it seems almost every laboratory is producing its own theory aimed at describing some large segment of operant behavior, it has become easy to ignore everyone *else's* theories. Devotees of molar regulatory theories appear to be a particularly insular and—rightly or wrongly—ignored band of theoreticians. Thus, several reviewers of earlier versions of this paper professed little hope for the ultimate impact of such theories. I too have reservations about the predictive power and usefulness of this approach. A related problem, and one which Timberlake appears to acknowledge, concerns the apparent lack of criteria, independent of behavior, for determining what is and what is not a set-point. We are told an organism has a set-point because a systematic change occurs in a behavioral measure under increased schedule challenge. Why does that systematic change occur? Because the animal is minimizing the deviation from a set-point. As I see it, there seems to be a degree of circularity here (and one less tractable than that facing reinforcement theory several decades ago, because regulatory theory is far more complex). Given that there is no way to identify a set-point independently of behavior, and given that there appears no way to specify in advance which of several putative set-points is more influential, the theory appears overly limited in terms of predictive power.

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This paper sets out to clarify what the author sees as misunderstandings of behavior-regulation theories of performance. Specifically, he proposes to explain the nature and importance of the approach, to define and describe the notion of behavioral set-point, to discuss the place of optimality in such theories, and to discuss molar versus molecular set-points. He assumes that all these have been misunderstood or misapplied.

Although the paper is logically arranged and generally clear, I am not sure that it really adds very much that is new with respect to behavior-regulation theories. As one who has not worked directly on the issue, I found little that made the approach any more (or less) defensible or that enhanced or altered my understanding of it. Consequently, I am not enthusiastic about recommending acceptance, even though I found the paper a nice exposition of the behavior-regulation approach. Perhaps I am incorrect, but it is my impression that most workers in the experimental analysis of behavior believe that the behavior-regulation approach to identifying, a priori, situations in which one will observe reinforcement (or punishment) is viable and important. Thus, I am not sure it is in need of much defense. And obviously I was not convinced by the author that the approach has been attacked to the point that it needs to be defended. A revision, then, should make the case for what seems to me to be largely a reiteration of previous work.

It is not entirely clear whether the regulation approach is offered as an explanation of reinforcement effects or as a completely independent notion. If the latter is intended, it needs to be more fully defended.

The largest portion of my original review was devoted to issues of clarity. I found several issues and concepts not clearly presented. The author responded effectively to most of my comments. Included in my confusion were the concepts of "instigation," "linkage," "set-point," "severity of challenge," and "minimal constraint." The revision makes what is meant by "instigation" and "set-point" more easily understood, and references to "linkage" and "severity of challenge" have been minimized. Also, the notion of "minimal constraint" has been dealt with. In the revision, however, the concept of "instigation" still troubles me. For example, it is stated that instigation refers to the combined effects of variables both within and outside the test session that lead to engaging in certain activities. Yet it is later claimed that "whether an organism is deprived of a commodity outside the experimental session is not . . . interesting." This certainly seems contradictory to me. I also am still a bit confused by what constitutes a "set-point." It seems that *any* activity, including latency to eat in a novel environment, that changes as a function of constraint can be viewed at least potentially as a "set-point."

The situation in which the author is trying to determine relative contributions of molecular and molar set-points is one in which the two are perfectly confounded. Even in the Marwine and Collier (1979) study, *total* contingent responding either is not constrained or is only minimally constrained. Once the subject completes one ratio, it can drink forever (theoretically). A discussion is needed of the kinds of tests (if they exist) that would allow one to separate molar and molecular set-point contributions.

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Theoretical approaches may differ in terms of how they organize or conceptualize interesting variables and effects. The usual move within the response-strengthening approach has been to group into separate conceptual categories variables that alter the potential of an event to function as a reinforcer or punisher (establishing operations; Michael, 1982) and variables that determine the effect of a reinforcer of given potency on the emission rate of a response (contingency operations). The establishing operation that we are most familiar with is, of course, food deprivation, which is an operation made outside the experimental session and one that has relatively prolonged and stable effects. It is recognized, however, that many significant establishing operations have relatively fleeting effects and are generated by events within the experimental session (Herrnstein, 1977; Michael, 1982). The result of any experimental procedure is viewed as the result of the various establishing operations (intended and unintended), the various contingencies, and the various discriminative-stimulus effects (e.g., Logan, 1964). It seems an open question whether the regulation-based theories, which conceptualize and organize these variables differently, will be more successful than response-strengthening-based theories in accounting for interesting aspects of behavior in different situations. At the present time, however, there seems to be no compelling reason to assume that theories developed within the response-strengthening tradition are incapable of dealing effectively with such data. Clearly, theorists in this tradition have been sensitive to the kinds of effects discussed here and have been interested in learning more about the various kinds of establishing operations.

In discussing molar and molecular set-points, it would seem important to emphasize that terms like *set-point* and *regulation* refer to concepts that might or might not turn out to be useful in organizing data. There seems to be no reason, in principle, why the same data could not be described using the concepts of the traditional response-strengthening approach. The reinforcing potency of a bite of food in a restaurant probably is a function of both long-term variables (e.g., the time since I've last eaten, the total calories that I've consumed during the last week, etc.) and short-term variables (e.g., the size of each bite and the time between each bite). If the waiter snatches my plate whenever I pause longer than five seconds between bites, I probably would come to pause less than usual. We could say that my eating more rapidly introduces a punishing effect (I don't like to eat so fast) but that this punishment is less severe than would result from my plate being taken away and from my having to order and pay for a new dinner. That these facts could be described in terms of molar and molecular set-points is not at issue. The point, simply, is that the data do not have to be described in those terms. What could be described as evidence that molar set-points are more strongly defended than molecular set-points (e.g., I'll change my eating tempo before I'll sacrifice my regular-sized meals) could be described in terms of different response strengths due to different punishment/reinforcement combinations.

Of course, if we could really predict in advance what set-points are going to be defended and how the set-points are weighted when circumstances place them in conflict, then the advantage of the regulation approach would be apparent. However, it is clear from the discussion that we are not yet in a position to do this. We even see that complex aspects of behavior such as the tendency to sample might be regulated. In view of these kinds of complexities and uncertainties, it seems fair to wonder how real the advantages of the regulation approach are relative to those of the traditional response-strengthening approach.

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