BEHAVIORAL AND BRAIN SCIENCES (2000) **23,** 73–130
Printed in the United States of America

Behavioral momentum and the Law of Effect

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Abstract: In the metaphor of behavioral momentum, the rate of a free operant in the presence of a discriminative stimulus is analogous to the velocity of a moving body, and resistance to change measures an aspect of behavior that is analogous to its inertial mass. An extension of the metaphor suggests that preference measures an analog to the gravitational mass of that body. The independent functions relating resistance to change and preference to the conditions of reinforcement may be construed as convergent measures of a single construct, analogous to physical mass, that represents the effects of a history of exposure to the signaled conditions of reinforcement and that unifies the traditionally separate notions of the strength of learning and the value of incentives. Research guided by the momentum metaphor encompasses the effects of reinforcement on response rate, resistance to change, and preference and has implications for clinical interventions, drug addiction, and self-control. In addition, its principles can be seen as a modern, quantitative version of Thorndike's (1911) Law of Effect, providing a new perspective on some of the challenges to his postulation of strengthening by reinforcement.

Keywords: behavioral momentum; clinical interventions; drug addiction; preference; reinforcement; resistance to change; response strength; self-control

1. Introduction

The stimulus presented by the experimenter, the response of the organism, and the reinforcer that follows the response are fundamental elements in the science of behavior. Skinner (1969) suggested that these three terms define the discriminated operant as a unit for analysis. This target article argues that there are two separable aspects of discriminated operant behavior that has been trained to asymptote: its rate of occurrence, which depends primarily on the contingencies between the response and the reinforcer, and its resistance to change, which depends primarily on the contingencies between the stimulus and the reinforcer.

The distinction between response rate and resistance to change is captured by the metaphor of behavioral momentum, in which the rate of a simple, repeatable response in the presence of a distinctive stimulus is analogous to the velocity of a physical body in motion. Following Newton's second law, when responding is disrupted in some way that is analogous to imposing an external force on a moving body, resistance to change of response rate is related to an aspect of behavior that is analogous to inertial mass in classical mechanics.

To pursue the metaphor, Newton's law of gravitation suggests that an analog to gravitational mass may be derived from the attractiveness or value of access to reinforced responding as measured by preference. Although they refer to different aspects of behavior – namely, resistance to change in the presence of a stimulus and responding that gains access to that stimulus – we will argue that resistance to change and preference covary and that they provide independent, convergent measurement of a single construct analogous to the mass of a physical body. In terms of the traditional distinction between learning and performance, velocity (identified with response rate) characterizes ongoing performance, whereas mass (derived from resistance to change and preference) reflects the learning that results from a history of reinforcement in the presence of a distinctive stimulus situation.

We begin by contrasting response rate and resistance to change as measures of the traditional construct of response

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https://doi.org/10.1017/S0140525X00242404 Published online by Cambridge University Press

strength and describe some research on resistance to change that distinguishes response rate and resistance. After a review of related work on preference, the convergence of resistance and preference is treated via the momentum metaphor. Supporting research involving concurrent measurement of resistance and preference is described, and the discrepancy between resistance and preference resulting from the partial reinforcement extinction effect is resolved by a model of resistance to change that incorporates generalization decrement. After considering some extensions to clinical interventions, drug effects, and self-control, we argue that the findings of research on behavioral momentum constitute a modern, quantitative version of Thorndike's (1911) Law of Effect and we review some challenges to the Law of Effect from the perspective of behavioral momentum.

2. The strength of steady-state free operant behavior

The idea that behavior varies along a dimension of strength appears in Sherrington's (1906) studies of reflexive behavior, where strength was measured by the latency and amplitude of response to an eliciting stimulus. It appears also in Pavlov's (1927) studies of conditional reflexes, where strength was measured by resistance to extinction or to external inhibition as well as by the latency or amplitude of response to a conditional stimulus. Hull's (1943) theorizing relied heavily on the construct of habit strength, which was established by reinforcement and expressed in performance measures such as latency, amplitude, probability, and resistance to extinction of a learned response. However, these measures did not always covary, casting doubt on the utility of the construct. Moreover, as was suggested by Logan (1956), responses that varied along dimensions such as latency and amplitude could be construed as different responses rather than as instances of a single response that varied in strength.

Following Skinner's (1938) relatively atheoretical approach, the experimental analysis of behavior has either identified response strength with the rate of a free operant (see, e.g., Vaughan & Miller 1984) or eschewed the notion altogether. In this section, we consider resistance to change as an alternative to response rate as a measure of strength. In addition, we suggest that resistance to change is related to learning, whereas response rate characterizes performance.

2.1. Determiners of response rate

It has long been recognized that response rate depends on the contingencies of reinforcement as well as the rate or magnitude of the reinforcer. For example, ratio schedules routinely maintain higher response rates than interval schedules with comparable obtained rates of reinforcement. However, it is not clear that ratio-schedule performance should be deemed stronger than interval-schedule performance, because the contingencies that shape and maintain them differ. Following Logan's (1956) argument for discrete responses, ratio and interval performances could be construed as belonging to different classes, rather than as instances of a single class that varies in strength.

Morse (1966) distinguished the shaping effects of reinforcement contingencies on response rate – for example, the difference between ratio and interval schedules in the reinforcement of interresponse times – from the strengthening effects of reinforcement on average response rate. Presumably, if shaping contingencies were kept constant across conditions that varied in the rate or amount of reinforcement, average steady-state response rate would give a direct measure of the strengthening effect of reinforcement.

In his review of the literature, however, Morse noted that the steady-state rate of a single response maintained by a single schedule of reinforcement was not always an orderly function of reinforcer amount when reinforcement contingencies were constant. For example, Keesey and Kling (1961) found that response rates maintained by variable-interval (VI) schedules were essentially constant when reinforcer amount was varied. However, Keesey and Kling also reported that response rate was positively related to reinforcer amount when each of three different stimuli signaled a different amount and alternated within each session (see also Shettleworth & Nevin 1965). In effect, Keesey and Kling's method established three discriminated operants defined jointly by the antecedent stimuli, the responses in their presence, and the consequences of responding signaled by the stimuli (Skinner 1969). Following Skinner, we take the discriminated operant to be a fundamental unit in the science of behavior. The relations between the strength of discriminated operant behavior and the signaled conditions of reinforcement will be explored in sections 3, 4, and 7.

2.2. Choice, strength, and value

Extensions of the steady-state operant paradigm to choice between two continuously available operants inspired a new approach to the measurement of response strength and reinforcement value. In a much-cited study, Herrnstein (1961) arranged concurrent VI VI schedules for pigeons' responses to two keys, effectively arranging two simultaneous discriminated operants defined by key location, and found that the relative frequency of responses to one alternative roughly equaled (matched) the relative frequency of food obtained from that alternative in each condition. Herrnstein's finding can be stated as:

$$B_1/(B_1 + B_2) = r_1/(r_1 + r_2), \quad (1)$$

where B_1 and B_2 designate the response rates to each alternative and r_1 and r_2 designate the reinforcer rates obtained from each alternative. This matching result proved to have remarkable generality (see de Villiers, 1977, and Williams, 1988, for review).

Herrnstein (1970) extended the matching law to the rate of a single response by assuming that all of an organism's behavior, including unmeasured behavior B_0, summed to a constant k, where B_0 and k are expressed in units of the measured response. From the matching law, Equation 1,

$$B/(B + B_0) = r/(r + r_0), \quad (2)$$

where B represents response rate, r represents the obtained rate of experimentally arranged reinforcers, and r_0 represents the rate of extraneous, unspecified reinforcers for other activities that occur in the experimental setting expressed in units of the measured reinforcer. Because $B + B_0 = k$, the sum of all possible behavior,

$$B = kr/(r + r_0). \quad (3)$$

Although it fails at very high reinforcer rates (Baum 1993) and in long experimental sessions (McSweeney 1992), Equation 3 provides an excellent description of the relation between response rate and reinforcer rate on interval

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schedules under most conditions. Moreover, it applies to discrete-trial as well as free-operant performance, to reinforcer magnitude as well as rate, and to negative as well as positive reinforcement (de Villiers 1977). In view of its generality, Equation 3 has come to be known as the Relative Law of Effect and is widely accepted as a modern version of Thorndike's Law. However, Equation 3 is limited in two ways: It does not address the effects of antecedent stimuli, and, although it describes asymptotic performance, it does not address other effects of a history of reinforcement.

2.3. Learning and performance

Learning has been defined as "a relatively permanent change in behavior *potentiality* which occurs as a result of *reinforced* practice" (Kimble 1961, p. 6; author's emphases). Kimble's reference to potentiality suggests that although changes in behavior as a result of reinforced practice may be directly observable in current performance, reinforcement may also have effects that can be detected only by a separate test. The possibility of differences between the effects of reinforcement on behavior as evaluated during training and as evaluated by a later test of "potentiality," such as resistance to change, accords well with intuition and is embodied in the long-standing distinction between "performance" and "learning" in the literature of learning theory.

2.4. Response strength and common sense

The construct of response strength is similar to behavior potentiality. It is presumed to increase with reinforcement, and the connotations of "reinforcement" in our everyday language may help to identify a useful way to characterize strength. For example, concrete is said to be reinforced with steel rods to make it stronger as a building material. In this expression, "reinforcement" implies an increase in durability or resistance: Under an added load, a reinforced concrete wall does not collapse as readily as an unreinforced wall. However, an observer could not determine, by looking at it before a load test, whether the wall had been reinforced or how many steel rods had been used; the load that makes the wall collapse must be known. By analogy, we suggest that more frequently or generously reinforced behavior becomes more resistant to challenge or disruption, and this increase in its resistance need not imply an observable increase in the rate or probability of currently observed behavior. Instead, the strengthening effects of reinforcement may be evident only when responding is disrupted in some way.

2.5. Resistance to change and response strength

A theoretical article by K. Smith (1974) proposed that reinforcement value could be measured by training the reinforced response to asymptote and then determining the intensity of "some standard attenuator required to just abort the behavior" (p. 141). The author noted a consequence of this approach: "'the most potent reinforcer' is the one able to engender behavior most highly resistant to attenuation. To reinforce – to 'strengthen' – is thus to make refractory to attenuation" (p. 141).

Nevin (1974) independently suggested that response strength be equated with resistance to change and explored this notion by arranging different conditions of food reinforcement in the presence of two successively alternating stimuli (the components of a multiple schedule) with pigeons as subjects. He found that resistance to disruption by an alternative source of food and resistance to extinction in a given component were both positively related to the rate or amount of food in that component during baseline training.

In sections 3 and 4 of this target article, we consider resistance to change as a measure of the strength of a discriminated operant. In sections 5 and 6, we consider preference for access to that discriminated operant as an independent measure of the value of the conditions of reinforcement maintaining it. Metaphorical relations between resistance and preference are treated in section 7, and empirical research linking resistance and preference is described in section 8.

3. Resistance to change in multiple schedules

The majority of empirical research on resistance to change has employed multiple schedules of reinforcement, which define two or more discriminated operants. We begin by describing the paradigm and then review research, most of which has used pigeons as subjects, illustrating the study of resistance to change and its determiners.

3.1. The multiple-schedule paradigm

Multiple schedules are arranged by correlating two (or more) successive stimuli with independent schedules or contingencies of reinforcement, where each stimulus-contingency combination defines a schedule component. The paradigm is illustrated in Figure 1, which shows the successive presentation of two stimuli, S_1 and S_2, separated by a brief time-out. In this illustration, a single response is intermittently reinforced in the presence of each stimulus according to independent VI schedules, with the schedules chosen so that the average rate of reinforcement per unit time in the presence of S_1 is greater than that in S_2.

The paradigm has the following features. (1) The experimenter can control the duration of each component and can arrange that they alternate, regularly or irregularly, a num-

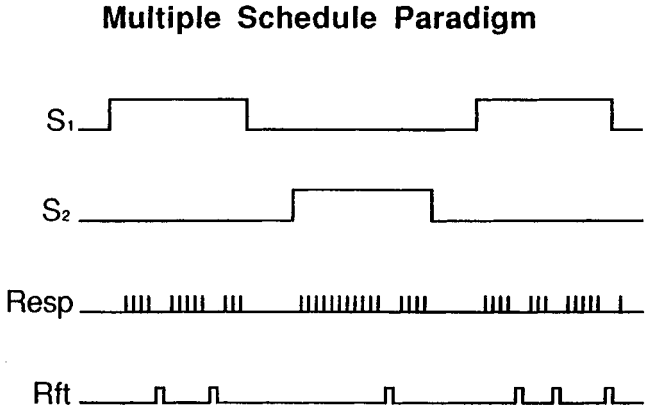


Figure 1. Schematic diagram of the multiple-schedule paradigm of discriminated operant behavior. Stimuli S_1 and S_2 are presented successively, separated by a brief time-out. A free-operant response is reinforced intermittently according to separate schedules in the presence of S_1 and S_2, defining two schedule components.

https://doi.org/10.1017/S0140525X00242404 Published online by Cambridge University Press

ber of times within each experimental session. Thus, response rates in both components can be measured and related to the component schedules within sessions as well as within subjects. (2) Interactions between components, such as behavioral contrast – an inverse relation between response rate in one component and reinforcer rate in the other component – can be minimized by arranging time-out periods between components. (3) Most important for present purposes, a disruptor can be applied equally to both component performances and its effects can be compared between components, again within sessions as well as within subjects. The use of VI schedules ensures that the number or rate of reinforcers obtained by the subject is roughly equal to the number or rate arranged by the experimenter even when response rate is moderately reduced.

3.2. A representative experiment

Nevin (1974, Experiment 1) trained food-deprived pigeons on multiple VI 1-minute, VI 3-minute schedules yielding 60 reinforcers per hour in Component 1 and 20 per hour in Component 2. Responding was disrupted by presenting food during the time-out periods between components for 6–10 hours at rates that varied across successive determinations. Average response rates for the final hour of baseline training preceding time-out food and for the first hour of time-out food are shown in the upper panel of Figure 2. Baseline response rates were somewhat higher in Component 1 than in Component 2, and, when food was presented during time-out periods, the decrease in response rate, relative to baseline, was always smaller in Component 1 than in Component 2.

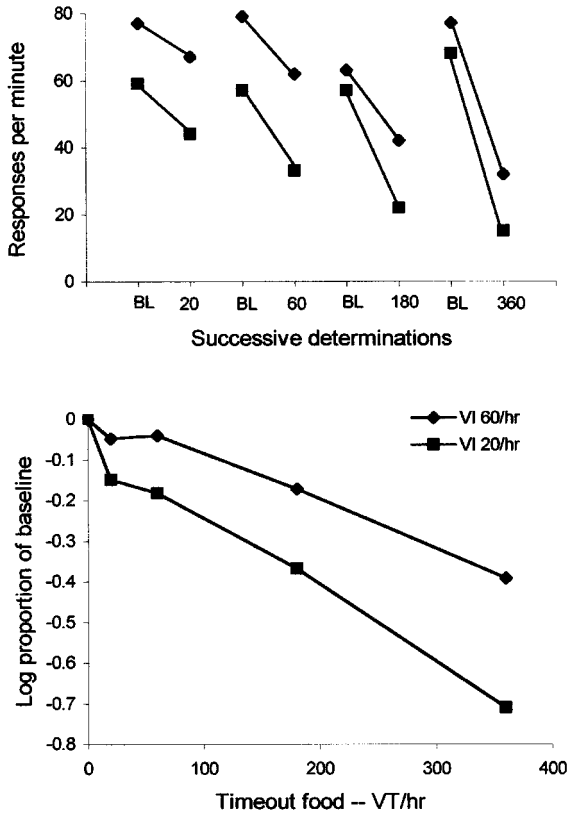


Figure 2. Average response rates of pigeons in two components of a multiple VI VI schedule with 60 reinforcers per hour in one component and 20 per hour in the other, showing the effects of the rate of free food presentations during time-out periods between components. The upper panel shows response rates during the last hour of baseline training and the first hour of disruption by time-out food across four conditions. In the lower panel, these data are re-expressed as log ratios of response rates with free food to response rates in the immediately preceding baseline and plotted as functions of time-out food rate. Adapted from Nevin (1974, Experiment 1).

To summarize the data, response rates in each component during the first hour of exposure to time-out food were expressed as logarithms of proportions of the immediately preceding baseline response rates for each pigeon and averaged across pigeons. These log proportions of baseline are shown in the lower panel of Figure 2 as a function of the rate of time-out food presentations.

The data are presented as logarithms of proportion of baseline for several reasons. First, proportion of baseline is a direct measure of resistance to change: The smaller the decrease, the larger the proportion. Second, logarithms are unbounded and permit examination of functional relations without distortion by floor effects. Third, the logarithmic transform renders equal proportional changes as equal differences. Suppose that presenting a given rate of time-out food reduces response rate to 50% of baseline. If this reduced level is construed as a new baseline and is further disrupted by doubling the rate of time-out food, one might reasonably expect another 50% reduction, to 25% of the original baseline. When expressed as logarithms, these successive reductions are the same, and, for this example, the relation between log proportion of baseline and time-out food rate is linear (for further discussion of measurement issues, see Nevin et al. 1983; 1987; Grace & Nevin 1997).

Both functions in Figure 2 are roughly linear (except for the initial portion of the function for Component 2) and can be characterized adequately by their slopes: −0.10 for Component 1, which arranged 60 reinforcers per hour, and −0.15 for Component 2, which arranged 20 reinforcers per hour. Strength of responding, construed as resistance to change, is inversely related to the slope of the function relating log proportion of baseline to the value of the disruptor: The shallower the slope, the greater the resistance. Thus, in this example, response strength is directly related to the rate of reinforcement in a schedule component.

3.3. Reliability and generality

The finding that time-out food has a smaller disruptive effect on performance in a multiple VI VI schedule component with more frequent reinforcement has been repeated several times (McLean et al. 1996; Nevin 1974, Experiment 5; Nevin et al. 1983). Similar results have been obtained with home-cage prefeeding (Nevin 1992a; Nevin et al. 1981; 1990), signaled alternative reinforcement (Nevin et al. 1981), and extinction (Nevin 1974, Experiment 2; 1992a; Nevin et al. 1983; 1990). Without exception, the rate of responding decreased relatively less in the component with the greater rate of reinforcement (the richer component) during training.

The effects of aversive disruptors are entirely consistent with those described above. For example, Bouzas (1978) arranged intermittent electric-shock punishers at equal rates in both components of a multiple VI VI schedule and observed relatively smaller decrements in the richer com-

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ponent. Lyon (1963) and Blackman (1968, Experiment 2) presented signaled unavoidable shocks during both components of a multiple VI VI schedule and observed less conditioned suppression to the signal in the richer component. Importantly, Blackman arranged that the same interresponse times were reinforced in each component to ensure that response rates were similar even though reinforcer rates differed between components.

Similar results have been obtained when reinforcer amount, rather than rate, has differed between components. For example, Shettleworth and Nevin (1965) observed greater resistance to extinction in the component with the larger reinforcer (see also Harper 1996; Harper & McLean 1992, Experiment 1; Millenson & de Villiers 1972; Nevin 1974, Experiment 3). In general, the effects of differential reinforcer rate and reinforcer amount on resistance to change are at least ordinally equivalent.

There have been some failures to find differential resistance in multiple VI VI schedules when drugs were used as disruptors (see, e.g., Cohen 1986; Lucki & deLong 1983; but see Egli et al. 1992, and sect. 10.2.1). Likewise, signaled or unsignaled within-component food appears not to have differential disruptive effects (Cohen et al.1993; Nevin 1984; J. B. Smith 1974), and Harper and McLean (1992) failed to find differential effects of within-component changes in reinforcer rate (but see Harper 1996). However, the overwhelmingly most general and reliable result is that asymptotic free-operant response rates in multiple VI VI schedules are more resistant to change in the presence of a signal for relatively frequent or large reinforcers than in the presence of a signal for relatively infrequent or small reinforcers. The convergence of these results across diverse disruptors confirms the utility of resistance to change as a measure of the strength of steady-state discriminated operant behavior.

3.4. Separating response rate and resistance to change

There are several ways to distinguish the effects of reinforcer rate on response rate and resistance to change. One is to arrange schedules of interresponse time reinforcement that produce similar rates of responding despite differences in reinforcer rates, as in the work of Blackman (1968) cited above. Another is to arrange identical schedules in separate components that are followed by different components signaling reinforcer rates that are either richer or leaner. For example, Nevin et al. (1987, Experiment 2) trained pigeons in a four-component procedure where identical VI 100-second schedules were arranged successively on the left- and right-side keys. One side-key component was always followed by a richer VI 20-second component signaled by one color on the center key, and the other side-key component was always followed by a period of nonreinforcement signaled by a different color on the center key. During baseline, response rates were higher in the side-key component that preceded nonreinforcement, an effect termed following-schedule contrast (Williams 1981). Resistance to extinction was greater in the side-key component that had preceded the richer center-key component. These results were replicated systematically and were shown to hold for resistance to satiation and for prefeeding as well as resistance to extinction by Tota-Faucette (1991; see also Nevin 1984).

Nevin et al. (1987) interpreted their resistance data in relation to stimulus-reinforcer relations. Specifically, they suggested that there is a stronger stimulus-reinforcer correlation for the side-key component that reliably preceded a higher rate of reinforcement than for the side-key component that reliably preceded nonreinforcement. Alternatively, one might argue that each side-key component was embedded within a serial compound stimulus, and resistance depended on the reinforcer rate correlated with the compound (for discussion, see McLean et al. 1996). The important result is that the reinforcer rate in the following component produced opposite effects on response rate and resistance to change in otherwise identical components.[1]

Two experiments by Nevin et al. (1990) employed a different method to separate the effects of reinforcer rate on response rate and resistance to change. Experiment 1 arranged a two-component multiple schedule in which key pecking was reinforced according to the same VI schedule in both components. Throughout baseline training, additional reinforcers were provided concurrently and independently of responding by a variable-time (VT) schedule in Component 1, and response rates were consistently lower in that component. However, resistance was greater in Component 1: When performance was disrupted by prefeeding or by extinction, response rate decreased more rapidly in Component 2 and fell below that in Component 1. The lower baseline response rate in Component 1 is consistent with Herrnstein's Relative Law of Effect (Equation 3), because the additional reinforcers increase its denominator. The fact that resistance was greater in Component 1 confirms the independence of baseline response rate and resistance to change and suggests that resistance depends on the total rate of reinforcement arranged in a component.

Similar results were obtained when the added reinforcers were contingent on a specified alternative response in a three-component multiple schedule (Nevin et al. 1990, Experiment 2). Component A arranged 15 reinforcers per hour on the right (target) key and 45 reinforcers per hour on the left (alternative) key of a two-key chamber; Component B arranged 15 reinforcers per hour on the right key and none on the left key; and Component C arranged 60 reinforcers per hour on the right key and none on the left key. Thus, relative right-key reinforcement was 0.25 in Component A and 1.0 in Components B and C. The critical comparisons involve the right key, where responding in Component C should be more resistant than that in Component B on the basis of reinforcer rate for right-key responding. The effects of alternative reinforcement are given by comparing Components A and B, and the effects of total component reinforcer rate are given by comparing Components A and C.

Average response rates and the slopes of functions relating log proportion of baseline to successive sessions of satiation, prefeeding, and extinction are displayed in Figure 3. Baseline response rates on the right key were highest in Component C, next highest in Component B, and lowest in Component A, in keeping with Herrnstein's formulation. However, when responding was disrupted by progressive satiation, prefeeding, or extinction, right-key responding in Component A was consistently more resistant to change than that in Component B and was similar to that in Component C. As was discussed by Nevin et al. (1990) these resistance results are not readily accommodated by Herrnstein's formulation. Most important for present purposes, the similarity of right-key resistance in Components A and

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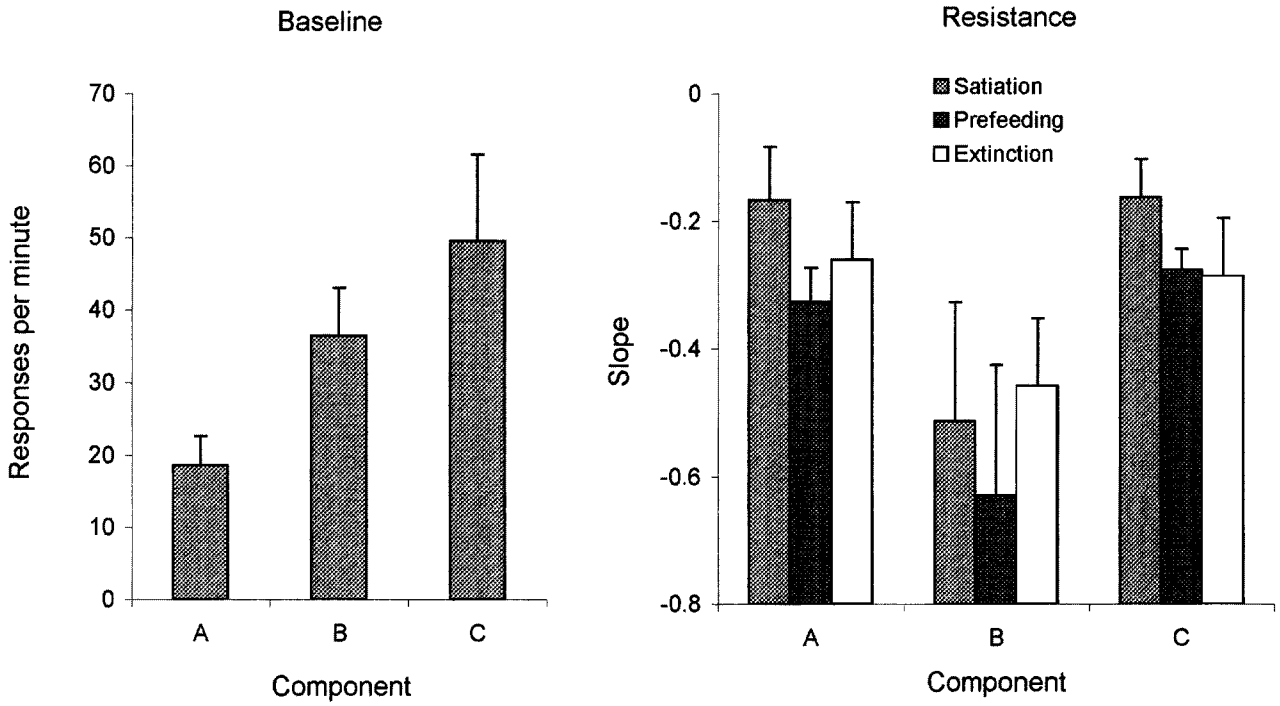


Figure 3. Left panel displays average baseline response rates on the right key in three multiple-schedule components. In Component A, reinforcer rate for right-key responses was 15/hour, with 45 reinforcers per hour available concurrently for left-key responses. In Component B, reinforcer rate for right-key responses was 15/hour, and in Component C it was 60/hour; no reinforcers were given for left-key responses in Components B and C. The right panel displays the slopes of functions characterizing resistance to satiation, resistance to prefeeding, and resistance to extinction in these three components. Standard errors are indicated by the error bars. Adapted from Nevin et al. (1990).

C, despite large differences in baseline response rates, again demonstrates the independence of these aspects of behavior. That result, and the greater right-key resistance in Component A than in Component B despite the same reinforcer rates for that response, again suggests that resistance depends on total reinforcement in the presence of a component stimulus.

These two experiments by Nevin et al. (1990) show that, although baseline response rate depends on relative reinforcement for the target response according to Herrnstein's Relative Law of Effect (Equation 3), resistance to change in a given component was independent of baseline response rate and depended directly on the total rate of food reinforcers obtained in that component, regardless whether the reinforcers were contingent on the target response, independent of that response, or contingent on an alternative response. All in all, the results reviewed in this section suggest that resistance to change depends on Pavlovian, stimulus-reinforcer relations.

3.5. Generality of Pavlovian determination

Key pecking by pigeons is notorious for its susceptibility to the Pavlovian relation between a key light and food. For example, pigeons will peck a key that signals food even if pecking cancels food presentations (see, e.g., Williams & Williams, 1969, for review; also see Schwartz & Gamzu 1977). This is an instance of biological preparedness (Seligman 1970). Virtually all of the research cited above has used pigeons as subjects, pecking at a lighted key as the response, and food as the reinforcer. Therefore, it is important that the results of Nevin et al. (1990) have also been obtained with other stimuli, responses, and species.

Mace et al. (1990) replicated Experiment 1 of Nevin et al. (1990) with retarded adults engaged in a sorting task, when performance was disrupted by turning on a television set. The results were strikingly similar to the pigeon data. Cohen (1996) also replicated Experiment 1 with college students engaged in a typing task, when performance was disrupted by providing a puzzle book, and again the results were similar to the pigeon data. Harper (1999) replicated Experiment 1 with rats, using separate levers to define the responses in the two components, and obtained similar results. Mauro and Mace (1996) replicated Experiment 2 with rats, and obtained similar results when they used visual (but not auditory) stimuli to define the three components. All in all, the effects of stimulus-reinforcer relations on resistance to change have considerable generality across stimuli, responses, and species.

To summarize, we have argued that resistance to change measures the strength of responding in a stimulus situation. The results presented above show that resistance is positively related to the total rate of reinforcement signaled by a stimulus and is independent of the asymptotic rate of responding in the presence of that stimulus. Asymptotic response rate, by contrast, depends on relative reinforcement of the response according to Herrnstein's Relative Law of Effect. Therefore, response rate and resistance to change are separate aspects of discriminated operant behavior: Response rate depends on response-reinforcer relations, whereas resistance to change depends on stimulus-reinforcer relations.

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4. Quantifying the determiners of resistance to change

We now consider a quantitative model characterizing resistance to change as a function of stimulus-reinforcer relations.

4.1. The Pavlovian contingency ratio

There are several ways of quantifying a Pavlovian contingency between stimuli and reinforcers (Gibbon et al. 1974). A simple, intuitively reasonable, and empirically useful way is to compute the ratio of the reinforcer rate in the presence of a stimulus to the overall average reinforcer rate in both the presence and the absence of the stimulus (Gibbon 1981). Intuitively speaking, this ratio measures the informativeness of the stimulus with respect to reinforcement. For example, if the reinforcer rate in the presence of a particular stimulus is identical to the overall average rate of reinforcement, the ratio is 1.0 and the stimulus is not informative. If the ratio is greater than 1.0, onset of the stimulus predicts an increase in the average rate of reinforcement, and, if it is less than 1.0, onset of the stimulus predicts a decrease.

Stimulus-reinforcer contingency ratios (CRs) for the two components of a standard multiple schedule may be expressed as

$$\mathrm{CR1} = r_{C1}/r_S \tag{4a}$$

and

$$\mathrm{CR2} = r_{C2}/r_S, \tag{4b}$$

where r represents the rate of reinforcement subscripted for the components C_1 and C_2 and for the overall session S. Even though r_{C1}, r_{C2}, and the intercomponent interval may vary from one experimental condition to another, r_S is the same for both components within each condition, so the relative contingency ratio for the two components reduces to r_{C1}/r_{C2}. Therefore, if the contingency ratio is an effective specification of the stimulus–reinforcer relation, relative resistance to change should vary with the relative contingency ratio, and, equally important, it should be unaffected by any variable that changes only r_S.

An experiment by Nevin (1992a) confirmed these expectations. Nevin arranged multiple VI VI schedules with 60 reinforcers per hour in a constant component and either 300 or 10 reinforcers per hour in the alternated component. In Experiment 1 the intercomponent interval was 2 seconds, and in Experiment 2 it was 2 minutes. Figure 4 shows that average relative resistance to change, calculated as the inverse ratio of the slopes of resistance functions for prefeeding and extinction, depends similarly on the relative contingency ratio for both resistance tests in both experiments. This similarity holds despite substantial differences in average baseline response rates produced by the intercomponent interval. We conclude that relative resistance to change is independent of the overall context of reinforcement as determined by the intercomponent interval.

4.2. Relative resistance to change as a function of relative Pavlovian contingencies

Nevin (1992b) reviewed all of the two-component multiple-schedule data collected in his laboratory since 1965 and related relative resistance to change to the relative contingency ratio, as shown in Figure 5. For experiments that varied reinforcer duration rather than reinforcer rate between components, the contingency ratio is expressed as the duration ratio. Across experiments, or across different conditions within experiments, the overall rate of reinforcement (r_S) varied substantially, and the the resistance tests employed time-out food, prefeeding, and extinction. There is no evidence that relative resistance was systematically affected either by r_S or by the testing method. Although there is a good deal of variation from one experiment to another, the overall trend of the data is adequately described by a linear function with a slope of approximately 0.35, which is quite similar to the slopes of the two-point functions shown in Figure 4. (Note: The example portrayed in Figure 2 appears as a single point, numbered 9, at $x = -0.48$, $y = -0.26$. The prefeeding and extinction data shown in Figure 3 appear as points numbered 5 and 6. The data shown in Figure 4 appear as points numbered 7 and 8.) To a first approximation, then, relative response strength, construed as relative resistance to change and measured as the reciprocal of the ratio of the slopes of resistance functions, is a power function of the ratio of reinforcer rates or durations experienced in the two components of a multiple schedule:

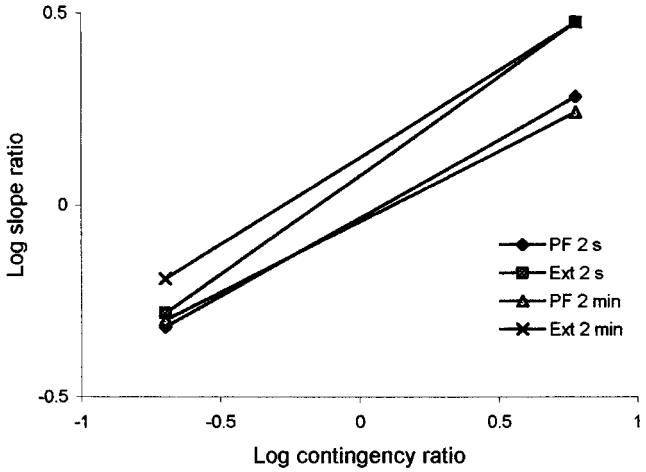


Figure 4. Logarithms of ratios of the slopes of functions relating response rates in the components of a multiple VI VI schedule to sessions of prefeeding (PF) or extinction (Ext), as functions of the logarithm of the ratio of contingency ratios or, equivalently, the log ratio of reinforcer rates in those components. Results are shown separately for conditions with 2-second and 2-minute time-outs between components. Adapted from Nevin (1992a).

$$m_{r1}/m_{r2} = [(r_{C1})/(r_{C2})]^b, \tag{5}$$

where m_{r1} and m_{r2} represent resistance to change and r_{C1} and r_{C2} represent reinforcer rates or amounts in Components 1 and 2, respectively, and b is a parameter reflecting the sensitivity of resistance ratios to reinforcer ratios. As we show below, preference between two schedules may be described by a similar function.

5. Preference and reinforcement value in concurrent-chains schedules

Nevin (1979) pointed out that there were a number of ordinal agreements between resistance to change and preference in the literature: Variables that increased resistance also increased preference relative to a constant alternative. We now describe steady-state research on preference in a way that parallels our discussion of resistance to change.

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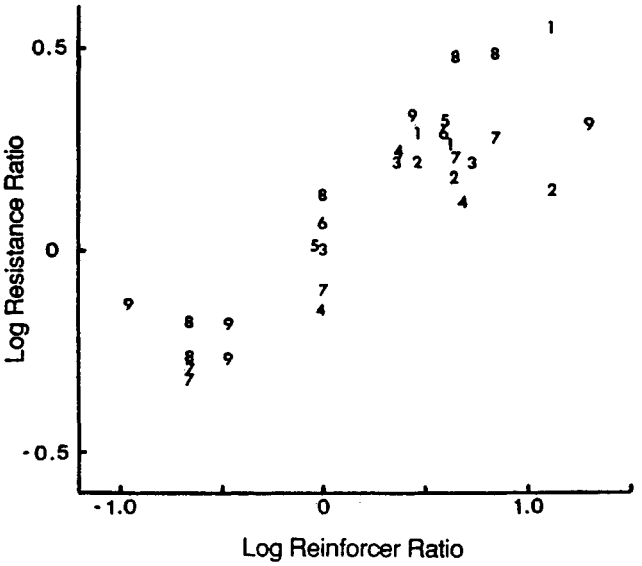


Figure 5. Logarithms of ratios of slopes of functions characterizing resistance to change in two-component multiple VI VI schedules that differ in reinforcer rate or amount are related to logarithms of the reinforcer ratio. The data points are averages across subjects for separate experimental conditions and methods for evaluating resistance to change, coded as follows: Nevin et al. (1983): 1, time-out food; 2, extinction. Nevin et al. (1990): 3, Experiment 1, prefeeding; 4, Experiment 1, extinction; 5, Experiment 2, prefeeding; 6, Experiment 2, extinction. Nevin (1992a): 7, prefeeding; 8, extinction. Points numbered 9 represent single conditions from Shettleworth and Nevin (1965), extinction; Nevin (1974), Experiment 1, time-out food; Nevin (1974), Experiment 2, extinction; Nevin (1974), Experiment 3, time-out food; and Nevin (1988), extinction. From Nevin (1992b).

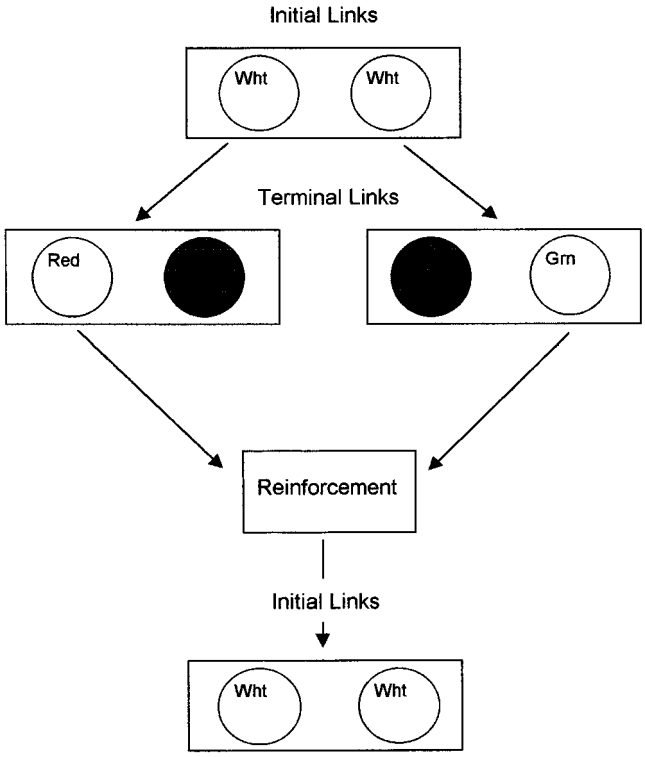


Figure 6. Schematic diagram of a typical concurrent-chains procedure. In the initial links, both keys are lighted white, and responding occasionally produces entry, according to equal concurrent VI schedules, into one of two mutually exclusive terminal links signaled by red or green. Responding in the terminal links produces reinforcement, after which the initial links are reinstated. The ratio of initial-link responses is taken as a measure of preference between the terminal-link discriminated operants.

5.1. The concurrent-chains paradigm

Preference has been studied extensively in a paradigm known as concurrent-chain schedules that is closely related to the multiple-schedule paradigm for evaluation of relative response strength. The basic concurrent-chain schedule paradigm is diagrammed in Figure 6. In a standard experiment, a pigeon is confronted with a pair of illuminated response keys where pecks on one key are followed by access to one signaled food-reinforcement schedule (C_1) according to a VI schedule and pecks to the other key are followed by access to a second signaled schedule (C_2) according to a separate VI schedule, where C_1 and C_2 are mutually exclusive and occur successively as in multiple schedules. The choice phase of the experiment, when both keys are lighted, defines the initial links of two chains, and the multiple-schedule phase, when only one or the other key is lighted and food is available, defines their terminal links. If the initial-link schedules are the same, the allocation of responding between keys during the initial-link choice phase provides a direct measure of preference for the terminal-link, multiple-schedule components. If VI schedules are used in the initial links, subjects rarely respond exclusively to one or the other, and preference is continuously related to variations in the terminal links.

Concurrent-chain schedules separate preference for a schedule from the response rate controlled by that schedule, thus avoiding a difficulty with concurrent schedules. When qualitatively different schedules are defined for two concurrent operants, preference is confounded with the response rates shaped by the different contingencies of reinforcement arranged by the two schedules. For example, variable-ratio (VR) schedules usually maintain much higher response rates than VI schedules, and the allocation of responding in concurrent VR VI may reflect the shaping effects of the different schedule contingencies as well as the values of the schedules (see, e.g., Herrnstein & Heyman 1979).

5.2. Some representative results

Autor (1960/1969) arranged identical initial links and varied the reinforcer rates in the terminal links using VI, VR, and VI DRO schedules (where VI DRO signifies that food was presented at variable intervals if the subject refrained from responding) in three separate experiments. He found that the relative rate of responding to one initial link approximately matched the relative rate of reinforcement provided by its terminal link, regardless of the terminal-link contingencies or response rates. Subsequent research has confirmed these conclusions: Herrnstein (1964a) repeated Autor's results with VI and VR terminal links arranged within conditions, rather than between experiments, and Neuringer (1969) showed that pigeons were indifferent between terminal links that arranged response-contingent and response-independent reinforcement after the same delay, even though the pigeons rarely responded when reinforce-

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ment was independent of responding. Neuringer (1967) varied reinforcer amount in the terminal links and found that preference was directly related to amount even though there were no effects on terminal-link response rates. Thus, preference evidently does not depend on response–reinforcer contingencies or response rates and depends directly on relative reinforcer rate or amount. In these respects, preference in concurrent chains is functionally similar to resistance to change in multiple schedules.

6. Quantifying the determiners of preference in concurrent chains

Several models of preference in concurrent chains have been proposed since Autor's initial research; here, we consider two that are relevant to the model of resistance to change summarized above.

6.1. Effects of the context of reinforcement

Although both Autor (1960/1969) and Herrnstein (1964a) observed approximate matching between relative response rates in the initial links of concurrent chains and relative terminal-link reinforcer rates, this matching result proved to be fortuitous when Fantino (1969) demonstrated that measured preference depended on the lengths of the identical initial links as well as the relative rates of food reinforcement in the terminal links. Fantino obtained matching with intermediate-length initial links, but preference approached indifference as the initial links were lengthened and approached exclusive preference for the richer terminal link as initial links were shortened. Thus, matching appeared to be just one of a continuum of possible results.

6.2. Delay reduction theory

To account for these and related results, Fantino (see, e.g., Fantino 1977) proposed that the value of a terminal link depended on the relative reduction in delay to food signaled by entry into that terminal link. More formally, Fantino's delay-reduction theory asserts that

$$B_1/B_2 = (T - t_1)/(T - t_2), \qquad (6)$$

where T is the overall average time from onset of the initial links to the delivery of a food reinforcer, t_1 and t_2 are the delays to food reinforcement in the terminal links, and B_1 and B_2 are the numbers of choice responses to the two keys in the initial links. The formulation is intuitively plausible: Signaled delays of 30 seconds and 1 minute differ by rather little, relative to an overall delay lasting 1 hour, but differ by a great deal relative to an overall delay of 2 minutes. Indeed, Fantino's delay-reduction theory predicts exclusive preference for the shorter signaled delay to food when the length of the overall average delay is less than the longer signaled delay.

Fantino's account of preference in relation to delay reduction has some properties in common with the contingency-ratio account of resistance to change presented above. Note that T, the overall average time to reinforcement in Equation 6, is the same as $1/r_S$, the average rate of reinforcement in Equation 2; likewise, t_1 and t_2 are the same as $1/r_{C1}$ and $1/r_{C2}$. Delay reduction theory suggests that the attractiveness or value of the terminal-link schedule in C_1 (for example) is an increasing function of the difference between $1/r_S$ and $1/r_{C1}$, whereas Nevin's account of response strength suggests that resistance to change in multiple-schedule component C_1 is an increasing function of the ratio of r_{C1} to r_S. However, both accounts embody the same intuition: The strength of responding in a multiple-schedule component and the value of access to a terminal-link schedule both depend on a comparison of component reinforcer rate (or terminal-link delay) with the overall average reinforcer rate (or delay) for the context in which the schedule appears.

Despite this similarity, there may be an important difference. Figure 4 suggests that relative response strength in C_1 and C_2 is roughly invariant with respect to the length of time-out periods between components, which influence r_S. If the initial-link choice periods that precede access to the terminal links in concurrent chains are functionally equivalent to the time-out periods that precede multiple-schedule components, initial-link length should also have no effect on preference. However, according to delay reduction theory and as shown by Fantino (1969), preference for the richer terminal link in concurrent chains varies inversely with the length of initial-link choice periods, which influence $1/T$. If resistance and preference are similarly determined, this difference must be resolved.

6.3. The contextual choice model

Grace (1994) has recently proposed a comprehensive account of performance in concurrent-chain schedules that assumes terminal-link values to be independent of the context of initial-link lengths within which they appear. Simply put, Grace's acccount assumes that terminal-link value depends only on the signaled delays to reinforcement, but the behavioral expression of relative value as preference in the initial links depends on the ratio of terminal-link to initial-link duration. The model is:

$$B_{i1}/B_{i2} = b(r_{t1}/r_{t2})^{a1}[(1/d_{t1}/1/d_{t2})^{a2}(x_{t1}/x_{t2})^{a3}]^{Tt/Ti}, \qquad (7)$$

where B_{i1} and B_{i2} represent initial-link response rates, r_{t1} and r_{t2} are the rates of terminal-link entries, and d_{t1} and d_{t2} are the delays to reinforcement in the terminal links. The parameters b, a_1, and a_2 represent response bias, sensitivity to number of entries, and sensitivity to delay, respectively. Other variables that influence preference, such as reinforcer amount, are represented by x_{t1} and x_{t2}, where a_3 is sensitivity to those variables. The exponent T_t/T_i is the ratio of average terminal-link duration to average initial-link duration, which accounts for the effects of initial-link length reported by Fantino (1969).

Unlike Fantino's delay-reduction theory, Grace's formulation has a number of free parameters; but, with the assumption that terminal-link value depends only on the delays to reinforcement, it provides an excellent descriptive summary of the results of a wide variety of concurrent-chain schedule experiments. Moreover, Grace (1996) has shown that estimates of value are consistent between the standard concurrent-chains procedure and Mazur's (1987) adjusting-delay procedure for determining indifference between two signaled conditions of reinforcement. The agreement between two different choice paradigms in their estimation of terminal-link value argues strongly for the identification of initial-link preference with the construct of reinforcement value. The contextual choice model also accords with Nevin's (1992a) results presented in Figure 4 in

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that relative terminal-link value in concurrent chains, as with relative resistance to change in multiple schedules, is independent of the overall context of reinforcement.

7. Linking resistance and preference through the metaphor of behavioral momentum

The functional similarity of resistance to change and preference may be understood within the metaphor of behavioral momentum, which was characterized briefly in section 1. Here, we explain it more fully.

7.1. Behavioral momentum and inertial mass

In classical mechanics, momentum is the product of the velocity and mass of a moving body. Momentum cannot be ascertained by observing the steady-state velocity of a body unless its mass is known. If its mass is unknown, it is necessary to impose a known external force, observe the change in velocity, and then calculate mass from Newton's second law:

$$\Delta v = f/m, \tag{8}$$

which states that the change in velocity is directly proportional to the imposed force and inversely proportional to the mass of the body.

Nevin et al. (1983) suggested that behavior can be treated similarly. Asymptotic response rate under baseline training conditions is a behavioral analog to velocity under constant conditions, and the change in that response rate when responding is disrupted by altering those conditions in a way that is analogous to an external force allows us to estimate a behavioral analog to inertial mass: the smaller the decrease in response rate, the greater the behavioral mass. Just as velocity and mass are independent dimensions of a moving body, so response rate and resistance to change are independent dimensions of behavior, determined primarily by response-reinforcer and stimulus-reinforcer relations, respectively.[2]

7.2. Some measurement issues

In physical science, the application of universal measurement units ensures dimensional consistency and comparability in measuring momentum across different external forces. In the science of behavior, however, there is no obvious system of units that can be applied to different disruptors. The change in response rate is dimensionless if postdisruption response rate is expressed relative to its baseline. Therefore, if the disruptor consists of imposed electric-shock punishment, the mass-like aspect of behavior must be expressed in units of electric shock to make Equation 8 dimensionally consistent; but, if the disruptor consists of prefeeding, it must be expressed in units of food. Moreover, any attempt to write an equation relating behavioral mass to the contingency ratio, which is dimensionless, must introduce a scaling constant having units of the disruptor.

Both of these problems can be resolved by imposing the same disruptor, x, on two independently measured ongoing response rates. Then

$$\Delta v_1 = x/m_1 \quad \Delta v_2 = x/m_2,$$

and, thus,

$$m_1/m_2 = \Delta v_2/\Delta v_1, \tag{9}$$

where the change in velocity (response rate) is measured as log proportion of baseline. Equation 9, which is dimensionless, provides a measure of relative rather than absolute behavioral mass. The two-component multiple schedule, which permits within-subject, within-session comparison of the resistance to change established by two different reinforcement conditions, is ideally suited for relative measurement of this sort.

7.3. Convergent measurement of relative behavioral mass

In physics, the inertial mass of a body, which is determined by imposing an external force and measuring the change in motion, is equal to the gravitational mass of that body, which may be determined independently by its force of attraction to another body of known mass at a known distance (e.g., its weight at the earth's surface). Newton's law of gravitation describes the relation:

$$a = (m_1 \cdot m_2)/d^2, \tag{10}$$

where a is the force of attraction, m_1 and m_2 are the masses of the two bodies, and d is the distance separating their centers. To determine the relative gravitational masses of two bodies with masses m_1 and m_2, it is sufficent to measure their relative attractiveness to a third body, equidistant from both, with constant (but unknown) mass m_3:

$$a_1 = (m_1 \cdot m_3)/d^2 \quad \text{and} \quad a_2 = (m_2 \cdot m_3)/d^2.$$

Thus,

$$m_1/m_2 = a_1/a_2. \tag{11}$$

The metaphorical connotations of "attraction" suggest that the behavioral equivalent of relative gravitational mass may be measured by the number of responses that bring the subject into contact with one or the other of two multiple-schedule components that are equidistant from choice – that is, preference in concurrent-chain schedules with equal initial links. Preference, transformed via Grace's (1994) contextual choice model (Equation 7), may be construed as an estimate of the relative reinforcement value of those components. If behavioral mass is similar to physical mass, the relative inertial mass of a discriminated operant estimated from resistance to change and its relative gravitational mass estimated from preference should be related by a simple function, perhaps even by identity. The schematic diagram in Figure 7 summarizes the relations between these terms.

8. A relation between response strength and reinforcement value

The convergence of strength and value suggested by the momentum metaphor is supported by quantitative relations derived from previous research, as indicated in Figure 7, and by recent experimental evidence.

8.1. Power-function relations linking relative strength, value, and reinforcement

The relative-value kernel of Grace's (1994) model is:

$$v_1/v_2 = [(1/d_1)/(1/d_2)]^a, \tag{12}$$

which is derived from the full model (Equation 7) by neglecting response bias and assuming that average terminal-

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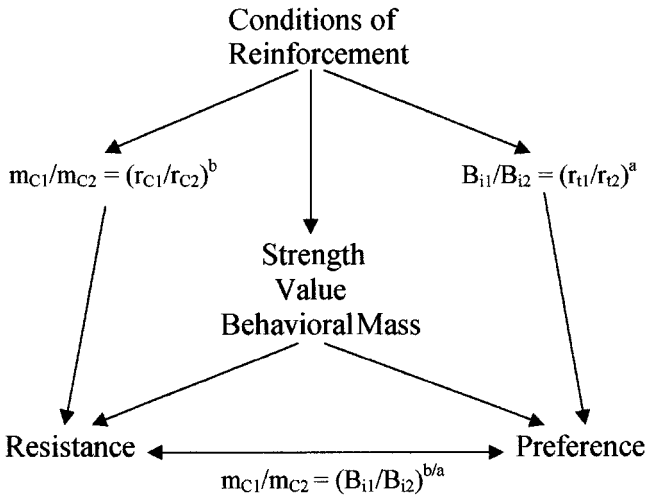


Figure 7. Summary of the relations between the conditions of reinforcement for two discriminated operants, their resistance to change (left branch) or preference between them (right branch), and the structural relation linking resistance and preference (bottom). Both resistance and preference are construed as expressions of a single central construct reflecting their strength, value, or behavioral mass.

link (T_t) and initial-link (T_i) durations are kept constant while relative terminal-link delay is varied and that the rates of terminal-link entries (r) and other variables (x) that affect preference are equated between alternatives. That is, the relative value of a signaled schedule of reinforcement is a power function of the relative reciprocal of delay (equivalently, relative immediacy of reinforcement or average reinforcer rate) in the terminal links of concurrent chains. Equation 12 is closely related to Equation 5 for relative resistance to change, which we repeat for convenience:

$$m_{r1}/m_{r2} = [(r_{C1})/(r_{C2})]^b, \quad (13)$$

where the exponent b seems not to depend on the duration of intercomponent intervals within which C_1 and C_2 are set (see Fig. 4 above). Likewise, Grace's account of relative value performs well if his exponent a is assumed not to depend on the duration of the initial links that precede the terminal links. If relative schedule value (preference) is a power function of the ratio of reinforcer rates arranged in the terminal links of concurrent chains, and relative response strength (resistance to change) is also a power function of the ratio of reinforcer rates in the components of multiple schedules, then the relation between relative response strength and relative reinforcement value must also be a power function:

$$m_{r1}/m_{r2} = (v_1/v_2)^{b/a}. \quad (14)$$

8.2. An experimental confirmation

Grace and Nevin (1997) devised a method for examining the power-law prediction directly by evaluating preference in concurrent chains in one portion of an experimental session and evaluating resistance to change in multiple schedules in the other portion. Specifically, in the concurrent-chains portion, two side keys were lighted white during the initial links, and pecks at one or the other side key gave access to its corresponding terminal link, signaled by lighting the center key red or green. In the multiple-schedule portion, the center key was lighted red or green after an intercomponent time-out, and the component schedules were identical to the concurrent-chains terminal links. After performance stabilized in both portions of the procedure, resistance to change was evaluated by presenting response-independent food during the time-out between components in the multiple-schedule portion of the session. Because only one time-out food rate was employed, we used a variation of the slope ratio to estimate relative resistance to change: $\log[(B_{x1}/B_{O1})/(B_{x2}/B_{O2})]$, where B refers to response rate subscripted for Component 1 or 2 and for time-out food (X) and baseline (O). If time-out food produces greater decreases in response rate in Component 2 than in Component 1, relative to their respective baselines, log relative resistance is positive, and, if it produces smaller decreases in Component 2 than in Component 1, log relative resistance is negative (see Appendix in Grace & Nevin, 1997, for discussion of this measure).

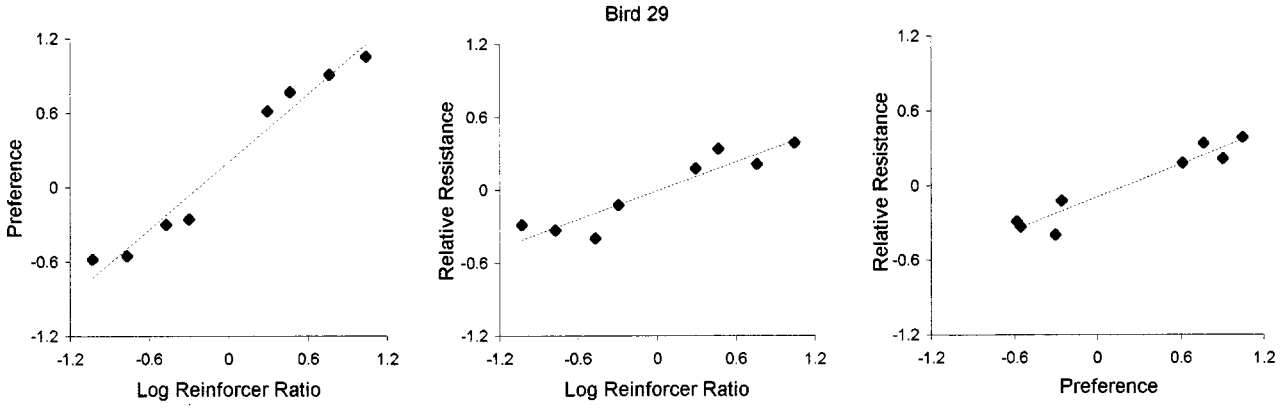


Figure 8. Left panel shows the relation between preference in concurrent chains, measured as the logarithm of the ratio of initial-link responses and the logarithm of the ratio of terminal-link reinforcer immediacy, for an individual pigeon. The center panel shows the relation between resistance to change in multiple-schedule components that were identical to the terminal links, measured as the logarithm of the ratio of response rate ratios with time-out food presentations to baseline and the logarithm of the ratio of multiple-schedule reinforcer immediacy. The right panel shows the structural relation between resistance to change and preference. Adapted from Grace and Nevin (1997).

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Preference and resistance to change were evaluated for five consecutive sessions in eight conditions, each of which arranged different pairs of variable delays whose sum was constant. Representative data for one pigeon (bird 29) are shown in the three panels of Figure 8. The left panel shows the relation between preference (the log ratio of initial-link response rates) and the log ratio of the relative immediacy of food in the terminal links. The center panel shows the relation between relative resistance to change (calculated as described above) and the log ratio of the relative immediacy of food in the multiple-schedule components, which is the same as in the terminal links. The right panel shows the structural relation between our two independently measured dependent variables, relative resistance and preference. That relation is a quantitative expression of the covariation of strength and value when the relative immediacy of reinforcement is varied.

It is important to observe that deviations from linearity in the left and center panels are correlated: pooled across all four of our subjects, the correlation is +0.52 ($P < .003$). The fact that deviations are correlated suggests that both preference and relative resistance are related to a common factor that is largely, but not completely, determined by the ratio of experimentally arranged reinforcer rates. Whatever its additional determiners, which may vary between individuals and experimental conditions, that common factor represents the relative behavioral mass of the two operants defined by the terminal links or multiple-schedule components.

8.3. Other preference-resistance relations

It is not surprising that the effects of reinforcer rate on resistance to change and preference are correlated within subjects, because both aspects of behavior have been shown to depend similarly on reinforcer rate in independent experiments. The same holds for reinforcer amount. However, some less obvious aspects of the conditions of reinforcement also have correlated effects on preference and resistance to change.

For example, Grace et al. (1998) degraded response-reinforcer contiguity by arranging a brief unsignaled delay before reinforcement in one terminal link of standard concurrent chains. All pigeons preferred the alternative terminal link, which arranged immediate reinforcement, even though the rates of reinforcement were about the same. In a separate multiple-schedule condition, the authors observed greater resistance to change in a component with immediate reinforcement. Moreover, the degree of preference covaried, across subjects, with the degree of differential resistance. Bell (1999) also found that resistance to change in a multiple-schedule component with immediate reinforcement was greater than in a second component with a brief unsignaled delay superimposed on the same VI schedule. In addition, Bell conducted choice probe tests in extinction and observed greater responding to the stimulus correlated with immediate reinforcement than to the stimulus correlated with unsignaled delayed reinforcement.

Signaling the delay to reinforcement may also have similar effects on preference and resistance. For example, with pigeons as subjects, Marcattilio and Richards (1981) reported preferences for a terminal link with a signaled delay over an otherwise identical terminal link with an unsignaled delay. Relatedly, Roberts et al. (1984) examined response rate, resistance to prefeeding, and resistance to extinction in a between-group study with rats as subjects, where one group received a brief signal before each reinforcer and the other received the same reinforcer delay but with signals presented randomly. Although baseline response rates were higher for the group with random signals, resistance to change was greater for the group with signaled delay.

Contingencies on response rate may also affect preference and resistance. Several experiments have shown that contingencies establishing low rates of responding generate greater resistance to disruption than high-rate contingencies when overall reinforcer rates are equated between multiple-schedule components (see, e.g., Blackman 1968; Lattal 1989; Nevin 1974, Experiment 5; but see also Fath et al. 1983). In concurrent-chains experiments, Fantino (1968) and Nevin (1979) found preference for low-rate over high-rate contingencies, and Nevin (1979) found that preference was greatest for the same birds that had most clearly shown an effect of low-rate versus high-rate contingencies on resistance to change in his earlier Experiment 5 (Nevin 1974).

These examples show that both resistance to change and preference may sometimes be affected by variables other than stimulus-reinforcer relations, but the effects are correlated. Although such findings challenge a purely Pavlovian account, they provide additional evidence that resistance to change and preference are independent measures of the strength, value, or behavioral mass of a discriminated operant.

An apparent exception to the agreement between resistance and preference arises when a fixed-interval (FI) schedule is compared to a VI schedule with the same arithmetic mean interval. Many studies (see, e.g., Herrnstein 1964b; Killeen 1968) have reported strong preferences for the VI schedule. Mandell (1980) confirmed this preference but found no difference in resistance between VI and FI schedules in the terminal links of chained VI VI and VI FI schedules. Mellon and Shull (1986) repeated part of her study and obtained modest evidence of greater resistance in the VI terminal links, but Mandell's failure to confirm the usual agreement between resistance and preference within her experiment remains to be explained. One source of interpretive difficulty is that FI performance is typically biphasic, consisting of an initial pause followed by rapid responding. Thus, changes in average response rate during resistance tests may not be a fair measure of the resistance of FI responding. In general, it may prove difficult to compare resistance to change between performances differing in temporal pattern or topography of responding, and apparent failures of agreement with preference may arise for this reason.

9. An augmented model of resistance to extinction

Our formulation of behavioral mass as a single construct expressed separately in resistance and preference is seriously challenged by any systematic dissociation between these aspects of behavior. The well-known and much-debated partial reinforcement extinction effect (PREE) presents a major challenge of this sort.

9.1. Preference, resistance, and the PREE

D'Amato et al. (1958) and several subsequent researchers (see, e.g., vom Saal 1972) have shown that animals respond

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more to a stimulus correlated with continuous reinforcement (CRF) than to one correlated with partial or intermittent reinforcement (PRF). However, responding is less resistant to extinction after CRF than after PRF in a wide variety of procedures (see Mackintosh 1974, for review). Thus, preference and resistance to extinction are related to the training schedule in opposite directions.

In addition, the PREE is a major exception to the general finding that resistance to change in multiple VI VI schedules, including resistance to extinction, depends directly on the rate of reinforcement. Clearly, the rate of reinforcement is greater when every response is reinforced (CRF) than when only some proportion of those responses is reinforced (PRF). Thus, the PREE is a major exception to the claim that resistance to any sort of change depends directly on rate of reinforcement and that it is correlated with preference.

9.2. Extinction and generalization decrement

When reinforcement is terminated after extensive training, there are two separable aspects of the transition to extinction that must be distinguished. First, reinforcers are no longer contingent on responding, and, second, the overall stimulus situation changes because reinforcers no longer occur. These effects are separable: Response rate decreases when the contingency is removed even though reinforcers are presented independently of responding (see, e.g., Rescorla & Skucy 1969), and response rate decreases when there is a change in the stimulus situation, at least temporarily, even though reinforcers may still be presented (see, e.g., Ferster & Skinner 1957, p. 78). The latter effect is known as generalization decrement, which has been invoked frequently to explain the PREE: Reinforcers, considered as stimuli, are part of the stimulus situation in which training occurs, and, when extinction begins, there is a smaller change in the overall stimulus situation after PRF than after CRF because the average reinforcer rate is lower.

9.3. Modeling generalization decrement during extinction

We suggest that CRF establishes greater behavioral mass than PRF, consistent with all the research on the rate of reinforcement reviewed above, but that the transition to extinction may decrease responding more rapidly after CRF than after PRF because of the greater generalization decrement. In terms of the momentum metaphor, the disruptive force of extinction must include both the suspension of the contingency and the decremental effect of situation change. We now consider a way to model these two forces during extinction by augmenting our basic model of resistance to change.

The basic model is:

$$\log(B_x/B_o) = -x/m, \tag{15}$$

where $\log(B_x/B_o)$ is the change in responding during disruption relative to baseline, x is the value of the disruptor, and m is behavioral mass. Grace and Nevin (1997) suggested that, for a given schedule component, m depends on reinforcer rate according to a power function, which is consistent with previous results for relative resistance to change (Fig. 5) and with Grace's (1994) model of preference (see sect. 6.3). Thus,

$$\log(B_x/B_o) = -x/r^a, \tag{16}$$

where r is reinforcer rate during training and a is the exponent of the function relating m to r. To capture the effects of suspending the reinforcement contingency and changing the situation by omitting reinforcers, the disruptor x, representing time in extinction, is multiplied by the additive combination of terms representing these separate factors:

$$\log(B_x/B_o) = -x(c + dr)/r^a, \tag{17}$$

where c represents the decremental effect of suspending the contingency and d represents the decremental effect of situation change arising from terminating reinforcer rate r. Thus, the force-like term in the basic momentum model is augmented by an additive term for the effectiveness of situation change in extinction (dr). The units of c and d must be such that the right side of Equation 17 is dimensionless.

The effects of disruptors that do not involve termination of reinforcement, such as deprivation change, can be captured by Equation 16 with the addition of a parameter f that scales the effectiveness of deprivation change in units that retain dimensional consistency:

$$\log(B_x/B_o) = -xf/r^a. \tag{18}$$

To illustrate the application of the model set forth in Equations 17 and 18, we estimated their parameters by fits to the average slope data of Experiment 2 of Nevin et al. (1990; see Fig. 3 of this target article). The parameter c in Equation 17 was set at 1.0, so that f_p (for prefeeding) and f_s (for satiation) express the effectiveness of those disruptors relative to the effect of suspending the contingency. Estimated parameter values are $f_s = 1.03$, suggesting that satiation was about as effective as suspending the contingency; $f_p = 1.45$, suggesting that prefeeding was about one-half again as effective; $a = 0.35$; and $d = 0.001$. The relation between obtained and predicted slopes is shown in the left panel of Figure 9. The model accounts for 91% of the data variance, and predicted values are usually within the range of the standard error of the data.[3]

These data do not constitute a good test of the model because there were only two reinforcer rates in the three schedule components, and four free parameters were estimated from nine slopes. Larger data sets would provide a more stringent test of the model. Nevertheless, the model predicts the effects of wider variations in reinforcer rates. The right panel of Figure 9 shows the predicted slope of the extinction curve when the reinforcer rate during training is varied from 10 to 5,000 per hour (the latter value may be unrealistically high, but reinforcer rates obtained on CRF, corrected for eating time, have sometimes exceeded 4,000/hour in our laboratory). Predictions were derived from Equation 17 using the parameter values estimated for a and d from the data of Nevin et al. (1990). Note that the slope becomes shallower as the reinforcer rate increases up to about 500 reinforcers per hour, and then becomes steeper as the reinforcer rate increases further. Thus, even though behavioral mass is a continuous positive function of reinforcer rate, resistance to extinction is predicted to be lower after training with high reinforcer rates under CRF than after training with the somewhat lower rates characteristic of PRF.

We have suggested above that behavioral mass is also measured by preference, which is directly related to reinforcer rate. Thus, the apparent dissociation between preference and resistance to extinction after training with CRF as opposed to PRF is resolved if this model is accepted.

https://doi.org/10.1017/S0140525X00242404 Published online by Cambridge University Press

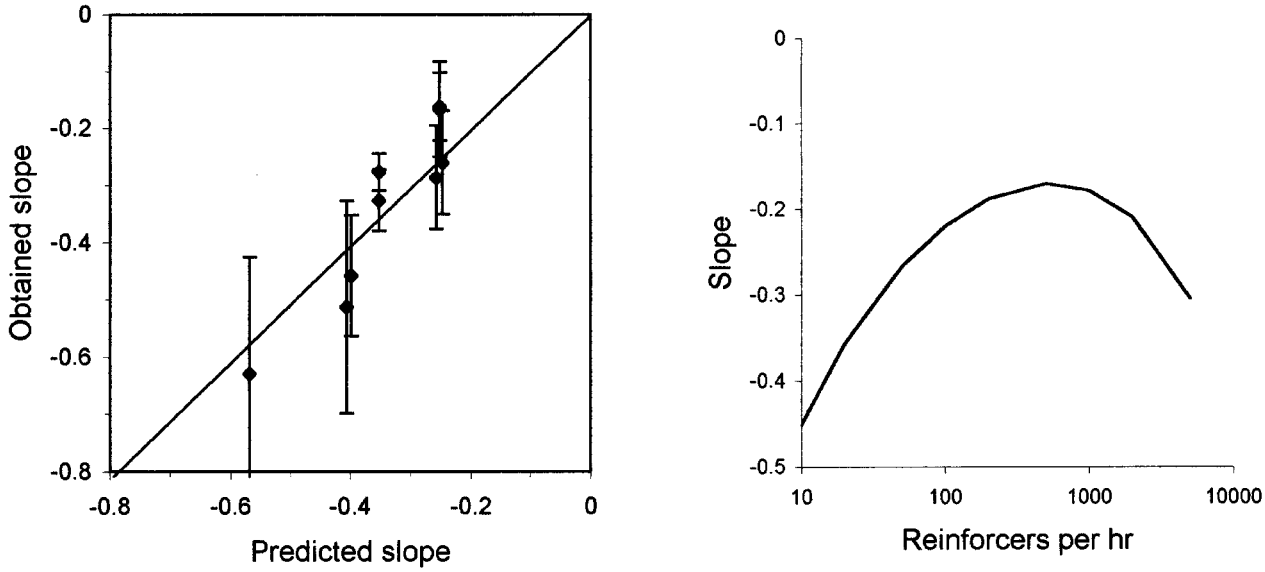


Figure 9. Left panel relates the average slopes of resistance functions obtained by Nevin et al. (1990; see Fig. 3 of target article) to the predictions of Equations 17 and 18. Error bars show the standard errors of the mean obtained slopes. The right panel shows the slope of the extinction curve predicted by Equation 17 as a function of reinforcer rate during training, with parameters a and d set at the values estimated by fits to the data of Nevin et al. (1990).

Equation 17 cannot be fully correct because the effects of situation change when reinforcement is terminated must decrease as time elapses in extinction, suggesting that the value of d must decay with time. Moreover, free-operant extinction often shows an initial increase in response rate that is sometimes described as a frustrative effect of reinforcer omission, whereas Equation 17 predicts only decreases. Modifying Equation 17, fitting it to the many extant data sets, and determining how its parameters depend on experimental variables such as reinforcer magnitude or length of training are tasks for the future.

10. Applications of behavioral momentum

In addition to guiding basic research and theory, the momentum metaphor may be fruitful in applied work. The next three sections describe some applications of our work to clinical intervention, drug addiction, and self-control.

10.1. Clinical intervention

An important goal of clinical intervention is to establish desirable behavior so that it occurs reliably during therapy and persists effectively when therapy ends – in metaphorical terms, to maximize both its velocity and mass. This goal suggests the use of high rates of contingent reinforcement during therapy, which should maximize both terms.

Many researchers have discussed the persistence of therapeutic gains in the client's natural environment without the reinforcers mediated by the therapist in relation to resistance to extinction, and have recommended partial reinforcement in order to capitalize on the PREE (see, e.g., Nation & Woods 1980). However, many other disruptors that inevitably occur in everyday life, including competition from the undesirable behavior that led to therapy, must also be considered. As summarized in sections 3 and 4, resistance to other disruptors such as distraction or competing behavior increases monotonically with increasing reinforcer rates, and the therapist must consider the relative importance of disruptors other than extinction in designing clinical interventions (see Lerman & Iwata 1996. for a review of extinction in relation to other factors in applied settings).

10.1.1. Effects of adding reinforcers. Biofeedback has been used extensively to help clients manage a variety of health problems, including muscle tension. However, effects established in the clinic have often failed to generalize to everyday life, presumably because of the absence of explicit biofeedback (unless the client acquires the necessary apparatus) as well as situation change. Tota-Faucette (1991) addressed this problem in a study of biofeedback for muscle relaxation with normal children. She arranged two distinctively signaled situations: In one, the children received tones plus points exchangeable for toys for meeting the relaxation criterion; in the second, they also received additional, noncontingent points or toys. During training, the children achieved significant reductions in muscle action potential (EMG) levels and significant increases in the proportion of time spent at or below the relaxation criterion. After 24 30-second trials with each situation, levels of relaxation were similar in both situations. However, when all auditory feedback and points were discontinued in an extinction test, relaxation was substantially more persistent in the situation that had included additional noncontingent reinforcers. This result is a systematic replication of the experiments described in section 3.4 with a clinically important response.

Unfortunately, added reinforcers should similarly increase the persistence of undesirable responses. This expectation is particularly important because a common procedure for reducing the rate of undesired behavior is to provide reinforcers for a competing alternative response or for unspecified behavior occurring in the absence of the target response. As was shown by McDowell (1982), the re-

https://doi.org/10.1017/S0140525X00242404 Published online by Cambridge University Press

duction in the target response is predicted by Herrnstein's Relative Law of Effect (see sect. 2.2). However, the addition of explicit reinforcers to the unknown reinforcers that maintain the undesired target response may increase its persistence even as they reduce its rate of occurrence. Mace (1991) obtained this perverse outcome with food stealing by a retarded child. After explicit reinforcement for proper eating, the rate of food theft was substantially lower than baseline, but, when thefts were physically blocked, attempts to steal food persisted far longer than in a previous condition where alternative reinforcement had not been provided. This outcome is entirely consistent with the research described in section 3.4 and with the metaphorical notion that, although alternative reinforcement reduced the velocity-like aspect of food theft, it also increased its mass-like aspect and thus tended to counteract the purpose of the intervention. At the least, the possibility of such outcomes must be considered by therapists who use alternative reinforcement to reduce undesired behavior.

10.1.2. Establishing compliance with requests. The momentum metaphor has been used effectively by Mace et al. (1988) to establish compliance with demanding requests that were normally resisted. Briefly, Mace et al. presented retarded adults in a group home with a series of easy requests that were fun to comply with (e.g., "Give me five"), and gave social reinforcement for compliance, immediately before a more demanding request (e.g., "Empty the trash"). They obtained substantially greater compliance than when the demanding requests were not preceded by easy requests. The metaphorical interpretation is that the series of easy requests endowed compliance as a general response class with both velocity and mass, thereby increasing its rate and reducing its disruptability by more demanding requests. Nevin (1996a) discussed the interpretation of this procedure for establishing compliance in relation to the momentum metaphor and concluded that its effectiveness can be understood and, perhaps, enhanced by reference to research on resistance to change.

10.2. Drug abuse and addiction

When people persist in efforts to procure and consume drugs, to the detriment of their health, occupation, and social life, their behavior is often characterized as addictive. As Heyman (1996) pointed out, the compulsive quality of addiction has led many researchers to conclude that it is not under the control of its long-term consequences. Heyman argued to the contrary and showed that a model of choice that incorporates changes in the value of drugs and nondrug reinforcers, together with control by local relative value, can account for addictive behavior. However, his model does not stress the role of environmental stimuli in addiction.

Evidence for control by environmental stimuli comes from studies of relapse after drug use has been eliminated during treatment in an inpatient facility. For example, relapse is very likely when a former addict returns to a situation in which drug use has previously occurred (Brownell et al. 1986; Hunt & Oderoff 1962). Conversely, when a former addict moves to a radically different stimulus situation, as when soldiers who were addicted to heroin in Vietnam returned to the United States, there is little evidence of relapse (Robins et al. 1977). As was noted by Nevin (1996b), these observations suggest that addictive behavior has considerable stimulus-specific mass.

In line with the distinction between response rate and resistance to change that has been made repeatedly above, we suggest that Heyman's choice model can account for the rate of addictive behavior, but its persistence may depend on historical stimulus-reinforcer relations. The effects of choice processes and stimulus-reinforcer relations may converge to endow drug taking with high momentum, thereby making addiction especially refractory to treatment and prone to relapse in the addict's normal environment (Nevin 1996b). In particular, drugs may be viewed as disrupting many everyday activities that do not involve drug taking and simultaneously reinforcing the behavior that procures them in a way that is particularly resistant to change. Here, we ask whether the effects of drugs are consistent with research on behavioral momentum.

10.2.1. Cocaine as a disruptor. There has been a vast amount of relevant research with nonhuman subjects, and we cannot review it systematically here. Instead, we will consider a few examples involving cocaine, a highly addictive drug that has created serious personal and public health problems. We begin by considering cocaine as a disruptor of ongoing operant behavior maintained by conventional reinforcers, which may be construed as a model for the deleterious effects of cocaine use on everyday activities.

The disruptive effects of acute and chronic cocaine adminstration have been studied by Hoffman et al. (1987) in a three-component multiple FR schedule with food reinforcement and pigeons as subjects. They found evidence that acute administration affected responding in ways consistent with other disruptors reviewed above: Relative to performance in vehicle control sessions, decreases in response rate were greatest in the component with the largest fixed ratio, and least in the component with the smallest fixed ratio, and thus were ordered with respect to obtained reinforcer rate. Hoffman et al. also found that development of tolerance was directly related to reinforcer rate: With repeated administration of a moderate cocaine dose, response rate recovered to near baseline levels in the component with the smallest ratio and recovered least, if at all, in the component with the largest ratio. Thus, cocaine administration is analogous to a disruptive force: Its effects are greatest on behavior maintained by a relatively low reinforcer rate, both upon initial administration and as its effectiveness wanes during the development of tolerance.

10.2.2. Cocaine as a reinforcer. Cocaine is also a highly effective reinforcer. In monkeys, characteristic patterns of operant behavior are maintained by fixed-interval (FI) and fixed-ratio (FR) schedules of cocaine reinforcement (see, e.g., Goldberg & Kelleher 1976), and choice between two concurrently available cocaine doses roughly matches relative dose level in a fashion similar to the relative magnitude of conventional reinforcers (Llewellyn et al. 1976). There is some evidence that increasing doses of cocaine reinforcement may also increase resistance to change. For example, Glowa et al. (1995) administered a dopamine reuptake inhibitor to their monkeys before selected experimental sessions with cocaine reinforcement. They found that reductions in cocaine-maintained responding were inversely related to cocaine dose per reinforcer. The presession inhibitor may be viewed as similar to prefeeding, which also

https://doi.org/10.1017/S0140525X00242404 Published online by Cambridge University Press

produces reductions in response rate that are inversely related to magnitude of food reinforcers in pigeons (Nevin et al. 1981).

Research by Carroll and Lac (1993) is more directly relevant to the prevention of cocaine addiction. They found that access to a glucose plus saccharine solution interfered with the acquisition of cocaine-reinforced autoshaping and subsequent cocaine self-administration, but only if glucose plus saccharine was given in the operant chamber. This could be interpreted as an instance of blocking by the experimental context (see, e.g., Tomie 1976). Alternatively, it may be that access to alternative reinforcers in the operant chamber endowed unmeasured behavior that competed with cocaine self-administration with high mass. This interpretation is admittedly speculative, but there might be some practical utility to the notion that arranging a high density of conventional (i.e., nondrug) reinforcers in a given environment increases resistance to the reinforcing as well as the disruptive effects of drugs.

10.3. Self-control

Here we discuss two approaches to the problem of self-control in relation to behavioral momentum. Self-control may be characterized as accepting some short-term deprivation (as in refraining from an addictive drug) and thereby obtaining some larger, long-term good (health and well-being).

10.3.1. Choice between small, immediate and large, delayed rewards. One experimental analog of self-control involves choice between small, immediate reinforcers and large, delayed reinforcers. Pigeons and (in many situations) humans generally exhibit impulsiveness by choosing the smaller, more immediate reinforcer (see Logue 1988, for review). These preference results are well described by a version of the generalized matching law, which assumes that the effects of reinforcer amount and immediacy are additive (Grace 1995; Logue et al. 1984). In a parametric study, Bedell et al. (1997) assessed preference and relative resistance to change in pigeons choosing between alternatives that differed in reinforcer amount and immediacy. They found that the effects of these variables on resistance to change were additive, consistent with the preference data.

Effective methods for enhancing choice of large, delayed reinforcers in nonhuman subjects include progressively lengthening the delay and presenting stimuli that bridge the delay to the larger reinforcer (Mazur & Logue 1978), or increasing the delay equally for both alternatives (Green et al. 1981). It would be interesting to determine whether these methods also enhance the resistance to change of responding for the large, delayed reinforcer, consistent with the general correlation between preference and resistance. Such an outcome would have immediate relevance for the transfer of self-control training in the clinic to everyday life.

10.3.2. Self-control as a temporally extended pattern. Rachlin (1995) has suggested a different approach that is related to the principles of behavioral momentum. Specifically, he argued that self-control involves an extended pattern of engagement in high-valued behavior (e.g., a healthy lifestyle) that persists despite occasional tempting alternatives, even though those alternatives, considered individually and locally, have a higher value than individual components of the pattern.

We suggest that Rachlin's extended pattern is analogous to sustained responding in the initial link of a chain schedule in that, from a molar perspective, continued access to the terminal-link reinforcer (analogous to health) depends on continued initial-link responding (analogous to moderate drinking, low-fat diet, etc.) throughout the experiment. In a study of resistance to change in chained schedules, Nevin et al. (1981, Experiment 2) showed that average initial-link response rates in pigeons were less disrupted by the occasional availability of a single immediate reinforcer on an adjacent key (mimicking temptation) when terminal-link food was relatively large and immediate. Similar results were obtained with prefeeding, suggesting that resistance to a tempting alternative is functionally equivalent to resistance to the other disruptors reviewed in this article.

In real life, as opposed to the pigeon chamber, the presumed ultimate reinforcer for living a healthy lifestyle – a long, healthy life – does not occur at any particular moment and, indeed, may not occur at all (one could be hit by a bus). Therefore, the contingency between living a healthy lifestyle and its ultimate benefits is at best remote. How, then, is the healthy lifestyle to be maintained? In view of the strengthening effects of added reinforcers (see sects. 3.4 and 10.1), we suggest that "self-control" – that is, maintaining a healthy lifestyle despite succumbing occasionally to the third martini or seconds on cheesecake – may be enhanced by arranging additional reinforcers that are unrelated to health, such as listening to music, in a person's normal environment. The same general approach may be useful in sustaining any desirable pattern of action where the intended consequences are remote, such as political efforts on behalf of world peace.

11. Relations to Thorndike's Law of Effect

Almost a century ago, Thorndike proposed his famous Law of Effect:

> Of several responses made to the same situation, those which are accompanied or closely followed by satisfaction to the animal will, other things being equal, be more firmly connected with the situation, so that, when it recurs, they will be more likely to recur; those which are accompanied or closely followed by discomfort to the animal will, other things being equal, have their connections with that situation weakened, so that, when it recurs, they will be less likely to occur. The greater the satisfaction or discomfort, the greater the strengthening or weakening of the bond. (Thorndike, 1911, p. 244)

Although many aspects of this oft-quoted law have been challenged, we suggest that its central principles are compatible with the work on resistance to change and preference that we have described above.

Thorndike (1911) proposed to define satisfaction and thereby achieve "more detailed and perfect prophecy" as follows: "By a satisfying state of affairs is meant one which the animal does nothing to avoid, often doing such things as attain or preserve it" (p. 245). In section 7.3, we suggested that the value of a discriminated operant may be estimated by its attractiveness, expressed as preference for access to that operant, a notion that is quite similar to attaining or preserving a satisfying state of affairs.

Thorndike also anticipated the identification of asymptotic strength with resistance to change:

https://doi.org/10.1017/S0140525X00242404 Published online by Cambridge University Press

> In certain cases in which the probability that the connection will be made is 100 per cent, the connections may still exist with different degrees of strength, shown by the fact that the probability of 100 per cent will hold for a week only or for a year; will succumb to a slight, or prevail over a great distraction; or otherwise show much or little strength. (Thorndike, 1913, p. 3)

11.1. Parallels with behavioral momentum

Although Thorndike's Law was principally concerned with acquisition, its initial statement that the probability of a response depends on its consequences relative to those of other responses is amply supported by molar analyses of steady-state response rate in relation to schedules of reinforcement, as summarized by Herrnstein's Relative Law of Effect. Thorndike's statement that satisfaction establishes a connection between the situation and the response is amply supported by research on resistance to change in a stimulus situation, which measures the strength of a discriminated operant and is directly related to the rate or magnitude of reinforcers contiguous with that stimulus.

Although Thorndike anticipated the possibility that strength is independent of the asymptotic rate or probability of responding before resistance is evaluated, he did not distinguish their determiners. As we have shown, response rate depends on response-reinforcer relations, whereas resistance to change is determined primarily by stimulus-reinforcer relations. Although other variables may also influence resistance to change, it appears that, whatever its determiners, response strength, as estimated from resistance to change in multiple schedules, is positively related to reinforcer value, as estimated by preference in concurrent chains. Thus, Thorndike's statement that the strength of connection is directly related to the magnitude of satisfaction is supported by the structural relation linking resistance to change and preference. Because these terms are measured independently, the relation between them is immune to the charges of tautology that have often been leveled against the Law of Effect (see, e.g., Postman 1947).

Thorndike's assertion that a stimulus-response bond is strengthened or stamped in by reinforcement appears to be at odds with research demonstrating abrupt changes in behavior when the reinforcer is changed or devalued. We now consider some of these studies from the perspective of behavioral momentum.

11.2. Changes in reinforcer magnitude or quality

A number of early studies demonstrated that reducing the magnitude or quality of the reinforcer resulted in abrupt decrements in behavior. For example, in a frequently cited study, Crespi (1942) trained rats in an alley with a large reinforcer and then shifted to a small reinforcer. Running speed decreased substantially in the next trial, to a level below that maintained by training with the small reinforcer only. The result suggests that, although running may have been acquired as a result of reinforcement, the reinforcer did not stamp in a habitual connection between the alley and running as would be expected according to the Law of Effect. In a review of this study and related studies, Mackintosh (1974) concluded that "reinforcers do not increase the strength of an association between stimulus and response; they are themselves associated with the response" (p. 216).

In Mackintosh's terms, we suggest that whether or not instrumental learning involves response-reinforcer associations, reinforcers do increase the strength of an association between stimulus and response as measured by resistance to change. Consider Crespi's result in relation to Equation 17 above. Abrupt reduction in reinforcer magnitude may be construed as a resistance test, on a continuum with reduction to zero – that is, extinction – and its effects may be attributed, at least in part, to the change in the stimulus situation that necessarily accompanies changes in the reinforcer, construed as a part of the set of events associated with training. These effects would compete with the persistence of running based on the behavioral mass established by the alley-reinforcer relation during training, which could be assessed independently by a resistance test such as prefeeding that did not involve changing the reinforcer. Thus, abrupt changes in behavior when the reinforcer changes are not incompatible with the development of a Thorndikean bond; they merely complicate its measurement.

11.3. Reinforcer devaluation effects

A number of studies have evaluated response–reinforcer associations by devaluing the reinforcer, usually by pairing it with a drug that causes gastric upset. For example, Colwill and Rescorla (1985a) arranged liquid sucrose or food reinforcers for lever pressing or chain pulling, counterbalanced across groups of rats. In a second phase of the experiment, they devalued one reinforcer by pairing it with gastric upset with the lever and chain removed from the chamber. In a final extinction test, they observed selective suppression of the response that had produced that reinforcer during training. This result suggests that the rats had associated each response with its respective reinforcer during training, as was suggested by Mackintosh (1974; quoted above), and then anticipated those reinforcers in the final test. These and related findings (see, e.g., Adams & Dickinson 1981) are contrary to Thorndike's original law because situation-response connections should have been equally strong for both responses.

Although our approach to the strength of discriminated operant behavior does not address the mechanism of response-specific reinforcer devaluation when responding is precluded, there is at least one aspect of the results that is related to analyses of resistance to change. Both Adams and Dickinson (1981) and Colwill and Rescorla (1985a; 1985b) found that, although responding established by a contingent and subsequently devalued reinforcer was suppressed relative to that established by a reinforcer that was not devalued, it was not totally suppressed despite the fact that the rats never consumed the devalued reinforcer. In other words, responding persisted despite the joint disruptive effects of reinforcer devaluation and extinction. In keeping with our arguments detailed above, the persistence of responding suggests that situation-response connections had been formed during training. This conclusion is consistent with Dickinson's (1994) suggestion that "instrumental training established lever pressing partly as a goal-directed action, mediated by knowledge of the instrumental relation, and partly as an S-R habit impervious to outcome devaluation" (pp. 51–52).

https://doi.org/10.1017/S0140525X00242404 Published online by Cambridge University Press

The effects of reinforcer devaluation may be isolated by comparing resistance to extinction of a response when its reinforcer had been devalued to that of a response when its reinforcer had not been devalued. Examination of Colwill and Rescorla's (1985a) Figure 1 shows that, when the reinforcer had not been devalued, training with sucrose led to substantially more responding during extinction than training with food. Thus, sucrose was the more effective reinforcer in that it established greater resistance to extinction. Consistent with this interpretation, sucrose-reinforced responding after devaluation was greater, relative to responding when sucrose had not been devalued, than was food-reinforced responding after devaluation relative to responding when food had not been devalued. Thus, sucrose apparently established stronger situation-response connections as evidenced by greater resistance to reinforcer devaluation as well as to extinction.

Because the data are presented as averages, we cannot determine whether this difference in relative responding is statistically significant, and in any case Colwill and Rescorla's experiment was not designed to evaluate relative resistance to devaluation. It would be interesting to examine devaluation effects in multiple schedules with substantially different reinforcer rates and ascertain whether responding during an extinction test depends on stimulus-reinforcer relations in the same way as responding that has been reduced by other disruptors. Experiments of this sort could lead to a fruitful interaction between analyses of resistance to change and research concerned with the associative structure of learning.

12. Conclusions

On the basis of the research described in this article, we propose a modern version of Thorndike's Law of Effect for discriminated operant behavior: When a response has been reinforced in a distinctive stimulus situation, its probability or rate of occurrence depends on the response-reinforcer contingencies. At the same time, it becomes connected to the situation and will tend to recur despite challenging disruptions. The greater the value of the situation, as determined by the conditions of reinforcement and as measured by preference, the greater the strength of connection as measured by resistance to change.

ACKNOWLEDGMENTS

Preparation of this target article was supported by National Science Foundation grant IBN-9507584. We thank the reviewers, especially Peter Killeen, for their thoughtful comments.

NOTES

1. Williams (1991) performed a related experiment in which stimuli signaling the two identical schedules were presented simultaneously in occasional choice probe tests. He found that his subjects made more choice-probe responses to the stimulus preceding the richer schedule than to the stimulus preceding the leaner schedule even though baseline response rate in the component preceding the richer schedule was lower. As will be seen in section 8.3, the agreement between relative resistance in Nevin et al. (1987) and probe choice in Williams (1991) may exemplify the general correlation between resistance and preference.

2. We recognize that the analogy between velocity and response rate is inexact, in that velocity is a vector that measures direction as well as distance per unit time. Nevertheless, the analogy to number of repeatable free-operant responses per unit time is suggestive and may be especially helpful in applied behavior analyses, as was suggested by Nevin (1996a).

3. Nevin and Grace (1999) have recently used a version of this model to interpret differences between resistance to prefeeding and resistance to extinction in their data.

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The stimulus-reinforcer hypothesis of behavioral momentum: Some methodological considerations

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Abstract: This commentary focuses on the stimulus-reinforcer hypothesis of resistance to change. The overall context of reinforcement can account for resistance to extinction. There are ways to systematically test the hypothesis that Pavlovian contingencies account for the behavioral "mass" of discriminated operant behavior.

Nevin & Grace's (N&G's) target article combines the behavioral momentum metaphor (Nevin 1974) with the contextual choice model (Grace 1994) to propose "a modern, quantitative version of Thorndike's (1911) Law of Effect" (Abstract) for discriminated operant behavior. The main ingredient of N&G's proposal is a Pavlovian stimulus-reinforcer relation that supposedly accounts for resistance to change in multiple schedules and preference in concurrent-chain schedules.

I am basically sympathetic to N&G's attempt to integrate behavioral momentum studies with other aspects of research on operant reinforcement. My main concern is with the method used to infer that stimulus-reinforcer contingencies explain behavior resistant to extinction in multiple schedules and preference in concurrent-chain schedules.

The stimulus-reinforcer hypothesis of resistance to change emerged from studies showing that response rate in one component of a multiple schedule is more resistant to extinction when: (1) response-independent reinforcers are added at variable times (e.g., Nevin et al. 1990), (2) the target component is followed by a richer schedule signaled by a colored center key (e.g., Nevin et al. 1987; Tota-Faucette 1991), or (3) the target component provides additional reinforcers contingent upon an alternative response (Nevin et al. 1990, Experiment 2).

A common denominator of the conditions listed above is that one component of a multiple schedule is correlated with a higher reinforcement rate. However, only the condition where response-independent reinforcers are added in presence of the stimulus associated with one component of a multiple schedule resembles the traditional Pavlovian way to establish a stimulus-reinforcer association. The other two conditions do not involve the standard Pavlovian procedure to establish conditioning to a target stimulus. The notion that stimulus-reinforcer contingencies account for re-

https://doi.org/10.1017/S0140525X00242404 Published online by Cambridge University Press

sistance to change seems to be inferred from the influence of the overall context of reinforcement.

To take care of this possibility, N&G quantify the Pavlovian determiners of resistance to change (sect. 4.1) in terms of stimulus-reinforcer contingency ratios. Relative resistance to change is accordingly expected to vary with the relative contingency ratio. This prediction has indeed been confirmed by Nevin (1992a), in a study suggesting that relative resistance to change is independent of the overall context of reinforcement. However, few systematic attempts to assess the role of stimulus-reinforcer contingencies have been conducted so far.

In particular, the context has rarely been manipulated systematically to identify the stimulus that supposedly is the best predictor of reinforcement. Nor have behavioral momentum studies included control procedures designed to explicity assess the role of Pavlovian stimulus-reinforcer contingencies in accounting for the behavior resistant to extinction. In the absence of proper control procedures, the Pavlovian hypothesis of resistance to change should remain tentative. Further research will need to include proper control procedures to show that stimulus-reinforcer contingencies in fact account for the "behavioral mass" of discriminated operant behavior. Meanwhile, a few possible ways to test the stimulus-reinforcer hypothesis of resistance to change suggest themselves.

For example, Rescorla (1968) argued that the best way to establish a stimulus-reinforcer association is to present the reinforcer contingently upon the occurrence of the stimulus. Rescorla's notion of contingency refers to the reinforcer's probability of occurrence in the presence of the stimulus, contrasted with its probability of occurrence in the absence of the stimulus. Rescorla's contingency idea (1968) can be used to test the stimulus-reinforcer hypothesis of resistance to change, using as baseline a multiple schedule with two identical components and a large intercomponent interval. In one group of subjects, response-independent reinforcers could be scheduled in one component and never at other times, whereas in another group, response-independent reinforcers could be provided during the intercomponent interval and not in the presence of any of the component stimuli. If N&G's stimulus-reinforcer hypothesis is correct, only the subjects in the first group should show higher resistance to extinction in the target component; subjects of the second group should show the same level of resistance to extinction in both components of the multiple schedule. Failure to find such results would point to a possible role of nonassociative factors in determining resistance to change.

Another simple way to assess a possible role of the overall context of reinforcement (as opposed to a specific stimulus-reinforcer relation) in determining momentum is to train behavior in one context and test its resistance to change in a different context. For example, a standard, two-key operant chamber for pigeons could be used to establish responding on a multiple schedule with two identical variable-interval schedules, one component including response-independent reinforcers as well. A different chamber (say, one with the walls painted black) could be used later to measure resistance to extinction in each component. If the stimulus-reinforcer hypothesis is correct, a difference in the level of resistance to extinction among the two components should be observed in the novel as well as in the training chamber. Finding the same level of resistance to extinction in both components when testing is done in the novel chamber would suggest an influence of the overall context (chamber) beyond specific cues.

Finally, in a traditional momentum study, each response-independent reinforcer could be preceded by a brief, discrete signal. If N&G's Pavlovian hypothesis is correct, this discrete signal should detract from the stimulus-reinforcer association, and therefore diminish resistance to change in comparison to a condition using unsignalled response-independent reinforcers. Finding the same level of resistance to extinction in both conditions would contradict the hypothesis that specific stimulus-reinforcer contingencies account for the behavioral "mass" of discriminated operant behavior.

Newton and Darwin: Can this marriage be saved?

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Abstract: The insights described by Nevin & Grace may be summarized without reference to the Newtonian concepts they suggest. The metaphor to Newtonian mechanics seems dubious in three ways: (1) extensions seem to lead to paradoxes; (2) many well-known phenomena are ignored; (3) the Newtonian concepts seem difficult to reconcile with the larger framework of evolutionary theory.

Nevin & Grace (N&G) are to be congratulated for having increased our understanding of reinforcement. The research reviewed shows that persistence may be quantified and that it is directly related to attractiveness.

These insights, however, in no way require the authors' analogies to Newtonian mechanics. We learn first that persistence of behavior in the presence of a stimulus depends on the rate of reinforcer delivery in the presence of the stimulus. Persistence is measured by resistance to a "disrupter," a category of operations that includes extinction along with variables like prefeeding. Such concatenation is novel and possibly problematic, because it mixes the traditional categories of contingency and motivation. Distinguishing between these categories is useful because extinction causes a long-term reduction in responding that prefeeding does not. Second, we learn that relative persistence of responding in two situations is directly related to the relative attractiveness of the two situations. In traditional terms, one would say that the relative strength of two discriminative stimuli matches their relative strength as conditional reinforcers. In more up-to-date terms, one might say that the relative power of a situation to sustain behavior matches its relative power to attract behavior. Nothing in these summaries requires the notion of "behavioral mass" in comparison with the physical mass of an object.

N&G may be fond of the metaphor, and it may have been useful in helping them to carry on the research and arrive at the understanding they describe, but the metaphor now seems to get in the way more than to enlighten. It seems problematic in three ways.

First, the metaphor of behavioral momentum belongs in the context of the larger framework of Newtonian mechanics. Do N&G want to suggest that the study of behavior will be advanced by further analogies to Newtonian concepts? If so, what could these be, and how would they fit with behavioral dynamics? Our attempts to extend the metaphor seem to lead to paradoxes. If reinforcement is analogous to a force, then why doesn't response rate (velocity) increase indefinitely? Is there an analog to friction? If so, then is extinction still a force, just like prefeeding?

Second, to be productive, a metaphor should embrace numerous well-known phenomena, but this one seems limited in scope and selective in its application. For example, suppose one compared a biologically prepared response with one that was unprepared. In the face of extinction, the rate of the prepared response would decrease relatively slowly, but, during acquisition, its rate would increase relatively rapidly. The analogy to mass suggests that both these changes should be slow, because a high-mass object acquires and loses velocity slowly. How would one resolve the contradiction?

Third, the metaphor offers no larger context in which to understand such matters as the difference between prepared and unprepared responses. Indeed, it offers no way to understand why response topography should matter at all. Yet we know it does matter. For example, the principle of least effort, which informs so much behavioral research, has no place in the authors' analogy. Least effort does tie in to another context, of course, the larger context that incorporates concepts like benefit and cost – that is,

https://doi.org/10.1017/S0140525X00242404 Published online by Cambridge University Press

optimality theory. Not just least effort, but reinforcement and punishment themselves are often viewed through the lens of optimality and, more specifically, evolutionary theory. Explanations of why organisms behave as they do frequently rely on natural selection, which tends to produce solutions to problems that are approximately optimal (e.g., Stephens & Krebs 1986). The explanatory basis of Newtonian concepts differs fundamentally from that of selection by consequences. This difference is recognized in evolutionary biology by the distinction between ultimate and proximate explanations (Alcock 1998). Ultimate explanations rely on selection across generations, whereas proximate explanations rely on development within a lifetime. Perhaps Nevin & Grace have in mind such a distinction. Perhaps they see Newtonian mechanics as a framework for proximate explanations. Then those mechanics of behavior would supplement ultimate explanations that are based on selection by consequences. It is hard to see how to arrive at such a solution, however, because the authors' appeal to rate of reinforcement to determine mass and attractiveness already seems to imply selection as a framework. If they assume selection as a framework, then the Newtonian concepts may be extraneous. How are the two sets of concepts to be reconciled?

Gaining (on) momentum

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Abstract: Nevin & Grace's approach is an interesting and useful attempt to find ways to measure "core" effects of a history of exposure to reinforcement. The momentum analogy makes intuitive sense, and the evidence for its utility is increasing. Several questions remain, however, about how the analogy will fare in the case of concurrent rather than sequential activities, about the use of extinction as a method to test resistance to change, and about the generality of some of the effects.

Nevin & Grace (N&G) are to be commended for presenting a novel approach to a longstanding issue, that of whether experience with reinforcement can be construed as producing some sort of unitary or core effect on subsequent behavior. As they note, attempts to equate core effects with those on response rate (at least of free-operant behavior) have not fared well, despite the superficial relationship between rate and probability. In the target article, they argue that core effects of reinforcement can be reflected either in resistance to change when additional variables are brought to bear or in choice for the situation in which behavior occurs. Both approaches to measurement, then, are based on the view that the situation in which behavior is reinforced is crucial, and that variables that influence response rate are not as important. This viewpoint has intuitive appeal and considerable empirical support, but there remain questions about the generality of momentum effects and also about logical consistency in its interpretation.

Some of the suggested applications of the momentum analogy left me somewhat confused. Primarily I did not understand what is supposed to happen when concurrent operants are involved. This is an important issue, because most of real life is filled with concurrent opportunities to act. It is unclear (at least to me) what one should expect about resistance to change when several activities are being measured concurrently, and I do not think N&G have been entirely unambiguous about the issue. In section 10.1.1, they suggest that adding reinforcement in a "situation" will increase the persistence of all reinforced behavior in that situation. In section 10.2.2, they suggest that added reinforcers for activities unrelated to health-maintaining behavior may give extra momentum to the health-related (rather than competing) activities.

Perhaps I am misreading them, but it seems that N&G are trying to have it several ways. In some cases extra reinforcers give extra momentum to all behavior, both good and bad; in others they give momentum to competing behavior that will interfere with bad behavior; and in still others extra reinforcement seems to give added resistance to change only to good behavior. Any of these interpretations involves a considerable conceptual leap from the available experimental data, which have focused on changes in the effectiveness of a particular reinforcer, not all reinforcers in a situation. There is little information about how one would expect momentum of activities maintained by different reinforcers to be affected by disrupters in the typical multiple-schedule arrangement, and even less about what happens to momentum of concurrently available operants that are maintained by different reinforcers. It may well be that resistance to change is reinforcer-specific. Certainly, its implications for therapy would be enhanced considerably if that were the case.

The literature concerning resistance to change when concurrent operants are measured is mixed. Some data (e.g., Farley 1980; Mellon & Shull 1986) seem at least superficially consistent with the idea that behavior maintained by higher rates of reinforcement will be more resistant. Other data, however, are not as supportive of an analysis based on the momentum metaphor when concurrent operants are maintained by the same reinforcer. For example, data from both Nevin et al. (1990) and McSweeney (1974) showed that when two performances were established under concurrent variable-interval (VI) schedules of food reinforcement, a disrupter led to larger proportional changes in the more frequently reinforced activity. This result is the opposite of what one sees if two different VI schedules are arranged in components of a multiple schedule. It also is not consistent with the idea that all activities in a situation will be similarly affected.

An interesting experiment that would be a tour de force in terms of tying preference to momentum would be to establish responding under a concurrent-chains procedure, and then introduce disrupters to determine what happens to the initial-link rates (cf. Mellon & Shull 1986). One would assume that initial-link responding that leads to a preferred outcome would be more resistant to change than responding that leads to an outcome that is not preferred.

N&G make a case for stimulus-reinforcer relations being the key determinants of momentum. They also, at another point in the target article, describe the results of Grace et al. (1998), which, at least on the surface, provide a severe challenge to the interpretation based on Pavlovian relationships among stimuli.

It is good to see N&G face directly the problems associated with using extinction as the disrupter in assessment of resistance to change. One of the strengths of the measurement of resistance to change is that ordinarily the potential problem of different units of behavior (operants) is circumvented. For example, suppose behaviors under variable-ratio (VR) and VI schedules are being compared. Under VR schedules it may well be that the "real" unit of behavior (functional operant; see Catania 1973) consists of rapidly emitted groups of experimenter-defined responses (descriptive operant; see Catania 1973), whereas under the VI schedule a class of interresponse times might emerge as the functional operant (Anger 1956; Morse 1966). As long as conditions of reinforcement remain in effect, application of disrupters should result in changes in the rate of the "real" units of behavior, and those changes should be reflected relatively faithfully in changes in rate of experimenter-defined responses. Using extinction as a disrupter, however, complicates the matter because as the conditions of reinforcement are removed, one might expect the units of behavior to "disintegrate." If that happens, the correspondence between rate of experimenter-defined responses and rate of the functional operants may break down, rendering measurement suspect. All this is to say that another perspective on the "problem" of extinction, in addition to the partial-reinforcement-extinction effect, is that it provides an experimentally unclean disrupter. It already seems a bit ad hoc to add a parameter that measures generalization decrement, and the response unit problem would need to be finessed by at least two additional parameters – one to monitor the change

https://doi.org/10.1017/S0140525X00242404 Published online by Cambridge University Press

in response units for each reinforcement condition. It may well be that a wiser choice would be to abandon extinction as a procedure to assess momentum.

N&G make good arguments about the utility of multiple schedules to examine resistance to change, but the generality of that utility still needs to be examined. Most of the research, for example, has focused on the use of VI schedules in the components of the multiple schedule. One reason for this is to provide control of reinforcement rate when response rate varies, but exclusive reliance on VI schedules limits generality. A really interesting comparison would be between VR schedules and VI schedules. That could be accomplished by studying a range of VR values in a multiple-schedule arrangement in which the VI values are yoked (cf. Ferster & Skinner 1957) to the interreinforcement times obtained under the VR schedule. This would provide a very strong test of the momentum idea that would predict equal resistance to change under the two schedules as long as reinforcement rates are matched.

Another area of useful research would be of the sort that examines whether multiple-schedule interactions have, in fact, been eliminated by the imposition of intercomponent timeout periods. For example, would responding under a VI 1-min schedule exhibit the same resistance to change if it were studied alone rather than as part of a multiple schedule of the sort usually employed? Another parameter that could be usefully examined is component duration, especially short durations. Component duration has been shown to affect response rates in multiple schedules (e.g., Killeen 1972; Shimp & Wheatley 1971). Would those changes occur with intercomponent timeouts in place, and, if so, would those changes in rate be independent of changes in resistance to change?

ACKNOWLEDGMENT

Preparation of this commentary was assisted by USPHS Grant No. DA04074.

To augment yet not contradict

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Abstract: Evidence from 45 early studies of resistance to extinction following reinforcement of differing amounts, taken in sum, challenges both the basic and the augmented models of Nevin & Grace. The augmented model seems too ad hoc in salvaging the analogy between persistence in behavior and concepts from physics, as my meta-analysis of these data affirms.

Nevin & Grace's (N&G's) augmented model in section 9.3 seems too ad hoc as a means of salvaging their analogy between persistence in operant responding and certain physical concepts: momentum, mass, inertia, and velocity/speed. If reinforcers have discriminative properties as suggested by the partial reinforcement extinction effect (PREE), then these need to be accommodated in predicting all the data, not just the PREE. Their basic model (Equation 5) must be inaccurate, because it ignores the discriminative properties and is contradicted by the PREE. Yet, paradoxically, that model nevertheless still fits the data reviewed up to that point in the target article. So, by inference, the supposedly improved augmented model applied consistently to those data (i.e., addressing the discriminative properties) must fail with those data. The trick for the authors to master is to augment yet not contradict. My commentary will illustrate the difficulty of doing so.

An adequate theory must account for evidence from a plethora of early studies of resistance to extinction that used simple procedures. I shall focus on one variable, the amount of reinforcement. Summarized in Table 1 are 45 findings that have been classed according to the effect on resistance to extinction of varying reinforcer amounts during training before extinction. Because type of schedule in training could be a critical determinant of the mixed nature of these results, as is later examined objectively in a meta-analysis, the studies were further classified according to whether the procedure involved continuous (CRF) or partial reinforcement (PRF) training prior to extinction. Excluded from this review are studies involving more complex designs for manipulating amount (e.g., mixtures of large and small reinforcers in particular sequences before the extinction test) and atypical schedules (e.g., extinction after progressive ratio training).

The most notable pattern in the table is that no study found greater resistance to extinction with a smaller amount of reinforcement in PRF training, whereas in 11 data sets there was a positive correlation between resistance and amount. This is in agreement with the target article's unaugmented model and nearly all multiple-schedule studies of resistance to change (e.g., Shettleworth & Nevin 1965; Harper & McLean 1992 is the main ostensible exception). This is expected, because the contingencies in the multiple schedule studies are more similar to PRF than to CRF. Three simple-schedule studies found no effect of amount. Using the conservative assumption that findings of no effect are failures in trials with equiprobable outcomes, 11 successes in 14 trials are significantly more than what would be predicted by chance using the binomial test ($p < .05$).

When considering studies regardless of schedule used in training, the probability that a larger reinforcer led to greater resistance to extinction than a smaller reinforcer does not reach conventional statistical reliability (normal approximation to the binomial, $z = 1.12$). Therefore, an intermittent reinforcement schedule used in training may be critical to a positive relation between resistance to extinction and amount of training reinforcement. To test this, chi-squared tests were conducted: The proportion of studies showing a positive correlation between amount and resistance to extinction differs depending upon CRF versus PRF training ($\chi^2 = 10.7, p < .01$; studies reporting no effect were excluded from this analysis). Considering only CRF training (left-hand column), obviously no consistent relation of resistance to reinforcer amount differences was found across studies. The CRF data, then, while variable across studies, clearly contrast with those from the PRF research and, in sum, contradict both the original and augmented models of N&G, because the original model requires a positive relation, whereas the augmented model requires an inverse one.

The effect of amount of reinforcement in these studies appears to be an integral aspect of the PREE (for a review, see Sutherland & Mackintosh 1971, pp. 338–403). Several theories have been proposed or applied to account for the PREE, including the discrimination hypothesis (Mowrer & Jones 1945), the generalization decrement hypothesis (Sheffield 1949), the frustration hypothesis (Amsel 1958), the cognitive dissonance hypothesis (Lawrence & Festinger 1962), the sequential/memory hypothesis (Capaldi 1966), stimulus-analyzer theory (Sutherland & Mackintosh 1971), and DMOD (Daly & Daly 1982), which is an iterative model that combines features of the Rescorla-Wagner model (1972; itself a variant on the linear operator model of Bush & Mosteller, 1951) and Amsel's (1958) frustration hypothesis. All these theories involve two or more hypothetical constructs that can be combined to predict inversion of the function relating amount of reinforcement to resistance to extinction, depending upon whether the training schedule is PRF or CRF.

All succeed in accounting for inversion of the function (but see Sutherland & Mackintosh, 1971, p. 373, for a critical viewpoint), though usually the effect of amount of reinforcement has not been as great a concern as the PREE. The outstanding difficulty for all these theories, and N&G's models, is to explain why the function does not always reverse. Nearly as often as not, resistance to extinction is positively related to amount after CRF training (Table 1). As possibilities, perhaps the amount function will invert only after lengthy training prior to extinction (perhaps giving sufficient time for a secondary factor or factors to gain in control relative to

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Table 1 (Case). *Early simple studies of resistance to extinction following training with differing reinforcer amounts*

	Continuous Reinforcement	Partial Reinforcement
Increased amount, increased resistance to extinction	Barnes & Tombaugh (1970) Gonzales & Bitterman (1967) Gonzales, Homes & Bitterman (1967) Gonzales, Potts, Pitcoff & Bitterman (1972) Hill & Spear (1962) Ison & Rosen (1968) Lewis & Duncan (1957) Marx (1967) Pavlik & Collier (1977) Pert & Bitterman (1970) Potts & Bitterman (1968) Zeaman (1949)	Capaldi & Minkoff (1969) Gonzales & Bitterman (1967) Gonzales & Bitterman (1967) Hulse (1958) Lamberth & Dyck (1972) Leonard (1969) Lewis & Duncan (1957) Likely, Little & Mackintosh (1971) Pavlik & Collier (1977) Ratliff & Ratliff (1971) Wagner (1961)
Increased amount, decreased resistance to extinction	Armus (1969) Capaldi & Sparling (1971) Fisher (1979) Gonzales & Bitterman (1969) Gonzales, Homes & Bitterman (1967) Hulse (1958) Ison & Cook (1964) Lamberth & Dyck (1972) Leonard (1969) Likely, Little & Mackintosh (1971) Marx (1967) Ratliff & Ratliff (1971) Roberts (1969) Traupmann (1972) Wagner (1961) Zaretsky (1965)	
No effect	Clayton (1964) Clayton (1964) Hill & Wallace (1967) Uhl & Young (1967)	 Roberts (1969) Uhl & Young (1967)

the primary, conventional component of response strength; see discussion by D'Amato 1969 and Mackintosh 1974, pp. 423–31; and, for recent support for the important role of training length on the PREE, see Zarcone et al. 1997). Or, perhaps species of fish, amphibians, and reptiles are less likely to show the paradoxical relation than species of birds and mammals (perhaps the second hypothetical construct or factor is a certain kind of inhibitory or discriminative process that is more manifest the greater the proportion of brain composed of cerebral cortex; see Bitterman 1975). What must not be overlooked in grand unifications such as the target article's attempt is that while there is a highly reliable positive correlation between resistance to extinction and reinforcer amount after PRF training in simple schedules, there are both negative *and* positive correlations between these variables in extinction after CRF training. Either Nevin & Grace's original or augmented models may possibly be modified to account for all or nearly all these data (not overlooking that due to the vagaries of empirical work, some of these studies may be unreplicable); or a completely different theory, such as some modification to one of those listed, may be required.

ACKNOWLEDGMENT

Manuscript preparation was supported by NSF Grants BNS 94-07206 and IBN-9870900 to the University of California, San Diego, with Edmund Fantino, principal investigator; and by other NSF grants to the University of California, San Diego, with Ben Williams, principal investigator. I thank Mathew Bell for assistance in researching Table 1 and for editorial comments.

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Metaphors, models, and mathematics in the science of behavior

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Abstract: Metaphors and models involve correspondences between events in separate domains. They differ in the form and precision of how the correspondences are expressed. Examples include correspondences between phylogenic and ontogenic selection, and wave and particle metaphors of the mathematics of quantum physics. An implication is that the target article's metaphors of resistance to change may have heuristic advantages over those of momentum.

In either everyday or scientific discourse, once we have noticed that different domains share a few properties, it may be fruitful for us to ask whether they also share other properties. Consider the

https://doi.org/10.1017/S0140525X00242404 Published online by Cambridge University Press

Darwinian account of evolution in terms of natural selection. In selectionist accounts, it is important to say both what is selected and what does the selecting. In phylogenic selection, members of a population are selected by their environments (and for each individual, the other members constitute part of the environment). Phylogenic selection is paralleled by ontogenic selection, the selection of behavior as it occurs within the lifetime of an individual organism (the operant behavior of Skinner 1981). In this case, the populations are populations of responses, and the members of these populations are selected by their consequences (i.e., reinforced). The contingencies according to which responses produce consequences are properties of environments.

It is easy enough to find parallels between phylogenic and ontogenic selection once one begins looking (Catania 1987). For example, artificial selection was taken for granted in Darwin's time. It was the acceptance of natural selection that presented difficulties. Similarly, the artificial selection of responses is not at issue in contemporary accounts of behavior. It is obvious that the shaping of a variety of response classes can be accomplished in laboratory environments and other settings. The critical question is whether natural selection has a major role in shaping behavior in natural environments (for discussion of some implications, see Catania 1995). The issue has been largely resolved at the level of phylogenic selection, but the parallel case for ontogenic selection has not yet been closed.

It is also easy enough to find places where the parallels break down. For example, if other individuals constitute parts of the environment in phylogenic selection, in what sense are responses other than the reinforced response part of its environment? Other classes of responses may have other consequences (consideration of these classes might lead us to study concurrent operants), but other responses that are members of the reinforced class do not.

The examination of parallel properties in different domains is a feature held in common by different types of scientific accounts, often distinguished by the formal or structural properties of the discourse that followed from the parallels. The types may also vary considerably in their explicitness and in the precision with which the parallels are stated. For example, mathematical descriptions allow parallels between the numbers derived from mathematical operations and those derived from experiment. The correspondences will hold only up to a point. When they fail, the stage is set for new kinds of mathematics (in physics, the obvious examples are the relations among Newton's mechanics, Einstein's relativistic equations, and quantum mechanics).

Depending on whether accounts take the form of verbal discourse, mathematics, or some combination, we may speak of them in terms of metaphors, models, or mathematics (or some combination). As Lakoff and Johnson (1999) have argued in considerable detail, metaphor is a pervasive feature of everyday as well as scientific discourse, and the present point is that such accounts have similar relations to their respective subject matters despite the formal differences in their presentations.

The nature of the correspondences and the problems that follow from them may differ at different levels. For example, the metaphors of light as wave and particle break down at quantum levels but need not even be an issue at the level of the mathematics that comprises quantum mechanics. Discourse and mathematics are both classes of verbal behavior (cf. Skinner 1957), but as behavior they have different properties. The metaphors of the former need not correspond to the numerical properties of the latter. (How often have nonphysicists been puzzled as well as charmed by the many dimensions of fundamental particles because they tend to interpret those dimensions in terms of spatial metaphors rather than taking them as equivalent to nonspatial dimensions such as brightness or color?)

This brings us to Nevin & Grace's (N&G's) target article, because it too involves both verbal and mathematical components. Inevitably its metaphors will break down, as we explore the correspondences between mechanics and behavior systems. We may ask heuristic questions: For example, will this metaphor give us good ideas for new experiments or new ways of looking at existing sets of data? But we must also ask whether features of the metaphor may sometimes be misleading.

As in relating the mathematics of signal detection to the behavior maintained by schedules of reinforcement (Nevin 1969), the present mathematical account elegantly relates the behavior maintained by schedules to preference and other phenomena. The mathematics will help keep us out of trouble by forcing us to be explicit about our assumptions and their implications. But, perhaps in a manner analogous to the relations of the wave and particle metaphors to quantum mechanics, the metaphorical parts of the account may cause difficulties.

A problem with the metaphor of momentum (and its related Newtonian terms) is that the behavior in question is maintained by the continuing delivery of reinforcers. It is not something that has been set into motion and then left to continue on its own. If we pursue the metaphor, we must see the maintained behavior as more like a vehicle that must continue to be fueled by having things constantly delivered to it. The present account may provide a reasonable description of how it slows down more or less quickly once those deliveries end, but if the slowing down occurs because the maintaining conditions have ended, the metaphor of momentum, despite its glorious association with the history of physics, may be more troublesome than the one with a more mundane history, that is, the metaphor of resistance to change. What, then, about possible extensions? Are the variables that produce resistance to change analogous to the resistance of some medium through which the behavior is traveling? And what is it that is doing the traveling (it may or may not be helpful to ascribe to it properties such as mass)?

As a subject matter, behavior is part of biology, and the behavior that we study has its origins in both phylogenic and ontogenic selection. Biology is a historical science, in the sense that it depends on the particulars of life as it has happened to evolve on our planet. So too is the science of behavior. As it applies to individual organisms, it depends on the particulars (the history) of the organism under study. N&G are to be commended for the parts of their metaphors that bring together aspects of behavioral data that were hitherto treated separately. But what about the mixing of metaphors?

It is reasonable to guess that we should be looking to metaphors, models, and mathematics that will show relations between the contingencies of selection that create classes of behavior (including the classes defined by stimuli, as in discriminative responding) and those that maintain and modify those classes. No doubt some of these issues will be addressed by other commentators as well as by N&G. In any case, it appears likely that, relative to the metaphor of momentum, the metaphor of resistance to change, with its implication that our main concern is with the rapidity with which behavior adjusts to new contingencies, will be the more lasting and robust metaphor. In other words, it may itself be more resistant to change than its alternatives (those others may lose their momentum).

Behavioral momentum: Issues of generality

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Abstract: Nevin & Grace's behavioral-momentum model accommodates a large body of data. This commentary highlights some experimental findings that the model does not always predict. The model does not consistently predict resistance to change when response-independent food is delivered simultaneously with response-contingent food, when drugs are used as response disrupters, and when responding is reinforced under single rather than multiple schedules of reinforcement.

Nevin & Grace (N&G) have presented a powerful model of behavior that accommodates a large body of data. The notion that

https://doi.org/10.1017/S0140525X00242404 Published online by Cambridge University Press

behavior has two independent characteristics, rate and resistance to change, has great intuitive appeal. When a student tells me that he really "knew" the material on an exam, but that his performance deteriorated under the disruptive pressures of the test, I can assure him that his behavior lacked sufficient mass to pass the course.

My commentary highlights some findings that the model does not always predict, with the hope that modifications can be made and the model can eventually accommodate all of the data. N&G cite some of these discrepant findings in section 3.3 (Reliability and generality, para. 4). The contradictory data fall into two categories: type of disrupters and type of baseline schedules.

In a typical resistance-to-change test, responding is reinforced under a multiple schedule where components alternate, and rate of reinforcement is high in one component and low in the other. When responding is disrupted, response rates decline relatively faster in the component providing the lower rate of reinforcement (e.g., Cohen 1986; 1998; Cohen et al. 1993; Nevin 1974). Ideally, all disrupters of behavior should produce this effect, but they do not. N&G have adjusted their model in order to deal with the special properties of extinction (i.e., the generalization-decrement problem, sect. 9.3) but not with the inconsistent effects of drug disrupters and response-independent food delivered during the session. In short, not all studies show a direct relationship between rate of reinforcement and resistance to change.

There is evidence that drugs may decrease response rates in a manner consistent with the behavioral momentum model (sect. 10.2.1; see also Hoffman et al. 1987), but clearly this effect is not consistent. Cohen (1986) presented a wide dose range of four drugs to rats responding under chained variable-interval (VI) 30-s VI 30-s, multiple fixed-interval (FI) 30-s FI 120-s, and multiple VI 30-s VI 120-s schedules. Response-rate reductions following drug administration were not related to rate of reinforcement in components of these schedules, as the behavioral momentum model predicts, and it is unclear why studies with drugs produced different results. Furthermore, when response-independent food is delivered simultaneously with response-contingent food, there is no systematic relationship between rate of reinforcement and relative reduction in response rate (Cohen et al. 1993; Nevin 1984). For example, in one experiment pigeons responded under a multiple fixed ratio (FR) 30 FR 60 FR 90 schedule (Cohen et al. 1993). Free food delivered under variable time (VT) 20-s and VT 40-s schedules during each component reduced response rates but not in a manner consistent with the behavioral momentum model, that is, there was no relationship between rate of reinforcement and resistance to change.

A more serious challenge to the behavioral momentum model comes from studies that have used single, rather than more complex, schedules of reinforcement. In a single schedule, only one schedule (e.g., VI 30-s) of reinforcement operates within a session, and that schedule is correlated with the entire stimulus context of the experimental chamber. In a complex schedule (e.g., multiple, chained, concurrent), more than one schedule operates within the session and each schedule is correlated with a different discriminative stimulus. Most studies that support the behavioral momentum model have used complex schedules of reinforcement. There are studies with single schedules of reinforcement that have also shown a direct relationship between rate of reinforcement and resistance to change (e.g., Church & Raymond 1967). However, there are other studies that have examined single schedules of reinforcement and have not shown this relationship (e.g., Clark 1958; Cohen 1998; Cohen et al. 1990; 1993). Cohen et al. (1990) trained rats to press a lever for food under FR 40, 80, and 160 schedules across successive conditions. Resistance to extinction was not directly related to rate of reinforcement, even though rats had extensive baseline training, and data were analyzed as proportions of baseline response rates. Rather, the partial reinforcement extinction effect (PREE) was observed, with greater resistance to extinction under the FR 160 than under the FR 40 schedule.

In a later study, Cohen et al. (1993) performed a systematic comparison of resistance to change in single and multiple schedules with rats and pigeons as subjects, using tests of prefeeding, response-independent food, and extinction. There was no direct relationship between rate of reinforcement and resistance to change in single FR, variable ratio (VR), FI, and VI schedules for rats or pigeons, but there was a direct relationship in the multiple schedules. Cohen (1998) also compared resistance to prefeeding and extinction in the same rats that were trained under both single VI 30-s and VI 120-s schedules across successive conditions and multiple VI 30-s VI 120-s schedules. With prefeeding there was no relationship between rate of reinforcement and resistance to change in the single VI schedules, but there was a direct relationship in the multiple schedules. In the single-schedule condition, resistance to extinction was greater under the VI 120-s schedule than under the VI 30-s schedule (i.e., the PREE), but again with multiple schedules resistance to extinction was greater under the VI 30-s schedule.

It is presently unclear why some single-schedule studies produce data consistent with the momentum model and other studies do not. Cohen's (1998) data suggest that the momentum model is most applicable in conditions where different rates of reinforcement and their correlated stimuli alternate frequently over a relatively short time span. N&G point out the potential applications of the momentum model in clinical interventions (sect. 10.0). A relevant question may be to what extent the "real" world is like a single schedule, and how much it is like a multiple schedule. Further research and modifications of the momentum model should answer these questions.

The role of context in choice

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Abstract: Nevin & Grace identify a difference between the predictions of delay reduction theory and the contingency-ratio account underlying behavioral momentum approaches to choice. This is shown not to be a true difference. The role of the overall context of reinforcement must be carefully incorporated by any theory of choice.

Behavioral momentum is a promising concept that does an admirable job of unifying and accounting for many of the important findings in the psychology of learning and motivation. Especially important is the suggestion that response rate depends largely on response-reinforcer relations and that resistance to change depends largely on stimulus-reinforcer relations. The theory also promises to make important contributions in applied areas including drug abuse and self-control. I will restrict my brief comments to the area of the target article that makes closest contact with our own work, namely, preference and reinforcement value in that venerable choice paradigm, the concurrent-chains approach.

According to delay reduction theory, the effectiveness of a stimulus as a conditioned reinforcer may be predicted most accurately by measuring the reduction in the length of time to primary reinforcement calculated from the onset of the preceding stimulus (Fantino 1969). Expressed differently, the greater the improvement (in terms of temporal proximity or waiting time to reinforcement) that is correlated with the onset of a stimulus, the more effective the stimulus will be as a conditioned reinforcer. Although this hypothesis has been extended to areas such as self-control, elicited responding, three-alternative choice, observing, operant analogues to foraging, percentage reinforcement, and the serial-position effect in short-term memory, the hypothesis was first developed to account for choice in the concurrent-chains procedure. Support for this theory comes primarily from the concur-

https://doi.org/10.1017/S0140525X00242404 Published online by Cambridge University Press

rent-chains procedure and from the successive-encounters procedure developed to assess aspects of optimal foraging theory (for references and background, see Fantino & Abarca 1985 and Fantino et al. 1993). Nevin & Grace (N&G) point out that while the accounts of preference provided by delay reduction theory and by the contingency-ratio account that underlies behavioral momentum theory are largely similar, "there may be an important difference" (sect. 6.2, para. 3). I will argue that this difference is more illusory than real. In presenting the argument, I will raise a theoretical question about behavioral momentum that the authors may choose to address in their reply.

N&G discuss Nevin's (1992a) clever experiment in which the interval between two multiple-schedule components is varied. As required by the contingency-ratio account, relative response strength in the two components is "roughly invariant with respect to the length of the time-out periods between components" (sect. 6.2) (which, of course, do influence the overall rate of reinforcement). N&G then make the following important point:

> If the initial-link choice periods that precede access to the terminal links in concurrent chains are functionally equivalent to the timeout periods that precede multiple-schedule components, initial-link length should also have no effect on preference. However, according to delay reduction theory and as shown by Fantino (1969), preference for the richer terminal link in concurrent chains varies inversely with the length of initial-link choice periods. . . . If resistance and preference are similarly determined, this difference must be resolved. (sect. 6.2, para. 3)

I submit that the intercomponent intervals in Nevin's (1992a) multiple-schedule study are more comparable with the intertrial intervals studied by Williams and Fantino (1996) and by Goldschmidt et al. (1998) with concurrent-chains schedules. These studies sought to answer the very question of whether time between schedules (or trials) is "functionally equivalent" to time in the choice phase (initial links). For example, as in Nevin's study with multiple schedules, Goldschmidt et al. (1998) manipulated overall time to reinforcement but did not change local contingencies on a given trial. We know, and the results confirmed, that lengthening the initial links (choice phase) has a dramatic and quantifiably predictable effect on preference. Would lengthening the time between trials (that is the prechoice interval) have the same effect? In both sets of studies, no matter what we tried, the answer was the same: The intertrial interval had no effect on preference. This raises the likelihood that the intercomponent intervals in Nevin's study are not at all "functionally equivalent" to the choice phase in concurrent chains, in which case the "important difference" between the two approaches may not be a difference at all.

I close with two questions. First, how does behavioral momentum theory deal with the very different effects produced by changing the duration of the choice phase and changing the duration of prechoice periods as in Williams and Fantino (1996) and Goldschmidt et al. (1998)? I suspect that the answer will be, "the same way delay reduction theory does – as in the Goldschmidt et al paper." Second, I am intrigued by how far one can go in discounting intercomponent durations and pose the following "thought experiment" (after Nevin, 1992a). Suppose that the intercomponent interval were better integrated into the rest of the multiple schedule (i.e., took on less of the trappings of "dead time" that the subject evidently begins to discount, both in Nevin's study and our own). In particular, instead of a 60-second period between the two components of the multiple schedule, apportion this period into 30 discriminable 2-second periods during the components. Would they now affect relative response strength? My suspicion is that indeed they would. Some subtle effects of reinforcement context on choice were reported by Jacob and Fantino (1988). N&G might explore how their view accounts for these data. More generally, I suggest that for both delay reduction theory and behavioral momentum theory, researchers should pay careful attention to the overall context of reinforcement in any constructed situation.

ACKNOWLEDGMENT

Preparation of this commentary was supported by NSF Grant IBN-9870900 and NIMH Grant MH57127-1 to the University of California, San Diego.

Experimenter momentum and the effect of laws

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Abstract: Nevin & Grace invoke a behavioral metaphor from the physics of momentum. The idealized assumptions they invoke are argued to translate to behavior only in the limited case of steady-state, constant-probability VI responding. Rather than further refine this limit case, mathematical models should be applied to generalizations of the limit case itself, broadening our understanding of behavioral processes.

Nevin & Grace present a metaphor of behavioral momentum in which frequently reinforced responding is increasingly difficult to disrupt. The analogy is good as far as it goes, but how far does it go? Is it a general principle of behavior from which to extrapolate clinical practice, or a mathematical "limit" case that models one quantitative relation at the expense of a more general understanding of behavioral processes?

The majority of this and other mathematical modeling in behavior analysis relies almost exclusively on rates of responding (typically pigeons' key pecking) under constant probability VI schedules. The prevalence of this procedure stems from its ability to muddle thoroughly momentary control over behavior. Although VIs generate wonderfully straight functions once the proper transforms and free parameters have been defined, one has to wonder how useful the uncovered "order" can be. In stripping away the moment, have we eliminated what makes behavior worth studying in the first place? If so, the limit case remaining (the ubiquitous VI) does little to model everyday experience, and as such can hardly be said to provide a general understanding of the behavioral processes at work.

This is not to disparage the development of mathematical models based on physics or any other science. We must recognize, however, the simplifying assumptions physicists use in generating and evaluting their models, and if they are adopted, be willing to abide by them. The physical model of momentum presented here, for example, describes the linear mechanics of completely elastic systems, in which momentum and energy are both conserved. Two billiard balls collide, and the velocity and kinetic energy of each is affected in a very specific, quantifiable fashion so that the total available to the system remains unchanged. (The authors present a model in which an environmental "disrupter" affects a "behavior," but in truth the analogy should have both affecting each other.) Furthermore, this physical interaction is entirely temporally symmetric: Reversing initial and resulting conditions does nothing to change the interaction or its mathematical characterization. In one temporal frame of reference, the balls enter from the left, collide, change course, and exit stage right. In the other, they enter from the right in mirror fashion and exit stage left. Newton cares not which direction the arrow points.

Is behavior such a system? Well, the answer depends, like physical systems, on your frame of reference and how closely you look. Clearly, the interaction between behavior and environment is not strictly elastic. Behavior is not conserved, that is, it does not continue unimpeded at a given strength without external support. Behavior is a dissipative system in the physicist's sense; energy is not conserved but is instead modified with each interaction. Environmental interactions add or subtract the energy associated with a

https://doi.org/10.1017/S0140525X00242404 Published online by Cambridge University Press

particular behavior. The occurrence of the behavior itself has an impact on future responses. Behaviors, unlike billiard balls, are not separate and independent events. If we align 1,000 meters and call it a kilometer, every meter therein is identical and interchangeable with every other. If we *run* 1,000 meters, however, few would argue every meter's interchangeability. Each meter run is different from the preceding one, *precisely because the preceding one preceded it.* Hence, although a collection of 1,000 responses generates a rate, it is a mistake to treat that rate as a singular entity. How those responses, and the consequences they generate, are distributed in time, can in only a few special circumstances be ignored. Under the vast majority of cases, the distribution of those responses and events are critically important to understanding either.

Thus, behavior, in addition to not being an elastic system, is also not temporally symmetric. Our billiard table model lacks a critical asymmetry. In truth the model works better if we first dip the balls in molasses. Now, each rolling ball changes the table, leaving a film of molasses where it passed, and leaving particular sticky pools around areas of past "collisions" with other responses and stimuli. Future rolls are neither entirely constrained or unconstrained, but will be affected whenever they intersect a previous path. Two special circumstances exist where previous rolls do not affect the current one: when no balls have rolled on the table previously (the very first response), and when the table is uniformly covered with molasses. The latter will occur only if we take extraordinary measures to ensure that collisions occur equally randomly around the table (i.e., if we explicitly discourage attraction to any particular portion of the table). Once the table is uniformly covered, all paths are again "isotropic" and a (now sticky) symmetry is restored. It is this characteristic that endows the constant-probability VI schedule with special significance as a behavioral baseline. By eliminating any temporal structure to behavior-environment interactions, it generates at asymptote a local behavioral arena that is, if viewed from sufficient distance, a uniform, gooey slate. Under these conditions, local response structure is isotropic and the asymmetry of acquisition yields to the symmetry of VI steady state.

In and of itself, this poses no problem, provided we recognize it as a particular limiting case of the model. What we do next is all-important. In modeling physical systems, there are two options. One is to recognize explicitly the various limiting conditions of the model and then begin independently relaxing each limitation systematically. The other is to maintain the limiting conditions, while increasing the complexity of the interaction studied. Once Galileo finished rolling individual balls down inclined planes, his successors could either investigate how changing the balls or the planes affected the relationship, or they could roll two balls down two identical planes and watch them collide in a central arena. The first requires a somewhat fresh start anchored firmly to the limit case (e.g., coefficients of friction can be defined for different materials, but they have to limit to zero on frictionless surfaces). The second builds on the established relation and extends it to more complex situations. The first generalizes, while the second further specializes. Our discipline has spent relatively little time on the former route and has increasingly taken the second path, progressively circumscribing an increasingly specialized behavioral paradigm that rarely reflects real-world behaviors. Are everyday responses truly "free" operants, emitted with equanimity at all points in time and under all circumstances, free of influence from prior responses, and generating consequences with a fixed but random rate? Few responses satisfy this description, and yet we model them as if they did. We seem to be caught up in a momentum of our own, imparted by the initial success of Herrnstein's formulation, wherein we cannot see past the limit case in which it works and begin to address the more difficult questions of why it does so.

The example of behavioral momentum illustrates the effect of laws, particularly when few in number. Laws tend to overly restrict the behavior of scientists and generate a momentum of their own. Although behavioral momentum may be an academically important refinement, what it adds to a general understanding of behavior is extremely limited because of the limited nature of the requisite paradigm. We applaud the attempt to model a behavioral relation mathematically, but wish to provide enough of a disruption to transfer some of this energy to expanding the limiting conditions, rather than further refining them. Can we make our mathematical relations handle temporally, topographically, and/or environmentally structured responding? Can we delineate the momentary changes that generate the steady-state limit case that forms our present mathematical foundation? If we cannot, we must seriously question the relevance of our models to everyday life, just as we would question physical models if they only worked in the idealized realm of the physicist but could not be extended to the broader, real world of the engineer.

Momentum feeds forward

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Abstract: Nevin & Grace present a fresh and thoroughly empirical argument undermining the traditional view of learning based on response contingent reinforcement. We endorse the main points in the target article and relate them to an ethological feed forward principle that complements the principle of behavioral momentum.

In the conventional view, response contingent reinforcement is the only plausible basis for so called "general process learning theory." Traditionally, the general process always works – except when it fails. Exceptions must be explained away one by one as unrelated artifacts of extraneous biological processes. The ethology of behavior appears in this traditional view as a fragmented antithesis of general process learning theory.

In contrast, arbitrary learning is a central feature of ethology. Von Frisch (1950, pp. 6–8) showed that bees under field conditions could learn to return to an arbitrary color after a single experience of food and color. Tinbergen (1951, pp. 97–100) showed that wasps in the field use arbitrary landmarks to return to nests and feed their larvae. Massive research documents the memory of birds for specific caches of food. Birds recover a high proportion of food that they cached themselves and a low proportion of food cached by other birds of the same species, indicating that they cache their food in arbitrary locations. Birds remember a staggering number of different caches, far beyond any feats of memory ever demonstrated in the operant conditioning chamber (Vander Wall 1990, pp. 158–69). They must remember each location after a single experience. Reinforcement must be irrelevant, because they consume all the food in each cache on the first return visit.

Learned patterns that at first seem unrelated to behavioral biology reveal their ethological roots on closer inspection. Popular animal shows amaze audiences with bears riding bicycles and porpoises walking backward on their tails, but successful trainers only capitalize on species-specific patterns (Hediger 1955). Male bears fight by striking with their fore paws while standing erect on their hind legs. They must balance on two legs to engage in this species-specific agonistic pattern. Bears transfer this two-legged pattern to dancing to music or riding a bicycle. Backward tail-walking appears spontaneously in untrained porpoises (Pryor et al. 1969). The dancing of the famous Lipizaner stallions is their species-specific mating display set to music. Arbitrary experience connects ethological patterns of response to arbitrary patterns of stimulation. Arbitrary learning emerges seamlessly from obligatory biology.

In the target article and throughout their work, Nevin & Grace (N&G) and their associates show that the common practice of calling food, water, and other commodities "reinforcers" creates much confusion. "If resistance to change is identified with re-

https://doi.org/10.1017/S0140525X00242404 Published online by Cambridge University Press

sponse strength, and reinforcement is presumed to strengthen responding, one must conclude that response-contingent reinforcement does not reinforce!" (Nevin et al. 1987, p. 29). In many common, easily replicated situations, increases in putative reinforcers yield decreases in reinforced responding. If a reinforcer is something that reinforces, then we cannot say that more reinforcers yield less reinforcement without doing violence to language. Such usage must confuse any discussion.

Suppose that we replace the interpretive term reinforcer with the descriptive ethological term sign stimulus or S*, a stimulus that evokes an ethological action pattern. Food evokes ethological patterns of lever-pressing and alley-running in rats and key-pecking in pigeons apart from any contingency (Gardner & Gardner 1988, pp. 430–40; 1988, pp. 152–69). Even in the case of so called "stimulus change reinforcement," stimulus change is an S* that evokes responding (Campbell & Sheffield 1953; Gardner & Gardner 1998, pp. 178–80). Putative reinforcers evoke the to-be-conditioned response, R^c, at the outset of experiments without any contingency whatsoever. More precisely, R^c begins early in an action pattern evoked by an S*, just as salivation begins earlier than chewing and swallowing in the action pattern of eating.

A mass of experimental evidence in the target article, plainly contradicts Skinner's (1938, p. 20) response-reinforcer (R–S*) formula. Taken literally, the R-S* formula implies that pressing and pecking blindly increase with reinforcement and only lead to lever-pressing and key-pecking by chance when these stimulus-independent responses happen to engage the levers or keys of an operant conditioning chamber. Rats do not press the air in the Skinner box, they press the lever. Pigeons do not peck the air in the Skinner box either, they peck the key. Real animals must always direct instrumental or operant responses at target stimuli.

Traditionally, Pavlovian conditioning is S-S*, but the concept of a behavioral unit without a response is as contrarian as the concept of a behavioral unit without a stimulus. Without some response indicator, it is impossible for any actual experiment to measure any actual effect of conditioning. In Pavlov's classical procedure, the food S* evokes an ethological action pattern that includes salivation. Furthermore, salivation begins early in the pattern. With repetition of light followed by food, salivation begins just before the usual time that food appears. Stretching out the time between light and food disturbs the pattern, at first. With repetition, however, Pavlov showed that salivation would appear as much as 30-min after the light, when that was the rhythmically repeated interval between light and food (Pavlov 1927/1960, p. 41). Salivation always appears just before the food as long as rhythmic repetition of light and food predicts the time that an S* will arrive. The light serves as a time signal rather than a food substitute.

All procedures for instrumental or operant conditioning include an S* that evokes an R^c at the outset of the experiment. Every time the apparatus in the operant conditioning chamber delivers food and every time that a rat finds food in the goal box of a maze, the procedure contains the elements of Pavlovian conditioning. Conditioning by contiguity must enter into every conceivable example of instrumental or operant conditioning. Is there anything that contingency can add to learning by contiguity alone?

R. Gardner and Gardner (1988; extended and amplified in Gardner & Gardner 1998) discuss the lack of evidence for the traditional notion of response-contingent reinforcement, the phlogiston of psychology. This target article, reviews a body of evidence that further undermines the traditional view. N&G's behavioral momentum acts forward like causal principles in the rest of natural science in welcome contrast to the teleological backward action of traditional response contingent reinforcement. The target article attributes behavioral momentum to S-S* association. The trouble with S-S* association as a principle of learning is that it entails stimulus substitution. The trouble with stimulus substitution is that the conditioned response R^c is distinctly different from the response to S*. An R^c is an appropriate response to an arbitrary stimulus that appears before S* (S^a) because it is an early fraction of the action pattern evoked by the S*: prefeeding as opposed to eating, predrinking as opposed to drinking, and so on.

While heartily agreeing with the target article on the experimental events that produce behavioral momentum and support resistance to disruption, this commentary urges Nevin & Grace to consider S^a-R^c contiguity as the mechanism of conditioning.

Amassing the masses

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Abstract: Nevin & Grace (N&G) buttress their metaphor with some good props. However, it is still not clear what momentum is analogous to. If momentum is a measure of strength, then the authors should say so *and* tell us how to calculate it. Furthermore, if "other" behavior can be introduced into the equation (and N&G's foray into the applied world suggests that it can), it is unclear when the masses are accrued and how much is accrued to each behavior.

The trouble with a metaphorical analysis is that sometimes you can't always see the wood for the trees. If velocity is analogous to response rate, and mass is analogous to resistance to change, what is behavioral momentum analogous to? Surely, behavioral momentum *is* that hypothetical construct we've all come to know and love so much: response strength. However, if resistance to change is analogous to response strength, as Nevin & Grace (N&G) suggest, then we've been using the wrong dependent variable; worse yet, N&G don't make it clear how to use the right one, momentum.

In section 2.4, they use the analogy of a "reinforced" concrete wall to explain the concept of behavioral mass. A better analogy was given by Nevin at a conference: two trucks traveling at constant (but different) speeds, one carrying a heavier load. The different speeds represent the different response rates in two components of a multiple schedule and the mass differential represents the resistance to change of responding in the two components. Crucially, we can't *see* the mass differential until both drivers simultaneously apply the brakes (analogous to extinction?) and the heavier truck takes longer to stop.

But hang on. If the truck takes longer to stop because it has more mass, will it not also take longer to get going? This means that latencies should be longer for behaviors with more mass. Conversely, latencies should be shorter for lighter behaviors. Is there any evidence for this? If so, this would strengthen N&G's metaphor but would also have serious measurement implications, because rate includes latency and anything that increases latency should decrease rate.

Measuring relative resistance is a nifty idea, however. No problem for two operants or a single operant in two components of a multiple schedule. But what about a single operant maintained by a single schedule? Presumably, "other" behaviors can also acquire mass, right? Why not measure resistance relative to "other" behavior? Problem. Just what is the response-independent free-food disrupter doing? Reinforcing and amassing other behaviors?

Of course, for applied behavior analysts such as myself, the most important implication of Nevin & Grace's work is for the assessment and treatment of problem behaviors. Reinforcing "other" behaviors incompatible with an undesirable behavior is a common tool for us. It will decrease the rate of the undesirable behavior. So far, so good. But Nevin & Grace suggest that this intervention might inadvertently increase the mass (resistance) of the undesirable behavior. This is bad news. But when does it do that? What are the laws for allocating mass to the target behavior versus the "other" behavior? If *both* get heavy, that would also be good news for a behavior analyst trying to maintain an appropriate behavior

https://doi.org/10.1017/S0140525X00242404 Published online by Cambridge University Press

in a client's repertoire. Quite simply, we need to know more than they're letting on.

Problems with the concept of force in the momentum metaphor

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Abstract: Although the momentum metaphor is successful in many ways, there remain problems with the adequacy of the notion of a force in the behavioral sense and the question of whether the conditions used to apply force can truly be separated from the conditions that establish and maintain behavioral mass.

The metaphor of behavioral momentum outlined by Nevin & Grace (N&G) has a great deal to recommend it in terms of both practical utility and generality. Two important challenges to the generality of the momentum metaphor are noted by N&G. One challenge arises from the failure to demonstrate differential resistance to change across multiple-schedule components when drugs are used to disrupt responding (Cohen 1986). Care needs to be taken, however, when interpreting the effects of drug-induced disruption on responding, because drugs may simultaneously degrade the stimulus control exerted by the discriminative stimuli (e.g., by impairing perceptual processes). Consequently, the conditions being used to maintain a differential resistance to change (the stimulus-reinforcer contingencies according to N&G) are not clearly associated with a given component. Harper (1999) demonstrated that whereas drugs that disrupt stimulus control fail to produce a differential resistance to change in responding dependent on baseline reinforcer conditions, drugs that do not disrupt stimulus control produce alterations in responding that are consistent with N&G's theory. Therefore, many drugs may not lend themselves to an analysis in terms of resistance to change, not because of a problem with the generality of the momentum metaphor, but because of multiple and confounding effects of those drugs on behavior.

Another challenge to the generality of the momentum metaphor comes from studies that have sought to disrupt responding via changes in maintaining within-component reinforcer conditions (e.g., Harper & McLean 1992). Such studies have found that responding in the presence of the discriminative stimulus associated with the lower rate or amount of reinforcement does not necessarily change more relative to baseline than responding associated with the higher rate or amount of reinforcement. Recent findings, however, suggest caution when interpreting the effects of within-component reinforcer manipulations on resistance to change. For example, Harper (1996) demonstrated that applying force (in terms of the momentum metaphor) via changes in maintaining reinforcer rate combine with those aspects of the reinforcement context being used to establish a resistance differential across components (e.g., reinforcer duration in the case of Harper & McLean 1992). Effectively, within-component alterations in reinforcer conditions confound the very conditions being used to maintain a differential resistance to change. For example, a change in the reinforcer rate from 60 to 20 reinforcers per hour applies a greater force to responding that is maintained by 2 seconds of access to food reinforcement compared to responding that is maintained by 6 seconds of access to reinforcement. Therefore, as with the drug research, an inability to demonstrate differential resistance to change following within-component manipulations may not be the result of a problem with the momentum metaphor itself but a problem in the way force is applied to assess behavioral resistance.

Although the momentum metaphor may turn out to be relatively resistant to the apparently contradictory evidence reviewed above, there remain several issues that challenge its generality and adequacy, especially with regard to the idea that various forms of disruption (e.g., response-independent food, extinction, prefeeding) act in a manner analogous to the physical concept of force. One major problem arises from a closer consideration of the issues surrounding within-component forms of disruption. Specifically, extinction not only disrupts responding, and thereby provides a means to assess resistance to change, but also removes the mass-producing conditions (i.e., the reinforcing conditions that establish the relevant stimulus-reinforcer contingencies). Therefore, introducing extinction should not reveal a differential resistance to change across components in which responding was originally maintained by different reinforcer rates. However, the studies employing the extinction procedure are some of the most commonly cited studies used to demonstrate the generality of the momentum metaphor. It is unclear how a disruptor such as extinction can simultaneously remove the conditions maintaining the stimulus-reinforcer contingencies but still produce a differential resistance to change (e.g., Nevin 1992b; Nevin et al. 1984).

In Equation 17, N&G suggest a modification to their basic model that incorporates an additional parameter (d_r) that might capture the additive effects of terminating the reinforcer rate in extinction. However, their approach not only fails to account for the observation that extinction sometimes produces increases in responding following continuous reinforcement (Mackintosh 1974) but also the observation that response rates can increase following the introduction of short durations of free food during an intercomponent interval (Harper 1996). Therefore, although the momentum metaphor spelled out in N&G's equations may work well in describing the extent of response rate decreases, it does less well accounting for response rate increases following the same type of disruptor or force.

The adequacy of the notion of disruptive events acting in the same way as a force in classical physics is also questioned by the observation that in the physical sense a constant force continues to slow a moving object (e.g., a brake continuously applied to a car rolling down a hill continues to decelerate the car until it stops), but, with regard to behavior, force does not necessarily appear to act in this same manner. For example, Harper and McLean (1992) demonstrated that in the first session of intercomponent food disruption, responding reduced to an extent dependent on the reinforcer conditions associated with responding in each component. This reduction in responding was still present even after 15 to 30 sessions of exposure to the disruptive event – but the extent of disruption was no greater the longer subjects were exposed to it. Thus, unlike a physical force, such as a constantly applied brake on a car, a behavioral force appears to suppress responding to a given level but then maintain it there.

Perhaps the greatest challenge is to elucidate exactly what is being disrupted when a force is applied. According to the momentum metaphor, responding does not decrease because of a disruption to stimulus-reinforcer contingencies (because these contingencies maintain the differential resistance to change observed across components). Perhaps such manipulations produce a weakening of the response-reinforcer relationship. But if a weakening of response-reinforcer contingencies is the mechanism by which a force reduces responding, we are left with the interesting situation that although reinforcement is coming from one source (e.g., food from a hopper), its relationship with the response it precedes appears to be quite independent to its relationship with the discriminative stimuli that preceded that reinforcement. That is, disruption degrades the value of the reinforcer in the context of the response-reinforcer contingency, while simultaneously maintaining the value of the reinforcer in the context of the stimulus-reinforcer contingency.

https://doi.org/10.1017/S0140525X00242404 Published online by Cambridge University Press

Implications of behavioral momentum for understanding the behavioral pharmacology of abused drugs

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Abstract: We briefly discuss some potential contributions of behavioral momentum research to the study of the behavioral effects of abused drugs. Contributions to the study of the direct effects of drugs on operant responding and to the study of drugs as reinforcers are addressed. Too little empirical evidence is available to thoroughly evaluate the relevance of behavioral momentum concepts to the study of drugs and behavior, but we note several reasons for optimism regarding its potential to make positive contributions.

Our comments address some possible implications of behavioral momentum for understanding the behavioral pharmacology of abused drugs. We are less concerned with the merits of the behavioral momentum metaphor per se, and more interested in considering possible implications of Nevin & Grace's (N&G's) notion of there being two fundamental aspects of the discriminated operant: rate of responding and resistance to change.

At the outset, we note the many substantial contributions that the field of operant conditioning has made to the study of the behavioral effects of abused drugs. A substantial proportion of all experimental behavioral research conducted on drug abuse involves operant conditioning procedures in some manner. To the extent that the historical record can be our guide, a safe assumption is that if behavioral momentum is important to understanding operant behavior, and N&G make a compelling argument that it is, it likely has much to contribute to our understanding of the behavioral effects of abused drugs.

In considering where in drug abuse research the potential of behavioral momentum may lie, we can divide the field into studies on the direct effects of drugs, that is, how drugs change rates of operant responding, and studies on the stimulus function of drugs, wherein the drugs themselves function as behavioral antecedents and consequences. With regard to direct effects, a robust experimental literature demonstrates that baseline rates of responding and the scheduled relationship between responding and its consequences are as important as the chemical structure of the compounds in determining how drugs affect operant responding (Dews & Wenger 1977; Kelleher & Morse 1968). Put simply, these studies have established the fundamental importance of the contingencies between the response and the reinforcer aspects of the discriminated operant as determinants of the behavioral actions of abused drugs. A reasonable question for this field is whether insufficient scientific attention has been allocated to understanding the extent to which drugs affect the resistance-to-change aspect of the discriminated operant. Excellent studies have demonstrated, for example, that drug effects on operant responding can be modulated by increasing stimulus control (Katz 1988; Laties 1972), but nothing comparable to the large body of work on the influence of schedules of reinforcement on the direct effect of drugs has been undertaken. This state of affairs may well reflect the relative importance of the two aspects of the discriminated operant to understanding the direct behavioral effects of drugs. That possibility notwithstanding, additional research examining how abused drugs influence the resistance-to-change aspect of the discriminated operant is warranted. For example, quite practical questions about how acute and chronic drug use affects resistance to change of behavior maintained by social, monetary, and other common classes of reinforcers would be informative.

With regard to the stimulus function of drugs, especially reinforcing effects, behavioral momentum research appears to have potentially important implications. There is an extensive scientific literature demonstrating that abused drugs can function as unconditioned positive reinforcers, and that those reinforcing functions are central to the genesis and maintenance of drug abuse (Griffiths et al. 1980; Higgins & Katz 1998). While little research has been conducted with the explicit purpose of examining the generality of behavioral momentum to drug-maintained responding, nevertheless there is evidence supporting its applicability. Some of the strongest evidence not already mentioned in the target article comes from studies using progressive-ratio (PR) schedules of drug reinforcement. Under a PR schedule, the response requirement for reinforcer delivery is progressively increased until responding ceases. The ratio at which responding ceases is termed the breakpoint, and is routinely used to compare the reinforcing efficacy of different drugs or drug doses. Consistent with behavioral momentum, breakpoint generally increases as a function of increasing dose (i.e., increasing reinforcer magnitude) across a wide variety of abused drugs, although decreases are sometimes observed at the highest doses (Stafford et al. 1998). Breakpoint came to be used in this area of inquiry as researchers realized that rates of responding were influenced by factors not directly related to the maintaining event's reinforcing efficacy and thus could be misleading. In the terms of behavioral momentum, breakpoint provides information about a drug's resistance to change, while response rate often does not. The research practices in this area reflect an implicit understanding of that distinction, but behavioral momentum provides a conceptual framework for making that distinction explicit and predictable. We see no reason to assume that this is an isolated example, and feel that behavioral momentum may make important contributions to understanding other aspects of the reinforcing effects of drugs.

Other interesting examples of the applicability of behavioral momentum to understanding drug abuse are found in clinical studies on contingency-management interventions (Higgins & Silverman 1999). In these studies, cocaine, heroin, or other substance abusers earn nondrug reinforcers (e.g., vouchers exchangeable for retail items) contingent on providing objective evidence of recent drug abstinence (e.g., negative urine toxicology results). These interventions are effective at increasing drug abstinence, but there are clear individual differences in how well they work. Consistent with predictions from behavioral momentum, one of the best predictors of patient response is the baseline frequency or amount of drug use (Preston et al. 1998). Patients with greater baseline drug use have a lower probability of a positive outcome. Also consistent with behavioral momentum, increasing the value or magnitude of the nondrug reinforcer increases abstinence in patients who were unresponsive at lower incentive values (Silverman et al., in press). Finally, patients who achieve the greatest duration of continuous abstinence during treatment, and the attendant increased material and social reinforcement associated with abstinence, have the highest probability of abstinence at post-treatment follow-up (Higgins et al., in press). This latter observation suggests a possible example of a healthy repertoire gaining mass during the rehabilitation process. Again, we find the behavioral momentum framework potentially helpful in organizing these empirical observations.

Space limitations prevent us from elaborating further on the potential we see for the concepts and findings of behavioral momentum to enhance research on the behavioral pharmacology of abused drugs. We commend Nevin & Grace for their informative review of this interesting and important area of research.

ACKNOWLEDGMENTS

Preparation of these comments was supported in part by research grants DA09378 and DA08076 from the National institute on Drug Abuse.

https://doi.org/10.1017/S0140525X00242404 Published online by Cambridge University Press

A passel of metaphors: "Some old, some new, some borrowed . . ."

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Abstract: Despite corrigible details, Nevin & Grace forge a clearer place for persistence as a fundamental attribute of motivated behavior and assay converging experimental operations in its measurement.

Strength, force, mass, resistance, extinction, and momentum are all borrowed objects, and, like neighbors' lawn mowers, they are inevitably modified before they are returned, if they ever are. All models start as metaphors; if they are taken seriously, they are tinkered with until they acquire a meaning *sui generis.* Mathematics is a precision tool for tinkering, and pieces of it inevitably get left behind in the metaphor, until the metaphor grows into a math model and is metaphor only in name. We are watching this happen to behavioral momentum.

Question 1: What's the problem that *momentum* is borrowed to fix? That's easy. The field lacks a theoretically justified dependent variable. Response rate won't do. Response rate, probability, and latency aren't highly correlated just by chance: Reinforcement affects some common thing, of which these are consequences. Versions of this thing have been called drive, excitatory potential, strength, intention, and so on. These are latent variables: Section 2.4 of the target article draws out their structural natures, which require environmental stress to become manifest as functions and be measured. Because they are latent, more or less of the independent and dependent variables have been crafted into them, giving each a different flavor. This is the kind of tinkering that Nevin and Grace are about.

Question 2: Given the long history of strength-type variables, is there anything new here? Yes. Two constructs are convincingly dissociated experimentally (sect. 3.4): One governs rate, and so on, and the other resistance to disruption. The latter is called behavioral *mass,* signified as *m.* This is the time constant in the exponential relation between response rate B_x and magnitude of a disrupter, *x;* from Equation 15 in the target article:

$$B_x = B_0 e^{-x/m}, \tag{1}$$

where B_0 is the rate in baseline ($x = 0$). The manufacture of a data base showing that masses as measured by resistance to change and by preferences are equivalent, and are subsumed in the same structure (Figs. 7 and 8), is new and important.

Question 3: What is the model like? In this journal, Nevin (1994) pointed out how my (Killeen 1994) model of schedule performance could account for resistance to change. In that model, the *specific activation* (a) gives the number of seconds of behavior that is elicited by each delivery of an incentive under the operative motivational conditions. (A separate parameter, the coupling coefficient, represents the contingencies of reinforcement that direct such incitement to one or another response form, and thus determine the measured response rate). Disruption of performance decreases *a,* by satiating or otherwise demotivating the organism. Nevin showed the conformity of my model to data like those pictured in Figure 2. Here I return the favor.

Where rate of reinforcement is constant and the specific activation/disruption (a_x) varies, my mechanics gives the response rate relative to baseline as approximately:

$$B_1 = \frac{B_{1,x}}{B_{1,0}} = \frac{r_1 + 1/a_0}{r_1 + 1/a_x} \tag{2}$$

Resistance to change is the (inverse) slope of this relation as x is varied. Slope is the derivative with respect to *x:*

$$\frac{dB_1}{dx} = \left(r_1 + 1/a_0\right)\left(a_x r_1 + 1\right)^{-2} \frac{da}{dx} \tag{3}$$

We do not know how specific activation changes with the disrupter (da/dx), but that cancels out of the ratio of resistances for different responses:

$$\frac{m_{r1}}{m_{r2}} = \frac{\left(r_2 + 1/a_0\right)\left(a_x r_1 + 1\right)^2}{\left(r_1 + 1/a_0\right)\left(a_x r_2 + 1\right)^2} \tag{4}$$

Now, this is a complicated expression that does not look at all like Equation 5 in the target article. But its predictions for a range of rates of reinforcement (varying r_1 and r_2 from 9 to 380 reinforcers per hour) for two representative values of a (25 s and 100 s) are shown here in Figure 1. The results approximate power functions whose exponents bracket the authors', shown in their Figure 5. The larger exponent in the bottom figure suggests that Nevin & Grace will find increases in b at higher values of a (better reinforcers, hungrier subjects).

What does this mean? Because their Equation 5 is so much simpler than my Equation 4, it is reasonable to prefer it. The present derivation gives that empirical regularity theoretical justification.

Question 4: So, what's blue? I am, because of the short shrift given to the central character, momentum, and some other modelling details. The metaphor of mass would mean that responses with larger m would not only decrease slowly; they would also increase slowly, as in recovery from disruption. I doubt it. Section

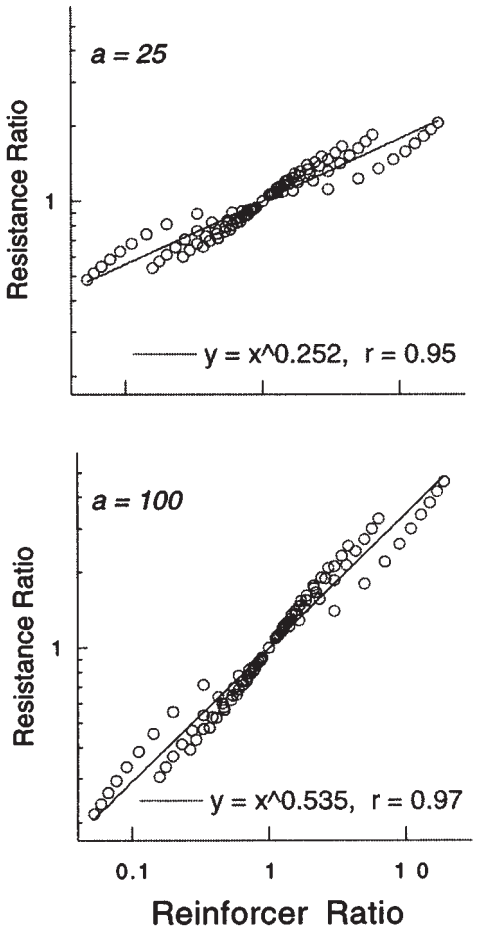


Figure 1 (Killeen). Equation 4 predicts mass at various rates of reinforcement and values of specific activation (a). Power functions have exponents $b = 0.25$ in the top panel and 0.54 in the bottom panel. This shows that Nevin & Grace's Equation 5 and Figure 5 are consistent with, and may derive from, an alternative model in which strength is a hyperbolic function of reinforcement rate.

https://doi.org/10.1017/S0140525X00242404 Published online by Cambridge University Press

9.3 confuses me, both in its goals and means. Dimensional consistency is not just political correctness; it's a useful tool (Stephens 1994) even for those oppressed by it. The authors note of Equations 16 through 18 that "units . . . must be such that [a logarithmic quantity] is dimensionless" (sect. 9.3). Absolutely true. But maybe not possible; and certainly not demonstrated. For those equations to be correct, their numerators must have the same dimension as their denominators, which requires that they be raised to the same power; this forces a to unity (or the raising of m and x to the power a). Tying up these loose ends will eventually lead to a tighter model.

Section 10 reviews the important implications of behavioral momentum in the engineering of behavior. But, wait a minute now, just what *is* momentum? Resistance to change? No. That's behavioral mass. Let's see, it must be here somewhere . . . nope. Can't find it. Let's make it.

Classically, momentum is the product of mass and velocity, and the authors have identified velocity with response rate, so momentum has to look like $p = mB$. Integrate Equation 1 over all values (0 to ∞) of the disrupter x to derive the total number of responses that will be emitted at all values of disruption. It gives momentum:

$$B_{\text{Tot}} = mB_0 \tag{5}$$

Behavioral momentum is simply the total amount of behavior that will be emitted as we sweep through the spectrum of all levels of disruption. If the disrupter is an extinction process, it is the total number of responses in extinction; a dependent variable that came under appropriate – and now ironic – criticism by Nevin (1988) as conflating rate and persistence. It does. But now that Nevin & Grace have deconstructed those factors, their recombination makes a new, and profound, sense of a classic dependent variable: Strength = momentum = output at all levels of disruption.

ACKNOWLEDGMENT

This research was supported by NSF grant IBN 9408022 and NIMH grant K05 MH01293.

Does conditioned suppression measure the resistance to change of operant behaviour?

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Abstract: Although conditioned suppression has face validity as a technique for assessing resistance to change of operant behaviour, it is not discussed by Nevin & Grace. However, application of their approach to the results of a conditioned suppression study that varied food deprivation and reinforcement magnitude (Leslie 1977) produces paradoxical results.

Nevin & Grace's (N&G's) account of behavioural momentum provides an opportunity to reexamine some puzzling data in the literature. One potential application of the notion of behavioural momentum is to the phenomenon of conditioned suppression, where positively reinforced operant behaviour is suppressed to an extent by the presentation of a stimulus (CS) that is correlated with an aversive event (US). Previous theorists (e.g., Millenson & De Villiers 1972) have construed this in terms of resistance to change and have suggested that pre-aversive CS presentation reduces the current level of motivation for the operant behaviour, and that consequently more highly motivated operant behaviour is less likely to be suppressed during CS presentation.

This hypothesis was examined experimentally by Leslie (1977). In that study, it was found that varying the level of food deprivation (between 100% and 75% of free-feeding body weights across blocks of experimental sessions) altered both the operant response rate (lever pressing of rats, maintained by a variable-interval schedule of 10% sucrose solution reinforcement), and the response rate during a CS followed by a brief foot-shock in a roughly linear fashion. However, varying the concentration of the sucrose solution used as the operant reinforcer across 5-day blocks (with 0%, 2%, 4%, 8%, and 16% solutions used on successive sessions) raised the baseline (VI) operant response rate much more than the CS response rate. These data are presented in summary form in Figure 1 (upper and middle panels).

Data from an additional experiment are also given in Figure 1 (lower panel). In this second experiment, it was found that the diverging functions for baseline and CS rate obtained by varying sucrose concentrations across sessions could be replicated by simply using this 5-day sequence (with 0%, 2%, 4%, 8%, and 16% solutions as reinforcement for lever pressing on a variable-interval schedule on successive sessions) at high (80% of free-feeding body weights) or low high (100% of free-feeding body weights) levels of food deprivation. Consequently, it was concluded that presentation of a pre-aversive CS had an effect that was equivalent to that of reducing food deprivation but not to that of changing the magnitude (concentration) of the reinforcer. In another terminology, CS presentation mimicked the effect of a motivational change but not that of a change in incentive.

N&G cite Nevin et al.'s (1990) finding – that within a multiple schedule, key pecking in one component was at a lower rate (because of response-independent reinforcer delivery) but was more resistant to change through prefeeding – as evidence that baseline response rate is independent of resistance to change. Leslie's (1977) findings show that baseline response rate is not an invariable predictor of the amount of resistance to change by a pre-aversive CS. It would be consistent with N&G's analysis to conclude that the higher response rates maintained by a higher level of food deprivation (see Fig. 1, upper panel) are, in effect, associated with a higher rate of reinforcement in the context of the Skinner box than the higher rates maintained by higher magnitudes of sucrose concentration (Fig. 1, middle panel), because the former were more resistant to change than the latter.

There is no direct evidence within Leslie's (1977) experiment that, for example, intermittent delivery of 10% sucrose solution to rats maintained at 75% body weight constitutes a higher rate of reinforcement than delivery at the same rate of 16% sucrose solution to rats maintained at 85% body weight, but it is a reasonable hypothesis. This is especially true in light of the often-observed insensitivity of the response rate of rats maintained by intermittent reinforcement to changes in reinforcement magnitude (see, e.g., Leslie & Toal 1994). However, the pattern of findings with sucrose solution manipulations in Leslie (1977) does seem to be itself inconsistent with Nevin & Grace's general thesis.

In a part of Leslie's (1977) conditioned suppression experiment not reported in Figure 1, concentration of the sucrose used as the reinforcement for lever pressing was varied between 20%, 10%, and 5%, either in alternating sessions (with 5%, 10%, and 20% each occurring twice in a semirandom sequence in each block of six sessions) or across blocks of 20 sessions. Neither procedure produced any reliable change in either baseline rate or CS rate. This led to use of the increasing sequence of sucrose concentrations (starting from 0%) for which the data are reported in Figure 1 (middle panel), when baseline and CS rates did change with the concentration of sucrose used as a reinforcer. If the conditioned suppression procedure is an acceptable technique for assessing resistance to change (N&G do not directly address this point), then this set of results poses considerable problems for the generality of the notion of behavioural momentum. With alternating of blocks of sessions, the baseline lever-pressing rates did not change, which is not itself a problem, but neither did the CS rates – and it would be predicted that that the rates maintained by the weakest reinforcer (5% sucrose solution) would be the most suppressed. When sucrose concentration increased in a fixed sequence across sessions, baseline rates did increase with reinforcer

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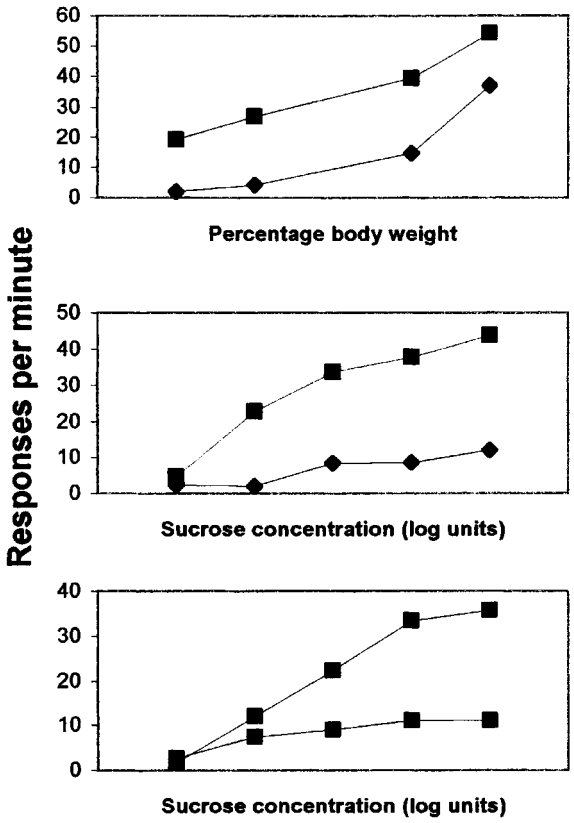


Figure 1 (Leslie). *Upper panel:* Baseline response rates (squares) and CS response rates (diamonds) for rats at 100%, 95%, 85%, and 75% of free-feeding body weights. Average data ($n = 4$) from Leslie (1977, Experiment 1). *Middle panel:* Baseline response rates (squares) and CS response rates (diamonds) for rats reinforced for lever pressing with sucrose solution at concentrations of 0%, 2%, 4%, 8%, or 16%. Average data ($n = 4$) from Leslie (1977, Experiment 1). *Lower panel:* Average response rates ($n = 3$) from Leslie (1977, Experiment 2), for rats reinforced for lever pressing with sucrose solution at concentrations of 0%, 2%, 4%, 8%, or 16%, at a high level of food deprivation (upper curve) or at a low level of food deprivation (lower curve).

magnitude, but the higher rates maintained by higher concentrations tended to be more (rather than less) suppressed by the pre-aversive CS.

N&G's final conclusion is that "the greater the value of the situation, as . . . measured by preference, the greater the strength of connection as measured by resistance to change" (sect. 12). This is an attractively simple notion, but perhaps (for reasons not yet identified) conditioned suppression will have to be excluded from the diverse list of techniques that can be used to assess resistance to change, given that differences in sucrose concentration as a reinforcer – which have been found to strongly influence preference in many published studies – either had no effect or had an effect opposite to that predicted on resistance to change by a pre-aversive CS.

Self-control's momentum outside of the laboratory

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Abstract: The goal of therapy is often to increase the momentum (persistence) of self-control behaviors. Determining how best to accomplish this goal necessitates conducting behavioral momentum research under a wider variety of conditions.

Nevin & Grace (N&G) have amassed an impressive amount of data in support of the analogy between the psychological concepts of response strength and preference and the physical concepts of velocity and mass (and their product, momentum). Their target article shows how this analogy can be used both to group together much seemingly disparate data and to make new predictions.

One of the aims of this article is to suggest possible clinical applications of the concept of behavioral momentum. N&G suggest that the behavioral momentum model can be useful in designing clinical interventions that result in behavior that occurs reliably after therapy is over, behavior that is resistant to change and that persists. They consider several possible specific areas of application, one of which is self-control (choice of a larger, more delayed reinforcer over a smaller, less delayed reinforcer; Logue 1995). [See also Logue: "Research on Self-Control" BBS 11(4) 1988.]

One of the primary suggestions that N&G give for how to increase the persistence of behavior such as self-control that has been established during therapy is to provide additional reinforcers during therapy, reinforcers that are independent of any responses that the client makes. N&G present several pieces of data in support of this notion. Discussed in most detail are the findings of Nevin et al. (1990). In that research, pigeons pecked response keys in order to obtain food. When the pigeons were given additional food reinforcers that did not depend on the pigeons' key pecking, pecking was more likely to persist both during extinction and when the pigeons had been fed prior to the start of the session. Therefore, N&G suggest, perhaps having people listen to reinforcing music in their usual environment will also make it more likely that people will show self-control and will persist in not choosing smaller, less delayed reinforcers.

The difficulty with this proposal is that the data on which it is based are very limited. To date, it appears that all of the experiments that have been conducted showing that behavior is more resistant to change if additional, noncontingent, reinforcers have been present in the environment have used pigeons, key pecking, and food reinforcers. In addition, none of these experiments have used a self-control paradigm. Furthermore, we know that, in general, humans tend to show more self-control than do pigeons, and they show far more self-control for points exchangeable for money than they do for food (Forzano & Logue 1994; Tobin & Logue 1994). For all of these reasons, the available data may not tell us very much about whether or not humans tested in a self-control paradigm in which they are responding in a variety of different ways for a variety of different reinforcers would show more self-control if they were given additional, noncontingent reinforcers that were qualitatively different from the contingent reinforcers. Predicting the effect of added, noncontingent reinforcers on humans' self-control under conditions outside of the laboratory is even more difficult.

There are many reinforcers for demonstrating practical applications of basic operant conditioning research, such as the research described in the target article. For example, such demonstrations can make it more likely to obtain funding for research, can suggest ideas for additional experiments, can increase nonspecialists' statements of approval of the research, and might even improve treatment protocols. However, very often, operant conditioners, in their (laudable) wish to be as precise as possible, con-

https://doi.org/10.1017/S0140525X00242404 Published online by Cambridge University Press

duct their experiments under extremely limited conditions, thus greatly limiting the generalizability of their findings. The research on behavioral momentum is no exception to this trend, although the concept of behavioral momentum is beginning to be used by researchers working with human subjects and by behavior analytic therapists (see, e.g., Plaud & Gaither 1996).

Nevertheless, if N&G want their metaphor to have momentum outside of the laboratory and to show persistence, the relevance of this metaphor to the world outside of the laboratory must be enhanced by conducting the research using different types of subjects, reinforcers, and situations, and by conducting more research in situations similar to the world outside the laboratory. If there is some value to extending the behavioral momentum analogy to the world outside of the laboratory, if the behavioral momentum analogy has any validity except under very specific laboratory conditions, more researchers need to conduct their research in such a way as to provide links between the world of the laboratory and the world outside of the laboratory.

The concept of behavioral momentum was first formulated almost two decades ago. For this concept to have lasting impact, the challenge for twenty-first century researchers is to carry this concept's momentum out of the world of the laboratory.

Clinical applications of behavioral momentum

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Abstract: An important measure of the validity and utility of basic behavioral research is the extent to which it can be applied in real life. Basic research on behavioral momentum and the model unifying choice and resistance to change (Nevin & Grace 1999) has stimulated the development of behavioral technologies aimed at increasing the persistence of adaptive behavior and decreasing maladaptive choices.

Basic researchers in psychology are concerned primarily with the specification of behavioral laws. Early learning theorists (e.g., E. L. Thorndike, J. B. Watson, B. F. Skinner) deliberately pursued research programs aimed at describing lawful relations that were characteristic of behavior *in general.* However, as basic researchers have refined these laws with increasing precision and quantification, some have questioned whether contemporary basic research informs us about behavior-environment relations relevant to human behavior in naturalistics contexts (Baer 1981; Mace 1994). An important measure of whether functional relations obtained with nonhuman subjects are pertinent to humans is the degree to which basic research findings can be applied in real-life. Even more important is the extent to which basic research stimulates new developments in behavioral technology that otherwise would not be considered.

Nevin & Grace (N&G) are addressing two aspects of behavior that are central to work in clinical psychology: choice and persistence. Modern behavior analytic views of abnormal behavior consider it to be a learned pattern of responding, the result of an interaction among genes, learning history, and current environment. Abnormal acts are considered choices to behave in one way versus another, and reflect a preference for the consequences abnormal behavior produce relative to the consequences available for alternative, more adaptive behaviors (McDowell 1982; Myerson & Hale 1984). Choice is a key concept in clinical psychology because understanding why individuals choose to behave abnormally is the first step in formulating a therapeutic intervention that discourages abnormal choices and promotes adaptive ones. Likewise, clinical psychologists are concerned with discouraging the persistence of abnormal choices, while at the same time increasing the persistence of adaptive behavior when individuals encounter novel situations or when treatments are unreliably implemented.

N&G argue that relative frequency (choice) and persistence are independent measures of the strength of behavior and are both a positive function of the relative value of reinforcement. They are independent measures, because choice is a function of response-reinforcer contingencies and persistence is determined by context-reinforcer contingencies (Nevin et al. 1990). What this means for clinical psychology is that, in order to effect a change in choice, therapeutic interventions should arrange a high rate of highly preferred reinforcement that is contingent on adaptive behavior, while decreasing the reinforcement available for maladaptive alternatives. This procedure is known as DRA (differential reinforcement of alternative behavior) and it has become a mainstay for intervention in behavior therapy. However, Nevin's analysis indicates that increasing the response-reinforcer contingencies to promote adaptive choices will also increase the context-reinforcer contingencies that affect persistence. The net result may be a reduction in the frequency of abnormal acts but, paradoxically, an increase in the persistence of these behaviors during times when treatments are not fully in place.

My colleagues and I conducted a study to see whether DRA does indeed increase the persistence of abnormal behavior in real-life situations (Mace et al. 1999). Three children with mental retardation and severe behavior problems (aggression, food stealing, hair pulling) were exposed to two separate phases of extinction (response blocking) to assess the persistence of their problem behavior. One extinction phase was preceded by a specified baseline rate of intermittent reinforcement. The other extinction phase was preceded by DRA treatment in which reinforcement of adaptive behavior was between 150% to 200% of the baseline reinforcement rate. Thus, relative to baseline, DRA constituted increased reinforcement in the same context in which abnormal behavior had a history of reinforcement. As predicted by Nevin et al. (1990), the frequency of abnormal behavior decreased with DRA, but it was much more resistant to extinction following DRA therapy than following baseline reinforcement for all three children. Although this raises concerns for the use of DRA in clinical psychology, the basic research also points to possible solutions to the problem.

DRA involves reinforcing or "teaching" a new behavior to replace abnormal behavior. Nevin's analysis suggests that the persistence-strengthening effects of DRA may be avoided if the alternative, replacement behavior is first taught in a context not associated with abnormal behavior. For example, a child with mental disabilities may have learned to act disruptively to get attention when his or her parents are preoccupied with other activities. A common DRA intervention is to teach this child a means to communicate his desire for attention rather than behave disruptively. To avoid increasing the persistence of disruptive behavior, communication could be taught in a separate room in the absence of the deprivation of adult attention (McComas et al. 1999), and later introduced to the relevant clinical context without adverse effects.

Finally, as N&G noted, the behavioral momentum metaphor prompted my colleagues and I to consider a novel clinical treatment for noncompliance (Mace et al. 1988). We reasoned that if high-rate reinforcement were arranged for a high-rate of compliance with instructions, compliance may persist when individuals are asked to do things they ordinarily resist. Treatment consisted of presenting a rapid sequence of high-probability (high-p) instructions immediately preceding a low-probability (low-p) instruction with which the client was normally noncompliant. Although the intervention was often effective, compliance to some low-p instructions was not improved. Faced with treatment failures, we considered N&G's model unifying choice and resistance to change and hypothesized that variables that affect choice may also affect persistence (e.g., Hollard & Davison 1971). In a series of experiments, we showed that increasing the quality of reinforcement for high-p compliance made the intervention effective

https://doi.org/10.1017/S0140525X00242404 Published online by Cambridge University Press

for low-p instructions that were resistant to treatment (Mace et al. 1997).

An important test of any theoretical model of behavior, especially one derived largely from laboratory research with nonhuman subjects, is the extent to which the model predicts human behavior in naturalistic situations. Nevin's basic research on behavioral momentum has stimulated new clinical interventions, predicted problems with traditional interventions, and pointed to novel solutions to these problems. By this measure, Nevin's model has a degree of predictive validity achieved by few behavioral researchers.

The partial reinforcement effect and behavioral momentum: Reconcilable?

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Abstract: This commentary considers factors that may account for the inconsistency between the behavioral momentum formulation and the partial reinforcement extinction effect. The method of testing, the variability of the schedule, the nature of the response-contingency, and response effort are considered. Some applications to real-world problems are also discussed.

Since 1974, Nevin's work on the relationship between response strength (defined as relative resistance to disruption) and relative reinforcement value has generated considerable research and contributed much to the analysis of existing data. In recent work, Grace and Nevin (1997) have extended this analysis by linking relative response strength (or behavioral momentum) to schedule preference. This commentary considers factors that may account for the inconsistency between the behavioral momentum formulation and the partial reinforcement extinction effect (PREE), a phenomenon in which low resistance to disruption is linked to high rates of reinforcement.

While working in Nevin's lab, I became interested in possible explanations for the inconsistency between the PREE and the predictions of the response-strength model. I observed that the method typically used to demonstrate the partial reinforcement effect differed from the typical response-strength paradigm in that the former used between-group comparisons and the latter used within-subject comparisons. The partial reinforcement effect was generally demonstrated by comparing a group of subjects that had experienced continuous reinforcement (CRF) with another that had experienced partial reinforcement. In contrast, response-strength evaluations typically involved comparison of the responses of single subjects to differentially signaled reinforcement schedules. To test the notion that the difference between these findings was related to the testing method, pilot work was conducted in which single subjects were exposed to alternating signaled schedules of continuous or partial reinforcement. Even in this single-subject context, the traditional PREE was replicated. We concluded, therefore, that it was not this difference in methodology that accounted for the difference between the predictions of the response-strength model and the empirically obtained PREE.

Next, we became interested in looking at differences other than frequency of reinforcement that might account for differences in disruptability between CRF and partial schedules. We observed that in the CRF schedule there is a perfect correlation between responding and reinforcement. In the variable interval (VI) schedules typically used to assess response strength, however, the relationship between responding and reinforcement is far less than perfect. Not only is reinforcement a joint function of time and responding, but also the times in variable schedules are designed to be unpredictable. It seemed reasonable to assume that terminating a highly predictable response-reinforcer relationship would produce a more discriminable change than would terminating a less predictable response-reinforcer relation and, hence, would produce less resistance to extinction. To test this, we arranged predictable (fixed) and unpredictable (variable) schedules, with equal programmed rates of reinforcement, in alternating signalled components of multiple schedules, and tested their resistance to disruption. We found that fixed and variable schedules with equal rates of reinforcement produced equal resistance to disruption despite producing strong preferences for variable-interval schedules in the initial links of concurrent chains (Mandell 1980). Our failure to obtain differential resistance to disruption for fixed and variable schedules was somewhat surprising, but, nonetheless, these findings were and are consistent with the original model of response strength, which states that relative resistance to change is a function of relative reinforcement rate. Equal rates of reinforcement *should* produce equal rates of disruption. In the target article, Nevin and Grace suggest that it is the patterned nature of responding in fixed schedules that renders resistance to change difficult to measure. Alternatively, it may be that the preference data for patterned schedules are anomalous and reflect something other than relative reinforcement value.

CRF and VI schedules differ along other dimensions as well. CRF schedules are response-based and VI schedules are time-based. The application of response-strength analysis to response-based schedules has yet to be thoroughly explored.

Other variables that invite analysis using the behavioral momentum metaphor are response effort and response complexity. Most of the data used to test this metaphor involve simple, uncomplicated responses. For such responses, effort may primarily influence the velocity of responding. Alternatively, response effort may effect the value and hence the strength of the response. When one attempts to apply this formulation to more complex behaviors or behavior classes (such as compliance, self-control, or political activism), the role of effort becomes more complex. Consider, for example, assembly-line workers who have only simple repetitive tasks to complete. Such workers often have high absentee and turnover rates, and their performances may be otherwise easily disrupted. Common wisdom tells us that increasing the difficulty of this job by requiring more repetitions, or increasing the force required per repetition, would only exacerbate the situation. If, however, the job were made more difficult by increasing its complexity and the responsibility of the worker for the whole process, then the results might be quite the reverse. And, undoubtedly, we have all experienced the phenomenon that a task that we struggle to learn often "stays with us" longer than a simpler task.

Two other examples of potential applications of behavioral momentum analysis come to mind from my current work. The first, based on work at the University of Massachusetts Lowell in conjunction with the Massachusetts Department of Mental Retardation, concerns the high turnover rate among human-service workers. Work in human services, though in some respects gratifying, is difficult, low-paying, and without significant reward from society. As the economy improves, the number of human-service workers declines (Larson & Lakin 1992; Razza 1993). The question of how to increase the momentum for human service work is unclear. It may be simply a matter of adding extra pay or additional tangible reinforcers to the situation, but it may also be a matter of increasing the responsibility or challenge of the work.

Another pressing problem being considered in conjunction with our College of Engineering is how to stem the declining numbers of young men and women entering math- and science-related fields. This decline typically shows up at the middle school level, even among students who do well in math (Pierson 1998; Vedula 1997). Would making math and science curricula less demanding, thereby increasing the rate of reinforcement, increase the likelihood of sustained effort? Alternatively, would making courses more challenging invest science with greater interest and value and thus sustainability? While these questions are not easily an-

https://doi.org/10.1017/S0140525X00242404 Published online by Cambridge University Press

swered, it is possible that considering these problems from the perspective of behavioral momentum will provide useful strategies for the future.

Happiest thought: Dynamics and behavior

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Abstract: Behavioral momentum is a part of the larger field of behavioral dynamics concerned with modeling conditions controlling changes in behavior. The analogy of behavioral momentum to Newtonian and Einsteinian dynamics is briefly treated along with additional physical intuitions related to resistance to behavior change and preference.

I am gratified to see Nevin & Grace's (N&G's) target article in *BBS.* Now behavioral momentum will be examined by the wider audience it merits, for the concept is the most significant to emerge from the field of behavior analysis since Herrnstein's Matching Law (Herrnstein 1970). Indeed, the Matching Law itself may be seen as an implication of behavioral momentum. The development of this concept in the 1970s (e.g., Nevin 1979) required breaking the bonds of the "steady-state," the predominant focus of perspective and research in behavioral analysis since Ferster and Skinner (1957) inspired a generation of behavior analysts to explore the astonishingly intricate and still unfathomable patterns of behavior engendered by contingencies of reinforcement. In this exploration, remarkably little attention was paid to the acquisition or real-time alteration of such patterns. The valiant efforts to analyze schedule patterns in some respects represented a deviation from Skinner's program in the *Behavior of Organisms* (1938), which emphasized the study of learning as opposed to performance. In that work and subsequently, Skinner argued for response rate as an appropriate measure of "response strength," a construct that has persisted implicitly or explicitly in behavior analysis, as the N&G paper demonstrates.

Skinner's rationale was founded partly on his method of observing repeated instances of an operant class (e.g., lever presses) and recording their cumulative occurrences to reveal orderly patterns of rate changes. Rate changes were seen as appropriate to descriptions of learning; rate itself was the most direct measure of the probability of a response, which was, in turn, a measure of response strength. But the myriad patterns of behavior engendered by schedules of reinforcement obscured the meaning of response strength as revealed by rate alone. A consideration of dynamics would tell us that two different response rates, like two different particle velocities, may provide little information about the conditions necessary to *change* the rates. We would prefer to be struck by a feather moving at 50 miles per hour than a truck moving at the same speed.

Behavioral momentum fits into a larger framework of behavior dynamics, a field devoted to the analysis and modeling of those processes controlling behavioral change under various contingencies (for general discussions, see, e.g., Killeen 1992; Marr 1992). Behavioral dynamics thus has a goal comparable to Newtonian dynamics, which sought to understand the causes of changes in motion. Behavioral momentum is an analogy or model derived directly from classical Newtonian dynamics, and what follows addresses the physical character of that model. Clearly, the model is remarkable in its range of account; at the same time, as no doubt N&G are aware, their analogy can be taken only so far.

Newton's second law *defines* force as the time rate of change of momentum (see José & Saletan, 1998, for a modern perspective on Newton's laws). This definition is derived from two principles: (1) An *inertial frame,* in which each isolated particle moves in a straight line, and, in addition, quantifies time so that if any isolated particle in the frame moves with a constant velocity, then all other isolated particles in the frame will move with constant velocity. This establishes Newton's First Law. (2) The *conservation of momentum,* observed from an inertial frame, characterizing the interaction of two (or more) particles so that, for example,

$$m_1 \mathbf{v}_1 + m_2 \mathbf{v}_2 = \mathbf{P}_{12} \quad (1)$$

Given $\mathbf{v}_1$ and $\mathbf{v}_2$, there exist scalars m_1 and m_2 and a constant, time-independent vector $\mathbf{P}_{12}$ so that (1) is satisfied. Moreover, while, for example, $\mathbf{P}_{12}$ may depend on the inertial frame, m_1 and m_2 do not. $\mathbf{P}_{12}$ is called *momentum,* a constant of motion. The m's are the masses, measured only relative to a standard, *in other words as ratios.* Taking the derivative of (1) yields:

$$m_1 \mathbf{a}_1 + m_2 \mathbf{a}_2 = 0 \quad (2)$$

Defining force as:

$$\mathbf{F} = m(d\mathbf{v}/dt) = m\,\mathbf{a} \quad (3)$$

(Newton's Second Law) yields along with (2):

$$\mathbf{F}_{12} + \mathbf{F}_{21} = 0 \quad (4)$$

which is Newton's Third Law, here characterizing the reciprocal interaction of particles 1 and 2.

Behavioral momentum makes no statement about constants of motion (although Herrnstein's Relative Law of Reinforcement contains something like that concept), nor about the vector nature of force, velocity, and acceleration. It ties velocity in the mechanical sense to rate of responding, but the latter is not velocity in the sense of Newton's First Law, or from the perspective of an inertial frame. Operant behavior is *dissipative,* requiring reinforcement to maintain it; rein*force*ment thus acts as a kind of force. A constant rate of responding might be analogous to an object falling through a viscous medium at terminal velocity so that the downward force of gravity just balances the upward force retarding its fall. Extinction, the common "disrupter" for studying behavioral momentum, reveals the dissipative forces on the target behavior, including alternative sources of reinforcement for other behaviors. N&G seem to approach this issue through their treatment of generalization decrement.

We might consider a model in which two unequal masses fall from one dissipative medium into another and consider the time of each mass to reach terminal velocity. If a mass at terminal velocity (i.e., at dynamic equilibrium) in one medium falls into a medium of greater viscosity, this is analogous to reduction in conditions of reinforcement. Alternatively, if the mass were to fall into a less viscous medium, the opposite would be the case. The time to reach terminal velocity is positively related to the mass, thus the ratio of times (T_{v1}/T_{v2}) to reach terminal velocity would be positively related to the ratio of the two masses (m_1/m_2). Because these sorts of functions are exponential, a log relation as presented in the bottom of N&G's Figure 2 might be modeled in this way.

Behavioral momentum, in accordance with Newton's dynamics, necessarily emphasizes the relative nature of mass. An equivalent form of Newton's Second Law related to the conceptual and empirical spirit of behavioral momentum is the impulse-momentum theorem:

$$\mathbf{I} = \int_{ti}^{tf} \mathbf{F}\, dt = \Delta\mathbf{p} \quad (5)$$

That is, the impulse, **I,** of the force **F** acting from t_i to t_f equals the change in the momentum ($\Delta\mathbf{p}$) of a particle. Studying the effects of a disrupter might be compared with golf swings of equal impulse striking alternatively a golf ball and a shot-put ball. The ratio of the resulting velocities will equal the inverse ratio of the masses. Presumably, disrupters of appropriately different impulse could be imposed to produce equivalent effects on two different behavioral masses.

Mass in the context of change in momentum refers to *inertial mass.* Alternatively, we might *weigh* two different objects (weight is a force) and take the ratio:

https://doi.org/10.1017/S0140525X00242404 Published online by Cambridge University Press

$$\mathbf{F}_1/\mathbf{F}_2 = m_1\,\mathbf{g}/m_2\,\mathbf{g} = m_1/m_2, \quad (6)$$

where **g** is the acceleration due to gravity. Mass in this context is *gravitational mass.* As was emphasized by Galileo, neglecting frictional forces, all objects fall with the same acceleration, **g,** toward the earth. This implies that the ratio of gravitational to inertial mass must be a constant. A larger mass is attracted to the earth with greater force, but more force is required to accelerate it, and these two effects apparently just balance. The equality of the two has been established to better than one part in 10^{11} (Foster & Nightingale 1995).

This equality remained a mystery from Newton's time until Einstein resolved it with the aid of thought experiments such as an elevator accelerating upward in empty space. Einstein's Principle of Equivalence says that it is impossible to distinguish between a uniform gravitational field and a uniform acceleration, establishing the relativity of the gravitational field, a cornerstone of general relativity. He described this realization as "the happiest thought of my life" (Pais 1982, p. 178).

It must have been a very happy thought as well for N&G to consider an equivalence between resistance to change and preference. Here there are further ties to classical physics when one considers a pair of preference conditions as a *potential function.* The function describing the force on a body at a given point is equal to the negative derivative of the function describing the potential at that point. Two-preference conditions are like a double-well potential where a particle under a driving force bounces back and forth just as an organism, driven by the concurrent contingencies, moves back and forth between preference conditions (for details, see Marr 1992). Here the relative *dwell time* is a key measure of preference, and the shape, including the "depth" of the potential function, is determined by the sort of variables N&G have addressed controlling both preference and resistance to change.

Contextual choice and other models of preference

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Abstract: Grace's contextual-choice model can account for the results from many studies on choice under concurrent-chain schedules. However, other models, including one that I call the "hyperbolic value-added model," can also account for these results. Preference and resistance to change may indeed be related, but the best model of preference remains to be determined.

Nevin & Grace (N&G) present an impressive set of empirical results in support of the theory of behavioral momentum. They also present evidence that animals' preferences, as measured in choice procedures, correlate with measures of resistance to change. This commentary will focus on one specific part of their theoretical presentation: Grace's (1994) contextual-choice model of choice and its application to data from concurrent-chain procedures (sects. 5 and 6).

Grace (1994) showed that his contextual-choice model (sect. 6.3) accounted for more than 90% of the variance in 92 data sets from 19 published studies on concurrent-chain procedures. This was clearly better than previous models, because Davison (1987) had tested three prominent models with some of the same data sets, and these models accounted for no better than 55% of the variance. However, Davison used no free parameters in fitting the predictions of these models to the data, whereas Grace used between two and four free parameters to fit the contextual-choice model. Because the addition of free parameters will improve the predictions of any model, it remained an open question whether other models could also provide good fits to these data sets if they had the same number of free parameters.

In my recent work, I have tried to develop a way to extend a model called the "hyperbolic-decay model" (Mazur 1984; 1987) to concurrent-chain procedures. This model states that as the delay to a reinforcer increases, the value or effectiveness of that reinforcer decreases according to a hyperbolic function:

$$V = \frac{A}{(1 + KD)} \quad (1)$$

where V represents the value of a delayed reinforcer, A is a measure of the amount of reinforcement, D is delay, and K is a parameter that determines the rate of decay. For cases where one alternative delivers reinforcers after a variable delay, this equation can be generalized to the following:

$$V = \sum_{i=1}^{n} p_i \left(\frac{A}{1 + KD_i}\right) \quad (2)$$

This equation states that the total value of an alternative that includes variable delays can be obtained by taking a weighted mean: Each possible delay, D_i, is weighted by p_i, its probability of occurrence in the schedule, and these individual values are summed to obtain the total value of the variable schedule. The hyperbolic-decay model could account for many phenomena observed in discrete-trial choice situations (see Mazur 1993), but whether it could be extended to choice situations that have initial links of extended durations (such as concurrent-chain schedules) was not certain.

I have suggested a way to incorporate the hyperbolic-decay model into the general framework of the contextual-choice model (see Mazur 1997). This modification yielded fits to the data that were virtually identical to those of the original contextual-choice model. There are, however, other ways to extend the hyperbolic-decay model to concurrent-chain schedules, including a model that I call the "hyperbolic value-added model." This model is described by the following equation:

$$\frac{B_{i1}}{B_{i2}} = b\left(\frac{r_{t1}}{r_{t2}}\right)^{a1}\left(\frac{V_{t1} - a2V_i}{V_{t2} - a2V_i}\right) \quad (3)$$

This equation is identical to the contextual-choice model except for the last parenthetical expression, which represents the contribution of the two terminal links. V_{t1} and V_{t2} are the values of the two terminal links, and V_i is the value of the initial links (all calculated with Equation 2, using the estimated delays to food from the start of the link). The multiplicative parameter $a2$ reflects the subject's sensitivity to differences between terminal links, similar to the way the exponent $a2$ does in the contextual-choice model. According to this model, the effect of each terminal link on choice depends on the amount of value added when the terminal link is entered (i.e., the value of the terminal link minus the value of the initial links). This process is similar in many ways to the principle of delay reduction (Fantino 1969), which states that the effect of a terminal link depends on the amount of delay reduction when a terminal link is entered.

I used a curve-fitting program to find the best fits of Equation 3 to the same 92 data sets that Grace (1994) had analyzed with the contextual-choice model. For each data set, I used the same number of free parameters (between two and four) that Grace used in his analyses. Across the 19 experiments, Equation 3 accounted for a mean of 89.6% of the variance, very close to the mean of 90.8% for the contextual-choice model. To test a third model, the same numbers of free parameters were added to delay-reduction theory (Squires & Fantino 1971), which then accounted for 83.0% of the variance in these data sets.

Based on these results, I conclude that, when equipped with a suitable number of free parameters, at least three different math-

https://doi.org/10.1017/S0140525X00242404 Published online by Cambridge University Press

ematical models can account for large percentages in the variance from published studies on concurrent-chain choice. It may well be that measures of preference can predict resistance to change, and vice versa. However, whether the contextual-choice model or some other mathematical model offers the most suitable measure of preference remains an unanswered question.

ACKNOWLEDGMENT

Preparation of this commentary was supported by Grant MH 38357 from the National Institute of Mental Health.

Behavioral momentum and multiple stimulus control topographies

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Abstract: We have analyzed many discrimination learning difficulties as reflecting multiple stimulus control topographies (SCTs). Nevin & Grace's analysis offers new variables to consider in the design of stimulus-control shaping procedures and cross-setting generalization of newly established behavior. A multiple-SCT perspective also suggests that fixed-trial discrimination procedures may offer advantages for reconciling momentum theory and partial reinforcement extinction effects.

Nevin & Grace's (N&G's) work is potentially significant, not only for understanding nonhuman animal behavior and animal models, but also human behavior inside and outside the laboratory. They have identified applications where the goal of a momentum intervention would be to make desired behavior more persistent, as well as those where the goal is to make undesired behavior less persistent. We address our commentary to this aspect and will emphasize potential contributions in educational situations.

To provide context, our laboratories have focused historically on analyzing discriminative stimulus control. Studies of complex environment-behavior controlling relations led us to resurrect and elaborate Ray's (1969) notion of the "stimulus control topography" (SCT), which refers to the physical features, structure, and controlling properties of discriminative stimuli (McIlvane & Dube 1992). The SCT is directly analogous to response topography; variations in both often go unmeasured in behavioral experiments. However, empirical analyses of the behavior of persons with developmental disabilities compelled us to differentiate among SCTs, to view discrimination baselines as sometimes consisting of multiple SCTs, and to study variables that governed their frequencies. N&G's analysis and its precursors have been extremely helpful in guiding our thinking (e.g., Dube & McIlvane 1996).

As one example, consider techniques for shaping stimulus control (fading, delayed prompting, etc.) that emerged from laboratory research and have been widely applied in educating individuals with developmental limitations. Such techniques are used in efforts to change the SCT – to transfer control of the participant's behavior from one stimulus (the prompt; e.g., brightness) to another (the target; e.g., form) with few or no unreinforced trials (errors). Typical practice is to present the prompt and the target simultaneously for a number of trials and then remove the prompt, either gradually or abruptly. When such procedures do not succeed in effecting transfer, longstanding practice has been to return to earlier program steps for "review." In our experience, when shaping fails initially, protracted review is usually not helpful.

N&G's analysis identifies new variables to consider in the design of stimulus-control shaping procedures. For example, preliminary training usually entails rich, often continuous, reinforcement schedules for responding to prompt stimuli presented alone; as the momentum analysis suggests, this increases the behavioral mass of prompt-controlled behavior. By contrast, target stimuli, which must compete with prompts for control, are typically novel, and thus any relevant behavior has little or no behavioral mass by definition. Moreover, the goal of eliminating unreinforced trials promotes constancy of stimulus-reinforcer relations and thus circumstances under which there is optimal resistance to change for behavior controlled by prompts. In sum, evolved shaping practices are the virtual opposite of those that N&G's analysis would recommend.

Good theoretical analyses often lead to predictions that are seemingly outside the scope of the original subject matter or contrary to standard practice or intuition. With respect to stimulus control shaping, N&G's analysis seems to predict that more successful transfer would result from: (1) after establishing reliable control by the prompt stimuli, reducing prompt-controlled behavioral mass by employing the leanest schedule that will maintain stimulus control; (2) using a continuous schedule on trials that pair the prompt and the target stimuli; and (3) interspersing such pairing trials in a greater number of trials that present only the prompt, also with a lean reinforcement schedule. If shaping procedures inspired by N&G's analysis do in fact improve efficiency or success rates of stimulus control shaping, that outcome would not only confirm basic principles but also contribute to the development of better instructional methods.

More generally, we suggest that studying fixed-trial discriminated operants may offer new opportunities for testing principles of behavioral momentum. Thus far, perhaps its major challenge has been the partial reinforcement extinction effect (PREE). We expect this section of N&G's target article to be the focus of much commentary. From our multiple-SCT perspective, we think the PREE test conditions are not particularly informative. On their face, continuous and partial schedules seem to encourage different SCTs. Continuous schedules minimize control by the organism's own behavior (i.e., response-produced stimuli) by interposing the reinforcer delivery immediately after the first response. By contrast, increasingly intermittent schedules increasingly encourage such control: Having responded, respond again. In our view, direct comparison of CRF and PREE conditions requires analysis of the relevant SCTs; if the stimulus control is different in the two situations, then the resulting performance differences may be difficult to interpret.

By contrast, fixed-trial procedures can be arranged to require a single response per trial, thus minimizing the possibility of response-based stimulus control. In human participants with mental retardation, we are currently studying reversal of simultaneous discrimination under continuous versus intermittent reinforcement conditions. Analogous to the PREE effect, are discrimination performances that have been intermittently reinforced more persistent (i.e., more difficult to reverse)? Thus far, the answer is negative; our preliminary data are consonant with N&G's analysis. Such findings bolster arguments that the PREE may be an incompletely understood anomaly rather than a real challenge to their analysis.

Finally, we address cross-setting transfer of behavior established in educational/therapeutic interventions. Is the goal of intervention to establish new behaviors having mass sufficient to survive naturally occurring disrupters in generalization environments? It would be unfortunate if readers came away with that message. Rather, a momentum analysis clarifies the tricky nature of programming for generalization, and it emphasizes the need to implement procedures for managing reinforcement schedules, disruptive influences, and controlling stimulus classes. Such an integrative behavior analysis will be necessary if we are ever to achieve an explicit technology to promote generalization.

ACKNOWLEDGMENT

Manuscript preparation was supported by NICHD grants HD 25995 and HD 33802.

https://doi.org/10.1017/S0140525X00242404 Published online by Cambridge University Press

Two cheers for behavioral momentum

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Abstract: Behavioral momentum is a useful metaphor reminding us that with constant conditions, ongoing behavior – in the form of response rate – would be expected to remain constant. But despite an impressive array of behavioral experiments, the concept has not yet been applied in a way that would make it useful as a general behavioral law.

There are many uses for the concept of behavioral momentum in the analysis of behavior. First, because it refers to a familiar process (physical momentum), it is relatively easy to grasp intuitively. The target article's frequent appeals to common sense are justified. Second, as it is applied within a domain (psychology) other than its original one (physics), it provides a new perspective on old facts: Extinction, acquisition, satiation, the partial reinforcement extinction effect, matching, and other familiar phenomena are seen in a new light. Third, it provides a heuristic for extension to other areas within the new domain such as clinical treatment, drug addiction, and self-control. The disadvantage of the concept of behavioral momentum, however, is that, like many analogies between physics and psychology, the correspondences it specifies may be misleading. The concept of physical momentum is a highly useful, precise, quantitative description of facts about the motion of rigid bodies. This concept has to be stretched, elaborated, and qualified when applied to the behavior of organisms.

As Nevin & Grace (N&G) note, this is not the first time that an analogy from physics has been brought into the psychology of learning. The original concept of the reflex and the connectionist theories of Pavlov, Thorndike, and Hull all rest on physical analogies. Although N&G admirably resist the tendency to physiologize, the function of the concept of response strength in their theory is the same as the function of a hypothetical physiological mechanism – to provide a continuous state that bridges between causes at one time and effects at another time. In other words, behavioral momentum is a dispositional concept. The comparison in section 2.4 of response strength with the hidden reinforcing rods in a concrete wall makes this clear.

Dispositional concepts are often meant to imply internal states rather than histories of external events. The dispositional concept *brittleness,* as applied to a porcelain cup, for example, implies that if the cup is dropped on a tile floor, it is likely to break. But is this because other cups made in the same way have broken when dropped or because the molecular structure of the cup is what it is? The answer depends on what you mean by "because." The structural explanation provides an immediate (efficient) cause and is of course valid in the case of cups because the relation between the forces on dropped cups and their molecular structure is well understood. But in psychology, where the relation between behavior and internal events (physiological, cognitive, or mental) is not well understood, structural explanations often serve only to paper over ignorance of immediate causes and to divert attention from prior history.

You could say, for example, that I went to the cash machine because I needed money and knew I could get it at the cash machine, or you could say that I went to the cash machine because under similar conditions (wallet contents, appointments, etc.) I frequently went to the cash machine in the past; *then* you could analyze my past behavior to find within it (within the behavior, not my head) my needs and my knowledge (see Rachlin 1994). But if you think you have *already* explained my behavior (in terms of *internal* needs and knowledge) you are unlikely to look for explanations elsewhere. The philosopher Rowland Stout (1996) calls the move from overt behavior over time to immediate but hypothetical internal causes, the "Internal Shift." The concept of response strength is not as conducive to the Internal Shift as such mentalistic concepts as need and knowledge, but the history of response strength as conceived by Pavlov, Thorndike, and Hull and by Skinner (1938) in the form of "reflex reserve" (but then abandoned by him), is essentially a history of the Internal Shift. All of these theorists, however externally grounded their original conceptions, ultimately saw response strength as an immediate internal cause of behavior. The analogy in behavioral momentum theory between resistance to change (held to be a direct measure of response strength) and physical mass implies that the thing being measured is the state of some body (in this case that of a behaving organism) rather than its past history.

The analogy between response rate (the psychological term) and velocity (the physical term) is also problematical. Velocity and change in velocity (acceleration) are continuous variables (which is what makes differential calculus applicable to them). If a body is moving at a constant velocity, it will be moving at that velocity no matter how narrow the time span. This does not apply to response rate. Response rate equals the number of responses in a period divided by its duration; a constant response rate will be found to vary as the timespan over which it is observed decreases. At the limit, response rate is either zero or infinity. At any moment a pigeon is either pecking a key or not pecking it. This difference might tempt an observer to hypothesize a constant and steady state within the organism that could be the immediate cause of the constant rate. Such a temptation, however satisfying, should be resisted, because, again, it adds nothing to explanation and diverts attention from reinforcement history.

The vast body of evidence presented and referred to in the target article demonstrates the heuristic value of the central metaphor. But despite the extensive evidence, there are several unexplored areas where the theory would seem to apply but is not tested. Behavioral momentum theory is fundamentally about the three-term contingency: (discriminative) stimulus; response; reinforcement. Traditionally, the effect on behavior of relations between response and reinforcement is the domain of instrumental (or operant) conditioning, whereas the effect on behavior of relations between stimulus and reinforcement is the domain of classical conditioning. Behavioral momentum theory posits a certain interaction of these two processes. It has long been recognized that classical and instrumental effects interact. Evidence that they may go in different directions (as when relative response rates in the initial links of a concurrent chain schedule are opposite to those in the terminal links) does not so much support behavioral momentum theory as it does the existence of the two processes.

Much of the cited experimental evidence demonstrates that a given response-change manipulation has less effect when reinforcement is high in magnitude, rate, or immediacy than when reinforcement is low in magnitude, rate, or immediacy. But this is what one would expect if the relation between the reinforcement parameter and response rate was negatively accelerated. A given abscissa change high on a negatively accelerated function would be reflected in a lesser ordinate change than one lower down.

One of the central implications of behavioral momentum theory is that resistance to change is a unitary variable – so that whatever method of decreasing response rate is applied (extinction and satiation being the two most common methods used in these experiments), one thing is basically happening. The experiments described or cited by N&G show corresponding effects of extinction, satiation, and prefeeding (e.g., see Fig. 3). But one wonders if this correspondence would hold up in wider applications. After all, extinction is a learning variable (dependent on discrimination of a zero-slope feedback function from a non-zero-slope feedback function) and would be expected to act slowly and persist, whereas satiation is a performance variable and would be expected to act rapidly and not persist.

Moreover, if we are to take the momentum metaphor seriously, it should apply to resistance to response rate increases as well as decreases. This implies that (1) change from extinction to a non-zero-slope feedback function and (2) increase in deprivation should have corresponding response-rate enhancing effects. Per-

https://doi.org/10.1017/S0140525X00242404 Published online by Cambridge University Press

haps this is so and some symmetrical effects will be presented in the Authors' Response.

In summary, Nevin & Grace have introduced a valuable and productive metaphor with wide application. As they note (sect. 10.3.2), I have used it to describe the coherence of patterned responses (Rachlin 1995). But, without a more systematic exploration of its power, behavioral momentum is a metaphor, not a behavioral law.

ACKNOWLEDGMENTS

The writing of this commentary was supported by grants from the National Institute of Mental Health and the National Institute on Drug Abuse.

Can the concept of behavioural mass help explain nonconstant time discounting?

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Abstract: The concept of behavioural mass provides one avenue for justifying (or making rational) the phenomenon of declining impatience, according to which decision makers put more value on delays that will occur in the near future than on those that will occur later.

The conventional economic model of intertemporal choice holds that delays of equal size should be given equal weight in decision making, regardless of when the delays begin. This assumption implies a constant rate of time discounting, instantiated in an exponential discount function. It has long been clear, however, even to academics, that humans routinely violate this assumption (Strotz 1955). An increasing number of researchers have adopted alternative models of intertemporal choice that incorporate a discount rate that gets smaller with increasing delays, a phenomenon which we can call *decreasing impatience.* The choice of such models has been justified in many ways. The major theoretical argument, mentioned in the target article, is that hyperbolic discounting, the most popular alternative to exponential discounting, is an extension of the generalised matching law (Ainslie 1975; Ainslie & Haslam 1992; Herrnstein 1997). The empirical argument is very compelling: People routinely display the preference reversals predicted by declining impatience (Kirby & Herrnstein 1995; Read & Van Leeuwen 1998), and hyperbolic discount functions fit human choice behaviour better than do exponential functions (e.g., Kirby 1997). Indeed, the arguments in favour of decreasing impatience are so compelling that variants of hyperbolic discounting are making significant inroads into the mainstream economic literature (Laibson 1997; O'Donoghue & Rabin 1997).

A major problem in accepting hyperbolic discounting as an account of human preference is that there are good reasons for calling nonconstant discounting irrational (Strotz 1955), and there are apparently no good reasons for calling it rational. How is it possible, therefore, that animals and humans could have acquired such a perverse disposition? One way of addressing this question is to "rationalise" irrational behaviour by arguing that just because we (as experimenters) represent a task in a particular way, that is no reason to assume that our subjects also represent it that way. The concept of behavioural mass offers one basis for such a rationalisation of decreasing impatience.

If we treat intertemporal choice as being determined by preference for variable interval schedules of reward rather than isolated rewards, then the optimal rate of time discounting will show decreasing impatience. For variable interval (VI) reinforcement schedules with a fixed reinforcer size, the longer the interval, the less the behavioural mass (the average amount of reinforcer per unit of time). The proportional decrease in the behavioural mass is not a function of the absolute increase in the length of the interval (which would justify constant discounting), but in its *proportional* increase. One prescriptive model of time discounting, which suggests that we discount future outcomes *as if* the delays under consideration reflect average delays in VI schedules, is Harvey's (1994) *proportional discounting,* which has the following discount function:

$$a(t) = \frac{b}{b + t},$$

where b is a discounting parameter, and $a(t)$ is the ratio between the amount to be received after time t. For example, if £100 in one month is worth £70 today, then $a(1\text{ month}) = 70/100 = 0.7$. For all values of t except very small ones, people who discount according to this function will value delayed alternatives in approximate proportion to the behavioural mass of a VI schedule having an average delay t.

Choices between delayed outcomes are frequently (implicitly or explicitly) choices between reinforcement schedules, rather than between single events. This can be illustrated with a familiar example that shows one way that proportional discounting could be learned. Consider how children are taught the virtues of patience. A child wants a reward (perhaps an ice cream) right now, but his parents prefer that he wait until tomorrow. Whatever delay is agreed, the child cannot open negotiations for a second ice cream until after the consumption of the first. If the ice cream comes tomorrow, that means that there is an entire day in which no ice cream negotiations can occur. Moreover, the agreed delay sets a precedent – the child knows very well that if he has to wait until tomorrow for the first ice cream, he will usually have to wait until tomorrow for all subsequent ice creams. Accepting a one day delay, therefore, is tantamount to agreeing to a VI schedule of reinforcement, with the agreed delay being the average interval. The total number of ice creams the child can expect to get in the future is inversely related to the proportional increase in the delay imposed by his or her parents; an increase from one day to two will have the same impact as an increase from two days to four. Such a child would rationally adopt some version of proportional discounting when dealing with his or her parents. He will kick up a real fuss to prevent even a few moments delay from the present, but will be relatively blasé about substantial additions to already long delays. The minimum reward that the child must be promised in order to induce him to accept a delay will be the amount that keeps behavioural mass constant, and this will increase as a linear function of delay.

In experimental studies of choice between delayed alternatives, the choices typically involve a number of independent decisions. Rarely does accepting a specific delay for one outcome mean that there will be no other choices offered during the delay, nor does it set a precedent for the future. We cannot assume, however, that just because we, as experimenters, represent the task in this way that our subjects do. Perhaps they use the same strategy when making one-shot choices as they do when making the developmentally and (perhaps) evolutionarily more significant choices between schedules of reinforcement that differ in their behavioural mass.

Toward a deconstruction of the metaphor of behavioral momentum

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Abstract: The metaphor of "behavioral momentum" exemplifies modernism at its best and follows in the wake of countless other applications of Newtonian mechanics and "the machine metaphor" to virtually every aspect of the human condition. Modernism, however, has fallen on hard times. Some of the chief reasons why are implicit in the target article by Nevin & Grace.

https://doi.org/10.1017/S0140525X00242404 Published online by Cambridge University Press

1. The machine metaphor and "modernism." Nevin & Grace (N&G) present one of the clearest and most successful applications in the history of psychology of the metaphor of man as machine. This metaphor originated, of course, in the seventeenth and eighteenth centuries, and was articulated explicitly by De La Mettrie (1748/1912). It contributed to that hugely influential philosophy of mind, British Empiricism, and was developed by Locke, Hume, Mill, and many others. This metaphor, commingled with British Empiricism and positivistic science, provided much of the basis for the original development of scientific psychology and, more generally, of the intellectual, artistic, and cultural movement often referred to as modernism.

Modernism means many different things to different people, but here it will refer to a belief in universal basic mechanisms that are independent in the sense that they work the same way across all possible contexts.

2. "Postmodern" criticisms of the machine metaphor. Postmodernism arose as a rejection of the machine metaphor and modernism as well. ("Man as computer" is a special case and is sufficiently different from the "man as Newtonian machine" metaphor to be outside the scope of this commentary.) The rejection was, and is, based on several issues. First, even within physics, Newtonian mechanics is only a special case, so that the universality of its claims needs qualification. Second, developments in research on human perception, memory, and naturalistic language have contributed to skepticism about the adequacy of the machine metaphor, especially in its Newtonian form, for the human condition. Some of these developments have placed greater emphasis on dynamic interactions among processes, on emergent phenomena, on the role of context, and, generally, on more Gestalt-like processes highly resistant to being reduced to independent component mechanisms in a manner compatible with Newtonian mechanics (Hanson 1969; Wittgenstein 1953). Third, countless literary works note troubling consequences of the machine metaphor for human society (Orwell 1949; Shelley 1818/1969; Stevenson 1886/1907).

3. Implicit modernist assumptions in Nevin & Grace. Are any of the usual postmodernist grounds for concern about the machine metaphor apparent in the target article by N&G? Yes. The article contains commitments to the idea that basic mechanisms are universal and independent, and there is no mention of how Gestalt-like properties of either perception or memory might cause difficulty for a machine metaphor. Let us examine just a few of these commitments in slightly greater detail.

In psychology, one of the reasons for skepticism about the universal adequacy of the machine metaphor is its typical emphasis on static functions and its general failure to deal adequately with the effects of time and with the dynamic interaction of processes. The unit of analysis is an especially troublesome problem in psychological applications of the metaphor. Logan (1956) presented a pioneering "micromolar" view that some have seen as particularly subversive to the primacy of mean response rate, and N&G cite Logan's work, but without noting the significant conceptual problems it causes for their molar analysis. In general, the concept of "response" is central to the metaphor, yet it remains ambiguous, especially in contexts where there is temporal organization of behavior. This ambiguity is acknowledged by N&G in the case of a Fixed Interval contingency, which they see as not a "fair" test context for their metaphor.

Other evidence also suggests that a molar account of behavior cannot provide an adequate general explanation of behavior (Hawkes & Shimp 1998; Hinson & Staddon 1983; Shimp et al. 1994).

The basic problem with the behavioral momentum metaphor is that it leaves virtually unexamined *why* the rate of a free operant should be analogous to the velocity of a moving body, and *why* resistance to change of that rate should be like inertial mass. N&G provide no principled argument why the specific laboratory settings upon which their metaphor is based justify claims of universal applicability. Of course, if universality is not intended, appropriate explicit qualifications would be most helpful.

4. Implications. The metaphor of behavioral momentum has been so successful that it seems eminently appropriate and constructive to continue to articulate it and to explore its limits. There seems more than ample reason to accept and applaud research on the machine metaphor, even while others choose to explore very different metatheoretical approaches (Putnam 1987). At the very least, we can expect the behavioral momentum metaphor to serve as a pragmatic rule of thumb. It may become ever more apparent, however, that the metaphor derives heuristic power from physical analogies but does not yet rest on a scientific understanding of basic psychological principles.

NOTE

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Preference and resistance to change do not always covary

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Abstract: Nevin & Grace's primary argument against theory and research on behavioral momentum is that preference and resistance to change may not covary. The method for evaluating preference and resistance to change seems problematic. Moreover, the theory fails to account convincingly for effects of average overall time to primary reinforcement on choice and preference for unsegmented schedules.

In their target article, Nevin & Grace (N&G) suggest that the metaphor of behavioral momentum is productive in guiding basic research on a discriminated operant. First, they advance a view that rate of response reflects performance, whereas resistance to change characterizes response strength. Then they propose a view that preference and resistance to change should covary because they are construed as convergent measures of a single construct, that is, response strength that results from a history of reinforcement.

I fully support the former view that rate of response and resistance to change are independent dimensions of behavior. As reviewed in the target article, there is ample evidence indicating that response rate depends on response-reinforcer relations whereas resistance to change is positively related to the total frequency of reinforcement.

The latter view, on the contrary, leaves me with more questions than answers. First, comparisons of results obtained with concurrent-chain schedules and multiple schedules (Grace & Nevin 1997) might not be an adequate method for evaluating preference and resistance to change. This method is based on the notion that contingencies between responding and reinforcement are different between multiple schedules and concurrent-chain schedules. In multiple schedules, presentations of component schedules are not contingent on subjects' behavior, whereas in concurrent-chain schedules presentations of component schedules are contingent on subjects' choice. However, as suggested by Neuringer (1967), results obtained from choice and nonchoice procedures might depend not only on such a difference in contingencies but also on another controlling variable, the frequency of shift between different reinforcement conditions.

Choice procedures generally produce larger effects on behavior of different reinforcement parameters, such as frequency, magnitude, and immediacy of reinforcement, than do nonchoice procedures. However, this might be partly because the frequency of shift is generally greater in choice procedures than in nonchoice procedures, because subjects usually change over between two alternatives very frequently in choice procedures.

It is therefore possible that multiple schedules with high fre-

https://doi.org/10.1017/S0140525X00242404 Published online by Cambridge University Press

quencies of shift (multiple schedules with relatively short durations of each component, say 5 seconds) produce distinct effects of different reinforcement parameters on responding. On the other hand, multiple schedules with extremely low frequencies of shift (multiple schedules with relatively long durations of each component, say 10 minutes) might produce no effects of different reinforcement parameters on responding. Based on this view, Neuringer (1967) compared effects of different reinforcement magnitude on responding in initial links (choice situations) and responding in terminal links (nonchoice situations) with the number of shifts kept identical.

Hence, one needs to examine preference in initial links and resistance to change in terminal links of concurrent-chain schedules with the frequency of shift kept identical between them before general conclusions can be reached about relations between preference and resistance to change.

Second, N&G ignored effects of T on preference when they derived Equation 12 from Equation 7. This implies that their model is restricted to a situation in which average initial-link and terminal-link durations are kept constant. However, even when we follow the contextual choice model (Grace 1994), there remains empirical evidence that choice is affected by the average durations of delay to primary reinforcement at the beginning of initial links (T). In other words, a possibility still remains that different values of T produce different preference in concurrent-chain schedules, but resistance to change in multiple schedules does not vary at all with manipulations of T.

Third, Mandell (1980) confirmed strong preference for a VI x-sec schedule to a FI x-sec schedule but found no difference in resistance between the FI and VI schedules. That is, there is distinct evidence against the notion that preference and resistance to change covary. The reason for this exception is unknown at the present. However, as mentioned in the target article, it is well known that a long exposure to a simple FI schedule engenders an initial pause and rapid responding before reinforcement. Thus, it is possible that after an extended period of training, a simple FI x-sec functions as chained FI y-sec FI z-sec with $(y + z)$ equal to x. If this conjecture is correct, it is suggested that the results obtained from Mandell (1980) have close relation to findings obtained from previous studies on schedule segmentation that used concurrent-chain schedules to examine choice between schedules with and without stimulus changes (for a review, see Takahashi 1996).

For example, Duncan and Fantino (1972) indicated that pigeons preferred unsegmented schedules (a simple FI) over segmented schedules (chained FI FI) with equivalent durations. Moreover, when subjects are required to choose between two segmented FI schedules, "responding in the initial links sometimes cease entirely" (Duncan & Fantino, 1972, p. 31; for a theoretical analysis of this phenomenon, see Takahashi 1996). What is important is that in concurrent-chain schedules, choosing a segmented schedule produces a fixed period of extinction situation (a first segment of the segmented terminal link) that is worse than a situation in initial links. In multiple schedules, on the other hand, responding produces a consequence that is better than a consequence following behavior other than responding. It is accordingly suggested that preference in initial links of concurrent-chain schedules is determined by comparing terminal links with initial links. N&G should also take into account these findings on schedule segmentation.

In conclusion, the metaphor of behavioral momentum is powerful in describing empirical findings on resistance to change and rate of response in terms of response strength. However, its prediction of agreement between preference and resistance to change remains a critical question for future research. The strength of the metaphor is estimated when it is disrupted by a wide variety of empirical findings.

Strength, limits, and resistance to change of operant theory

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Abstract: The research Nevin & Grace report is impressive in its integrative power, but it also shows the current limits of operant theory: There is tremendous concentration on understanding how existing behavioral relations are modulated in rate or time allocation, but little on dealing with the origin of the behavioral relations themselves. Specifying what should count as a behavioral unit will require source principles sensitive to the composition of the units being related.

Over the last 20 years, the momentum hypothesis has grown into an impressive research program linking various applications to basic behavior science, and Nevin & Grace (N&G) are to be commended for bringing their results to the attention of a wide readership. I will leave discussion of empirical issues to others, who might take exception to N&G's evaluation of Equation 3 (Warren-Boulton et al. 1985) and to the prevalence of power functions in N&G's modelling (see Timberlake 1982). I will instead discuss some conceptual problems with operant theory that the target article exemplifies.

Although N&G's proposal is innovative in various ways, it, like standard operant theory, focuses on the rate or time allocation of existing environment-behavior relations, and studies how this rate can be modulated by temporal correlations with other events. The resulting *allocation laws* now include behavioral momentum as well as delay reduction and various forms of matching. The amount of integration shown by N&G's model is impressive, but the allocation laws of operant research will remain incomplete to the extent that their terms are defined functionally and not formally. The nature of the stimulus and response terms in the matching law, for example, is often determined post hoc by adherence to the very allocation principle being tested (cf. Rachlin 1971).

Although "functional analysis" is often touted as one of the most successful features of operant research, its prevalence hints at circularities more troubling than that discussed by N&G. Without any previous (hence formal, not functional) specification of what counts as a stimulus, a response, or a reinforcer, the predictive power of behavioral analyses remains limited (see Weimer 1984) and difficulties quickly crop up. Consider N&G's treatment of concurrent schedules, for example. In their discussion of the matching law, N&G state that concurrent VI–VI schedules involve "two simultaneous discriminated operants defined by key location" (sect. 2.2). But when it comes to defining stimulus-reinforcer relations, N&G aggregate reinforcers across keys (sect. 3.4). What were two different stimuli (parts of two "discriminated operants") is now a single stimulus, associated with a single reinforcer rate. A more consistent way of defining stimuli in concurrent schedules would require adjustments either in N&G's view of matching or in their computation of stimulus-reinforcer rates (Equations 4a, 4b).

A formal specification of what counts as a stimulus or a reinforcer, independently of the allocation laws being tested, will evidently contribute much to behavior analysis. I have argued elsewhere (Tonneau 1998) that the *source laws* explaining the origin of novel environment-behavior relations (Epstein 1991) should be sensitive to the organization of the complex stimuli and responses being related, thus making contact with what cognitive scientists discuss in terms of compositionality and systematicity (Fodor & Pylyshyn 1988).

Shifting one's account from punctate events to organized sequences is one way to increase the explanatory power of behavior theory without sacrificing on issues of organization (Tonneau 1995). Consider the phenomenon of reinforcer devaluation (sect.

https://doi.org/10.1017/S0140525X00242404 Published online by Cambridge University Press

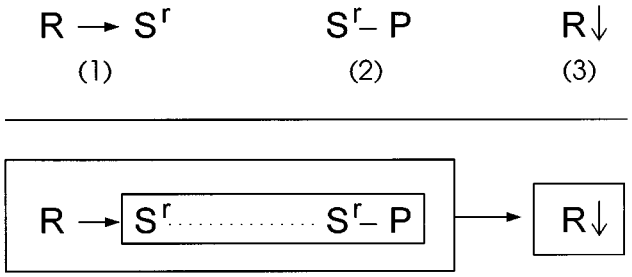


Figure 1 (Tonneau). *Top line:* Three stages of a typical devaluation experiment. Response rate is suppressed (↓) in phase 3. *Bottom line:* On a molar view of reinforcer devaluation effects, R is directly punished by the S^r . . . S^r-P sequence. Time flows from left to right. Solid arrows indicate causal relations. (R: response; S^r: reinforcer; P: punisher.)

11.3), which N&G's model cannot explain at its present stage of development. In the first phase of a devaluation experiment (Fig. 1, top line, phase 1), a response R is reinforced by access to S^r. In a second phase, S^r is devalued in the absence of R, say by pairing S^r with a punisher P (Fig. 1, top line, phase 2). In a final test phase without programmed consequences (Fig. 1, top line, phase 3), the rate of R turns out to be lower than in a control condition lacking the S^r-P pairing stage. This effect on response rate is puzzling from a traditional operant perspective, because the S^r-P pairing took place in the absence of responding.

However, devaluation results can be explained by assuming that R is punished by the pattern, S^r . . . S^r–P, an organized sequence of events that spans *both* the first and second phases of the experiment (Fig. 1, bottom line). That this sequence, including as it does a punisher P, itself punishes behavior should hardly be a matter of surprise. Response rate in test phase thus presumably reflects both the reinforcement of R by a discrete event (S^r) and the punishment of R by a P-containing molar sequence, S^r . . . S^r-P.

Since behavior analysis will most likely progress by moving away from a restricted set of concepts, especially of Skinner's molecular kind (Malone 1987), I was surprised by N&G's endorsement of the "discriminated operant" as a "fundamental unit" of behavior (sect. 2.1). What an odd claim, in a paper showing so clearly that the alleged foundation is permeated from within by Pavlovian relations and from above by devaluation effects. The "discriminated operant" obviously shows high resistance to change when confronted with new, but necessary, theoretical developments.

Behavioral momentum and behavioral economic metaphors for excessive consumption

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Abstract: Metaphors "highlight and hide" different aspects of phenomena. A behavioral economic metaphor for excessive consumption highlights the contextual features of behavioral-environment relations. Can the behavioral momentum metaphor generate a representation of context that is at least as useful as that generated by behavioral economics? Maybe, maybe not; or maybe a mixed metaphor will do a better job than either alone.

The metaphor of operant-as-body-in-motion is impressively exploited in the Nevin & Grace (N&G) target article to identify heretofore unknown relations between resistance to change, preference, learning, and performance. In a different context, Nevin (1995) showed that both behavioral momentum and behavioral economic analyses could account for a more limited set of laboratory-based empirical relations, even though those analyses are derived from fundamentally different conceptions of behavior. Whether a behavioral economic analysis could be successfully applied to the relations discussed in the target article is an interesting question but is beyond the scope of this commentary. Instead, the focus here is on extensions of the behavioral momentum (sect. 10.2, "Drug abuse and addiction") and behavioral economic metaphors (Vuchinich 1999) to excessive consumption (e.g., alcohol and drug abuse) in the natural environment.

Lakoff and Johnson (1980) cogently demonstrated the centrality of metaphor in our conceptual systems. The power of metaphors to influence conceptual development derives to a significant degree from the manner in which they "highlight and hide" different aspects of a phenomena. That is, while a metaphor facilitates (i.e., highlights) comprehension of certain aspects of the phenomena under consideration, at the same time it inhibits (i.e., hides) comprehension of other aspects. In lay and general scientific and professional communities, the dominant conceptions of excessive consumption have been and currently are based on disease (bodily dysfunction) and addiction (obligation or enslavement) metaphors. Both the disease and addiction metaphors highlight internal corporeal and psychological variables and hide environmental and contextual variables that influence excessive consumption. Because of this historical and contemporary imbalance, relatively less attention has been and is paid to the contextual features of behavior-environment relations that prevent, generate, perpetuate, and reduce excessive consumption.

A positive aspect of the behavioral economic metaphor for excessive consumption is that it reverses the priorities of the disease and addiction metaphors and highlights the contextual determinants of consumption. Because a behavioral economic metaphor entails a choice perspective, characterizing the context of the "choice" to consume excessively is inherent in the analysis. The theoretical and empirical elaborations of the metaphor have proceeded with already available concepts related to characterizing context (e.g., unit price, own and cross price elasticity of demand, substitutability and complementarity relations, temporal discounting). These elaborations have generated empirical regularities regarding excessive consumption that have considerable generality and have generated novel prevention and treatment strategies that can be readily integrated with existing efforts across the range of clinical to public policy approaches (Vuchinich 1999).

Like the behavioral economic metaphor and unlike the disease and addiction metaphors, the behavioral momentum metaphor would tend to highlight the contextual determinants of excessive consumption. However, it is relevant to wonder if the behavioral momentum metaphor can generate a representation of the molar context of consumption that is at least as useful as that generated by the behavioral economic metaphor. Because physical mass is "an intrinsic property of the object itself" (Rothman 1989, p. 29), at the outset it would seem that adhering to the terms of the operant-as-body-in-motion metaphor would generate a more constrained representation of that context. High velocity alcohol or drug consumption would have more or less momentum (and be more or less resistant to change) depending on its mass. Clearly this is a worthwhile (and perhaps important) distinction, and one that is not immediately apparent in the behavioral economic metaphor. But what are the contextual forces that will push and pull the excessive consumption so as to raise or lower its velocity or mass? Will those forces be significantly different from those already identified by behavioral economics? Answers to such questions await the theoretical and empirical elaboration of the behavioral momentum metaphor in relation to excessive consumption. Will that elaboration produce concepts that are as useful as, for example, demand elasticity and substitutability? Maybe, maybe not, or maybe this is not an either-or issue. A mixed metaphor for excessive consumption that includes the environ-

https://doi.org/10.1017/S0140525X00242404 Published online by Cambridge University Press

mentally constrained utility seeking of behavioral economics and the stimulus-reinforcer relations of behavioral momentum may produce an analysis more useful than either one alone.

Resistance to change, contrast, and intrinsic motivation

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Abstract: Many studies have demonstrated differential resistance to change in the context of negative behavioral contrast. That is, as a result of introducing a disruptor, response rates decrease to a greater extent when the maintaining reinforcement schedule is leaner. Resistance to change also applies to positive contrast, in that increases in response rate are greater in leaner schedules. The negative contrast effects seen in studies of intrinsically motivated behavior reflect an increase in resistance to change as a result of adding extrinsic reinforcers.

Many researchers treat response frequency or response probability as the measure of response strength, and reinforcer probability as its primary determinant. For example, the relative law of effect (Herrnstein 1970) established that response probability is a function of reinforcers produced by the responses, relative to reinforcers gained from other sources. In their target article, Nevin & Grace (N&G) propose an alternative index of response strength, resistance to change. Because resistance to change is determined by the stimulus-reinforcer association, it also is sensitive to reinforcer rate. So, in most cases, resistance to change and response rate covary in their sensitivity to changes in reinforcer probability (Harper & McLean 1992).

The notion of resistance to change is appealing because it describes instances where the rate of a response changes to a greater extent when it has been maintained by a leaner or less favorable schedule of reinforcement. It corresponds to the personality attribute of stoicism or persistence in that the persistence of behavior may be unrelated to its overall frequency or probability. As summarized by N&G, there is indeed a range of situations in which response rate and resistance to change are uncorrelated. For example, the sensitivity of ratios of response frequency to changes in reinforcer ratios in multiple schedules depends on intercomponent time (White 1995), but the sensitivity of ratios of resistance to change does not (N&G). Here we argue that the useful notion of resistance to change may be further generalized to instances where behavior increases as a result of the introduction of a "disruptor," and where behavior might be assumed to be maintained by "intrinsic" reinforcers.

Resistance to change is typically measured in terms of the extent of reduction in a component of a multiple schedule where reinforcement conditions are constant, as a result of changing conditions in another component. For example, Harper and McLean (1992) showed that free food in a third component of a multiple schedule, compared with extinction, reduced response rates in two other components where responding was maintained by variable-interval 120-s schedules. The effect in one component, where responses produced 2-s reinforcers, was greater than in the other component, where responses produced 6-s reinforcers. That is, resistance to change was greater in the component where responding was maintained by the longer-duration reinforcers. Harper and McLean showed this resistance to change effect for both transient responding, thus replicating Nevin's (1974) result, and for steady-state behavior. The reduction in response rates as a result of free-food in another component is an example of negative behavioral contrast.

Most demonstrations of resistance to change are instances where negative behavioral contrast is greater when the maintaining reinforcers have a weaker effect on prevailing behavior. Some previously unpublished data from our laboratory related to positive behavioral contrast provide clear confirmation of the resistance-to-change principle. That is, the effect generalizes to instances where response rates increase above baseline levels as a result of interpolating a disruptor. In this experiment, five pigeons performed in a single-key, four-component, multiple schedule. Each session comprised 48 consecutive 60-s components without intercomponent timeouts, with each of the four components following each other equally often. the components were signalled by four distinctively different colors and line orientations. In three, the variable-interval schedules always arranged average interreinforcement intervals of 30 s, 60 s, and 120 s. The fourth component was a variable-interval 60-s schedule (baseline) or extinction. Five conditions were conducted. The first, third, and fifth were repetitions of the baseline condition, each lasting for an average of 30 days. The second and fourth were repetitions of the extinction condition, each lasting for an average of 44 days.

Data were taken from the last 5 days of each condition and averaged over repetitions of the baseline and extinction conditions. The resistance-to-change measure was that described in the target article by N&G, namely the log of the ratio of response rates in extinction and baseline conditions, for each of the three constant components. Averaged over birds, the log resistance to change measures for the variable-interval 30-s, 60-s, and 120-s components respectively were 0, 0.069, and 0.195 ($F(2,8) = 4.54$, $p < .05$). That is, as a result of introducing extinction in the changed component, there was far greater change in the constant component maintained by the leanest reinforcement rate. Equivalently, the component with the richest reinforcement rate was associated with the greatest resistance to change. Resistance to change therefore applies also to situations where behavior is increased as the result of introducing a "disrupting" condition. But note that because the present experiment involved alternating conditions in which the changed component was variable-interval 60-s or extinction, the result can equally be interpreted as showing greater negative contrast (with the sign of the log resistance measure changed), and hence less resistance to change, when the maintaining schedule is leaner.

Incentive motivation is an issue that may be understood in terms of resistance to change. Many popular writers and scientists have claimed that explicit reinforcers have a detrimental effect on intrinsic motivation, that is, engaging in an activity for its own sake (Deci 1971). From a meta-analysis of 96 studies, Cameron and Pierce (1994) concluded that there is no evidence to suggest that rewards decrease a person's intrinsic motivation to engage in an activity, except in instances where tangible rewards are expected. For example, Carton and Nowicki (1998) arranged three conditions that closely followed the procedure of early studies that claimed to show a detrimental effect of extrinsic reinforcers (Deci 1971). In the control condition, participants solved "word-find" puzzles at the rate of about one per minute without any intervention. In two experimental conditions where word-finding was rewarded with a .25 probability, success rates increased to about two words per minute. In a subsequent return-to-baseline, participants who were told that rewards would cease solved puzzles at a rate of 0.5 per minute, whereas participants for whom rewards ceased without their knowledge continued at the baseline level. For the first experimental group, therefore, there was a detrimental effect of extrinsic rewards for word-finding, as shown by the reduction in performance below the baseline level.

A simple translation of engaging in an activity for its own sake is that performance of a task may involve reinforcing consequences that are intrinsic to the task itself (such as learning to play the piano) or that have their effect owing to the individual's prior history of social or extrinsic reinforcers for the behavior. In terms of the relative law of effect, extrinsic reinforcers (rewards for learning to play the piano) are pitted against intrinsic reinforcers R_i. A detrimental effect of extrinsic reinforcers, R_e, on intrinsi-

https://doi.org/10.1017/S0140525X00242404 Published online by Cambridge University Press

cally-motivated behavior B_i, is predicted by $B_i = k.R_i/(R_i + R_e)$. That is, an increase in R_e results in a reduction in B_i. This is not what is observed, however. In Carton and Nowicki's (1998) experimental groups, adding extrinsic rewards doubled the rate of word-finding. Extrinsic reinforcers are therefore not pitted against intrinsic reinforcers but may supplement or amplify their effects.

The conclusion that rewards had a detrimental effect was based on the result of the subsequent return to baseline, in extinction conditions. This effect is a negative behavioral contrast effect. Interpreted in terms of resistance to change, behavior during the baseline extinction conditions is maintained by the intrinsic reinforcers associated with the task (otherwise called intrinsic motivation). Adding extrinsic reinforcers strengthens resistance to change by the mechanism described by N&G. Negative behavioral contrast is obtained on return to baseline, as in the studies described above. But it has yet to be directly demonstrated whether adding extrinsic reinforcers increases intrinsic motivation, interpreted as resistance to change. To date, the appropriate control condition has not been run. What is needed is a return-to-baseline extinction condition in which a disruptor is added, such as a concurrent competing alternative. If our application of the resistance-to-change principle to incentive motivation is correct, we expect the facilitatory effect of extrinsic rewards for performance on the main task to be indicated by greater disruption by the concurrent task of performance in the control condition where extrinsic reinforcers were not added.

The uncertain domain of resistance to change

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Abstract: Two important assumptions of behavioral momentum theory are contradicted by existing data. Resistance to change is not due simply to the Pavlovian contingency between a discriminative stimulus and the rate of reinforcement in its presence, because variations in the response-reinforcer contingency, independent of the stimulus-reinforcer contingency, produce differential resistance to change. Resistance to change is also not clearly related to measures of preference, in that several experiments show the two measures to dissociate.

Resistance to change has major limitations as a general measure of response strength. It appears to be inapplicable to simple schedules of reinforcement (Cohen et al. 1993) and also fails to enlighten the determinants of choice. Its domain is thus limited to multiple schedules of reinforcement, and even there the controlling variables remain obscure (e.g., Cohen 1998).

The most problematic feature of the current version of behavioral momentum theory is its claim that the resistance to change for a given response is determined by the rates of reinforcement that occur during the presence of the discriminative stimulus for that response, regardless of whether that reinforcement is actually contingent on the response. Thus, resistance to change is said to be determined by the Pavlovian relation between the discriminative stimulus and the rate of reinforcement in its presence. At least some available data clearly contradict the view that the response contingency for producing the reinforcer is irrelevant to the degree of resistance to change. Bell (1999) compared the resistance to change of several components of a multiple schedule as a function of the delay contingency, manipulating the way a response produced the reinforcer. Only the response-reinforcer contingency was varied, while the stimulus-reinforcer relations were unchanged. As currently formulated, behavioral momentum theory predicts that resistance to change should have been similar for the different components of the schedule. In fact, the component with an unsignaled delay-of-reinforcement contingency had significantly less resistance to change.

Nevin & Grace (N&G) do acknowledge that Bell's results cause difficulty for the view that resistance to change is due solely to the Pavlovian contingency, but seem not to appreciate the contradiction between Bell's results and those the authors cite in favor of the Pavlovian relation being critical. Nevin et al. (1990) demonstrated that resistance to change associated with a given stimulus was increased by a concurrently available second source of reinforcement, regardless of whether that alternative reinforcement was contingent on a second response or presented independently of responding on a variable-time schedule. It is important to recognize that response-independent reinforcement is very similar to response-dependent reinforcement presented after an unsignaled delay (see Williams, 1976, for a direct comparison). The issue raised, therefore, is why the unsignaled delay used in Bell's experiment decreased resistance to change (and preference), while the variable-time food schedule of Nevin et al. (1990) increased resistance to change. Until that issue is resolved, the claim that resistance to change is determined by the rate of reinforcement in the presence of the stimulus must remain suspect.

The second serious problem with the current formulation of behavioral momentum theory concerns the controlling variable in concurrent schedules. In fact there appears to be some conflict between the treatment of concurrent schedules given in the target article, and the previous treatment of concurrent schedules provided by Nevin (1992b). In the earlier paper, Nevin notes that behavioral momentum theory requires the variable controlling resistance to change in a concurrent schedule to be the local rate of reinforcement, in much the same way as specified by the melioration theory of matching (see Williams, 1988, for a discussion). Thus, choice is always to the higher-valued alternative defined by the local reinforcement rate (and greater resistance to change), and matching occurs because the VI feedback functions causes the local reinforcement rates to reach an equilibrium only when matching occurs. The question posed is how the local rate of reinforcement, being the critical variable controlling resistance to change, can be reconciled with the notion supported by Nevin et al. (1990) that the sum of the rates across the two alternatives determines resistance to change. Are there two different kinds of resistance to change?

Other studies make clear that preference and resistance to change do not provide a common index of an underlying response strength. This research (Williams & Royalty 1989) was begun as an attempt to test the melioration theory of matching, which Nevin (1992b) argued is essentially similar to resistance to change in its predictions. Several subsequent studies (Belke 1992; Gibbon 1995; Williams 1993) have corroborated the original findings. In all of these experiments, a multiple schedule has been used in which separate concurrent schedules were available in each component of the multiple schedule. For example, in Belke's (1992) experiment, a concurrent VI 40-s VI 80-s schedule alternated with a concurrent VI 40-s VI 20-s schedule. Then, during probe preference tests, the stimuli correlated with the two VI 40-schedules were presented together. The results were that the VI 40-s stimulus paired in training with the VI 80-s alternative was strongly preferred over the VI 40-s stimulus paired in training with the VI 20-s schedule. Note that the rate of reinforcement summed over both choice alternatives and the local reinforcement rate as calculated by melioration theory were substantially greater for the VI 40 paired with the VI 20 than for the VI 40 paired with the VI 80. Thus, the account of Nevin and Grace should predict greater preference, and also greater resistance to change, for the VI 40 paired with the VI 20. Given that the obtained preference was contrary to this prediction, local rate of reinforcement is not the variable controlling choice. If resistance to change is assumed to be functionally similar to preference, it too must also not be determined by the local rate of reinforcement (or the total rein-

https://doi.org/10.1017/S0140525X00242404 Published online by Cambridge University Press

forcement in the presence of the stimulus. Alternatively, resistance to change and preference may covary only in some situations. Unpublished data (Bell 1997) suggest that the latter possibility is correct, in that separate resistance to change tests following training on Belke's procedure showed greater resistance to change for the components of the concurrent VI 40-sec VI 20-sec schedule despite preference being in the opposite direction. Clearly much remains to be learned about the circumstances defining when resistance to change may or may not be a valid measure of response strength.

Authors' Response

Behavioral momentum: Empirical, theoretical, and metaphorical issues

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Abstract: In reply to the comments on our target article, we address a variety of issues concerning the generality of our major findings, their relation to other theoretical formulations, and the metaphor of behavioral momentum that inspired much of our work. Most of these issues can be resolved by empirical studies, and we hope that the ideas advanced here will promote the analysis of resistance to change and preference in new areas of research and application.

We thank the commentators for their thoughtful observations about our target article. Before responding, we will restate its central theses: (1) The traditional notion of response strength is effectively captured by resistance to change; and (2) independent measures of preference and resistance to change converge to estimate a single construct representing both the strength of responding maintained by the conditions of reinforcement and the value of those conditions.

In support of these theses, we have argued that within the multiple and concurrent-chain schedule paradigms that have been employed to obtain the relevant data: (1) Both resistance to change and preference depend directly on the rate or amount of reinforcement signaled by a stimulus (although other variables may modulate this dependency); and (2) neither resistance to change nor preference depends on the rate of responding maintained by the conditions of reinforcement.

Note that these central theses and the results supporting them make no reference to the metaphor of behavioral momentum. However, the metaphor has been helpful to us and others; we discuss its uses and limitations at the end of this response.

Many of the commentators have questioned the generality of the findings we have relied on to make our case. Others have proposed theoretical alternatives or suggested areas where new research is needed. The empirical and theoretical explorations they suggest can only increase the understanding of resistance to change and preference. We will try to respond to these relatively specific questions before addressing comments that are concerned primarily with extension to applied settings, or with broader conceptual issues.

R1. Generality within the multiple and concurrent-chain schedule paradigms

R1.1. Pavlovian determination of resistance to change. Aparicio, Branch, and **Williams & Bell** point out that neither resistance to change nor preference is solely determined by Pavlovian, stimulus-reinforcer relations. We agree, and noted several exceptions in section 8.3. However, the arguments for a major role for Pavlovian factors are compelling. First, research with serial schedules showed that resistance to change in components with the same rate of food reinforcement depended directly on the reinforcer rates in the following components that were signaled by initial component stimuli, regardless of whether transition to the following components was response-contingent or noncontingent (Nevin 1984; Nevin et al. 1987). Second, adding alternative reinforcers to a component with a given rate of reinforcement contingent on a target response increased its resistance to change relative to a second component with the same rate of contingent reinforcement, again regardless of whether those alternative reinforcers were noncontingent or contingent on an alternative response (Nevin et al. 1990). In both cases, resistance to change depended on the current or following rate of reinforcement correlated with a stimulus, independent of response-reinforcer contingencies. These results imply a role for Pavlovian stimulus-reinforcer relations. It would certainly be valuable to pursue research along the lines suggested by Aparicio to increase understanding of Pavlovian processes in resistance to change.

The argument for Pavlovian determination of preference is essentially the same as for resistance to change: Preference depends similarly on the relative rates or amounts of reinforcement correlated with the terminal-link stimuli in concurrent chains, regardless of the terminal-link contingencies and response rates (Autor 1969; Herrnstein 1964a; Neuringer 1967); and with stimulus-reinforcer relations constant, pigeons show no preference for terminal links with response-contingent or noncontingent reinforcers (Neuringer 1969).

We agree that unsignaled delay raises a substantial problem for a Pavlovian account: With stimulus-reinforcer relations constant, resistance to change is greater with immediate than with delayed response-contingent reinforcement, and a stimulus signaling immediate reinforcement is preferred (Bell 1999; Grace et al. 1998). The question is whether the effects of unsignaled delay can be understood from a Pavlovian perspective. Unsignaled delay may have conditioned inhibitory effects, as suggested by inverted U-shaped gradients of stimulus control (Richards 1974; Richards & Hittesdorf 1978); other assessments of inhibitory control, such as combined-cue tests, are needed to evaluate this possibility.

Williams & Bell state that noncontingent reinforcement is similar to response-contingent reinforcement after an unsignaled delay. This may be true for maintained response rate in a single schedule (as in Williams 1976), but not for preference. Combining Neuringer (1969) and Bell (1999), we would predict preference for noncontingent re-

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inforcement over contingent reinforcement after an unsignaled delay, with overall delays equated. This would be consistent with the pattern of resistance-to-change results.

R1.2. Agreement between resistance to change and preference. Whatever the eventual account of the effect of unsignaled delay, it does not violate the agreement between preference and resistance to change. **Mandell, Takahashi,** and **Williams & Bell** note that this agreement is violated by some other variables. The most widely replicated is the strong preference for variable-interval (VI) over fixed-interval (FI) schedules in the terminal links of concurrent chains, which contrasts with the failure to find large, reliable differences in resistance to change when VI and FI schedules are arranged in the components of a multiple schedule (Mandell 1980) or the terminal links of multiple chains (Mellon & Shull 1986). The problem with comparing resistance to change in VI and FI components is that FI produces a structured behavioral unit – an initial pause followed by rapid responding – whereas VI does not. There is no obvious way to measure the resistance to change of a structured unit to permit comparison with the resistance to change of average response rate.

A solution to this problem requires exploratory analyses to determine which aspects of structured behavioral units change under disruption, and then to determine whether their resistance to disruption depends on the conditions of reinforcement. Although our published work to date has used preparations that do not involve the added complexities of structured units, we agree with **Galbicka & Kessel** and **Shimp** that our approach should be extended to temporally or topographically structured responding. Recently, we have used the peak procedure (Roberts 1981) in multiple schedules and concurrent chains to examine both preference (Grace & Nevin 1999) and resistance to change (Grace & Nevin, submitted) of FI performances. Our resistance data indicate that both peak response rate and the relative variability of peak timing are less affected by prefeeding in the component with the shorter FI. When FI values are equal, these measures are less affected in the component with the larger amount or higher probability of reinforcement. Whether this approach will help resolve the discrepancy between preference and resistance to change with FI and VI schedules remains to be seen.

Takahashi raises a related question for behavioral units that are structured by sequences of stimuli, as in the segmented terminal links of concurrent chains. Duncan and Fantino (1972) observed preference for a simple FI over a chained FI FI with the same total delay to reinforcement. Would there be a comparable difference in resistance to change? Nevin et al. (1981) showed that resistance to change in the initial links of multiple chained VI VI schedules was substantially lower than in the terminal links, but did not compare chained and simple schedules. Again, comparison of preference and resistance to change must await research designed to measure the aggregate strength of units comprised of successive stimuli and their correlated conditions of reinforcement.

Takahashi also notes, correctly, that quantitative comparisons of preference and resistance to change can only be made when the frequency of shifting between the terminal links of concurrent chains and the frequency of alternation between components of multiple schedules are the same. For this reason, we equated those shift frequencies in our 1997 study described in section 8.2. We also used interdependent schedules in the initial links to ensure equal frequencies of exposure to both terminal links, and we recommend this procedure in all studies concerned with preference and resistance to change.

Williams & Bell raise a problem that we did not consider. Belke (1992) reported that when probe choice tests are conducted after training on concurrent VI 20-sec, VI 40-sec schedules in Component 1, and VI 40-sec, VI 80-sec schedules in Component 2, pigeons respond much more to the stimulus correlated with VI 40-sec in Component 2 than to the stimulus correlated with VI 40-sec in Component 1. However, if the relative value of these stimuli depends on the total reinforcer rate obtained in their respective components, as suggested by the resistance-to-change result sof Nevin et al. (1990, Experiment 2) and by our argument that strength and/or value depend on overall Pavlovian stimulus-reinforcer relations, the VI 40-sec stimulus appearing concurrently with the richer VI 20-sec schedule in Component 1 should be preferred.

The same expectation arises if probe choice is based on local reinforcer rates obtained while the pigeon is actually responding to the VI 40-sec schedules. (The question of whether local or overall reinforcer rates determine resistance to change in concurrent schedules, which is raised by several other commentators, as well as Williams & Bell, will be considered in R2.2.) **Williams & Bell** note that Bell (1997) found resistance to change to be greater in Component 1 than in Component 2, which is entirely consistent with our expectations (see also McLean et al. 1996), and it seems likely that if pigeons were allowed to choose between Components 1 and 2 as the terminal links of concurrent chain schedules, they would prefer Component 1.

The apparent dissociation between preference and resistance to change noted by **Williams & Bell** may be caused by the type of choice probe used (cf. Grace & Savastano, 1997, Experiment 2). Specifically, responding in choice probes of the sort used by Belke (1992) can be influenced by carryover of response patterns from concurrent-schedule baseline training (Gibbon 1995). We suggest that the relative value of the two VI 40-sec components in Belke's procedure be evaluated by arranging them as terminal links of concurrent chains in interspersed preference tests (Grace & Savastano's "reinforcing strength" tests) for comparison with their relative resistance to change.

R1.3. Failures to find differences in resistance to change related to reinforcer rate or magnitude in multiple schedules. When the components of multiple schedules arrange different reinforcer rates, and a disruptor is applied equally to both, the vast majority of data demonstrate greater resistance in the richer component. As we noted briefly in section 3.3, some studies have failed to find this result. For example, **Cohen** notes that when drugs serve as disruptors, there may be no difference in resistance between components. **Harper** points out that this could arise from disruption of stimulus control: If the drug reduces or abolishes discrimination between the stimuli signaling the components, any difference in resistance to change would be reduced or abolished. Harper (1999) used a two-lever procedure to evaluate stimulus control, and found that when discrimination was unaffected by a drug, the usual relation between resistance to change and reinforcer rate was ob-

https://doi.org/10.1017/S0140525X00242404 Published online by Cambridge University Press

tained. His method is recommended whenever a disruptor is likely to affect stimulus control as well as response rate.

A second sort of failure arises when the disruptor is the presentation of noncontingent reinforcers during schedule components, rather than between components (e.g., Cohen et al. 1993), or changes in the component schedules themselves (e.g., Harper & McLean 1992, Experiment 2). **Harper** suggests that these failures may arise because "within-component alterations in reinforcer conditions confound the very conditions being used to maintain a differential resistance to change." For example, the addition of frequent noncontingent reinforcers would tend to equalize the overall reinforcer rates in components with fairly lean VI schedules. However, extinction is an instance of a within-component alteration, which nevertheless produces reliable differences in resistance to change, at least with fairly lean VI schedules (see sects. 3.3 and 9.3). To accommodate resistance to extinction after training on richer schedules, including continuous reinforcement, we proposed a model (Equation 17) that includes the magnitude of the within-component change in reinforcer rate during the transition from training to extinction as a separate, additive disruptor. It may be that other within-component changes can be treated similarly, thereby maintaining the distinction between the conditions that determine differential resistance and the variables that disrupt responding.

R1.4. Is differential resistance to change limited to disruptors that decrease response rate? All of our research has involved disruptors that decrease response rate, such as intercomponent food, prefeeding, and extinction. **Rachlin** asks whether the dependency of resistance to change on reinforcer rate holds for procedures that increase response rate, as well. **White & Cameron** provide an answer: Changing one schedule of a four-component multiple schedule to extinction – a contrast operation – leads to greater increases in the leaner components. Similar findings have been reported between groups by Spealman and Gollub (1974). Finally, Harper (1999) found that response-rate increases produced by doses of fluoxetine were greater in the leaner component. Thus, the limited data available on this issue suggest that resistance to disruptors that increase response rate is symmetrical and therefore consistent with the effects of disruptors that decrease responding.

R2. Generality to other schedule paradigms

R2.1. Simple schedules. It is pointed out by **Cohen** that many studies employing simple schedules of reinforcement fail to find that resistance to change is directly related to reinforcer rate. Failures of agreement between multiple and simple schedules may result from infrequent alternation between different schedules (cf. **Takahashi,** as well as Cohen), which is typically on the order of weeks in within-subject experiments, such as Cohen et al. (1993), or from the absence of distinctive stimuli signaling the different schedules. When distinctive stimuli are correlated with different schedules, alternation rate is probably the critical variable (e.g., Cohen 1998). When there are no stimuli correlated with different schedules, or when comparisons are made between independent groups, as in the majority of simple-schedule studies, the stimuli signaling conditions with different reinforcer rates consist of the reinforcer rates themselves, which may not be salient stimuli. It is interesting to note that Church and Raymond (1967) observed greater resistance to punishment with VI 12-sec than with VI 5-min in a between-group, simple-schedule study, perhaps because their reinforcer rates differed by a factor of 25, whereas other studies have generally used much smaller differences between groups or conditions. We suggest that when the schedules themselves are the only signals for differential conditions of reinforcement, resistance-to-change results are likely to be equivocal unless the conditions of reinforcement differ substantially.

In a related matter, **Branch** wonders whether responding under a given schedule would exhibit the same resistance to change if studied alone rather than as part of a multiple schedule with its attendant discriminative stimuli. **Higgins & Sigmon** note that resistance to disruption by drugs may be increased by stimulus-control procedures, perhaps by enhancing the distinctiveness of the stimulus situation and its correlation with reinforcement. In a brief report, Wenrich (1963) suggests that stimulus control greatly increases the persistence of responding established and maintained by a conditioned reinforcer under conditions of satiation. Conversely, Cohen's (1998) data suggest that, if anything, average resistance to prefeeding or extinction is lower in multiple-schedule components than in corresponding simple schedules. Clearly, more research is needed to answer Branch's question.

Leslie states correctly that disruption by conditioned suppression should be greater with leaner conditions of reinforcement, and cites some contrary data when the concentration of a sucrose reinforcer was varied either daily or in 20-session blocks. Perhaps his failure to find the expected effects arose from the absence of distinctive stimuli signaling sucrose concentration. When the conditions of reinforcement are signaled by different stimuli in multiple schedules, the effects of conditioned suppression are consistent with those of other disruptors. For example, Blackman (1968) and Lyon (1963) used multiple VI VI schedules and found less suppression in the richer component. More directly relevant to Leslie's concern, Millenson and de Villiers (1972) used 8% and 32% sucrose concentrations as reinforcers in alternating daily sessions with distinctive stimuli signaling the current condition (in effect, a multiple schedule with a 23-hr intercomponent interval) and found less suppression in the 32% sessions. Together with Cohen's (1998) findings for a comparable procedure, the Millenson and de Villiers result suggests that both distinctive signals for the conditions of reinforcement and at least daily alternation may be necessary to produce results with simple schedules that are comparable to those obtained with multiple-schedule components alternating frequently within each session.

Based on a thorough review of the literature, **Case** points out that after training on simple schedules with intermittent reinforcement, usually in discrete trials, resistance to extinction is an increasing function of amount of reinforcement. The agreement between these results and those obtained with multiple schedules is impressive, suggesting that the differences between conditions of reinforcement are better discriminated when they differ in reinforcer magnitude rather than rate.

Case's review also shows that the results are mixed after training with continuous reinforcement, and he challenges us to account for the diversity of outcomes. If r in Equation

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17 is taken to represent reinforcer magnitude, our theory predicts a bitonic relation between resistance to extinction and *r*. If the location of the maximum depends on procedural variables and/or individual differences, an inconsistent pattern of ordinal results would be generated.

Although it is not germane to questions of resistance to change in simple schedules, **Case**'s claim that our treatment of the partial reinforcement extinction effect (PREE) in section 9.3 contradicts all our previous work on resistance to change, summarized by Equation 5, will be addressed here. He asserts that if we include the discriminative properties of reinforcement in accounting for resistance to extinction after training with rich schedules, we must also include those properties in all other resistance analyses. He is wrong. Disruptors other than extinction leave the training schedule, and hence its discriminative properties, unchanged. Therefore, *dr* in Equation 17 is zero, so it reduces to Equation 18 (which illustrates resistance to deprivation change). Writing Equation 18 separately for the different reinforcer rates or amounts arranged in two multiple-schedule components, and taking their ratio, gives a version of Equation 5.

R2.2. Concurrent schedules. Our account predicts the same resistance to change for two concurrent operants, regardless of their reinforcer rates. This prediction holds under two different interpretations. First, the two responses should be equally resistant because both occur in the same overall stimulus condition and hence are exposed to the same stimulus-reinforcer relation (cf. Nevin et al. 1990). Second, if time spent on each alternative matches the relative rate of reinforcement (as in Herrnstein's [1970] matching law, Equation 1), the local rates of reinforcement correlated with each alternative will be the same; if resistance depends on local reinforcer rate, the two responses should be equally resistant (cf. Nevin 1992b). **Williams & Bell** (see also **Tonneau**) point out that these accounts are quite different, but they converge on the same prediction.

Unfortunately, the results vary widely across experiments. As **Branch** notes, Nevin et al. (1990) found somewhat greater resistance to satiation, prefeeding, and extinction for the less frequently reinforced response. Branch also states that McSweeney (1974) found a similar effect when body weight was varied; however, the numerical data published by McSweeney (1975) suggest the opposite effect, or no difference. Branch also notes that, by contrast, Farley (1980) found that resistance to punishment was greater for the more frequently reinforced response. Finally, Myerson and Hale (1988) found equal resistance to extinction, for two concurrent responses reinforced at different rates, in accordance with either approach to our prediction. It is difficult to extract any simple generalization about resistance to change of concurrent operants from this array of results. Although it would appear that multiple and concurrent schedules are closely related, the subject's ability to switch between concurrent but not multiple schedules may be critical (cf. Williams 1993), and research on the determiners of switching may help elucidate sources of variation in these results.

Because it may be relevant to resistance to change in concurrent schedules, we discuss here the study by Jacob and Fantino (1988), which **Fantino** asks us to interpret. They examined the effects of two interventions on performance on a standard two-key concurrent VI VI schedule of food reinforcement, and concluded that preference was affected only when transitions to a richer or leaner schedule were response-contingent. However, changes in absolute response rate were not systematically correlated with either positive or negative transitions on either key, so differences in resistance to change cannot be evaluated.

Branch suggests that we examine resistance to change in the initial links of concurrent chains. In unpublished research, we have found that responding is more resistant to discontinuing the initial-link contingency in the initial link that leads to the richer terminal link, as in multiple chains (Nevin et al. 1981). This finding should be replicated with other disruptors to ascertain its generality. If found to be general, the results would suggest that our prediction of equal resistance to change for two concurrently available responses needs to be reconsidered.

R3. Areas where new research is needed

R3.1. Resistance to change in multiple schedules. Virtually all of our research has employed multiple VI VI Schedules with fixed-duration components lasting from 20 sec to 3 min, and separated by time-out periods ranging from 0 to 2 min. The results have been quite consistent within this domain. However, we agree completely with those commentators who have noted the need to move beyond this domain.

Branch suggests that a comparison of variable-ratio (VR) and yoked VI schedules could be important. Specifically, if resistance is determined entirely by stimulus-reinforcer relations, there should be no difference in resistance to change. We agree, and a study of resistance to change in VR and VI schedules, together with preference between them, is nearing completion. Tentatively, it appears that for both VI and VR, preference and resistance to change are directly related to reinforcer rate. However, with reinforcer rates equated, VI is slightly more resistant than VR, and likewise is somewhat preferred. If confirmed, this result would join the findings of Grace et al. (1998, discussed in R1.1) to show that response-reinforcer relations may modulate the effects of stimulus-reinforcer relations. In a related study, Lattal et al. (1998) found that responding was more persistent under a yoked interval schedule than under a progressive-ratio schedule: Specifically, interval responding continued beyond the "break point" at which progressive-ratio responding ceased. As noted by **Higgins & Sigmon,** progressive-ratio methods are widely used to study the reinforcing efficacy of drugs, and their use with other problems in the study of resistance to change would be welcome.

Branch also notes the need for research on simple versus multiple schedules, a problem on which **Cohen** and associates have worked extensively and that we discuss in R2.1. Finally, Branch joins **Takahashi** in noting that component duration may be a critical variable, especially very short components, and **Fantino** asks what would happen if very short time outs were interpolated into components. These are all researchable questions within the general domain of resistance to change.

R3.2. Determiners of the relative-strength exponent *b*. Based on his interpretation of resistance to change in relation to the level of activation produced by each reinforcer, **Killeen** suggests that relative resistance to change may be more sensitive to the ratio of reinforcer rates (see our Equa-

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tion 5) if activation is increased by the use of large incentives or increased deprivation. We are not aware of any studies that specifically address this question. A study that examines resistance to change in multiple schedules with components that differ in reinforcer rate, with deprivation and reinforcer magnitude as parameters across successive conditions, could evaluate Killeen's suggestion and thereby his account of our relative resistance results.

R3.3. Resistance to extinction. Several commentators have suggested, from different perspectives, that extinction is "an experimentally unclean disrupter" (**Branch**). They point out that different schedules involve different controlling variables or response units, which may break down during extinction (**Branch, Mandell, McIlvane & Dube**), and that extinction removes the stimulus-reinforcer relations that established different degrees of resistance to change in training (**Harper**). **Baum & Mitchell** and **Rachlin** also note that extinction is a "learning" variable, and should produce somewhat different results from satiation, for example, which is a pure "performance" variable. We agree that extinction involves complexities that are absent from other tests of resistance to change, and that modeling all aspects of behavior during extinction, including transient rate increases, might require additional parameters. However, extinction is a member of the set of disrupters that converge on the estimation of relative response strength, at least with fairly lean VI schedules, and despite its complexity, it is too important a process to be ignored. Our model of resistance to extinction adds one plausible parameter representing a process that is widely acknowledged in the literature, and does a respectable job of accounting for the PREE. Nevin et al. (submitted) tested the model against larger archival data sets, and found that it explained a great deal of variance – but by no means 100%. This is just the beginning of the development of an effective model of resistance to extinction that is compatible with resistance to other disruptors. **Killeen** points out that dimensional consistency will be essential in this endeavor, and although the present statement of Equation 17 can easily be made dimensionally consistent, the dimensions of its free parameters are cumbersome. Dimensional analysis (e.g., Stephens 1994) may help refine interpretation of the processes involved in resistance to extinction.

R3.4. Recovery from disruption. Both **Killeen** and **Rachlin** question whether recovery from disruption would be slower for responding in the richer of two components, as predicted by our general approach to resistance to change. The question will be tricky to answer, because reinstatement of predisruption conditions may not be a disrupter applied equally to ongoing responding (or its absence) in both components. Consider training subjects on a multiple VI VI schedule with different reinforcer rates, and then disrupting responding by extinction. If the original schedules were reinstated in a third phase, this would be an asymmetrical operation in that responding would make contact with the original schedules sooner and more often in the richer component. This difference would have to be addressed with an added term analogous to generalization decrement in our model of resistance to extinction. Perhaps better, one might disrupt responding by reductions in deprivation, and then examine resistance to increases in deprivation, so that the conditions of reinforcement remain in effect throughout. Carlton's (1961) data suggest that recovery from reduced deprivation might indeed be greater in the leaner component, but further data are needed.

In a related comment, **Hall** suggests that response latencies – the time to initiate responding in a component – should be longer for the richer component. However, if one construes the onset of the component stimuli as disrupting not-responding during time outs between components, the onset of the richer component would constitute the greater disrupter and therefore produce shorter latencies. The limited data available (Fath et al. 1983) show no consistent differences in latencies between components, but these come from pigeons pecking lighted keys – a highly prepared response (cf. **Baum & Mitchell**). Latency differences might emerge for less prepared or more effortful responses, especially under experimental conditions that explicitly controlled the subject's behavior before onset of the component stimuli, for example, by arranging a third schedule on a separate manipulandum.

R3.5. Biological factors. According to **Baum & Mitchell,** we may find differences in resistance to change between "prepared" and "unprepared" responses, where preparedness is related to rapidity of acquisition. **Gardner & Scheel** suggest that learning involves the acquisition of stimulus control over a component (R^c) of the response elicited by the US (reinforcer), and preparedness may relate to the degree of overlap between R^c and the elicited response. If so, one might expect some differences between food-reinforced key pecking by a pigeon, where the topographical relation between the key peck and the consummatory food peck is well documented, and popcorn-reinforced sorting or point-reinforced typing by a human, where the overlap between the operant and elicited responses is far from obvious. Nevertheless, relative resistance to change is essentially the same for these three preparations (for a summary see sect. 3.4 and Nevin, 1998, Fig. 12-5). It might be instructive to compare, within a single experiment with pigeons, a highly prepared response, such as key pecking for food in one multiple-schedule component, and a presumably less prepared response, such as treadle pressing for food in an alternated component, and ascertain whether resistance and/or preference depend on degree of preparedness. Starin (1989) found that pigeons preferred a terminal link requiring pecking over a terminal link requiring treadle pressing with the same programmed delay to reinforcement. Despite confounding by small differences in obtained reinforcer delays, his results suggest that key pecking is preferred, and should therefore be more resistant to change, consistent with Baum & Mitchell's argument. To make contact with the literature on biologically based predispositions in learning, it would be valuable to explore the effects on resistance to change of the modalities of stimuli signaling qualitatively different reinforcers (e.g., Garcia & Koelling 1966; for review see Mackintosh 1977), as well as the degree of topographical overlap between the target and unconditioned responses.

R3.6. Structured behavioral units. We heartily agree with **Galbicka & Kessel** and **Shimp** on the need for research with structured behavioral units, where the structure may be defined either by complex response contingencies or by complex stimulus-response relations. In particular, the accuracy or quality of skilled performance may be more or

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less disruptable depending on the conditions of reinforcement. As mentioned above, we have done some work along these lines with the accuracy of timing in a multiple-schedule peak procedure, and found that timing was less disrupted by prefeeding in the component with the richer schedule. However, Nevin and Grosch (1990) examined the resistance to change of accuracy in a matching-to-sample paradigm with signaled reinforcer amount, and found no relation to the magnitude of food reinforcement. Clearly, there are many opportunities for research along these lines. Finally, an explicit comparison of the resistance to change of a temporally extended and perhaps effortful structural unit with that of a simpler, easily repeated response, under equated conditions of reinforcement, might begin to address **Mandell**'s concerns about application in work settings and classrooms.

R3.7. Resistance to change of stimulus control. Commentators **McIlvane & Dube** propose that whenever a researcher is interested in transferring control of a reinforced response from one stimulus dimension to another, as in instructional settings with retarded subjects, preliminary training on one dimension should be limited in duration and should use the leanest schedule possible, thereby minimizing resistance to acquisition of control by a new dimension. In effect, their argument suggests that a stimulus-response relation acquires resistance to changes in the stimulus as well as other sorts of disruption. For example, lengthy training with a rich schedule of reinforcement for responding in the presence of a specific stimulus should make control by that stimulus harder to disrupt. McIlvane & Dube describe some results showing that successive reversals may be especially appropriate for measuring the resistance to change of stimulus-response relations. Another interesting way to examine the strength of stimulus control is via the postdiscrimination gradient of generalization, which often shows "peak shift" – that is, maximal responding at a stimulus displaced from the training stimulus in a direction away from the negative stimulus. The peak shift is often interpreted in relation to the generalization of inhibition from the negative stimulus (for summary, see Rilling 1977). Terrace (1966) found that peak shift diminished and eventually disappeared with extended training, and White (1973) found that peak shift was attenuated or eliminated by a rich schedule of reinforcement. In other words, responding in the presence of the training stimulus was more resistant to the presumed inhibitory effects of extinction at the negative stimulus with longer training or a richer schedule of reinforcement. Other tests of the resistance to change of stimulus control, including transfer tests of the sort described by McIlvane & Dube, are needed to support this conclusion.

R4. Relations to other formulations

R4.1. Resistance to change. It is suggested by **Rachlin** that our resistance-to-change results are consistent with any increasing, negatively accelerated function relating response rate to reinforcer rate. Herrnstein's (1970) hyperbola (our Equation 3) is such a function, and Nevin (1979; see also Nevin et al. 1990) acknowledged that many resistance results were at least ordinally consistent with predictions based on Herrnstein's formulation. There is, however, one effect that is not predicted by Herrnstein's hyperbola: the "crossover" obtained when alternative reinforcers are arranged concurrently with a target response in one component of a multiple schedule (e.g., Nevin et al. 1990; see sect. 3.4). As Herrnstein would predict, target response rate in the component with alternative reinforcers is lower than in an otherwise identical component without alternative reinforcers during baseline training. Response rates decrease in both components during disruption, but the rate of the target response does not decrease as rapidly and becomes higher in the component with alternative reinforcers. By contrast, Herrnstein's hyperbola predicts, incorrectly, that response rate will always be higher in the component without alternative reinforcers. The crossover result has been replicated with human subjects by Cohen (1996) and by Mace et al. (1990), and constitutes a major counterexample to Rachlin's argument.

Killeen adroitly shows that his version of the hyperbola, derived from the joint operation of coupling and activation mechanisms (Killeen 1994), can give an excellent quantitative account of relative resistance to change. Perhaps his model can be adapted to handle the crossover. With reference to Nevin et al. (1990, Experiment 2; see sect. 3.4), his Equation 12 (Killeen 1994) for Component B, without alternative reinforcers, is

$$B_B = kR/(R + 1/a), \quad (1)$$

where k includes response-reinforcer coupling, R is rate of reinforcement for the target response, and a is seconds of activation per reinforcer. For Component A, with alternative reinforcers, the equation becomes

$$B_A = (kR + k'R_{alt})/(R + R_{alt} + 1/a), \quad (2)$$

where k' is less than k because alternative reinforcers are only weakly coupled to the target response. Killeen's comment and Nevin (1994) suggest that disrupters decrease a. If a decreases to the same extent in both components, the crossover can be predicted. Substituting R = .0042/sec (VI 15/hr) and R_{alt} = .0125/sec (VI 45/hr), with k = 3, k′ = .5, and a = 100 sec, we obtain B_B = 53.0 and B_A = 42.2 responses per min. If a decreases to 10 sec, B_B = 7.2 and B_A = 9.6 responses per min. (We chose these parameters simply to illustrate the feasibility of predicting the crossover with response rates on the order of those reported.) We believe that we have applied Killeen's (1994) model correctly, and we look forward to a continuing series of mutually reinforcing exchanges.

R4.2. Preference in concurrent chains. Both **Fantino** and **Takahashi** note, correctly, that preference between a given pair of initial links in concurrent chains varies with the value of T, the average total time between reinforcers. Fantino's finding (e.g., Goldshmidt et al. 1998) that time-out periods before initial-link onset do not affect preference, and therefore the effective value of T, is important for understanding the determiners of the temporal parameters in choice models. It is not critical, however, for Grace's (1994) Contextual Choice Model (CCM); it merely indicates that CCM's temporal-context exponent, Tt/Ti, is unaffected by time out. It is also not critical for the relation between preference and relative resistance to change. Nevin (1992a; sect. 4.1) found that relative resistance was unaffected by the intercomponent interval (ICI or time-out), which seems entirely consistent with the results of Goldshmidt et al. (1998). So we now agree with Fantino: The ICIs in Nevin (1992a) were

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not functionally equivalent to the initial links in concurrent chains, and there is no discrepancy to be resolved.

Mazur proposes an alternative model for concurrent chains, which he calls the hyperbolic value-added (HVA) model, based on his original work showing that a hyperbolic equation provided an excellent description of choice between fixed and variable delays to reinforcement in a discrete-trial procedure (Mazur 1984). Mazur's simulations show that the HVA model accounts for almost the same percentage of variance as CCM across the range of studies analyzed by Grace (1994), and concludes that the best model for concurrent chains remains to be determined. Although a full evaluation of the HVA model must await a more detailed presentation, there appears to be at least one problem. It requires that preference should converge to the terminal-link value ratio as initial-link duration increases indefinitely (because the $a2V_i$ terms in Mazur's Equation 3 go to zero). By contrast, other models for concurrent chains such as CCM and Fantino's delay reduction theory (DRT) predict in this case that the limit of preference is indifference (see sect. 6.1).

R4.3. Behavior-economic approaches. Commentator **Vuchinich** notes that behavior-economic analyses of changes in consumption under variations in unit price generally agree with analyses based on resistance to change in relation to reinforcer rate or magnitude. The convergence of these two quite different analytic approaches suggests the possibility of combining them into a more comprehensive account than either now provides. Behavioral economics is typically concerned with the level of consumption maintained by a given type of reinforcer in relation to its unit price, where the latter is expressed as units of reinforcement per response as determined by ratio schedules. The relation between consumption and unit price, expressed as elasticity of demand, is modulated by the substitutability and cost of alternative reinforcers. By contrast, analyses of resistance to change have concentrated on rate of responding in situations where access to the reinforcer has been controlled by the use of VI schedules. Also, no studies of resistance to change have explored the effects of substitutability when qualitatively different reinforcers are used for concurrent operants in training or for intercomponent reinforcers in resistance tests. It is important to extend the analysis of resistance to change to ratio schecules of the sort usually used in behavior-economic research (cf. **Branch**) and to explore the effects of qualitatively different reinforcers. At the same time, behavior-economic analyses should explore the effects of extended training with stimuli signaling different reinforcers or unit prices on elasticity of demand.

Read asks whether behavioral mass can help explain the phenomenon of nonconstant temporal discounting, in which individuals discount shorter delays at a greater rate than longer delays. We find his suggestion intriguing. His example of a child negotiating with her parents for an ice cream shows that if persistence (i.e., behavioral mass) is determined by the average interreinforcer interval, then the child should resist much more if an expected small delay is increased by the same amount as an expected long delay; that is, the child will show nonconstant discounting. As Read demonstrates, this is consistent with Harvey's (1994) proportional discounting model (Read's Equation 1). However, if we assume that mass is a power function of the average interreinforcer interval rather than strictly proportional (see Equation 16), the resulting function is:

$$a(t) = \frac{b}{b + t^q}, \tag{3}$$

which is closely related to the discounting function proposed by Grace (1999). Grace's two parameter function $(1/(c+1^q))$ can apply to all of the nonhuman data on choice between delayed reinforcers, and also accounts for some representative human data (Myerson & Green 1995) better than the generalized hyperbola proposed by Loewenstein and Prelec (1992). The possible relevance of behavioral mass for explaining "irrational" temporal discounting is worth pursuing, and we thank Read for bringing it to our attention.

R5. Relations to applied settings

As **Mace** notes, a strong test of any behavioral theory is its applicability to real life. Although real life may resemble concurrent schedules (**Branch**) or simple schedules (**Cohen**) in some respects, it surely involves successive encounters with distinctively different situations involving different conditions of reinforcement, as in multiple schedules, and it may offer repeated occasions to choose between them, as in concurrent chains. Accordingly, applications based on the principles of resistance to change and preference in those paradigms should have some chance of success, and interpretation of real-life situations based on those principles should be possible.

We begin by addressing some issues raised by **Branch** and **Hall** concerning the effects of alternative reinforces. In section 10.1, based on the research literature summarized in sections 3.4 and 3.5, we argued that adding alternative reinforcers to a distinctive situation where an ongoing target response is reinforced would increase the resistance to change of all responsed, including the target response. **Mace** provides further examples in a clinical setting, giving strong support to our argument. We also pointed out, in section 10.2.2, that providing noncontingent access to reinforcers in the experimental situation interfered with the acquisition of cocaine-reinforced responding, an apparently opposite result. Here the sequence of events is crucial. It may not have been clear, in our brief summay, that Carroll and Lac (1993) presented noncontingent reinforcers before introducing cocaine reinforcers for autoshaped responding; therefore, the cocaine-reinforced target response could not have been strengthened by the noncontingent reinforcers because it had not yet been established.

Mace describes a way to deal with the problem that using alternative reinforcers to reduce the frequency of an undesirable target response might also increase its persistence (cf. **Hall**). The method is to establish desirable behavior with alternative reinforcers in a distinctly different situation, and then introduce the new, desirable behavior into the situation where undesirable behavior has normally occurred. Although our research does not address this approach directly, it makes sense and deserves systematic study, both in the laboratory and the clinic.

Our proposal (sect. 10.3.2) for using alternative reinforcement to increase the resistance to change of living a healthy lifestyle is based on personal experience, as well as

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extrapolation from research. To maintain mobility in his left shoulder, Nevin was required to do some excruciatingly painful exercises daily. Not surprisingly, he readily found excuses to do something else and exercised only sporadically, if at all, until he began listening to favorite pieces of chamber music while exercising. (With reference to **Branch** and **Hall,** note that listening to music is not topographically incompatible with exercise.) He is pleased to report that this procedure made his exercise regime highly resistant to tempting alternatives, and his shoulder has improved markedly. Moreover, in accord with our expectations, he now listens to chamber music more frequently despite competition from a busy personal schedule. It appears that both exercise and music-listening have become more resistant to disruption by the temptations or distractions of daily life, at least within the situation he calls home. We certainly agree with **Logue** that more than anecdote or plausible extrapolation from research with nonhuman subjects is needed, and hope that her comment will stimulate research to determine whether noncontingent reinforcement enhances self-control in real-life settings.

In a related comment, **Higgins & Sigmon** suggest that the maintenance of a healthy lifestyle (refraining from drug abuse) may depend on the material and social reinforcers that accrue during long periods of abstinence. Although these reinforcers may be correlated with an abstinent lifestyle, they are only remotely contingent on saying "No to drugs" in a particular instance. Nevertheless, they should enhance resistance to temptation by drugs as long as they occur in the same situation.

Higgins & Sigmon also suggest that resistance to interventions designed to reduce drug abuse may be interpreted in relation to determiners of resistance to change, and we agree. It is interesting that both Higgins & Sigmon and **Mace** have found that when a treatment appears to fail in some individual cases, greater incentives can produce the desired outcome. For Higgins & Sigmon, a larger incentive for abstinence apparently functioned as a more effective disrupter of a pattern of drug use, whereas for Mace, a larger incentive for compliance served to enhance its resistance to change.

As **White & Cameron** point out, adding alternative (extrinsic) reinforcers during engagement in a class of behavior that appears to be intrinsically reinforcing ought to enhance persistence of that behavior. However, some data seem to show the reverse: When expected extrinsic reinforcers are discontinued, rates of responding fall below those in baseline, before extrinsic reinforcers are added. Nevertheless, a history of extrinsic reinforcement may make that reduced level of responding more resistant to subsequent disruption, and we hope that research will address White & Cameron's interesting suggestion.

The use of alternative reinforcement may also be relevant to some of **Mandell**'s concerns. For example, she asks how one might reduce absenteeism in assembly-line jobs that involve simple, repetitive tasks. Perhaps some alternative reinforcers, either tangible or social, could be arranged within the work situation (but not necessarily concurrent with the task itself) to enhance persistence in tedious, mechanical tasks. Indeed, Mandell suggests this sort of approach to the problem of retaining human-service workers, and it might work as well in classrooms devoted to math and science without dumbing down the curricula.

R6. Conceptual and philosophical issues

Rachlin argues that the construct of strength, value, or mass in the Newtonian metaphor provides a continuous internal state that bridges between causes at one time and effects at another time, and that this may serve to "paper over ignorance of immediate causes." In contrast, **Gardner & Scheel** seem to like this construct as a proximate explanation because it solves the retroflex-action problem of reinforcement. Although the experimental analysis of behavior has generally shied away from invoking internal states for reasons given by Rachlin, our approach exemplifies what Staddon (1993) calls a "state" model, where the state is a function of the organism's history. "Behaviorists would therefore do well to acknowledge the utility of internal, albeit historically defined, states and judge a theory on its explanatory merits" (Staddon, p. 446). Because the construct of strength, value, or mass is tied to a history of reinforcement, and because it is measured independently by resistance to various disrupters and by preferences, it is unlikely that it will be used in circular explanations of the sort Rachlin properly condemns.

Tonneau argues for formal, a priori specification of the terms of a theoretical formulation. As an example of the problems that arise in the absence of formal definition, he notes that we refer to concurrent schedules as involving two discriminated operants defined by key location in section 2.2, but then aggregate reinforcers across key locations in section 3.4. His example is well chosen. As we noted in R2.2, these two ways of describing concurrent responses and reinforcement lead to the same prediction: Resistance to change should be the same on both keys. To distinguish these accounts, some method must be found to separate them experimentally. McLean (personal communication, 1999) is exploring whether resistance is more closely related to local or aggregated reinforcer rate by using VI schedules to control frequency of changeovers, and thereby local reinforcer rates, in switching-key concurrent schedules (Findley 1958). This would be a functional analysis, which could then lead to a formal identification of the controlling variables. Tonneau's explanation of response-specific reinforcer devaluation in terms of molar sequences may also exemplify the need for a functional approach. If the molar sequence is a formally specified unit, the sequence and temporal relations of events within it should not matter. However, Murray and Nevin (1967) and Williams and Barry (1966) showed that the sequence and temporal relations between shocks and reinforcers was critical for response suppression and recovery during punishment. Only research can determine whether the same is true for reinforcer devaluation. The whole point of Skinner's (1938) argument for the generic nature of stimulus and response classes was that one could not tell, *a priori,* what was included in a class. Rather, it is the functional equivalence of the effects of several physically different stimuli, or covariation of several topographical variants of behavior, that determines whether they are members of the same class or not. Inevitably, form follows function.

The need for functional definition is clearest for the stimulus presentations called reinforcers. In a thorough review of the literature on the nature of reinforcement, Kling and Schrier (1971) concluded that "the only thing which reinforcers have in common is that they have been found to increase the probability of the reinforced response" (p. 691).

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We have used the term in this functional sense, which may be interpreted as the selection of a response by its consequences. We have also used it to describe presentation of a stimulus, presumed to be a reinforcer in this functional sense, which strengthens responding in the presence of a discriminative stimulus. In our experiments, the reinforcer – food for a hungry pigeon – can and usually does have both effects. However, as **Gardner & Scheel** note, quoting from Nevin et al. (1987), the selective function (increasing the rate of a designated response) and the strengthening function (increasing resistance to change) can be independent, which complicates the vocabulary of reinforcement. In the metaphor of behavioral momentum, selecting and strengthening may occur at the same time, but they are separate processes, with the former determining velocity and the latter determining mass. **Baum & Mitchell** assert that because mass depends on reinforcer rate, we have assumed a selectionist framework, which they view as incompatible with mass as a proximate explanatory construct. However, we believe that these two aspects of reinforcement – selection and strengthening – are complementary, not contradictory, and that their marriage will be durable.

R7. The momentum metaphor

Nevin's (1974) initial studies and his interpretation of response strength as resistance to change did not invoke the metaphor of behavioral momentum. The metaphor was first used explicitly by Nevin et al. (1983) in the first attempt to quantify relative resistance to change, and its utility was enhanced by early findings suggesting that velocity (response rate) depended on response-reinforcer relations, whereas resistance to change (mass) depended on stimulus-reinforcer relations. The experimental dissociation of response rate and resistance to change was consistent with the independence of velocity and mass in classical mechanics.

The metaphor has also been useful for explaining our work to nonspecialists. As **Rachlin** says, it is easy to grasp, provides a new perspective, and suggests application or interpretation in other areas. However, **Vuchinich** cautions that although metaphors may highlight previously unrecognized relations, they can also hide inconsistencies or problems. For example, the analogy between response rate and physical velocity is imprecise (Rachlin; our Note 2), and it may be misleading (**Baum & Mitchell, Catania, Galbicka & Kessel, Marr**) because reinforcement is needed to maintain responding, prompting questions of the sort raised by Baum & Mitchell. The velocity analog is also limiting because it does not apply comfortably to aspects of behavior that do not involve rate, such as accuracy of timing or stimulus control, even though these aspects of behavior may also acquire situation-specific strength. In this respect, we welcome Catania's suggestion that resistance to change may be the most enduring aspect of the metaphor.

Several commentators have also questioned the force component of the metaphor. For example, in section 11.3, we treat the pairing of a reinforcer with gastric upset as a disruptive force, despite the fact that as **Tonneau** notes, our approach does not specify the mechanism underlying response-specific reinforcer devaluation. Neither does it specify the mechanism(s) underlying the other disruptors that have been used in the study of resistance to change. For example, if prefeeding changes activation (**Killeen**), extinction may change activation differently, in addition to its discriminative effects (see also **Baum & Mitchell** and **Rachlin**). Perhaps intercomponent food degrades response-reinforcer relations while sparing stimulus-reinforcer relations (**Harper**), or perhaps it adds strength to other competing behavior (**Hall**). Although prior formal specification of effective disruptors and understanding of how they work would be desirable, it is the functional equivalence of these and other force-like disruptors, as demonstrated by the convergent estimation of relative inertial mass, that is the essence of our approach.

The truly central construct in our work is behavioral mass, which we construe as summarizing what is learned as a result of a history of reinforcement in the presence of a distinctive stimulus. Convergent measurement of this summary construct is given by resistance to change (strength) in multiple schedules and by preference (attractiveness or value) in concurrent chains. It is unlikely that convergent measurement of behavioral mass would have occurred to us without the metaphor, and we thank **Marr** for reminding us of Einstein's insight into the equivalence of inertial and gravitational mass as the happiest thought of his life.

Perhaps the metaphor of behavioral momentum has done its job and should be set aside (e.g., **Baum & Mitchell, Catania**) or replaced by other physical metaphors (e.g., **Galbicka & Kessel, Marr**) that will encourage extensions to new behavioral domains. Or perhaps, as **Shimp** suggests, mechanistic metaphors are inappropriate to the complexities of behavior in the first place. However, his criticism of modernism, which we take to mean a deterministic and analytical approach that attempts to identify proximate causes, would seem to apply to virtually all of experimental psychology. **Killeen**'s elegant demonstration that momentum, the product of velocity and mass, is the total amount of behavior emitted under all levels of disruption, exemplifies what can be done with a modernist approach and breathes new life into the metaphor. This total, this integral, needs a name. It is more than response rate, the traditional Skinnerian measure of response strength; it is more than resistance to change, which we have identified with the notion of response strength. Thorndike would have liked it because it combines response probability (velocity) and strength of the bond (mass). We think "behavioral momentum" is a pretty good name for it, and it may prove to be more important than its constituents.

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Letters "a" and "r" appearing before authors' initials refer to target article and response, respectively

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