JOURNAL OF THE EXPERIMENTAL ANALYSIS OF BEHAVIOR 1989, 51, 277-286 NUMBER 2 (MARCH)

MYSTERIES OF THE ORGANISM: CLARK L. HULL'S PRINCIPLES OF BEHAVIOR *AND SOME PROBLEMS IN CONTEMPORARY SCHEDULE THEORY*[1]

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When approaching a classic work such as the *Principles of Behavior* (1943), the contemporary writer risks falling prey to two contrary temptations. One is to use hindsight, that most exact of sciences, to castigate the book for errors and misconceptions that more recent work has revealed. This seems a pointless exercise. Not only would one have expected significant advances in knowledge in the 46 years since the *Principles* was published, but as Hull revised his theories substantially between its appearance and his death (see Hull, 1952), the *Principles* does not in any case represent the Hullian system in its final form. Another temptation is to take almost the opposite route and regard Hull as a kind of Nostradamus of psychology, possessed of miraculous and unerring insight into modern trends. This also seems inappropriate because, although from time to time material in the *Principles* strikes a modern resonance, the overwhelming impression that this reader obtained was how *different* Hull's preoccupations were from those concerning workers in animal learning today. It is the relation between some of Hull's concerns and those of contemporary workers in behavior analysis that provides the structure for this review, as it seems that pointing out such relations is important for two reasons. For one thing, the basic message of the *Principles* is that behavior is lawful, and that the often bewildering complexity of observed data can be understood in terms of simple, quantitative, and above all discoverable, laws. If any research program is currently pursuing the basic scientific aim of formulating such laws it is surely that of contemporary workers involved in the quantitative experimental and theoretical analysis of behavior. For another thing, as will be argued below, several of the foci of the *Principles* serve as useful pointers to problems that contemporary behavior analysts have neglected to a greater or lesser degree.

SKINNER'S REVIEW OF *PRINCIPLES OF BEHAVIOR*

It would, however, be inappropriate to discuss the *Principles* in this journal without some reference to Skinner's own review (1944) of the book. A degree of antagonism between workers following the behavior-analytic tradition and those sympathetic to Hull is often taken for granted, and sometimes even surfaces as overt hostility, as in Malone's (1987) association between Hullian theory and cognitivism, an insightful connection which Malone did not, evidently, intend as a compliment to either. Given Skinner's revolutionary questioning (1950) of the value of learning theory of the hypothetico-deductive sort of which Hull was the principal exponent it is perhaps surprising to find that his review of the *Principles,* although incisive in its criticisms of technical aspects of the way that Hull's theory was developed and presented, does not criticize the Hullian enterprise in general nor usually take issue with the data presented or the conclusions drawn. In fact, Skinner's main concern seems to be the quasi-axiomatic system of postulates, corollaries, and deductions that Hull employs in the *Principles* to expound his theories.

The use of such a system, perhaps more appropriate to an axiomatic area such as

[1] Hull, Clark L. (1943). *Principles of behavior.* New York: Appleton-Century-Crofts. x + 422 pp.

I am grateful to Eliot Shimoff and Philip Hineline for providing me with copies of Hull's *Principles.* Reprints can be obtained from J. H. Wearden, Department of Psychology, The University, Manchester, M13 9PL, England.

geometry than an experimental science, has been widely criticized (even from within the Hullian camp by Spence, 1956, p. 98, see also Bergmann & Spence, 1941), and has been followed by very few (Voeks, 1950, is a rare example). Skinner's review of the *Principles* exposes the many failures of this approach in just over five printed pages. Skinner first discusses the historical evolution of Hull's system of postulates, which are allegedly primitive axioms of his learning system, and the theorems and corollaries that are deduced from them. Skinner notes that Hull's postulates are far from homogeneous: some are "in fact inverted definitions; others described quantitative processes"; other postulates are descriptions of empirically derived functional relations between behavior and controlling variables; others are derived from speculative neurology.

Skinner comments that Hull's "deductions" from postulates are

> often concerned merely with showing that complicated instances of behavior may be analyzed into simpler instances ... capable of being studied experimentally. This is an unavoidable task in a science of behavior, but to regard the simple case as postulate and the complicated as theorem is to extend the postulational framework beyond its sphere of usefulness.... To force simple scientific inferences into the postulative mold does not contribute to clarity, but rather to awkwardness and confusion. (p. 278)

Skinner points out that the "postulates" used by Hull are often complex, and that the use of symbols to represent various processes contributes little, because "they are used only to paraphrase what has already been said in words. There is apparently no instance in the book of a productive manipulation of symbols" (p. 278). Furthermore, Skinner argues that it is not clear which principles are "primary" and which "secondary" or derived, because new postulates are sometimes introduced when "facts cannot otherwise be accounted for" (p. 279).

Skinner objects specifically to Hull's Postulates of Afferent Neural Interaction and Behavioral Oscillation, remarking that both are neural fictions "with the single negative function of accounting for failure to predict." He also adds that "In his introductory chapter Hull inveighs against certain traditional psychological ghosts, but it is doubtful whether any of them is quite so ghostlike in function as Afferent Neural Interaction or Behavioral Oscillation" (p. 279).

When finally discussing the behavioral data that Hull presents in the *Principles,* towards the end of his review, Skinner identifies three principal difficulties. First, the various measures of Hull's theoretical constructs (e.g., the manifestation of "Effective Reaction Potential" in terms of response latency, response amplitude, resistance to extinction, etc.) do not always covary sufficiently well to measure properly the variable in question. Second, many of Hull's experimental demonstrations are prone to methodological problems, often resulting from his repeated use of the "complicated and unexplored motive of escape ... which is likely to confuse eliciting and reinforcing stimuli ..." (p. 280), and failures to distinguish between Pavlovian and operant conditioning processes. Skinner finally notes that the exposition provided in the *Principles* exhibits some mathematical weakness (such as describing three experimental points by an equation with three fitted constants and providing detailed instructions for habit strength calculation without describing the techniques necessary for measurement of this variable). In general, however, Skinner seems to approve of Hull's attempts at quantification and credits him both with "a willingness to abide by the experimental facts" and with "setting a record for the use of experimental material in a primarily theoretical work" (p. 280). Overall, therefore, although Skinner's review is clearly critical in tone, it does not read today as hostile, either personally or professionally.

In a fascinating foreword to the seventh printing of Hull's *Principles,* Spence (1966) discusses Hull's work in the context of two other books that "not only decisively determined the course" of the area of animal learning but "also literally instigated most of the research carried out in it" (p. vii). These are Skinner's *The Behavior of Organisms* (1938) and Tolman's *Purposive Behavior in Animals and Men* (1932). As Spence notes, most commentators comparing the three specific books, and the body of work produced by Hull, Tolman, and Skinner generally, have tended to emphasize the differences among them. Spence, however, takes a different position and discusses the similarities in the work of these three influential figures. In the first place, Spence notes (p. viii) that all three are "thoroughly behavioristic in approach" by virtue of their focus on behavior per se, rather than the mind or consciousness. Second, all three employ a molar and (usually) nonphysiological approach. Spence further points out that in terms of the actual definition and measurement of behavior, all three, including even Tolman in practice, work along very similar lines. Third, all three had "similar conceptions as to the task confronting them," this being the development of law of the form $R = f(S,A)$ where R stands for response measures, and S and A are independent variables such as the presence of certain discriminative or eliciting stimuli (S) and variables relating to an organism's motivational state or the experimental conditions (A). Next, all three were concerned with "the defining of potentially significant behavioral measures and the identification and specification of the relevant environmental variables" (p. ix), and agreement between them even extended "in principle at least, [to] their conceptions as to the role of theory and the form it should take in the total scientific enterprise" (p. ix).

Spence (1966) remarks that "to include Skinner as a party to this accord, however will no doubt come as a considerable surprise, if not shock, to some" (p. x). This rings even more true today but, as Spence makes clear, numerous similarities can be noted between the theoretical activities pursued by Hull and Tolman and Skinner's early work, particularly that presented in *The Behavior of Organisms* (1938). According to Spence, examination of Skinner's early work will show that "he not only engaged in a kind of theorizing, but that the type of theoretical construct he used was the same as that which Tolman, in principle, advocated and Hull, in practice, employed" (p. x). Spence further credits Skinner with being the "first to describe the role of intervening variables in psychological theorizing" (p. x) in Skinner (1931) and gives several examples of the similarities between the "hypothetical intermediate terms" and "states" used by Skinner (1938) and Hullian theoretical constructs.

Spence (1966) notes that Skinner's early theoretical work did not receive the attention it deserved (particularly that "neither Hull nor Tolman ever gave any evidence that they were aware of Skinner's excellent analysis and use" (p. xi) of theoretical terms), whereas developments from his empirical work, such as teaching machines and behavior modification, attracted wide interest, and he speculates that Skinner may have been discouraged from further theorizing by this history of differential reinforcement. Spence then ruefully concludes that "this is most unfortunate as it renders unlikely the probability of his interests ever shifting back to the task of developing the more abstract kind of knowledge that all sciences strive to achieve, a task for which *The Behavior of Organisms* showed him to be brilliantly qualified" (p. xi–xii).

It may be clearer, after reading Spence's (1966) comments, why Skinner's review of the *Principles* was milder in tone than might have been expected; perhaps, in 1944, the gulfs separating behavior analysis from other approaches to learning were narrower than they would later become. Spence's remarks are also relevant to present-day controversies as to whether different approaches to animal and human learning (radical behaviorist, associationist, cognitive, etc.) are incompatible or can, and should, be reconciled. Shimp (1976, 1984) has argued in the pages of this journal that cognitive constructs such as memory are useful in understanding some current problems in behavior analysis, but this view has provoked dissent (see Branch, 1977). The question of the relevance of studies of animal cognition to modern behavior-analytic research has likewise been controversial (see Wasserman, 1978, 1982, and Malone, 1982, for contrary views). Most recently, Williams (1987) has argued that associationist explanations of behavior may be relevant to some problems studied by behavior analysts. The tone of Spence's preface clearly puts him on the side of those who seek common ground between different approaches to animal learning. Furthermore, his comments suggest that retrospective examination can often find strong similarities between approaches that appear very different, particularly to their active contemporary protagonists. Currently, some developments in quantitative analysis of behavior seem to deviate from the traditional concerns of "prediction and control" of behavior, principally by developing accounts of schedule performance that are clearly couched in terms of hypothetical internal processes. These developments appear to put much current work in schedule theory close to conventional theoretical accounts of behavior based on intervening variables (such as those of Hull and Tolman) rather than to post-1950 radical behaviorism, although they may in fact represent a return to the philosophical position implicitly underlying the type of theoretical analysis carried out by Skinner in the early part of his career. Perhaps, like Spence, some future commentator may note that the differences that divide behavior analysts at the present time turned out to be less important than the common approaches and principles that united them.

LEARNING AND MOTIVATION: TWO HULLIAN PROBLEMS

Most of the remainder of this review is concerned with how modern research in behavior analysis deals with two issues that occupy a large amount of the space in Hull's *Principles,* the acquisition of behavior and behavior change, and motivation. Both of these problems seem neglected in the sense that modern schedule theory offers little or no systematic treatment of them, although in some cases substantial amounts of relevant data have been collected both by workers within the behavior-analytic tradition and those outside it.

Acquisition and Behavior Change

Acquisition of associations between impinging stimuli and responses occurring in their presence, represented theoretically in the *Principles* by the growth of habit strength, is a central topic of the book. Four chapters discuss the way in which habit strength changes as a function of the number of reinforced trials (chapter VIII), reinforcer magnitude (chapter IX), delay of reinforcement (chapter X), and asynchrony between stimuli and responses (chapter XI). Sometimes the emphasis is on the development of habit strength over trials, at other times the focus of interest is the asymptotic level of habit strength achieved. The empirical data with which Hull illustrates such processes are examples of behavior such as amplitude of conditioned responses (pp. 103 and 125), response latency (p. 126), running times (p. 149), and choice of one response over another (p. 151).

Recent systematic theories of learning in animals have afforded the details of behavior acquisition only scant attention, even when continuity between their theoretical concepts and those in the *Principles* seems clear. For example, modern Pavlovian theories, such as those of Rescorla and Wagner (1972) and Pearce and Hall (1980), use the core concept of *associative strength,* a variable that represents the degree of learning in a way similar to that in which habit strength does, although the associations formed involve conditioned and unconditioned stimuli, not stimuli and responses. However, discussions of these theories rarely involve consideration of the actual pattern of acquisition of behavior. Acquisition of schedule performance after initial operant training, or acquisition of performance after change of schedule, has likewise attracted little theoretical interest. The *JEAB* cumulative index for Volumes 21–40, for example, lists only 12 articles which have been classified by their authors in terms of the key word "acquisition." None of these articles is concerned with performance on standard reinforcement schedules such as variable-interval (VI) and variable-ratio (VR), and the material in them covers a wide range of issues, from autoshaping to the acquisition of trust in humans.

The general neglect of acquisition and behavior change in the operant field is particularly difficult to understand for two reasons. For one thing, Ferster and Skinner's (1957) classic work on reinforcement schedules, a cornerstone of behavior analysis, is concerned extensively with acquisition and behavior change, to the extent that a large proportion of the data presented comes from conditions in which steady-state operant responding has not been reached. For another, it is clear that some current theoretical approaches to schedule performance can, in principle, deal easily with acquisition data. For example, Myerson and Miezin's kinetic theory (1980) can be applied to behavior acquisition and change under concurrent schedules. Even more strikingly, computer simulations modeling the process of interresponse-time reinforcement (Peele, Casey, & Silberberg, 1984; Wearden & Clark, 1988) must *necessarily* produce predictions about acquisition data, because the simulations acquire behavior over sessions just as animals do. Yet, at present, no articles have been published in which extensive theoretical analysis of data on acquisition or change of schedule performance has been conducted.

One objection to studying acquisition or transition data might be that it is disorderly relative to that found in steady-state conditions, and as such might pose insurmountable problems to understanding. However, actual data on acquisition and change do not support this view. Ferster and Skinner (1957), Weiss (e.g., 1970), and Nevin's group of researchers (Nevin, 1974, 1979) have all presented data from transition states that show impressive regularities, suggesting that theoretical analysis of the effects of changing conditions should not be impossible.

For example, Ferster and Skinner (1957) found that the transitions from continuous reinforcement to fixed-ratio (FR) and fixed-interval (FI) schedules were sufficiently orderly to be represented by stylized plots of responses versus time (see their Figure 12 for FR and Figure 117 for FI). They also devoted considerable space to discussing data both from acquisition of schedule performance after continuous reinforcement and transitions from one schedule value to another (see pp. 42–57 for FR schedules and pp. 135–185 for FI).

It seems, therefore, that the current neglect of the Hullian problem of "learning" is justified neither by historical precedent in behavior analysis nor by fundamental deficiencies in present theoretical models, and it seems to be an area ripe for rapid developments in experimental and theoretical analysis. It may be that parallels between animal foraging in natural environments and performance on certain operant tasks (Abarca & Fantino, 1982; Commons, Kacelnik, & Shettelworth, 1987; Lea, 1979) will provide a spur for such developments, because foraging situations inevitably involve behavioral adjustment to change. I have suggested elsewhere (Wearden, 1988) that the fact that foraging research bears on this central problem of behavior change may make it ultimately the most valuable of recent work on biological influences on animal learning.

Another relatively neglected process intimately connected with changes in behavior is extinction. Although the decline in operant response rate when reinforcer delivery is discontinued is a commonplace result in the laboratory, systematic quantitative analysis and theory have been rare. As in the case of acquisition of operant behavior and the effects of changes in schedules, Ferster and Skinner (1957) showed a lively interest in this topic and presented data showing the changes in response rate consequent on extinction after a variety of conditions. These included extinction after simple VI and FR schedules (pp. 346–351 and 57–63, respectively) as well as extinction after training on more complex schedules such as multiple and tandem schedules with various sorts of components.

Attempts to understand extinction occupy two chapters (XV and XVI) of the *Principles,* where Hull's theory somewhat resembles that of Pavlov in being based on inhibition, and, like Pavlov, he assumes that extinction occurs because of the accumulation of inhibitory forces that suppress responding. The original learning, in Hullian terms the accumulation of habit strength, is unaffected by the extinction process in the sense that the stimulus–response links established by initial conditioning remain. As is well known, Hull proposed two inhibitory processes in the *Principles.* The first of these, reactive inhibition, is "a condition or state which acts as a primary negative motivation in that it has an innate capacity to produce a cessation of the activity" which produced it. Each response emitted (whether reinforced or not) generates reactive inhibition, and the actual amount occurring is a function of the effort required to produce the response (pp. 278–280). Reactive inhibition that accumulates also "diminishes progressively with the passage of time according to a simple decay or negative growth function" (p. 281).

Hull also proposed another inhibitory process, conditioned inhibition, that results from the reinforcement of behavior associated with the cessation of activities giving rise to reactive inhibition. This behavior is conditioned to currently impinging stimuli, as Hull's stimulus–response theory of learning holds that all behavior is, so reactive inhibition is "the somewhat paradoxical phenomenon of a negative habit, i.e., a habit of *not* doing something" (p. 282).

As Mackintosh (1974, pp. 414–418) points out, Hull's theory of extinction has several curious features. For one thing, it does not involve any new learning. Because the process of reactive inhibition operates whether or not responses are reinforced, the only difference between reinforced training and extinction is that reinforcer delivery in the former case counteracts the response-decrementing effects of reactive inhibition. As a result of reinforced training, habit strength eventually reaches an asymptote, whereas reactive inhibition will continue to accrue. Any response associated with cessation of responding will be reinforced by reduction of reactive inhibition and, therefore, even when responses are consistently reinforced, the tendency not to respond will grow until responding eventually ceases. This prediction is contradicted by the persistence of operant responding for hundreds or even thousands of sessions of schedule training and receives little support even from studies of extended training in lengthy alleyways.

A related oddity of Hull's theory of extinction is that, unlike that of Pavlov, the inhibitory process suppressing responding does not result from nonreinforcement, so there is no sense in which the animal learns, in extinction, that responses are no longer reinforced. Some other problems with Hullian extinction theory have been discussed by Gleitman, Nachmias, and Neisser (1954).

The difficulties encountered by Hull's attempts to deal with inhibitory tendencies and their opposition to reinforced responding have perhaps acted as a deterrent to modern schedule theorists attempting a more adequate analysis. It seems that contemporary workers treat the decline and eventual cessation of responding under extinction, as well as the role of nonreinforced responses under partial reinforcement schedules, either not at all or as an aside to other issues (e.g., Killeen, 1979). Herrnstein's matching-consistent equation (1970) and optimization models (Baum, 1981) both correctly predict a zero steady-state response rate under extinction because the rate of reinforcement delivered by the schedule is zero. Neither, however, constitutes an adequate theory of extinction apart from this, because neither is able to predict that different types of schedule will generate different amounts of responding in extinction, nor are phenomena like spontaneous recovery of responding after extinction (e.g., Boakes & Halliday, 1975; Ellson, 1938) obviously within their grasp. Computer models simulating interresponse-time reinforcement (Peele et al. 1984; Wearden & Clark, 1988) fare even more poorly, because in these models all systematic changes in behavior are driven by the occurrence of reinforcers, and thus in extinction responding continues indefinitely.

The treatment of extinction offered by Hull in the *Principles* regards the eventual cessation of responding as resulting from the buildup of some competing, inhibitory force. The principal determinant of resistance to extinction is thus the prevailing level of response strength obtaining at the start of extinction. As various commentators have pointed out, treatments such as Hull's have particular difficulty with the partial reinforcement effect (see Mackintosh , 1974, pp. 434–467), which is generally exemplified by the fact that responses like alley running that are established under partial reinforcement training extinguish more slowly than those established under consistent reinforcement, in spite of the fact that the latter training regime may engender more rapid responding initially. Indeed, some theories of extinction after instrumental learning have been designed primarily to account for partial reinforcement effects (e.g., Capaldi, 1966). A partial reinforcement effect under free-operant schedules would manifest itself in schedules that delivered low rates of reinforcement producing responding that was more resistant to extinction than those delivering high reinforcement rates, although the response rate before extinction would usually be greater in the latter case (e.g., Herrnstein, 1970). Work by Nevin and his associates (Nevin, 1974, 1979; Nevin, Mandell, & Atak, 1983) has not only been exceptional in its concern for the effects of extinction after operant training but has also made the notable discovery that partial reinforcement effects are difficult to observe under free-operant conditions (e.g., Nevin et al., 1983, pp. 54–55). Ironically, such a finding suggests that an inhibitory theory somewhat like the one that Hull advances in the *Principles* might enjoy more success explaining free-operant extinction than in accounting for the types of behavior to which it was originally applied. At present, however, the lack of a contemporary theoretical account of operant extinction is definitely remarkable, given the ubiquity of the phenomenon in day-to-day laboratory practice.

Motivation and Reinforcement

Motivation and reinforcement are central to Hullian theory. In the *Principles* Hull expounds his basic drive theory in chapter V, presents the well-known need-reduction theory of reinforcement in chapter VI, and introduces his famous equation expressing reaction potential (relating more or less directly to the strength of behavior observed) as a multiplicative function of habit strength and drive level in chapter XIV.

The effect on operant behavior of variations in deprivation level and reinforcer magnitude or type is another topic that has not been central to modern schedule theory. Once again, however, the precedent for the general lack of concern is not set by Ferster and Skinner (1957), who showed a keen interest in these problems. They conducted extensive analyses of the effect of deprivation level on FR schedules (pp. 71–77), presented data on satiation under FI (p. 320), and examined VI performance when pigeons' body weights were varied over a wide range (pp. 365–371). Their investigation of motivational variables further involved effects of prefeeding on VI (pp. 371–373), results from VI with water deprivation and reinforcement (pp. 373–376), and analysis of performance under multiple VI schedules with different reinforcers in each component (pp. 567–577).

In Hull's *Principles,* changes in deprivation level are manifested as drive differences, which interact with habit strength values to produce different reaction potentials and ultimately differences in performance. Reinforcer magnitude is not, however, treated in the *Principles* as a motivational variable, but instead as one that influences learning (in the form of habit strength) directly. Thus, animals enjoying larger reinforcer magnitudes actually accumulate more habit strength, both on each trial and in terms of asymptotic levels reached (p. 134), than those receiving smaller reinforcers. This formulation was later altered to encompass effects of reinforcer magnitude on *performance* rather than learning, (Hull, 1952; for a discussion of reinforcer magnitude as an incentive variable see Spence, 1956, and also Bolles, 1967, and Mackintosh, 1974).

How does modern schedule theory deal with the quintessentially Hullian problem of motivation, with its double aspect of drive (related to deprivation operations) and incentive (related to changes in reinforcer magnitude, type, or status)? In general, it seems that some types of schedule theories can encompass these problems, but their grasp overall is hardly more sure than that of Hull. Motivational questions appear to lie at present only within the province of molar schedule theorists. Ironically, although the processes embodied in molecular models of schedule performance (e.g., Peele et al., 1984; Wearden & Clark, 1988) are driven by the occurrence of reinforcement, these occurrences are considered to be punctuate events, devoid of properties such as magnitude or quality. Motivational level is, likewise, not represented in such models, but merely assumed adequate for reinforcers to be effective. Accounts of schedule performance based on the idea that behavior is generated by allocation of time between measured responding and other activities (Herrnstein, 1970), or general economic models assuming that animals respond so as to maximize the value (defined as the balance of gains obtained from and costs incurred by responding) such as Baum (1981), may deal with motivational effects more readily.

For example, consider the effects of deprivation level (in Hullian terms manipulation of drive) on operant responding. Modern operant theory generally treats deprivation operations as affecting the absolute or relative value of the reinforcers delivered (e.g., Herrnstein & Loveland, 1974; Michael, 1982). In general this approach works well, as in Bradshaw, Szabadi, Ruddle, and Pears (1983), who exposed rats to a range of VI schedules under two conditions that varied the deprivation level (from 80% to 90% of free-feeding weights) but kept reinforcer magnitude constant. All rats responded more rapidly at any particular VI value when 80% deprived, and the relation between response rate and reinforcement frequency was well described by Herrnstein's equation (Herrnstein, 1970) at both deprivation levels. Determination of the two parameters of Herrnstein's equation (k and r_0) showed that deprivation changes altered only r_0, generally held to reflect the rate of extraneous reinforcers present in the experimental conditions (Herrnstein, 1974). Such a result is clearly consistent with the view that changing deprivation level increases the "effectiveness of the reference reinforcer"

(Bradshaw et al., 1983, p. 272). Charman and Davison (1983) likewise found increasing r_0 with decreases in deprivation when pigeons were trained under multiple schedules of food reinforcement.

It seems, therefore, that an analysis of deprivation effects based on Herrnstein's equation can predict that response rates increase with increases in deprivation without undue difficulty. This analysis does not, however, contain any means of relating deprivation level to actual level of responding, a step that would require an assumption similar to Hull's assertion (p. 144) that drive increases linearly with duration of deprivation. Furthermore, it may be that not all the effects of drive changes on behavior can be expressed in terms of the effect of reinforcer value on current responding. Effects of establishing types of behavior with different drive levels may persist in extinction, as in the results of Perin extensively discussed in the *Principles* (e.g., p. 227). In this case, obviously, no reinforcers are delivered, so the different drive levels must either (a) have a direct energizing effect on behavior or (b) have led to the learning of different things initially. The former possibility is obviously similar to Hull's concept of general drive (*Principles,* chapter XIV), the latter (taken in conjunction with the general argument that deprivation level changes alter effective reinforcer magnitude) to Hull's assertion in *Principles* that different reinforcer magnitudes produce actual *learning* differences.

Bradshaw's laboratory also provides two thorough studies of the effect of changes in reinforcer magnitude on responding under VI schedules. In the first (Bradshaw, Szabadi, & Bevan, 1978) the concentration of sucrose in a constant volume of liquid reinforcer was increased from zero (i.e., distilled water). Increasing concentrations produced systematic increases in response rate in rats at any particular VI value, and Herrnstein's equation described the changes in response rate with reinforcement frequency well. Both parameters of the equation varied with reinforcer concentration. The asymptotic response rate, *k,* increased as reinforcer concentration increased (contrary to the prediction from Herrnstein, 1974), and the value of extraneous reinforcement rate, r_0, decreased with increases in reinforcer concentration, consistent with Herrnstein's prediction.

In the second study (Bradshaw, Ruddle, & Szabadi, 1981), the concentration of sucrose was kept constant, but the volume delivered was varied between conditions. Here, only the r_0 parameter of Herrnstein's equation was affected by the manipulation, with r_0 increasing with decreases in reinforcer volume in accordance with Herrnstein's (1974) prediction. However, there were no significant differences between the r_0 values derived from the two higher reinforcer volumes (0.1 and 0.05 mL), but the smallest (0.02 mL) produced a significant change from the 0.1-mL condition. Taken together, these two experiments suggest that reinforcer magnitude manipulations may be more complex than have previously been supposed. However, there is no doubt that, overall, increasing reinforcer magnitude tends to increase response rate under VI schedules, as do increases in deprivation level.

Overall, therefore, the analysis of deprivation and reinforcer magnitude effects deriving from Herrnstein's equation fares reasonably well, particularly considering that it was not specifically designed to accommodate data on these issues. Economic approaches (e.g., Baum, 1981), which derive response rate from a balance of reward value received from responding versus costs incurred, might provide similar discussions of deprivation and reinforcer magnitude effects via changes in the reward value of reinforcers, and some other contemporary theories (e.g., McDowell & Wood, 1984) can also produce ordinal predictions for changes in motivational variables. No current theory, however, is able to predict how much response rate should change as a result of some particular variation, in physical units, of reinforcer magnitude, a type of specification which Hull tried to provide in the *Principles.*

CONCLUDING COMMENTS

I have argued above that two quintessentially Hullian problems, learning and motivation, deserve more attention, experimentation, and theoretical analysis than they have received up to now in contemporary research on reinforcement schedules. The reasons for their neglect relative to some other problems remain obscure, but perhaps clues are offered by the very novelty and success of Skinner's own early researches, culminating in the publication of *The Behavior of Organisms* in 1938. In this work, Skinner was able to provide instances of response acquisition resulting from administration of just a single reinforcer, a result very different from, and in some ways much more immediately compelling than, the trial-by-trial acquisition of associations and habits as they were viewed by Pavlov and Hull. Furthermore, the dramatic effects of response shaping argued strongly for the virtues of the theoretical concept of direct strengthening of behavior itself, rather than explanations in terms of the more indirect effect of environmental events on internal processes as advanced by Hull in the *Principles*. Indeed, it is the emphasis on direct environmental control of behavior that gives experimental analysis of behavior some of its most distinctive features. Nevertheless, even for the purposes of prediction and control of behavior, an interest in changes in performance over sessions after changes in schedule conditions, or as a result of experimental manipulations of motivational variables such as deprivation and reinforcer magnitude, seems almost certain to be productive, as the work of Ferster and Skinner (1957) themselves shows. Perhaps Hullian problems have been neglected unduly by contemporary schedule researchers because of an implicit assumption that they might be attacked only in terms of Hullian ideas, involving confused notions about "drives," elementary forms of associationism, and beliefs about inhibitory processes that were later shown to be illogical. But this pessimistic view ignores the enormous progress that has been made in understanding many aspects of behavior in the years since the appearance of *Principles*, a substantial portion of which has arisen from work appearing in this very journal. Perhaps with the tools more recent researchers have provided we can take a fresh look at learning and at problems of motivation and even make some progress towards the future condition, of which Hull writes movingly in the closing pages of the *Principles*:

> There is good reason to hope that the behavioral sciences will presently display a development comparable to that manifested by the physical sciences in the age of Copernicus, Kepler, Galileo, and Newton. . . . But we should not deceive ourselves. The task of systematically developing the behavior sciences will be both arduous and exacting, and many radical changes must occur. . . . The great task can be no more than begun by the present generation of workers. Hope lies, as always, in the oncoming youth, those now in training and those to be trained in the future. Upon them rests the burden of the grinding and often thankless labor involved, and to them must rightfully go the thrill of intellectual adventure and the credit for scientific achievement. Perhaps they will have the satisfaction of creating a new and better world. . . . (pp. 401-402)

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