

# How adaptive behavior is produced: a perceptual-motivational alternative to response-reinforcement

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**Abstract:** The sway that the response-reinforcement framework (Spencer, Thorndike, Hull, Skinner) has held on the behavioral sciences for nearly a hundred years is finally ending. The strength of this framework lay in providing concepts and methods for studying the effects of hedonic (reinforcing) stimuli on the repetition of specified responses acquired in instrumental training situations of various kinds. Its weakness lay in the invalidity of its central assumptions, stimulus-response association and response-reinforcement, which could not deal with motor-equivalence and flexibility (or "intelligence") in behavior. To the four decades of incisive criticism on particular theoretical and empirical grounds, a more comprehensive challenge to the response-reinforcement framework is now added by the newer ideas about the nature of cognitive, motivational, and response-production processes that have emerged from the work of ethologists, neuroscientists, and cognitive psychologists. An alternative framework, incorporating the newer ideas, is clearly needed.

The particular framework proposed here is based on the ideas of perceptual learning of stimulus-stimulus correlations and of a motivational (rather than reinforcing) role of hedonic (incentive) stimuli. According to it, an act is produced when its *act-assembly* is activated by a pexgo (perceptual representation) of a certain eliciting stimulus complex (ES). When certain eliciting stimuli are correlated with incentive stimuli, they acquire motivational properties that serve to strengthen the pexgos generated by those eliciting stimuli and thereby increase the probability of activation of the corresponding act-assemblies. Motivation thus influences response production, not by directly instigating "existing" responses, but by modulating the strength of pexgos of eliciting stimuli for the succession of acts that comprise a response. Therefore, a response is always constructed afresh on the basis of current perceptions; not even a stable and stereotyped response occurs as a mere activation of a preformed motor program. The topography of any response that emerges is determined by the nature of the motivational state and the momentary spatiotemporal distribution of eliciting stimuli of changing motivational valence.

By suggesting that the animal learns the overlapping and nested correlations between the stimulus events that commonly occur in a given situation, and by separating what is learned from the processes of response production, the proposed perceptual-motivational framework seems capable of dealing with the problems of motor equivalence and flexibility in adaptive behavior. Some implications of this approach for further behavioral and brain research on such problems as behavior modification, learning by observation of models, analysis of causality, and search for neural substrates of learning and response production, are outlined.

**Keywords:** act-assembly, behavior modification, central motive state, classical conditioning, contingency organization, eliciting stimulus, incentive motivation, instrumental training, learning, response production, two-factor theory

More than a hundred years ago, even before the publication of Darwin's (1859) theory of natural selection, two "evolutionary philosophers," Alexander Bain (1868) and Herbert Spencer (1872 - 73), combined the idea of selection with that of hedonism and proposed the first psychobiological view of adaptive actions of animals and man. Their view was that through diffuse discharges of nervous energy to muscles, many random movements are produced spontaneously, and that those movements that are followed by pleasure tend to be selectively strengthened and maintained, and that those that are followed by pain are selectively enfeebled and abandoned. This favors successful adaptation and species preservation because evolutionary pressures have determined that things that are affectively pleasant for a species are by and large beneficial to that species, and things that are affectively unpleasant for the species are generally harmful to the species. The mechanism Bain and Spencer invoked was this: hedonically pleasant stimuli release large discharges of nervous energy toward the nerves and muscles that are or have

recently been active, so that the motor channels underlying action that successfully leads to pleasant stimuli are made more permeable; painful stimuli act in the reverse way, making the motor channels less permeable.

This postulate of the strengthening or weakening influence of hedonic stimuli on the neural channels involved in recent movements served as the core around which the response-reinforcement view of learning and behavior modification gradually took shape (see Postman, 1947). Early in this century, Thorndike (1911) suggested that learning is the formation of new situation-response connections, which are strengthened or weakened by reward and punishment - the *law of effect*. He applied this to the problem of teaching. Hull (1943) developed a complete system of behavior based on the idea of stimulus-response associations and their *reinforcement* by drive reduction. This system was elaborated for the study and interpretation of a wide range of phenomena, such as those of social learning, anxiety, and psychotherapy (Dollard & Miller, 1950; Miller & Dollard,

1941; Mowrer, 1939, 1960a). Concurrently, Skinner (1938, 1953), less interested in the explanation of behavior and more in the principles of its control, developed a response-reinforcement technology that proved useful in training and remedial procedures for varied problems of education, health, and social management (see Skinner, 1959). All this produced a climate of opinion in which the response-reinforcement framework or paradigm came to dominate both the theoretical and practical aspects of the study of behavior.

This climate of opinion seems to have cushioned the response-reinforcement framework from a variety of criticisms that were articulated against it over nearly half a century. But now the cumulative thrust of several incisive theoretical and empirical objections is beginning to make the criticisms appear more pertinent and to be taken more seriously. However, such remains the sway of the response-reinforcement framework that those who work within it have not ventured beyond offering minor modifications or extensions, presumably believing the basic tenets of the framework to be essentially sound. The purpose of this paper is (*a*) to summarize and restate the reasons why the basic tenets of the response-reinforcement framework are invalid – that the difficulties cannot be simply patched up with minor revisions; (*b*) to define the elements of a theoretical *alternative* to the response-reinforcement idea; and (*c*) to propose a tentative framework that indicates the direction in which we might reasonably proceed in the long task of developing a coherent and complete account of the production of adaptive behavior.

The first step is obviously to describe what the response-reinforcement framework and its basic tenets really are. This is perhaps best done by contrasting the response-reinforcement and incentive-motivational assumptions about the role of reinforcing stimuli (pleasure and pain) in the learning and performance of responses. The idea of incentive-motivation is an old one (Crespi, 1942; Hull, 1952; Logan, 1960; Rescorla & Solomon, 1967; Seward, 1956; Sheffield, 1966), and it was often invoked as a subsidiary principle within the response-reinforcement framework. During the past two decades, attempts have been made to use the concept of incentive-motivation as the basis for developing an alternative approach to the study and interpretation of adaptive behavior (Bindra, 1968, 1969a, 1974, 1976; Bolles, 1972; Mowrer, 1947, 1960b). A comparison of the concepts of response-reinforcement and incentive-motivation should therefore bring out and illustrate the differences in their basic tenets.

### **Response-reinforcement versus incentive-motivation**

Everyone recognizes that hedonic stimuli – objects and events that typically give pleasure or pain – are important in the production and modification of behavior. There are two ways in which they may exert their influence. First, they may serve as reinforcers – rewards or punishments – of the actions that the animal has just performed, and may thereby influence the future probability of occurrence of those actions. Second, they may serve as incentives – stimuli that induce particular motivational states – and thereby influence a wide range of subsequent actions. If you receive some pleasant news in a letter, you may repeat the actions that led to the reading of the news (going to your mail box, taking the letter out of the envelope, unfolding it, etc.), but it may also induce an expansive mood that may alter several aspects of your behavior in the hallway, at the morning coffee break, and in relation to casual visitors. Clearly, a pleasant stimulus may influence subsequent behavior either by making it more likely that prior ("reinforced") responses will be repeated or by inducing motivational changes that facilitate the same or other responses, or by some combination of the two.

Common sense sees no difference between the reinforcing and motivational interpretations of the role of hedonic stimuli in behavior; many popular aphorisms, such as "spare the rod, spoil

the child" and "flattery will get you everywhere," are vague enough to be consistent with both interpretations. Nor was the difference clearly seen by those thinkers who wrote about the role of pleasure and pain in the control or explanation of human actions; Machiavelli, Hobbes, Bentham, and Freud remained obscurely ambiguous on this point. Modern psychologists and other serious students of behavior have too often failed to take account of the difference. Since hedonic stimuli used as reinforcers (rewards and punishments) for particular responses on a given occasion may, on subsequent occasions, induce a certain motivational state (i.e., lead to anticipation of those hedonic stimuli), a clear conceptual separation between the possible response-reinforcement and incentive-motivational roles of hedonic stimuli has been slow to emerge.

The three essential differences between the two may be explained with reference to Table 1. First, the response-reinforcement hypothesis assumes that behavior modification produced in a given stimulus situation (SS) is dependent on the contiguity or correlation between what the animal was *doing*, under the given stimulus and organismic conditions, at the time of presentation of some hedonic stimulus, that is, on the response-reinforcement contingency ( $R:S^H$  or  $S:S^{Reinf}$ ). The incentive-motivation hypothesis assumes that the behavior modification is dependent on the contiguity or correlation between what the animal was *perceiving*, under the given stimulus and organismic conditions, at the time of presentation of the hedonic stimulus, that is, on the stimulus-incentive contingency ( $R:S^H$  or  $S:S^I$ ).

Second, at least implicitly, the response-reinforcement hypothesis attributes the learning reflected in the animal's behavior to the selective strengthening of some sort of neural connection between the central representation of the stimulus situation ( $s_s$ ) and the central representation of the response ( $r$ ); the locus of association is the  $s_s-r$  connection (stimulus-response or S-R learning). According to the incentive-motivation hypothesis, on the other hand, the learning consists of the selective strengthening of some sort of neural connection between the central representation of the stimulus that was being perceived ( $s_2$ ) and the central representation of the hedonic stimulus ( $s^H$ ); the locus of association is the  $s_2-s^H$  connection (stimulus-stimulus, or S-S learning). I shall call the momentary central representation that underlies the perception of a stimulus a pexo of that stimulus (see Bindra, 1976, Chapter 5); pexo is an acronym for presently excited gnostic organization.

The third difference concerns the postulated or implied mechanism of response production on the test trial. According to the response-reinforcement hypothesis, a pexo of the stimulus situation ( $s_s$ ), since it has become more strongly connected to a particular response (say,  $R_2$ ), gives rise to  $R_2$ , rather than to other responses (say,  $R_1$  or  $R_3$ ). According to the incentive-motivation hypothesis, on the other hand, a pexo of a particular stimulus (say,  $S_2$ ) generates a pexo of the hedonic stimulus ( $s^H$ ) and thereby generates the same motivation (central motive state or cms) as is normally generated by the hedonic stimulus ( $S^H$ ) itself; this central motive state, in combination with the detailed sensory-spatial features of the situation, then determines what response will emerge. Thus, according to the response-reinforcement view, the response that is produced is a repetition of the response that was selectively strengthened from among other preexisting responses; in contrast, according to the incentive-motivation view, the (unspecified) response that is produced is a fresh construction dependent on the motivational state and the pexo generated by the prevailing stimulus situation. I shall explain this further in a later section.

### **Objections to the response-reinforcement idea**

As early as the 1920s, Lashley took strong exception to the type of neural model implied in the idea that adaptive behavior de-

Table 1. Response-reinforcement and incentive-motivation hypotheses of the role of hedonic stimuli ( $S^H$ ) in response production

(1) Hypothesis	(2) Training trial (s)	(3) Test trial	(4) Critical contingency	(5) Association learned	(6) Mechanisms of response production
Reinforcement	[SS] $S_1, S_2, S_3$ $R_1, R_2, R_3$	: $S^H$	$R_2:S^H$	$ss-r_2$	$SS \rightarrow ss \xrightarrow{r_1} r_2 \rightarrow R_2$ $r_3$
Motivation	[SS] $S_1, S_2, S_3$ $R_1, R_2, R_3$	: $S^H$	$S_2:S^H$	$s_2-s^H$	$S_1 \rightarrow s_1$ $S_2 \rightarrow s_2-s^H \rightarrow cms \rightarrow R?$ $S_3 \rightarrow s_3$

Note: An animal is put in a certain stimulus situation (SS). The animal perceives various stimulus configurations ( $S_1, S_2, S_3$ , etc.), and it also displays some responses ( $R_1, R_2, R_3$ , etc.). Now the animal is presented with a hedonic stimulus ( $S^H$ , say a highly palatable food). At the time of presentation of  $S^H$ , the animal is both doing something (say,  $R_2$ ) and perceiving something (say,  $S_2$ ), as in column 2. When the animal is subsequently tested in the same situation (column 3), its behavior is modified in some respects (learned behavior modification). So far there is no difference between the two views. The difference arises when one asks how  $S^H$  influenced the behavior of the animal. These differences concern what contingency is considered important (column 4), what associations of neural representations (small case letters) are formed (column 5), and what mechanisms are postulated for response production (column 6).

pends on the activation of distinctive stimulus-response (i.e., sensory-motor) neural channels, and that learning consists in the strengthening of connections in particular channels (e.g., Lashley & McCarthy, 1926). He pointed out that the same response, defined in terms of a certain outcome, may be produced on different occasions by the use of different muscles, involving the activation of different motor channels. One may protect one's eyes against strong light by shielding them with a hand, by turning one's head, or by turning the whole body around. How can this "motor equivalence" (substitutability of movements) involved in a response be reconciled with the idea that learning consists of the selective strengthening and weakening of specific sensory-motor connections? There is no way in which a reconciliation can be achieved within the response-reinforcement framework, because it rests on the idea of a selective influence on the specific action the animal has just performed. With the required selective reinforcement of particular sensory-motor channels there can be no motor equivalence or flexibility, and the strengthening of such sensory-motor junctions (stimulus-response associations) is the only mechanism of learning provided by this framework.

Over the next three decades, the above fundamental argument against the idea that the locus of learning is stimulus-response association was repeated, in different terms, by Köhler (1929), Allport (1937), and Tolman (1932, 1948, 1949). In view of these criticisms, the only way left to hold on to the stimulus-response association idea was to abandon concern with the problem of how different movements (sensory-motor channels) can produce the same response, and to consider the response as the ultimate unit for behavioral analysis. This is essentially what Skinner (1935) had advocated, saying that a response is defined not by its specific movements, but in terms of certain measurable environmental outcomes. Learning theorists thus turned to the "functional relations" between generic stimuli and generic responses, without regard to what specific sensory or motor components they were made of (see Hilgard, 1956). While this circumvention of the problem of motor equivalence may have "emancipated" psychology from a neurological straitjacket, it did so only by evading the fundamental problem of how adaptive behavior is put together; it sacrificed explanation to the empirical goal of enunciating descriptive principles.

This atheoretical approach was extended to the second main tenet of the response-reinforcement framework, the idea that hedonic (reinforcing) stimuli strengthen stimulus-response functional relations. Skinner (1950), being more concerned with controlling behavior than with understanding its mechanisms, had taken no clear stand on how reinforcing stimuli reinforce responses. This position was now more explicitly articulated by

others (e.g., Premack, 1959). Concern with the processes by which hedonic stimuli influence learning and performance was thus gradually abandoned by learning theorists in favor of the empirical principle that response-reinforcement somehow changes the functional relations between stimuli and responses.

But difficulties arise even within such an empirical reinforcement principle. First, the phenomena of latent learning, sensory preconditioning, and problem solving show that a good deal of learning and behavior modification can occur without the reinforcement of any specific responses (see Kimble, 1961, Chapter 8). Second, experiments on learning without responding (e.g., response substitution, learning while paralyzed) make it clear that not even the occurrence of a response during learning, let alone reinforcement, is necessary for demonstrating a learned behavior change (e.g., Dodwell & Bessant, 1960, Köhler, 1925; Solomon & Turner, 1962). If the response is not there, then what functional relation is reinforced? Third, it has been shown that many phenomena of adaptive behavior, such as "instinctive actions" described by ethologists, "adjunctive" (or displacement) activities, and observational learning may fall outside the domain of the response-reinforcement framework (e.g., Bandura & Walters, 1963; Falk, 1971; Tinbergen, 1951). Finally, it has been repeatedly demonstrated by the proponents of the response-reinforcement view that this framework provides an inadequate account even of the details of behavior in operant training experiments that use explicit response-reinforcement contingencies in training (e.g., Breland & Breland, 1961; Shettleworth, 1975; Staddon & Simmelhag, 1971).

Agreeing that the response-reinforcement framework, as traditionally conceived, is inadequate, several authors have offered new schemes that try to deal with the above findings by supplementing the response-reinforcement idea in some way. Some (e.g., Bolles, 1972; Estes, 1969; Walker, 1969) have suggested that, in addition to S-R associations, S-S, R-S, and R-R associations may also be learned. Two-factor theorists (e.g., Mowrer, 1947; Rescorla & Solomon, 1967) have explicitly incorporated a motivational factor as an essential aspect of learned behavior modifications; thus while the response "selection" is thought to depend on the principle of response-reinforcement, response instigation is said to depend on classically conditioned incentive-motivation. Still others (e.g., Hearst & Jenkins, 1974; Staddon & Simmelhag, 1971) have proposed that there are two components of adaptive behavior; one component (e.g., "sign tracking" or "interim" activities) involves S-S associations and incentive-motivation while the other component (instrumental responding or "terminal" activity) involves S-R associations and response reinforcement. Though all these theoretical schemes show some advantage in handling the above findings at an empirical level,

they do not come to grips with the problem of how a new stimulus-response relation actually gets learned. They continue to resort to the principle that reinforcement strengthens stimulus-response association, without explaining how the problem of motor equivalence is to be solved. As we have seen, without explaining how different movements or acts can function as the same response, the idea of the strengthening of specific stimulus-response associations by reinforcement turns out to be inconsistent with the observed flexibility of behavior. If the focus is on *explaining* the flexibility of adaptive behavior, the response-reinforcement idea must be completely abandoned – even as a subsidiary principle.

In view of these difficulties, some thinkers have tried to imagine ways in which hedonic stimuli may influence behavior without strengthening specific stimulus-response associations. One approach is cybernetic (e.g., Estes, 1972; Miller, 1963; Mowrer, 1960b). It assumes that hedonic stimulation serves as an energizer of some process underlying the response-in-progress, so that once a response has occurred, its continuance or repetition depends on continual modulation by the energizing process, variously described as "amplifier," "go mechanism," "emotion," etc. This is essentially a motivational view, but one that fails to deal with the initial production of the response; it also assumes that the role of motivation is merely to energize responses that lie ready-made in some store. A related view (e.g., Bolles, 1972; Estes, 1972; Konorski, 1967) is that the animal learns the relation between a response as a stimulus entity and reinforcement, so that the "idea" of a response leads to the expectancy of reinforcement, and this expectancy somehow instigates the production of *that* response, or the energizing of the idea of a response by the expectancy leads to *that* response. The difficulty with this view is the same as that with William James's (1890) theory of voluntary action: It ignores the problem of the translation of a response-reinforcement expectancy or the energizing of the idea of a response into the production of that response. There is little doubt that animals learn the correlations between their actions and the consequences of those actions, and this explains the surprise animals show when normal consequences fail to follow, but the learning of this correlation in no way explains how that specific action gets produced. What, for example, is the functional relation between the (motor) processes that produce a reaching response and the (sensory or conceptual) processes that represent the idea of that response?

#### Elements of a new approach

If the response-reinforcement idea is to be wholly discarded, how might we proceed toward formulating an alternative approach to the study and explanation of adaptive actions? Traditionally, those who have criticized the inadequacy of the response-reinforcement framework for handling flexibility of behavior have invoked some cognitive (Tolman, 1932, 1948) or motivational (Mowrer, 1947, 1960a, 1960b) concepts. The general thrust of such concepts (e.g., cognition, expectancy, emotion, etc.) is that the motor output comprising a response is separated from current sensory inflow by the intervention of some central system of knowledge or motivation, and it is this system, not sensory-motor associations, that determines what the response will be. It is assumed that such a central system *somewhat* flexibly adjusts the motor output to the ever-changing stimulus and organismic conditions.

The idea of a central system (cognitive or motivational or something else) not rigidly tied to any specific response is certainly the minimum requirement for dealing with the problem of flexibility. But a difficulty with this idea is that it has remained too general and vague, and no specific principles of response production have emerged from it. An adequate account must indicate not only how flexibility might be achieved but also how the occurrence of whatever specific response occurs on a given

occasion is determined. The response-reinforcement view was developed to deal with the latter but failed to cope with the former; the reverse appears to be the case with cognitive (Tolman, 1932) and motivational (Mowrer, 1960b) views.

During the past three decades, the work of ethologists, neuroscientists, and cognitive psychologists, as well as of learning theorists, has produced many new findings. I have recently reviewed these findings and have abstracted a few neuropsychological concepts as the elements of a new approach to the explanation of adaptive behavior (Bindra, 1976). These neuropsychological concepts suggest a view of cognitive, motivational, and response-production processes that is quite different from the conception of these processes on which the earlier accounts of adaptive behavior were based. Before outlining a new framework I shall briefly review some of the important emerging ideas. This review deals with four suggestions that are pertinent to any account of adaptive behavior. These questions are: (1) How are stimuli perceived? (2) How is knowledge of the environment represented in the brain? (3) What is the nature of the motivational factor? (4) How does motivation influence response production?

**Perception (pexo).** How are the environmental stimuli, in relation to which an animal acts, represented in the brain – how are they perceived? The idea that has been gaining acceptance since the 1940s is that a stimulus is perceived when an *existing* representation of it in the brain is activated by the current sensory inflow. The existing representation has been called cell-assembly, prototype, template, ideal, and so forth. The essential idea is that structural representations of *stimuli as such* (triangles, apples, words, etc.) exist in the brain and that they need only to be triggered by a certain complex of stimulus features to cause perception. In opposition to this view, I have suggested that no structural representation of a stimulus *as such* exists in the brain, and that perception involves the generation of a pexo – spatially and temporally ordered activation of certain structural gnostic elements or pexo components. The gnostic elements are gnostic-neurons and gnostic-assemblies, the latter being acquired through individual experience. The generation of a pexo depends on both the current sensory inflow and central neural influences from contingency organizations representing knowledge of the environment (see below).

It is unnecessary here to reproduce the reasons for introducing the concept of pexo or to show in detail how it differs from other perceptual constructs (Bindra, 1976, Chapter 5; Bindra, 1977). However, it is important to note two points. First, my view implies that there is no such thing as *the* pexo of a certain object; the same object is likely to generate different pexos on different occasions, depending on the exact stimulus features (as well as the sensory modalities) represented in the current sensory inflow and on the exact nature of the central influences. The second implication is that the pexo generated describes not some abstract ideal or concept of the stimulus, but that the content and temporal ordering of the activation of gnostic-neurons and gnostic-assemblies that make up a pexo is determined by moment-to-moment fluctuations in both current sensory inflow and central influences. In other words, the primacy and critical importance of the details of *current* sensory and central inflow is retained in my view of perception; this is essential if the animal is to adjust to moment-to-moment stimulus fluctuations of its internal and external environment.

**Knowledge (contingency organization).** It now seems fairly well agreed that the knowledge that an animal acquires of its environment consists not of stimulus-response units or habits but of the *correlations* that exist between various events that typically occur in a given situation (Bindra, 1976, Chapters 6 and 7; Mackintosh, 1974). For example, a rat learns that being put on a carrying stand (Event 1 or E<sub>1</sub>) is correlated with being taken to an experimental room (E<sub>2</sub>), E<sub>2</sub> is correlated with being put in an ex-

perimental chamber ( $E_3$ ),  $E_3$  is correlated with an empty (waiting) period of 50 seconds ( $E_4$ ),  $E_4$  is correlated with the onset of a tone ( $E_5$ ), and  $E_5$  with the target event, delivery of food ( $E_6$  or  $E_T$ ). Elsewhere I have explained how learned correlations are different from associations, and how a neural *contingency organization* representing a correlation (positive or negative) between two events may develop through experience (Bindra, 1976, Chapter 6).

There are three points about situational correlations and contingency organizations that should be borne in mind for the present discussion. First, a contingency organization that represents a positive correlation,  $E_1:E_2$ , links the pexo components of  $E_1$  ( $pE_1$ ) with the pexo components of  $E_2$  ( $pE_2$ ) in such a way that the activation of  $pE_1$  (by  $E_1$ ) results in the anticipatory priming of  $pE_2$  even before (in the absence of) the occurrence of  $E_2$ ; the momentary level of such anticipatory excitation of the pexo components of  $E_2$  is a function of the predicted imminence of  $E_2$  (which in turn depends on the average time interval between  $E_1$  and  $E_2$  during learning). Second, the correlations that exist between the events of a situation are multiple and *overlapping*;  $E_1$  is correlated not only with  $E_2$ , but also with  $E_3$ ,  $E_4$ ,  $E_5$ , and  $E_T$ , and  $E_2$  is correlated not only with  $E_3$ , but also with  $E_4$ ,  $E_5$ , and  $E_T$ , and so on. This means that the total anticipatory primed excitation of  $E_T$  is determined not only by  $E_5$ , but by the context as a whole – the usual sequence of events ( $E_1$ ,  $E_2$ ,  $E_3$ , and  $E_4$ ) that precedes  $E_5$ ; the disruption of this context would attenuate the primed excitation of  $E_T$  produced by  $E_5$  (alone). The third point is that correlations that describe the usual sequence of events in a situation are *nested*; the correlations of  $E_1$  encompass the correlations of  $E_2$ , and those of  $E_2$  encompass those of  $E_3$ , and so on. This means that a greater number of contingency organizations are weakly excited by the earlier events (in the given situation), but fewer contingency organizations are strongly excited by later events; this corresponds to the fact that the target event ( $E_T$ ) is weakly predicted by the earlier events and is strongly predicted by the later events in the given context. Thus the nested arrangement insures that the level of primed excitation of a certain event is proportional to the imminence of its occurrence.

It should be clear that the main functional outcome of the perception of an event,  $E_1$ , in a given, familiar situation, is the anticipatory priming of the pexo components of events ( $E_2$ ,  $E_3$ , etc.) correlated with or predicted by  $E_1$  in that situation. Thus the perception of subsequent (predicted) events is facilitated by the earlier (predictive) events; priming of, say,  $pE_2$  by  $pE_1$  facilitates the perception of  $E_2$ . This means that the knowledge that an animal acquires of a situation provides a basis for facilitating the perception of ("attending to") the likely future events. This general view of knowledge representation in terms of overlapping and nested correlations, contingency organizations that prime pexo components and facilitate the perception of predicted events, and the dependence of the level of primed excitation on context, offers considerable scope to any new approach for dealing with the problem of flexibility.

**Motivation (central motive state).** Concerning the nature of the motivational factor, the traditional idea has been that motivation is primarily, if not exclusively, a matter of internal, organismic conditions (food depletion, dehydration, changes in hormonal levels, and other metabolic variations). In contrast, the newer concept of motivation rests on the observation that motivation is as much a matter of external stimulation by hedonic stimuli (incentive objects and events) as it is of internal organismic conditions. The evidence for the importance of environmental stimulation in the generation of motivational states has come from three independent lines of inquiry: neurophysiological (e.g., Pfaffmann, 1969), psychological (e.g., Bindra, 1968; Pfaffmann, 1960; Young, 1948), and ethological (see Hinde, 1966, Chapter 15). As opposed to neutral stimuli, hedonic (or "affectionate," or "emotional," or "reinforcing," or "releasing," or "incentive" – whatever one calls them) stimuli have the property of reliably producing appetitive or aversive reactions and being affectively pleasing or discomforting. They include sights, sounds, tastes, odors, temperatures, and cutaneous textures that are provided by such biologically important objects, events, and situations as food, water, a sexual partner, a nest, the call of a distressed offspring, the shape of a predator, and injurious levels of heat or cold. The organismic conditions are important in that they serve as "gates" or limits within which certain particular incentive stimuli become effective (Bindra, 1969b). However, no motivation for a particular goal is generated by the organismic conditions per se; the generation of particular goal-directed motivational states is critically a matter of environmental incentive stimulation. (The earlier anticipatory-goal-response view, that incentive-motivational effects are themselves dependent on sensory feedback [the rg-sg mechanism] from previously reinforced responses [e.g., Amsel, 1962; Spence, 1960], is also now known to be untenable. Conditioned stimuli can generate motivational effects independently of the occurrence of any particular visceral or consummatory responses that might have been reinforced [for a review, see Bindra, 1968; Rescorla & Solomon, 1967].)

One implication of this incentive view of motivation is that the environmental stimulus that generates a motivational state can also serve as the goal stimulus to which the action is directed. This provides an uncomplicated mechanism for the coordination of motivation and action. For example, an eating central motive state would direct (or attract) the animal to the very environmental stimuli that played a part in generating that state. This kind of mechanism appears to be a more plausible basis of coordinating motivation and action than the "drive stimuli" idea of Hull (1943), which has been found to be inadequate (see Kendler, 1946).

**Motivational influence (pexo priming).** Concerning the role of motivation in response production, the idea implicit in earlier accounts has been that the performance of a response involves two steps: its *selection* by a certain associative factor (e.g., habit, discriminative stimulus) or a cognitive factor (e.g., expectancy, reasoning), and its *instigation* by the motivational factor (Hull, 1943; Skinner, 1953; Tolman, 1932). Thus the motivational factor has been thought to influence behavior primarily by a direct effect on response production. Recent evidence makes this traditional but still widely held view quite untenable.

The critical evidence comes from experiments involving electrical stimulation through electrodes implanted in the hypothalamic-limbic "motivational sites" of the brain. When appropriate pulses are passed through an electrode implanted, say, in the "feeding" site, even a sated animal will begin to show food-directed behavior. It has been shown that the site of electrical stimulation is not tied to any specific class of responses, but determines only the general goal (Flynn, 1967; Miller, 1961; Roberts & Kiess, 1964). For example, the site of stimulation determines whether an animal would become inclined to attack, eat, or copulate; the specific actions (approach, lever-press, maze-traversal) depend on the requirements of the situation and may vary greatly. Thus motivational influence is not specifically tied to any particular forms of motor organizations.

An important point about the above experiments is that the situation and the organismic state of the animal may be held constant, yet the electrical stimulation at different sites makes the animal act in relation to different objects (food, sexual partner, attackable object, etc.) in the situation or to different aspects (stimulus features) of those objects (Flynn, 1967; Roberts, Steinberg, & Means, 1967). This means that the generation of a motivational state makes certain environmental stimuli, or some features of those stimuli, relatively more effective than they would otherwise be at that time. In other words, a motivational state may serve to bias the generation of pexos of environ-

mental stimulus complexes that characterize particular types of goal or incentive objects. I have suggested that the mechanism of such ("attentional") bias could be the selective motivational priming of the pexgo components of certain relevant environmental stimuli through contingency organizations and motivational states (Bindra, 1976, Chapters 9 and 10; 1977). Whether or not the mechanisms of motivational influence I have suggested turn out to be tenable, it now seems reasonable to assume that motivational states influence the production of directed responses, not by a direct influence on motor outflow, but by influencing perceptual processes.

The above brief account of the change in ideas about perception, knowledge representation, nature of motivation, and the locus of motivational influence suggests that any new framework for the study and explanation of adaptive behavior should, at least in these respects, be quite different from the earlier ones. Of course, the proposed concepts of perception, knowledge, and motivation can serve as elements of several different types of new frameworks. One such framework that I have been gradually putting together over the past several years (Bindra, 1968, 1969a, 1974, 1976) is outlined here.

### A perceptual-motivational framework

The framework I outline here is tentative and incomplete, offered more as a fresh starting point and avenue of approach rather than as a formal theory. It incorporates the elements formulated in the last section, but also makes some additional assumptions. Though conceived as a framework for the study of behavior at all levels, I shall be concerned here with only animal behavior in typical learning experiments – the type of problems to which the response-reinforcement framework was most directly addressed. In order to encourage its further elaboration and revision, I shall outline the framework in the form of a few tentative assumptions and principles.

**Motor organization.** The motor system generates three forms of muscular action: viscerosomatic reactions, transactional (consummatory or defensive actions), and instrumental responses. Viscerosomatic reactions, such as phasic changes in heart rate, insulin release, salivation, and breathing, are produced either as reflexive responses to specific, simple stimuli (e.g., a loud sound) or as accompaniments of particular emotional (motivational) states, such as those of pain, fear, and anger, which in turn are produced by particular stimulus complexes (shapes, sounds, textures, etc.). Transactional actions, such as eating, drinking, sniffing, grooming, copulating, biting, and struggling to get free of a predator, are performed when the animal is in contact with an environmental object, including parts of its own body. Instrumental responses, such as walking, climbing, lever-pressing, and head-turning, bring the animal close to a particular stimulus (or otherwise make the stimulus available) or take it away from a particular stimulus (or otherwise make the stimulus disappear). Though adaptive behavior frequently requires close integration of the three forms of action, the mechanisms that produce each are probably quite different, and there is no fixed or necessary relation among the three forms even when they are aspects of a common adaptive sequence. Here we shall be concerned mainly, though not exclusively, with instrumental responses.

Transactional actions and instrumental responses are composed of sequences of certain acts. An act (e.g., reaching, standing) is a unit of behavior that consists of a unified set of movements; in the mature animal, an act occurs as a whole in an all or none fashion and its occurrence is completed in no more than half a second. The final step in the production of an act is the activation of an *act-assembly* – a neural organization that temporally orders the excitation of certain movement commands (for further details, see Bindra, 1976, Chapters 3 and 4).

An act-assembly, like a gnostic-assembly, is organized through individual experience; both these neural organizations are structural and both are "small" (i.e., account for no more than half a second of organized neural activity). All the longer instances of behavior, that is, most transactional actions and instrumental responses, represent only functional organizations that are put together fleetingly from act-assemblies for a particular occasion. There are no (structural) motor programs lasting more than half a second; thus, there is no lasting or "memory" basis for the long sequences of acts that comprise a typical transactional action or instrumental response. The integrated appearance of such actions is achieved by continual influences on act-assemblies, so that an old sequence can be interrupted and changed within a fraction of a second; ultimately, this possibility of rapid change is the basis of behavioral flexibility.

**Perception and action.** The production of any act – the activation of an act-assembly – is determined jointly by the animal's current organicistic condition, motivational state, and sensory inflow from within its body and from the environment. Assuming that all the factors except the external environment are held constant, the production of any action will depend critically on the presence of a certain stimulus complex in the situation. An environmental stimulus complex that is critical for the production of a certain act may be called an *eliciting stimulus (complex)* or ES for that act. The main concept of eliciting stimulus is derived from the ethological concept of "releasing stimulus" (Baerends and Kruijt, 1973; Tinbergen, 1951). Since the same motor and muscular system must serve a variety of needs, any defined act may be produced by several different eliciting stimulus complexes; there is many-to-one mapping of eliciting stimulus complexes to acts. An eliciting stimulus complex may involve only one sensory modality or more than one.

Note that an eliciting stimulus complex need not be equivalent to an object as such; it may be a complex consisting of certain aspects (features) of objects and certain features of a background; for example, a stimulus complex that has been found to be effective in producing egg-rolling in gulls is a combination of certain features of the egg and certain features of the background. Typically, it is the relative rather than the absolute dimensions of stimulus features that comprise an eliciting stimulus complex. An aspect of an eliciting stimulus complex that is extremely important in the production of acts is the spatial location of the stimulus complex in relation to the animal; for example, a monkey may use its right arm to reach for a toy presented to its right visual field and its left arm when it is presented in its left visual field.

Any eliciting stimulus complex, to be effective in producing an act, must be perceived – it must generate a pexgo involving the activation of several gnostic-neurons and gnostic-assemblies. Other things remaining constant, what an animal does depends on what it perceives. The probability that a specified act will be produced is a function of the strength of the pexgo generated by an eliciting stimulus complex for that act. The factors that determine the type and strength of the pexgo generated will be discussed below.

The mechanism by which a certain pexgo is translated into a particular act is not known and little attempt has been made to investigate it. I have speculated that the basis of the correspondence between pexgos and acts lies in the period of perceptual learning when gnostic-assemblies and act-assemblies are being simultaneously formed under the influence of the same eliciting stimulus complexes (Bindra, 1976, Chapter 5). This problem of the meaningful relation between perception and action is a fundamental one for psychology; the general approach I have proposed invites a serious study of it.

**Spontaneous activity.** Any experimental chamber may be thought of as made up of many eliciting stimulus complexes

(ESs) distributed in the enclosed space. The effectiveness of the various ESs in activating an act-assembly producing the corresponding act (sniffing, scratching, scanning, grooming, freezing, etc.) is dependent, other things being equal, on the salience and incentive properties of the ESs. Salience is a function of stimulus prominence (in relation to the background stimulus features) and novelty. With increasing familiarity (habituation) to the situation, an animal will gradually stop acting in relation to the less prominent stimuli, and eventually show little activity of any kind. However, if some of the ESs have some appetitive or aversive incentive properties, and the animal is in a relevant organismic condition, then it will continue to act in relation to those ESs. The point of these observations is that the spontaneous activity that an animal displays in an enclosure is not "random" activity, but consists of very specific acts produced jointly by highly specific ESs and particular organismic and motivational conditions (Bindra, 1961; Bindra & Palfai, 1967).

Suppose the animal is hungry and there is a piece of food in the chamber. The smell or sight of food in the presence of the organismic state of hunger will create a central motive state of feeding, and this motivational state, together with the proximal and distal stimuli, will determine the viscerosomatic, transac-tional, and instrumental aspects of the animal's action (as shown in Figure 1A). In general, the animal will, after some exploration of the chamber, move in the direction of the food (instrumental response) and, once in contact with the food, will begin to eat it (consummatory action). The basis of this action is that a pexgo generated by the ES (food) creates a motivational state that in turn further excites (motivational priming) the gnostic-assemblies representing certain features of the food. Thus a moment later a stronger pexgo of food-in-a-certain-location is generated and this results in the activation of the corresponding act-assembly; each successive act is thus determined, moment by moment, by a fresh pexgo.

**Learning.** As explained in a previous section, learning or acquiring knowledge of the environment involves the building of contingency organizations that represent correlations between various stimulus events in particular situations. We can now say that the correlations an animal learns are between the various eliciting stimulus complexes (ESs) that make up a given situation. Some of the ESs are neutral ( $ES^N$ ) while others have appetitive or aversive incentive properties ( $ES^I$ ). Thus, the animal can learn four types of correlations:  $ES^N:ES^N$ ,  $ES^N:ES^I$ ,  $ES^I:ES^I$ , and  $ES^I:ES^N$ . A main tenet of the proposed framework is that building contingency-organization representations of such stimulus-stimulus ( $ES_1:ES_2$ ) correlations is the sole basis of learned modifications of behavior. The correlation of primary interest in typical learning experiments is that between a neutral or conditioned stimulus (CS or  $ES^N$ ) that predicts something about the probability, place, and time of occurrence of a reinforcing, incentive, or unconditioned stimulus (US or  $ES^I$ ).

There are three main propositions that describe how stimulus-stimulus correlations are learned and how the learning of correlations is related to modifications of behavior:

1. The learning of a stimulus-stimulus ( $e.g.$ ,  $ES_1:ES_2$ ) correlation depends on the perception of the concomitant and nonconcomitant occurrences of the two ESs. Several conditions ( $e.g.$ , stimulus salience) are known to determine the type of correlation acquired and the rate of correlation learning (see Bindra, 1976, Chapter 6). Once learned, a pexgo generated by  $ES_1$  will prime the pexgo components of  $ES_2$ , thereby producing, in advance, *some* of the same neural (pexgo) consequences that are produced by  $ES_2$  itself (see Figure 1B).

2. If  $ES_2$  happens to be an incentive stimulus ( $ES_2^I$ ), then  $ES_1$  will acquire incentive-motivational properties of the same type (appetitive or aversive, etc.) as are possessed by the  $ES^I$ . This means that the learning of the correlation  $ES_1:ES_2^I$  will give  $ES_1$  the capability of generating a central motive state and thereby priming pexgo components of  $ES_1$ , and facilitating the generation of a stronger pexgo of  $ES_1$  at the next moment (Figure 1C). The animal is therefore likely to act in relation to  $ES_1$ , the

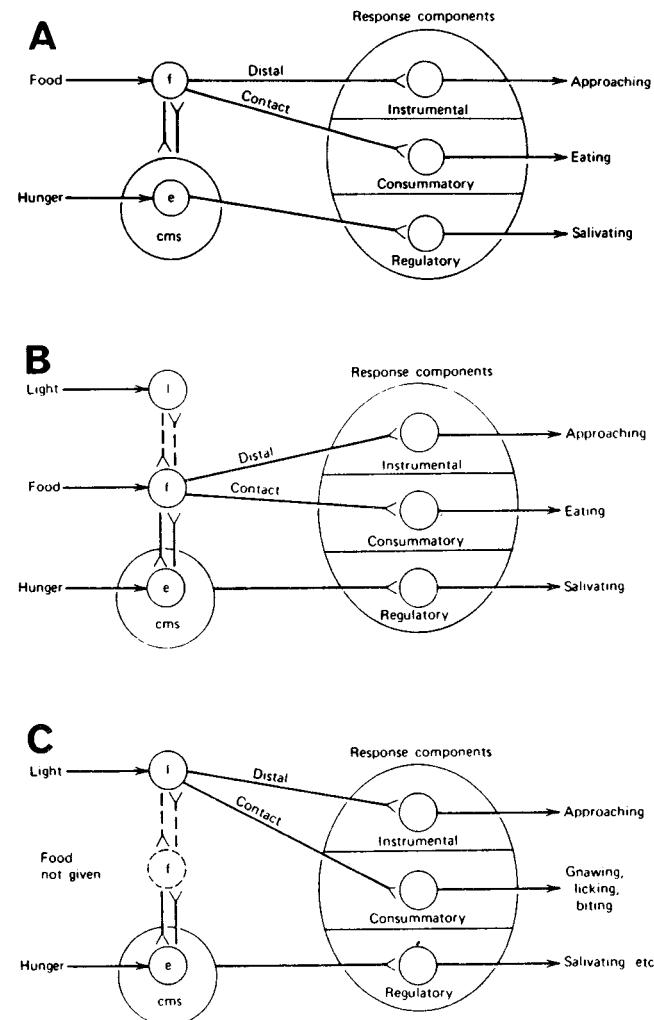


Figure 1 The process of response production. (A) Before Learning. This shows what happens when food is presented to a food-deprived animal. The central (perceptual) representation of food odor ( $f$ ) combines with the central neural consequences of food depletion to generate an eating central motive state ( $e$ ), which excites the sensory-motor coordinations involved in appetitive actions generally and directly activates the regulatory autonomic mechanisms, which produce viscerosomatic reactions. The central motive state also further excites the specific food-odor representation and thereby produces an instrumental approach toward the food. The excitation of the central representation of the contact-stimulus features of food ( $e.g.$ , its taste) activates consummatory response components.

(B) During learning. This shows what happens during the early stages of conditioning. The pairing of a light with food results in the learning of a CS:US contingency, which is shown as broken-line reciprocal connections between the central representations of light ( $l$ ) and food ( $f$ ). These connections make the light an effective predictor of food.

(C) After learning. This shows what happens after conditioning has been successfully achieved. On a test trial, when the presentation of food is withheld, the central representation of light ( $l$ ) through its learned contingency with food, excites ( $f$ ) and thereby creates the eating central motive state, which then enhances the excitation of first ( $f$ ) and then ( $l$ ). Again the perception of the distal stimulus features of the light produces instrumental approach responses directed at the light. If the animal is able to make contact with the light, it may also display some consummatory acts that may resemble to some degree those elicited by the contact stimuli of food ( $e.g.$ , gnawing, licking, biting), but the whole consummatory action would not occur because the light source lacks the stimulus features of an edible object. This model explains why the conditioned response may only partly resemble the unconditioned response. Actions in relation to the light would, of course, continue only until the food gets presented; then the greater motivational value of food would make the animal's actions directed in relation to it.

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exact form of the action being determined by both the nature of the central motive state generated and the stimulus features of  $ES_1$  (not  $ES_2$ ). Though based on the Pavlovian conditioning concept of signaling, this view of learning is not the old response-substitution view of classical conditioning. That view suggests that the response produced by the initially neutral, conditional, or signaling stimulus is the same as the one elicited by the primary incentive or unconditioned stimulus, but this is seldom true (see Kimble, 1961, Chapter 3; Konorski, 1948). According to my view, the new learned response is determined, not by the primary incentive or unconditioned stimulus, but by the type of motivational state generated by the conditioned stimulus ( $CS$  or  $ES$ ) and by its detailed stimulus features in relation to the total situation.

3. For the acquisition of a new, specified response, the  $ES$ s for the various component acts ( $A_1, A_{11}, \dots, A_n$ ) of that response, say,  $E_1, ES_{11} \dots ES_n$ , must acquire incentive-motivational properties. That is,  $ES$  for each component act of the response must be treated as a conditioned stimulus predicting an incentive stimulus ( $ES:ES'$ ) as described above. Further, in order to make each component act occur at the right place in the sequence, we would need to ensure that the strengths of pexgos  $E_1, E_{11} \dots E_n$  would reach their maxima in that order. This would be best achieved by arranging the type of overlapping and nested contingencies described before, that is, by arranging  $ES'$  to be presented when the total response ( $ES_1-A_1, ES_{11}-A_{11}, \dots, ES_n-A_n$ ) has been completed. This is essentially what is done in instrumental training.

When a response is said to be reinforced in a typical instrumental training experiment, we have the condition whereby the  $ES$ s for the successive acts required to complete that response are followed, after different durations, by  $ES'$ , so that the  $ES$ s become conditioned incentive stimuli and enhance their own efficacy for producing the corresponding acts at the correct place in the sequence.

It should be clear that the fundamental processes of correlation learning, generating conditioned incentive-motivation, pexo priming, and the activating of act-assemblies, are the same in both the classical and instrumental training procedures. In the classical procedure, an extraneous, arbitrary stimulus is used as the  $CS$ , while in the instrumental procedure the  $ES$  for an arbitrarily selected response serves as the  $CS$ ; in both cases the basis of learning is the  $ES(=CS):ES'$  contingency. Since the  $ES$ s for a to-be-trained *instrumental* response are usually not known, the procedure of instrumental training serves as a way of ensuring that the unknown  $ES$ s for the specified response come to be followed by the incentive stimulus. In other words, "... arranging a response-incentive contingency is the best way so far discovered for ensuring the animal will observe the critical stimulus features [ $ES$ ] which must enter into the stimulus-incentive contingency [ $ES:ES'$ ] for producing the specified response. . ." (Bindra, 1974, p. 208).

The concept of eliciting stimulus was not used in the traditional accounts of either classical conditioning or instrumental training. The reason for this is not far to seek. In the case of classical conditioning, the Unconditioned Stimulus or  $US$  (e.g., food) serves both as the eliciting stimulus for the response (e.g., for salivation) and as the hedonic stimulus, the unconditioned or incentive stimulus ( $US$  or  $ES'$ ), so that it would be redundant to recognize an  $ES$  apart from the  $US$  that elicits the response to be conditioned. In the case of instrumental training, the reason for not employing the concept of eliciting stimulus lies in the lack of concern with the first occurrence of a response; accounts of instrumental training deal mainly with how a response is reinforced by a reinforcing stimulus ( $S^{reinf}$ ) *after* it has first occurred "spontaneously" in the training situation. The fact that eliciting stimuli are not formally considered in instrumental training does not mean that they do not exist or that they do not have a role in the production of a specified response or its particular components. Knowledge of appropriate eliciting stimuli is often informally used in designing a suitable experimental environment for a given species and a specified response (see Hinde & Stevenson-Hinde, 1973; Seligman & Hager, 1972). The proposed framework allows a formal recognition of  $ES$ s in accounts of

learning. This is important, for to say that the first response occurs "spontaneously" is to ignore an essential part of the phenomenon to be explained.

**Response-production.** As described above, a typical experimental situation consists of a spatial distribution of several  $ES$ s of varying unconditioned or conditioned incentive value, and the relative valence of  $ES$ s keeps on changing from moment to moment as successive events occur and new contingencies come into play. Given an animal that has been trained in such a situation, how can we predict what it will do at a given moment? The most general principle determining response production seems to be this: An animal will move toward an  $ES$  that is of greater appetitive motivational valence or of lesser aversive motivational valence than other  $ES$ s in that situation at that time.

This principle that links response production to a spatial gradient of motivational valence is sufficient to explain the production of the sequence of acts that, say, take a hungry rat from the start box to the goal (food) box of a runway, or take a rat from an electric-shock chamber to a safe chamber, and so on. The readiness and speed with which such locomotory responses are made depend on the steepness of the gradient. If a tone previously associated with food is sounded while the rat is still in the start box, its departure to the food goal box will probably be delayed, as will that of a rat trained to avoid the start box if a tone signaling an electric shock is sounded at or near the goal (safe) box. So far as I know, no systematic studies of the effects of changing the spatial location of  $ES$ s on the topography and speed of responses have been undertaken, though there has been considerable interest in the effects of extraneous conditioned stimuli on instrumental responding (Rescorla & Solomon, 1967).

The above considerations suggest that response production is determined, not by the absolute motivational valence of an  $ES$  but by its valence relative to other  $ES$ s and their spatiotemporal location in the situation. One implication is that no stable response will emerge in a situation in which there is no spatiotemporal gradient of motivational valence. There is some support for such an idea. Working with pigeons, Gamzu and Williams (1971) arranged an experiment in which the incentive stimulus (grain) had a certain probability of occurrence during  $CS$  presentation (illumination of a key) and a certain probability of occurrence during the inter- $CS$  interval. They found that birds began to approach and peck the light key, but only when the incentive probability was greater during the  $CS$  than during the inter- $CS$  interval. When, without reducing the probability of incentive presentation during the  $CS$  interval, Gamzu and Williams made the incentive probabilities in  $CS$  and inter- $CS$  intervals equal (nondifferential conditions), the pigeons stopped pecking at the key during its illumination ( $CS$ ) as well as when it was not illuminated (inter- $CS$  interval). Differential motivational valence for the  $CS$  and the background were necessary for the response to develop and be sustained. Other studies point to the same conclusion (see Lajoie & Bindra, 1976). It should be noted in passing that these findings are inconsistent with the idea that responding under such  $CS:ES'$  contingencies is maintained by adventitious  $R:ES'$  (response-reinforcement) contingencies; if this were the case, then pecking at the  $CS$  should have persisted during the nondifferential condition, because the probability of grain presentation was not altered in the  $CS$  period.

Since the relative incentive valence of a particular stimulus may be enhanced by discriminative training, the more differentiated the  $ES$  is made through discriminative training, the more reliable would be the emergence of a response corresponding to that  $ES$ . In the instrumental procedure, since the incentive stimulus is presented only when the animal has made the specified response (i.e., has observed the critical  $ES$ ), the eliciting stimulus easily becomes a highly differentiated discriminative stimulus compared to other situational stimuli. This accounts for the emergence of reliable and stable environmentally directed responses in the instrumental procedure, as compared

to the relative variability of environmentally directed responses in the classical procedure.

In applying this account to instrumental responses that are not defined in relation to obvious spatial landmarks, that is, to instrumental skills such as arbitrary movements (head-turns, lever-pressing, chain-pulling, etc.), I have suggested that response differentiation in the learning of these skills is ultimately stimulus discrimination learning, that is, learning that different specific features have different correlations. This discrimination is between some of the detailed spatial and temporal features of the ESs in relation to which the skill is performed. Thus, all forms of instrumental learning, whether involving gross approach and withdrawal acts in relation to spatially differentiated stimuli (e.g., goal box) or arbitrary skills involving refined acts, may be regarded as special cases of stimulus-stimulus correlation learning. The apparent differences in, say, maze learning and learning an arbitrary skill may result from the finer perceptual differentiations the animal is required to learn when the experimenter makes reinforcement ( $ES^1$ ) contingent on the occurrence of a highly skilled response (with highly refined ESs). This suggestion needs to be examined experimentally.

**Flexibility.** A corollary of the above account of response production is that a response as such is not a fixed, "preexisting," entity that is "selected" for production, but that it takes shape in the course of being produced, and that the main determiner of the detailed form or topography of any response is the momentary effectiveness of the various ESs in the situation. Thus, a response has no continuous existence in the brain; it is a fresh construction each time it occurs. This dependence of a response on the moment-to-moment changes in the current sensory inflow and the central influences from contingency organizations, as well as central motive states, allows for variations in response components from occasion to occasion; so long as the overall context remains the same, variation or substitutability of acts can occur without altering the total outcome. This substitutability of acts is motor equivalence at the behavioral level.

But, now, if flexibility is not a problem, how can we account for the stereotypy (uniformity and stability) that is characteristic of most responses trained by the instrumental procedure in the laboratory? The essential answer to this is that the basis of the stereotypy of a laboratory-trained response lies in the fixedness (lack of variation) of the spatiotemporal distribution of stimuli throughout the training and test trials; the response stereotypy disappears as soon as the spatiotemporal distribution of the situational ESs is substantially altered (see Bindra, 1974, 1976, Chapter 12). Because this spatiotemporal distribution is seldom rigidly fixed in the natural environment of animals, the development of stereotyped instrumental responses is rare.

An aspect of flexibility other than substitutability of acts in a response is the relative "functional autonomy" of a response after substantial training. For example, it is well known that a habit (e.g., a maze habit), once well established, will be performed effectively even if the animal is deprived of some sensory functions that were necessary or important during the initial acquisition (e.g., Honzik, 1936). This appears to raise a problem for the proposed framework, for if each act is produced by a particular ES, then how can a response as a whole be completed effectively in the absence of what was, during training, a critical ES?

The traditional answer to this question has been to postulate the development of some kind of motor program (based on feedback or central motor neural integration) that can be completed without some of the stimulus (or sensory) elements initially required for its development. But, as has been repeatedly observed, a motor program of this kind is inconsistent with the flexibility observed in normal behavior (Bindra, 1976, Chapter 4; Lashley, 1951). On the other hand, one of the tenets of the present framework is that motor organizations – act-assemblies – are "small," producing acts lasting a fraction of a second. If this

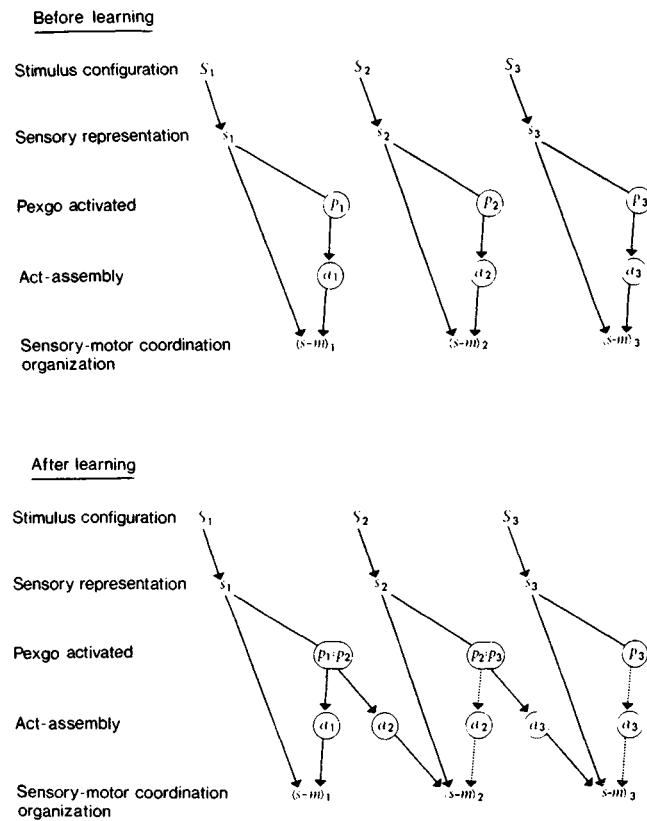


Figure 2 A perceptual hypothesis of response integration. Before learning the act-assembly (a) of each act in the sequence is activated by a pexgo (p) of a certain eliciting stimulus complex (S). After learning, contingency organizations ( $p_1:p_2$ ;  $p_2:p_3$ ) enable  $S_1$  to complete the whole sequence without requiring any support from  $S_2$  and  $S_3$ .

is so, how can the occurrence of long chains of integrated acts be explained without the presence of the ESs that determine each act in the sequence?

The general answer suggested here is that responses consisting of long chains of acts are integrated, not through feedback or motor organization, but through central contingency organizations that make a pexgo of one environmental stimulus (ES<sub>i</sub>) capable of priming the pexgo components of the next stimulus (ES<sub>ii</sub>) even in the absence of that stimulus (ES<sub>ii</sub>) or its sensory inflow. Thus, once contingency organizations representing the correlations of a certain situation have been developed, the production of the response will become partly independent of the environmental support needed initially, as shown in Figure 2. The integration is not motor, but a correlational integration of pexgo components. By removing the mechanism of integration from the motor system to the perceptual system, the present suggestion (a) leaves the motor system free to serve many masters, and (b) makes substitution and innovative adjustments to sudden stimulus changes possible in the course of the performance of the response.

### Some implications

Elsewhere I have explained how the kind of framework I have outlined here may be used to derive plausible interpretations of several hitherto perplexing phenomena of learning, including "instrumental conditioning" of visceral reactions, motivational modulation of instrumental behavior, observational learning, and compound conditioning (Bindra, 1974, 1976). Lajoie and I have also shown how the present ideas can clarify some of the

difficulties that have arisen in the interpretation of certain results of experiments on autoshaping, omission-training, and superstitious responses (Lajoie & Bindra, 1976). Here I briefly indicate some research questions and redirections of research effort suggested by looking at the problem of adaptive behavior within the proposed framework.

First, if the basis of learned modifications of behavior lies in the acquisition of specific types of incentive properties by certain eliciting stimuli, then we should try to learn more about eliciting stimuli. Considerable progress has recently been made by ethologists in the elucidation of the stimulus configurations that tend to be salient for a species and elicit or promote the occurrence of particular acts (see Eibl-Eibesfeldt, 1974; Hinde, 1966). The proposed framework provides a theoretical base for findings of this type and encourages a more refined analysis of the eliciting stimuli for various movements and acts. Such knowledge is likely to be useful to those interested in the shaping of desired transactional and instrumental actions, as well as in the improvement of perceptual-motor skills.

Second, a central postulate of the proposed perceptual-motivational framework is that the form and frequency of an instrumental action are determined, not so much by the incentive value of any particular stimulus in the situation, but by the *relative valence* of the various situational stimuli and by the pattern of their distribution in space and time. This means that the emphasis in the analysis of behavior should be not on relating specified responses to specified stimuli, but on the study of the total range of actions that occur in relation to larger spatial and temporal segments of an animal's environment. Further, one of the deductions from the present framework is that the stereotypy and persistence of certain responses, such as superstitious routines, avoidance responses, and neurotic symptoms, may arise from the continuing uniformity and stability of the spatiotemporal distribution of incentives in the situations to which individuals happen to be confined. This idea has obvious implications for therapeutic behavior modifications in man.

Third, if the occurrence of a specified response at a given time in a certain situation is determined not by whether or how much that *response* has been reinforced previously, but on the extent to which the eliciting stimuli for that response currently predict – are contingently related to – the occurrence of certain incentive stimuli, then the principles for the training or modification of specific responses must be worked out anew without reference to response-reinforcement. It follows that the control of behavior in a given situation is to be gained not by reinforcing (rewarding or punishing) specific responses, but by making the eliciting stimuli for the specific responses more effective by linking them to incentive stimuli and by rearranging the spatiotemporal distribution of the critical stimuli in the situation. Further, if learning involves the building of contingency organizations through *observing* environmental events, then, in order to learn to perform a new response, an animal does not necessarily have to be first made to display that *response*. This makes learning by observation an important principle of learned behavior modification, especially in the adult animal. Thus, learning by observation of models (copying, imitation, etc.) need no longer be considered as a special form of learning, but as one in which the demonstrator or model plays a part in making the novice or the learner observe the critical stimuli and their relations (Bindra, 1974).

Fourth, the proposal that learned behavior modifications rest on the building of contingency organizations that reflect the dependencies or correlations among various situational stimuli makes possible a unified treatment of learning and cognition. Elsewhere I have elaborated upon the proposition that "cognitions" postulated to account for complex forms of perception, meaning, recall, and problem solving are ultimately constellations of overlapping and nested contingency organizations, each developed by the observation of stimulus-stimulus relations as in a simple learning experiment (Bindra, 1976, Chapters 6, 13, 14,

15). The study of the development of the ability to abstract knowledge of causal relations – what has been called "development of causality" – is from the present viewpoint an aspect of the general problem of learning the correlations we observe in our environments.

Fifth, and finally, the proposed perceptual-motivational framework suggests a vastly different approach to the study of the neural mechanisms of learning and motivation than the approach that follows from the response-reinforcement framework. At present, the search for the neural substrate and mechanisms rests largely on the assumption that the process of "selection" of a response is separate from the motivational processes that instigate its occurrence. This assumption has led to the search for (a) the loci of particular motivational states, (b) the loci of the motor organizations of particular responses, and (c) the pathways through which *a* influences *b*. But, contrary to the implications of this approach, particular motor organizations cannot be tied exclusively to a single motivational state, and most motor organizations must impartially serve various motivational states in order to generate adaptive actions.

The present perceptual-motivational framework suggests that the motivational states influence response production by priming the central perceptual representations (pexgos) of environmental eliciting stimuli, thereby rendering those stimuli more potent or effective. From this viewpoint, the important pathways of motivational influence must be those that project from motivational (say, limbic) sites to the sensory-perceptual representation of the very stimuli in relation to which the (appropriate) response is to be made. Since responses as such do not lie ready-made in particular motor structures but are constructed afresh on each occasion, the precise motor output involved in the production of a response must be activated through continual impulses from perceptual organizations to motor organizations. The elucidation of the neural relations of perceptual structures with motivational structures on the one hand and with act-organization structures (premotor areas) on the other thus becomes an important task for physiological psychology.

The replacement of the response-reinforcement framework by the perceptual-motivational framework does not, of course, mean that all aspects of adaptive behavior will now, suddenly, be fully understood. The painstaking task of doing careful, analytic studies must go on as ever; the point of this article is that future studies are likely to be more fruitful in elucidating the principles and mechanisms of adaptive behavior if the questions to be studied are formulated in terms of the proposed perceptual-motivational framework. If this framework is adopted as a working base, the emerging analyses and interpretations of classical conditioning (Asratyan, 1974; Bindra, 1976; MacKintosh, 1974, 1975; Rescorla & Wagner, 1972; Sutherland & MacKintosh, 1971) must form an important part of the further elaboration of the framework.

#### ACKNOWLEDGMENT

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**Bindra's S-S contiguity theory of instrumental learning.** Bindra proposes what is in essence an S-S contiguity theory of instrumental learning, recognizing that the principal problem for any such theory is to arrange for the translation of purely afferent linkages into action; the animal must not be left, as Guthrie (1935) put it, "buried in thought." Various solutions to this problem have been offered in the past, and it is too bad that Bindra does not consider their relation to his own. O. H. Mowrer (1960; also this *Commentary*) has suggested, for example, that the animal continually scans its repertoire of responses. Once initiated in this way, a previously rewarded response ( $R_a$ ) is facilitated by interoceptive feedback from the response ( $S_{Ra}$ ), which is assumed to have acquired activating properties by contiguity with reward. Much earlier, Morgan (1894) suggested a preexisting connection between the sensory center for the feedback from any response and the motor center for that response ( $S_{Ra}-R_a$ ). When the animal is returned to the experimental situation ( $S_1$ ) and remembers the previously rewarded response ( $S_1-S_{Ra}$ ), the response tends, therefore, to be initiated. At the same time, the reward ( $S_2$ ) is remembered ( $S_{Ra}-S_2$ ), and the response is facilitated by a "control center" that reads the hedonic value of  $S_2$ , an unrewarded response ( $R_b$ ) may be remembered ( $S_1-S_{Rb}$ ), but it is not made because its feedback ( $S_{Rb}$ ) has no facilitating associations. Bindra relies on a preexisting connection between some component of  $S_1(S_a)$  and  $R_a(S_a-R_a)$ , which is responsible for producing the response in the first place ( $S_a$  is the "eliciting stimulus" or ES for  $R_a$ ). Rewarding  $R_a$  ensures the contiguity of  $S_a$  with reward, in consequence of which  $S_a$  acquires "incentive-motivational" properties that facilitate  $R_a$ .

What all of these theories have in common is the assumption of conditioned activation - the occurrence of  $R_a$  is held to be facilitated because some stimulus (or sensory center) acquires activating properties as a result of pairing with reward. They differ as to how  $R_a$  is selectively activated. It should be noted in passing that Bindra's dichotomy of response-reinforcement and incentive-motivational theories is a false one. Hull (1952) and Spence (1956) were no less incentive-motivational theorists than is Bindra, although they traced the acquisition of incentive-motivational properties to S-R ( $S-R_G$ ) rather than to S-S linkages and the selection of rewarded responses to other S-R linkages established and strengthened in the course of training. In any case, all of these theories face a common difficulty. If, with food as  $R_2$ , we accept salivation as an index of the incentive-motivational properties acquired by some afferent precursor of  $R_a$ , the theories lead us to expect that  $R_a$  will always be preceded, or at least accompanied, by salivation, but that is not the case. For example, Ellison and Konorski (1964) trained dogs to press a panel in response to one stimulus, the ninth press producing a second stimulus that soon was followed (noncontingently) by food. The animals pressed but did not salivate to the first stimulus, and when

the second stimulus was turned on they salivated but did not press. As Mackintosh (1974; also this *Commentary*) has recently concluded in a review of this literature, the "data make it difficult to see how the initiation of an instrumental chain of responses can be motivated by an incentive system whose operation is in any way directly mirrored by overt CRs." One might argue, of course, that other measures would give results more favorable to these theories, but if incentive-motivational properties are conditioned according to the same laws as salivation, why should results for other measures be different? The results for salivation are, in fact, just what conditioning theory would lead us to expect: in instrumental training, reward is best predicted, not by the sensory antecedents of the rewarded response, but by its sensory consequences.

While Bindra's unique contribution to the theory of instrumental learning is the idea of a preexisting connection between some eliciting stimulus and the rewarded response that is activated as the stimulus acquires incentive-motivational properties by contiguity with reward, he relies heavily on another and more familiar mechanism of behavioral change: as the ES is paired with reward, it comes also to elicit responses like those elicited by the reward itself, among the most important of which, apparently, is approach (Birch & Bitterman, 1949). "The most general principle determining response production," Bindra tells us, is that "an animal will move toward an ES that is of greater appetitive motivational valence than other ESs in that situation at that time." He notes that conditioned approach alone will account for instrumental performance in locomotor situations, such as runway or maze, in which the animal is rewarded for going to a particular place, and then asserts that the learning of manipulative responses, such as pressing a lever or pulling a chain, is like maze learning except that "finer perceptual differentiations" are required. Is manipulative learning a special case of conditioned approach, or are locomotor and manipulative learning alike only in that both involve "stimulus-stimulus correlation"? The relation between the two kinds of task and the mechanisms of mastery is far from clear.

Although Bindra criticizes response-reinforcement theorists for defining responses in terms of "measurable environmental outcomes" rather than "specific movements," he himself is apt to define them in the same way - see, for example, his list of "transactional acts" (eating, struggling to get free of a predator) or instrumental responses (walking, climbing, lever-pressing). There seems, in fact, to be no other practical way of thinking about behavior. Furthermore, for all his talk about the "fresh construction" of responses each time they occur, Bindra fails in my opinion to provide any new insight into the problem of response equivalence that plagues all connectionist theories (of which Bindra's certainly is one). Mere variability is not very difficult to deal with; one has only to assume that a range of stimuli and a range of movements with a common environmental effect become involved in the learning over a series of trials (Hull, 1943). The real difficulty is presented by the occurrence of appropriate movements clearly outside the range of those displayed in training when new constraints are imposed on the animal, as, for example, when a monkey that has been trained to operate a mechanism with one hand operates it successfully with the other hand upon being deprived of the use of the first (Lashley, 1924). I find no solution to this problem in Bindra's theory, nor for that matter even the promise of a solution in any nonconnectionist theory.

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## Commentary/ Bindra: A perceptual-motivational view of adaptive behavior

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**Is Bindra's theory of adaptive behavior radical enough?** There is much to be admired in Bindra's effort to formulate a new theory of how adaptive behavior is produced – in particular, the attempt to deal with complex psychological processes in a rigorous and consistent manner. There are, however, some questions raised by the formulation. These fall into two categories. The first is concerned with the details of the formulation. The second asks whether Bindra has gone far enough in breaking with the behaviouristic tradition.

**1 Bindra's formulation** Bindra rejects the position that particular responses are reinforced because it does not deal with the fact that animals can substitute one act for another in order to reach a goal without being specifically trained to do so. Bindra labels this "motor equivalence" and deals with it under the general heading of the flexibility of adaptive behaviour. I do not want to argue with Bindra's point that S-R formulations cannot deal with response flexibility, although one might do so, I suppose, by calling upon notions such as the habit family hierarchy. Rather, I would question Bindra's formulation because it seems to have difficulties in accounting for the specificity of the effects of reinforcement.

Suppose that one shapes a rat to raise one limb, then to lower it, and so on. Bindra's account of such learning is as follows. An animal learns correlations between stimuli that elicit a given act and reinforcing outcomes that follow the act. The central representations of these stimuli acquire incentive motivational properties because they predict reinforcing outcomes. As a result, the animal's motivational states biases it to attend to the eliciting stimuli which elicit the appropriate act. It seems to me that this view implies that two different stimuli must have been paired with the reinforcing outcome in order for two responses to have been elicited, because all other factors were presumably the same during the shaping of each response. To identify the eliciting stimulus with the response itself won't do for obvious reasons. To identify the eliciting stimulus with the idea of the response would help, but Bindra rejects this because, I think, it would result in his theory's becoming indistinguishable from the response-reinforcer theory for all practical purposes. The eliciting stimuli must, therefore, be environmental. But what are the specific stimuli that elicit limb raising, and what are the specific stimuli that elicited limb lowering? The identification of such eliciting stimuli is crucial to Bindra's formulation. But they are not identified. Bindra criticizes the response-reinforcement theories because "they do not come to grips with the problem of how a new stimulus-response relation actually gets learned." The elicitation principle that Bindra proposes (and which he points out has not really been investigated) is too vague to provide much of an improvement.

One may raise the same question in another way. Bindra notes that animals learn about the correlation between responses and outcomes. Suppose that the following sequence of events and responses is reinforced: stimulus 1, stimulus 2, response 1, reinforcement. Why not assume that the central representation of the response (or the idea of performing the response, to put it another way) is primed by stimulus 2, and that this can influence response performance? Why is priming by some unspecified eliciting stimulus a more satisfactory assumption?

**2 The break with tradition** In some sense, Bindra's formulation is a radical break with the response-reinforcement view. He tries to explain instrumental learning as a form of learning about the relationships among certain representations of stimuli. But it is not radical enough, I think, on two counts.

First, he does not, it seems to me, permit the brain to carry out operations much more complex than those specified by traditional learning theorists – in particular learning about correlations. What about, for example, the sorts of things Tolman talked about – the formation of spatial cognitive maps, and the complex operations that can be performed on such maps?

More important, Bindra's formulation admits to only one form of learning – about correlations among central representations of stimuli. A main tenet of the proposed framework is that building contingency-organization representations of such stimulus-stimulus ( $ES_1:ES_2$ ) correlations is the sole basis of learned modifications of behavior. It seems to me more satisfactory to accept that animals can learn different things under different circumstances, and, as a consequence, employ different strategies to solve a given problem. One might argue, for example, that animals learn about correlations among stimuli, some of which elicit responses under one set of circum-

stances, and may form S-R associations under other circumstances, and perhaps both under still other circumstances. When an animal is reinforced in a given arm of a T-maze, it may associate that location with food and move toward that location because of the location's "appetitive motivational valence," or it may learn to make a particular right or left turn at the choice point, and so on. Data reported by Hicks (1964) and Mackintosh (1964; see also this Commentary) suggest that animals learn about locations during the early stages of training in the T-maze and learn particular turns after longer periods of training. In short, perceptual-motivational learning affects adaptive behaviour under one set of conditions and response-reinforcement learning under another. A theory which incorporates the possibility for different types of learning is more satisfactory, at least for me, than the single type of learning theory proposed by Bindra.

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**The more things change . . .** This paper, Bindra's latest theoretical statement, provides a clear view of the position he has been developing for the last few years. I am quite in sympathy with this theoretical position and would endorse several of its basic tenets. I heartily approve of Bindra's rejection of the response as the fundamental unit of learning and his reciprocal emphasis on stimulus learning. I like his rejection of reinforcement mechanisms and his emphasis on complementary motivational mechanisms. I also share Bindra's belief that the long-neglected problem of response production now demands the careful attention of learning theorists, and I was most interested to see how he addressed this problem. I enjoyed reading the paper.

But I had a strange feeling that I had been through it all before – not in Bindra's writings, but somewhere else. Where? Was it *déjà vu*? No, I finally realized, it was Guthrie. Guthrie had said it all back in 1935. I had not recognized it at first because the language was so different. But it was Guthrie all right. Bindra uses quite different language and he is concerned with quite different behavioral phenomena, but these superficial differences notwithstanding, his theory is clearly Guthrian in content, just as Estes' superficially different mathematical learning theory was clearly Guthrian. The flavor is unmistakable; Bindra is a Guthrian! Consider the following points:

**Reinforcement** Bindra has no special reinforcement mechanism; when learning occurs it is said to be due entirely to experience. The critical feature of experience is correlation rather than contiguity, and that is a real difference between Bindra and Guthrie.

**Motivation** Bindra has no drive-type of motivator; there is no energizing effect of deprivation or intense stimulation. Such treatments produce organismic conditions that act as gates enabling some stimuli rather than others to be effective. The stimuli most affected by this switching action are "incentive" stimuli, which are known by their capacity to elicit behavior. In effect, then, motivation is created by the presence of certain behavior-eliciting stimuli; for Guthrie, too, motivation was created by the presence of what he called "maintaining stimuli." And in both theories, motivation is simply manifest in the occurrence of certain behaviors.

**The stimulus** The situation is too molar for Bindra's explanatory purposes. Stimulus objects are too molar. Even stimulus dimensions and features will not suffice. The stimulus field is viewed, as Guthrie viewed it, as an ever-changing panorama of minute events, partly external and partly internal. Elements in this vast, complex field come and go as the organism moves about and changes its posture and orienting behaviors. This is pure Guthrie. While Guthrie had trouble specifying before the fact what the effective stimulus was, Bindra must have still more trouble, because his stimulus field can include not only stimulus elements actually present but also previously experienced elements that have been associated with those actually present.

**The response.** Behavior is also viewed atomistically. Actions are composed of acts, which are events lasting no more than half a second. After

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raising our hopes by bringing up the question of response production, Bindra dashes them by announcing that responses are elicited by eliciting stimuli. Just how the translation from stimulus to response is made is not specified; this awaits further investigation. Meanwhile, the response is assumed to occur because of the eliciting stimulus. This is precisely the kind of relationship that Guthrie called an S-R association.

**Reductionism** Both Bindra and Guthrie indicate that ultimate reality lies in the nervous system, and that current psychological theorizing is little more than a stopgap measure until the operation of the nervous system is better understood.

**Timeliness** At a time when nearly everyone believed in the S-R association, Guthrie attributed all learning to the acquisition of S-R associations. Today, when S-S mechanisms are in vogue, Bindra attributes all learning to the acquisition of knowledge about stimulus events and their correlations. This shift from response learning to stimulus learning appears to be a major change in content. But is it such a big change, or is it more a matter of perspective and terminology? Between stimulus input and response output Bindra interposes two theoretical linkages, a learned one to a hypothetical effective stimulus (pexgo) and a fixed one from there to the response. Guthrie treated the second linkage as learned rather than fixed, but it was a strange one-trial, all-or-none kind of learning. For Guthrie, the trial by trial changes that occur in behavior were due to the changing relationship between the input stimuli presented to the animal and the effective stimuli that actually control behavior. But this is just Bindra's first, learned linkage. Thus, the difference between the theories seems to be more semantic than syntactical.

**Complexity** By making all learning stimulus learning and by emphasizing the enormous wealth of stimuli that might be effective at any given moment, Bindra has put the entire burden of explaining the complexity of behavior on the complex organization of the stimulus field. Then the syntactical rules that generate behavior can be made simple indeed. This was also Guthrie's strategy.

**Testability** Guthrie's theory has stood for more than 40 years, almost unscathed by the results of laboratory investigation. Bindra's renovation should likewise stand for many years to come.

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**by D. A. Booth**

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**A long stride towards sense in psychology.** Behavior is the pattern of responses to an interpreted complex of current stimuli. Behavior is acquired by classical conditioning, that is, the building of representations of the contingencies amongst stimuli.

Bindra's advocacy of these two theses, central to his position, is an important contribution to the current development of behavioral and brain sciences. Moreover, these themes now appear more widely and are pushed further than Bindra's account implies. The commonsense realistic view of mind and behavior has been crippled for two centuries or more by fallacies from physicalist and dualist metaphysics, but scientific theorising and experimental design is at last returning to reality. Thus, Bindra has my general support – while he also has my specific criticisms for reflecting the new mood in only a limited way. He adds physiologising and antimentalism, which, in my view, are otiose. He retains unnecessary elements of drive theory. He does not attempt to defend conditioning theory against charges that it is cumbersome and slippery. I take these limitations briefly in turn.

Bindra's analysis is behavior theory. Yet, even more in his book (1976 *op cit*) than in this article, he presents his view as brain theory. Dressing one up as the other helps neither psychology nor neuroscience, nor does it advance the final defeat of dualism. The neurologising beloved of McGill psychologists and Russian conditioners typically neither adds to the behavioral predictions from the theory nor provides neuroscientists with manageable experimental issues. The help that the neuroscientist currently needs from the psychologist is accurate detailing of the brain's input-output relationships – the mental processes within behavioral organisation. Furthermore, psychology will not be reduced to neurophysiology; the two will be

married. In my specialty at least (motivation), no such marriage has yet even been consummated (Booth, 1976, pp. 453-58; Friedman & Stricker, 1976).

Decades after the later Wittgenstein (1953), Bindra (not alone) regards percepts, thoughts, concepts, desires, and the like as occult entities or ghostly machinations. At least his terminology suffers in consequence. Words like "pexgo," "gnostic assembly," and "central motive state" mean what percept, belief, and motivational or emotional state have ordinarily meant, despite what bad philosophy and its manifestations in introspectionist and environmentalist psychologies have tried to twist them to mean. Thinking or other mental activity does not subsist in some unobservable nonbehavioral world. Of course, the detailed organisation of someone's thoughtful silence and immobility is not usually evident from the instantaneous physical appearance of that person and his situation – but then neither is the nature of almost any piece of behavior.

Bindra's account of action seems weakened by this residual physicalism, notwithstanding his appreciation for Lashley's and Skinner's awareness of the problem of response equivalence. There can be no burking the fact that action is intentional – that is, directed by the perceived consequences of physical movement or nonmovement of limb, larynx, or whatever. Giving an account of such behavioral structures is in the first instance a problem in psychology, and not in neurology, as Bindra takes it. Furthermore, the mere bundling of involuntary reactions may not create intentionality. In his concern to refute response-reinforcement learning, Bindra appears to regard the notion that a reinforced response can contribute to a conditioned incentive stimulus complex as more of a problem than learning to understand conventional symbols, for example. Where I am in accord with Bindra is in his program of working out the results of assuming that intentional aspects of behavior are based on the same learning mechanism as involuntary aspects of behavior. Uttley (1976), for example, has illustrated this approach in mathematical brain theory, relating it to both neurophysiology and the Rescorla-Wagner conditioning theory. Bolles's (1976 and this Commentary) new edition of *Theory of motivation* is remarkable for its persistence in advocacy of the R-S "reinforcement" view of learning in the face of repeated admissions that the S-S "associative" analysis does at least as well. Motivations and emotions, operants and respondents, could both be selected into the stream of behavior by biases resulting from conditioned expectancies.

Bindra follows the tradition that bodily states of the organism set the effectiveness or otherwise of external stimuli. I have suggested that we could take the S-S view even more radically and consider the possibility that any perceptible bodily state can also be a conditioned or unconditioned stimulus that elicits behavior in conjunction with other eliciting stimuli from the external environment (Booth, 1977a,b). Extreme drive states and their energising of behavior or limiting of learning have been studied, but the more relevant exploration of the normal range of need states or hormonal or drugged conditions as stimuli for selecting behavior has hardly begun.

Bindra's account of conditioned motivation seems to assume that conditioning is stimulus substitution, not adaptive expectancy formation (his Figure 1). Conditioned responses are different from unconditioned responses, not only because the eliciting stimuli are presented in different contexts, as Bindra rightly emphasizes, but also because the appropriate reaction inbuilt to a predictive stimulus is not necessarily the same as the reaction inbuilt to the predicted stimulus.

Bindra's view that "what an animal does depends on what it perceives" and on what perception it has acquired, is being applied much more widely than Bindra indicates, even in the much broader treatment in his book (1976 *op cit*). Miller, Galanter, and Pribram (1960) overprecipitately rejected all forms of conditioning theory as incompatible with the hierarchy that appeared necessary to action and cognition. The motivational processes generally treated hierarchically by ethologists can be accounted for by parallel S-R connections in interaction, e.g. by reciprocal inhibition (Ludlow, 1976). A wide range of human problem-solving behavior can be described in terms of minimally structured collections of situation-act connections (Newell & Simon, 1972). Such an organisation is highly flexible and adaptive because the particular connection that is operative at any instant is the one that is most closely specified by the momentary situation that has been created by the environment and the previous operation of other connections. Such a theoretical approach to cognition has the great advantage that it can be operationalised in a way that can be checked directly against experimental results (including computer simulation if desired). Like Harrison (1972), I have urged (against the psycholinguistic establishment) that

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mechanisms of language reception and production should be treated in similar terms, not just at the early stage when the infant's language is obviously S-R, but even in adulthood (Booth, 1978) With language, as with other cognitive behavior, theories based on complex hierarchical fixtures may have more to do with logic than with mental processes

A realistic theory of human behavior – even of the rat's spontaneous behavior – may have to be cumbersome Collation of myriad facts and execution of routine calculations within a theory may have to be delegated to computer programs This can cause antagonism Yet simplistic theory is delusion Minds and brains are highly engineered universes Nevertheless, there is a further issue of whether conditioning theory is unnecessarily cumbersome Suppes's (1969) reduction of finite automata (and hence all specifiable behavior) to a theory of S-S conditioning and S-R processing has been countered by Arbib (1969) as implausibly time-consuming because of a highly constrained learning process Suppes and Scandura have their own ripostes (Scandura, 1976) and it would be interesting to have Bindra's Clearly, Bindra would not permit us to compare cognition with a discrete approach to a single conditioned incentive stimulus It would be foolish to compare motor control with elicitation of a reflex or perception with the firing of a bug detector, and yet motor and perceptual integration are readily treated as combinations of reflex-mediating mechanisms and of detector-type neural connections, respectively [cf Kupfermann and Weiss, this issue] Two areas in which the complexity of coordinating such theorizing to experiment seems to be adequately tackled would be applications of Newell and Simon's approach in developmental psychology (e.g., Young, 1975) and the editor of the *Journal of Experimental Psychology Animal Behavior Processes* presiding over experimental rivalries between his own and other variants of conditioning theory

This last example may also be helping to refute the notion that "cognitive" behavior theory is too slippery to be scientifically useful Nevertheless, Bindra's approach, and any liberal expectancy conditioning or causal perception theory of behavior, should be recognised as in part a paradigm, not a specific set of hypotheses Furthermore, the conditioning paradigm can explain anything, just like the Skinnerian radical environmentalist language, because each is a paradigm centred on a tautology For Skinner, an operant is reinforceable behavior and a reinforcement is what maintains behavior (and all behavior is apparently prejudged operant until proved otherwise) In conditioning theory, the circular definition would be that the conditioning stimulus (or US) is whatever consequence of a stimulus (the CS) changes the response to that CS, and the changed response (or CR) is whatever behavior the CS comes to elicit when the CS-US contingency is imposed In both cases, the tautology is an apparatus for getting a grip on the particular facts Bindra has plenty of good criticisms of particular response-reinforcement hypotheses However, in the end neither his approach nor his rival will be refuted, I suspect One will prove more effective to elaborate than the other

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### by A. Charles Catania

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**What constitutes explanation in psychology.** I have no inclination to argue that behavior is modified through contiguity of responses with hedonic stimuli, or that learning consists of strengthening neural connections between central representations of stimuli and central representations of responses, or that the mechanism of response production involves some kind of connection between gnostic organizations and particular responses Yet these are the terms in which Bindra characterizes the "response-reinforcement idea," and it is for these conceptions that he is proposing "incentive-motivational" alternatives I find so little of the theoretical framework of the contemporary experimental analysis of behavior represented in either of the positions outlined by Bindra that the temptation is to say nothing Models of the CNS (conceptual nervous system) have been recurrent features in the history of learning theory, but except for Sherrington's deduction of the existence of the synapse from behavioral data, such models have not much contributed to our progress

The crux of the problem seems to lie with what constitutes explanation in psychology Bindra hints at this when he contrasts his views with those of other learning theorists; his references to Skinner seem more peripheral than those to others such as Hull and Mowrer (see also this Commentary) He states that Skinner was "less interested in the explanation of behavior and more in the principles of its control," and that the concept of the operant "sacrificed explanation to the empirical goal of enunciating descriptive principles" But without descriptive principles based on empirical findings, what would there be to explain? It is curious that those who give priority to explanation over description see even the descriptive concepts of other theorists as explanatory Thus, Bindra says, "If the focus is on explaining the flexibility of adaptive behavior, the response-reinforcement idea must be completely abandoned" The contemporary operant view of reinforcement, however, is not explanatory at all Instead, reinforcement is a name for a particular type of behavioral phenomenon When we have shown that a response has become more likely because the response has produced a particular consequence, we say that the response has been reinforced The name involves no commitment to how widespread this phenomenon is in the behavior of organisms, nor does it rule out the possibility that changes in behavior can occur in other ways In this context, Bindra's critique of the "response-reinforcement idea" seems simply irrelevant

The history of learning theory includes many debates over the nature of explanation One argument is that research effort can be misdirected by theories that give the appearance of explanation In this context, consider the explanatory substance of Bindra's account For example, in discussing motivational influence, Bindra states that "the generation of a motivational state makes certain environmental stimuli, or some features of those stimuli, relatively more effective than they would otherwise be at that time" But how can we define motivational state? Clearly, we speak of motivation when we are considering those variables that change the relative effectiveness of stimuli as reinforcers or as punishers (Bindra would prefer to speak of their hedonic valence) In Bindra's statement, therefore, I find not an explanation of some property of behavior, but rather an indirect definition of motivation Yet for Bindra the central motivational state now takes on a life of its own, and in other places becomes part of the explanation of behavior The tautology that is implicit in such theorizing sometimes even becomes explicit (For example, when Bindra states that "Assuming that all the factors except the external environmental are held constant, the production of any action will depend critically on the presence of a certain stimulus complex in the situation," or that "Other things remaining constant, what an animal does depends on what it perceives")

Related problems exist with valences and salience and other terms, but for me Bindra's most critical terminology is that of hedonic stimuli There are two

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major empirical developments that bear on the nature of those stimuli that we call reinforcing or punishing, and I do not see how Bindra's system deals with them. In both instances, the difficulty is that such stimuli are relatively and not absolutely defined. Bindra admits the relativity of "motivational valences," but for him "hedonic" stimuli have the property of reliably producing appetitive or aversive reactions and being affectively pleasing or discomfiting. They include sights, sounds, tastes, odors, temperatures, and cutaneous textures that are provided by such biologically important objects, events, and situations as food, water, a sexual partner, a nest, the call of a distressed offspring, the shape of a predator, and injurious levels of heat or cold." I do not see how Bindra's account deals with the reversibility of reinforcers, as demonstrated by Premack, or with the maintenance of behavior by schedules of response-produced aversive events, as demonstrated by Kelleher and Morse. Premack, for example, has shown that running can reinforce a rat's drinking or drinking can reinforce its running, depending on the organism's relative deprivation of drinking or running. How, then, can running and drinking be classified in terms of Bindra's distinction between transactional and instrumental responses? Kelleher and Morse have shown that an electric shock that suppresses or punishes a monkey's lever presses when every press produces shock generates high rates of responding when presses produce shock according to a 10-minute fixed-interval schedule. How does the language of incentive or hedonism help us to deal with these different effects of electric shock? My guess is that these findings will have substantial impact on the fundamental concepts of the analysis of behavior, but I do not see much recognition of or room for them in Bindra's account (Kelleher & Morse, 1968; Premack, 1959 *op. cit.*)

Over roughly the past two decades, psychology has been suffering from an epidemic of paradigm clashes. The major clash, of course, was between cognitive psychology and behaviorism, as shown by Chomsky's 1959 review of Skinner's *Verbal Behavior*. The "alternative framework" proposed by Bindra, with its heavy emphasis on perception, provides another instance, characterized as a clash between "response-reinforcement" and "incentive-motivation" paradigms. Bindra speaks of the "climate of opinion in which the response-reinforcement framework or paradigm came to dominate both the theoretical and practical aspects of the study of behavior" and which "cushioned the response-reinforcement framework from a variety of criticisms that were articulated against it over nearly half a century." One irony is that a compelling issue in Chomsky's psycholinguistics is not dealt with in Bindra's system. Chomsky had objected to behavioral accounts in terms of stimulus-response associations (S-R theories) on the grounds that they were inadequate to the flexibility of verbal behavior. Associative chains, described in terms of sequential dependencies or Markov processes, could not generate grammatical structure. Chomsky's criticisms were not particularly relevant to Skinner's behavioral account because Skinner was not an S-R theorist. But they are relevant to Bindra because the treatment of sequential behavior as chaining remains problematical whether the sequences are generated as S-R units through feedback from the periphery or are moved inside as successively activated central systems (Bindra's pexgos). Perhaps this issue could be dealt with in terms of branching and converging concurrent sequences, but I do not see any such developments as part of Bindra's system.

Pexgos and motivational states are not behavior, and Bindra's account of the production of responses or act sequences by these central organizations allows so many degrees of freedom that it can hardly be called explanation. The concepts are necessarily anchored only remotely to the phenomena of behavior. For this reason, I find Bindra's account more reactionary than revolutionary. Although it is vastly different in detail, its philosophy of science has more in common with Hull than with the contemporary analysis of behavior. Cognitive and behavioral approaches exist in great variety, and we will undoubtedly continue for some time to hear the differences among them spoken of in terms of paradigm clashes. We have yet to establish an effective language for the science of behavior, and that task must be one of our highest priorities. An effective descriptive system may change our criteria for explanation; we will look for behavioral explanations of behavioral events, instead of appealing to alternative levels of analysis that have only hypothetical status. For example, it is a kind of explanation to say that a particular response occurred because it was elicited rather than because it was reinforced. Such distinctions are an essential component of an experimental analysis, but they are attenuated by accounts, like Bindra's, that assume an eliciting stimulus complex as part of the explanation of every act.

I believe that in the long run the advantage will go to those approaches that stay close to the subject matter of behavior. Perhaps the perspective to be gained will make the clashes discussed here seem like little more than skirmishes on the outskirts. The defense of the experimental analysis of behavior will certainly remain simple as long as attacks fall only on other varieties of behaviorism, with which it has little in common.

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**Requisition for a pexo.** Bindra's points about the flaws in his predecessors are well taken – and could be taken farther – but his own view, for all its apparent novelty, has something naggingly familiar about it. What is it exactly that makes the proposed theory look so old-fashioned (to me)? I think the answer can be seen if we reflect on a trend discernible in the history of behaviorism. As Bindra points out, such phenomena as latent learning and response substitution soon showed behaviorists that the laws and mechanisms proposed were insufficient; animals (even rats) were too smart to be explicable by the simple mechanistic models of reinforcement and conditioning then available. Skinner's response, as Bindra notes, was to retreat from models of conditioning mechanisms and settle for an analysis of "functional relations," and, as Bindra observes, this evaded "the fundamental problem of how adaptive behavior is put together." For those who did not follow Skinner's lead, but continued to grapple with the "fundamental problem," the result was an inexorable if reluctant migration of behavioristic theory toward more "cognitive" characterizations of the theoretical entities posited. (See Broadbent, 1961; Taylor, 1964 and BBS 1:2; and Dennett, 1975, for overviews of this trend from different perspectives.)

The rationale for this trend can be seen more clearly in retrospect than it could be at the time, especially now that cognitive or intentional (Dennett, 1971; Haugeland, this volume, next issue, forthcoming) formulations are no longer viewed with horror. It was usually easy enough for the behaviorist to say (in the privacy of his own room) what the shortcomings of his particular theoretical entities were: they did not vary, but remained constant (as they ought to have) in rough concomitance with what one would call the perceivable meaning of the situation, if one could only permit oneself such locutions. One had hoped for laws of nature, on the model of the laws of chemical combination for instance, but when the laws that suggested themselves turned out not to handle the data, a subtle and relatively unrecognized (one might even say repressed) shift in tactic occurred: the search for laws of nature relating presumably simple theoretical entities (e.g., stimuli and responses) was displaced by what in retrospect can be seen to have been an effort to design theoretical entities of some complexity with the requisite powers to accommodate the data. It was a shift, in Haugeland's terms, (this volume, next issue, forthcoming) from a *derivational-nomological* enterprise to a *systematic* enterprise, but without abandoning the trappings and preconceptions of the former style of science. Thus a premium was still placed on framing *laws* to relate the theoretical entities and on *equations* to characterize their interaction, even though what was being done was essentially system-designing at ever more cognitive, less mechanistic levels of abstraction. As Bindra says: "The general thrust of such concepts (e.g., cognition, expectancy, emotion, etc.) is that the motor output comprising a response is separated from current sensory inflow by the intervention of some central system of knowledge or motivation, and it is this system, not sensory-motor associations, that determines what the response will be. It is assumed that such a central system somehow flexibly adjusts the motor output to the ever-changing stimulus and organism conditions."

Somehow, but how? The question invites speculation about systems: "what if we tried to do it with some parts with functions A,B,C, . . . ?" What Bindra offers us is a fairly advanced set of design proposals in this tradition, but still couched in vestigial "laws of nature" talk.

What this tends to conceal, from theorist and onlooker alike, is that the design proposals are not full-fledged structural blueprints (the *end-product*

of a solved design problem), but an elaborate collection of functional specifications of sub-system parts, specifications that set the problem for the designer, rather than solve it. For instance, Bindra says, "According to the incentive-motivation hypothesis, a pexgo of a particular stimulus (say,  $S_2$ ) generates a pexgo of a hedonic stimulus ( $s^H$ ) and thereby generates the same motivation (central motive state or cms) as is normally generated by the hedonic stimulus ( $S^H$ ) itself; this central motive state, in combination with the detailed sensory-spatial features of the situation, then determines what response will emerge." This is presented as a hypothesis (a sketch of a law to be tested), when in fact it can only be viewed as a sketch of a design proposal the feasibility of which is utterly uncertain because of the semi-disguised intentionalistic characterization of the sub-system parts and functions.

The disguises come off when one asks what the equivalence classes are and what the individuating characteristics of the entity-types are. What makes a pexgo a pexgo of a stimulus  $S_2$  (i.e., a stimulus of type  $S_2$ —a solution to the problem of stimulus generalization or pattern recognition is presupposed from the outset)? Not merely that the pexgo was simply caused or triggered by an  $S_2$  type stimulus, for pexgos are representations of stimuli or stimulus situations (Direction to the engineer: make me some pexgos to represent each important type of stimulus, please). Next, this pexgo "generates" a pexgo of the hedonic stimulus ( $s^H$ ) (Note to engineer: do not forget to build in a conditionable hedonic-stimulus-pexgo generator as part of each pexgo.) And this thereby ("thereby" because that is the way it ought to work) "generates the same motivation (central motive state or cms) as is normally generated by the hedonic stimulus ( $S^H$ ) itself." What counts as same central motive state? Our equivalence class here is undisguisedly intentional: a particular cms is a token of a particular cms-type if and only if that token has the function of determining "in combination with the detailed sensory-spatial features of the situation" a response appropriate to that particular cms type (See Dennett, 1971, on intentionality and appeals to appropriateness).

Is it possible to design parts with these features and powers and then get them to work together in the ways proposed? Who knows? One might get some hint of the answer by turning Bindra's proposal over to the Artificial Intelligence (AI) types (see Pylyshyn et al., this issue), whose stock in trade is taking such sketchy design proposals and trying to come up with ways of implementing them. But I suspect we will not have to wait long for an answer. The sorts of suggestions Bindra makes seem to me to propose structures much less sophisticated than structures already known by the AI community not to be powerful enough to model very intelligent behavior. I do not expect that they will believe Bindra when he claims that "'cognitions' postulated to account for complex forms of perception, meaning, recall, and problem solving are ultimately constellations of overlapping and nested contingency organizations, each developed by the observation of stimulus-stimulus relations in a simple learning experiment."

There is an abundance of reasons for viewing askance the freewheeling system-designing of the AI school, but one makes a mistake if one contrasts that enterprise with the apparently "more scientific," apparently law-seeking enterprise instanced here by Bindra, for the two are really the same: both schools are trying to design systems powerful enough to explain the data, but the latter school suffers from the great disadvantage of thinking it is doing something else.

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**Reinforcement, explanation, and B. F. Skinner.** In the famous 1924 debate between John Watson and William McDougall, McDougall predicted that Watson's "peculiar dogmas" would soon "fade away like the insubstantial fabric of a dream, leaving not a wrack behind" (Watson and McDougall,

1928, p. 92). Bindra now tells us that at least one dogma that Watson's successors have successfully fostered for more than fifty years has finally seen its day. But his attack has two major shortcomings: First, it is based on certain misconceptions about at least one important formulation of the reinforcement concept, and second, the "new" view that he offers is not so new.

Bindra spends much time criticizing a straw man, namely a hedonic view of reinforcement. For Bindra, a "basic tenet" of the reinforcement view is that behavioral consequences are effective by virtue of their value as "pleasure and pain," "rewards and punishers," and in terms of how "pleasant" they are. But a hedonic view of reinforcement was rejected by many early behaviorists (cf. Meehl, 1950) and is far from the representative view of modern behavioristic formulations.

Skinner clearly and explicitly rejected the hedonic view of reinforcement (e.g., Skinner, 1938, p. 62 *op. cit.*), and even Thorndike was careful to define "satisfiers" and "annoyers" in terms of their observable effects rather than their putative hedonic value (e.g., Thorndike, 1931, p. 36). Skinner's concept of reinforcement, as elaborated by his students (e.g., Catania, 1973; and this *Commentary*), may be briefly stated as follows: If we can show that a response increases in frequency because (and only because) it is followed by a particular stimulus, we call that stimulus a *reinforcer*, and its presentation, *reinforcement*. Reinforcement, so defined, is a term we invoke when we observe certain relations between events in the world. It is neither a circular concept, nor a hedonic concept. It is simply a name we give to certain facts.

Bindra lists what he believes to be three important characteristics of the reinforcement concept. He says, first of all, that a reinforcement "hypothesis" "assumes" that behavior is modified because of a contiguity between behavior and "hedonic" stimuli. But there is no reinforcement "hypothesis"; the term is invoked only to label certain facts. Moreover, that response-stimulus contiguity affects subsequent responding is not an "assumption"; it, too, is a fact. And, as mentioned above, hedonism need not be part of the reinforcement concept.

Second, Bindra claims that implicit in the reinforcement view is an assumption about neural connections, and he then proceeds to pose his own set of assumptions about these connections. But Skinner's view of reinforcement explicitly avoids such weak assumptions, as Bindra himself briefly notes later in his essay:

Finally, Bindra objects to the fact that reinforcement is said to act only on one particular response, which is then "more strongly connected" to a stimulus situation. But Skinner has managed without any connectionistic assumptions, and, as Bindra correctly notes, he avoided the so-called motor equivalence problem by defining a response class in terms of its measurable effects.

In short, not one of Bindra's characterizations of the concept of reinforcement is appropriate to Skinner's formulation. Bindra's presentation would have been more effective had it addressed itself more directly to Skinner's view, especially considering the great impact that Skinner's work has had on modern psychology.

The few comments that Bindra does make about Skinner's position are inaccurate. He says, for example, that Skinner promoted a position that "sacrificed explanation to the empirical goal of enunciating descriptive principles." But Skinner never "sacrificed explanation"; he simply dispensed with "explanation" that appealed to hypothetical constructs in favor of explanation in terms of antecedent and current observables. It is also not true, as Bindra claims, that Skinner took "no clear stand on how reinforcing stimuli reinforce responses" (e.g., see Skinner, 1953, pp. 81-84 *op. cit.*). Finally, Bindra is disturbed because a variety of experimental phenomena indicate that learning can take place although no obvious response-reinforcement pairings have occurred and that reinforcement does not account for all behavior. But these phenomena do not indicate a failure of the concept of reinforcement, only that the concept has been misunderstood (Skinner, 1977). Reinforcement is not an account of all behavior; it is a name for one important relationship between behavioral and environmental events.

Bindra's own model suffers from some of the faults of the straw man it is supposed to replace. For one thing, it ignores certain data. Bindra looks to research in classical conditioning for understanding of the learning process. He seems to believe, in fact, that *all* behavior is elicited and thus arbitrarily excludes data from his consideration that demonstrate consequential control.

Bindra's program, furthermore, is reminiscent of early British associationism, clearly evident in a main "tenet" of his program that "building

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representations of stimulus-stimulus correlations is the sole basis of learned modifications of behavior." He is concerned with the acquisition of "knowledge," and, as John Locke did in the fourth edition of his *Essay*, Bindra invents a phenomenon of association ("correlation") of ideas ("representations" of stimuli) as an explanation for observed changes in behavior. As was true for Locke, Bindra's theory of unobservables is not supported by current physiological knowledge, and hence, is still more philosophy than science.

As is true of any theory that depends on hypothetical constructs, Bindra's theory is ultimately at the mercy of observed events in behavior and environment, no matter what physiological mechanisms are eventually unearthed. The theory could not have arisen if orderly relationships between such events had not been observed, and the theory's maintenance must always be subject to these relationships. Skinner's work has done much to clarify the relationships between behavioral and environmental events and has established a body of data that construct theories such as Bindra's can only serve.

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**Defining behavioral representations.** Bindra addresses two important and related questions. The first is how we divide processing stages into meaningful dimensions for subsequent analysis. The second is how we account for the observed fact of response variability.

These are largely matters of behavioral taxonomy. How do we divide the universe into meaningful compartments and seek rules of relationship among these compartments? Several points arise here. First, divisions in behavioral research (e.g., "pexgos") are abstractions. They are not "things." Second, the abstractions of "pexgos," "gnostic units," and so forth, are in some sense arbitrary. Their reality is difficult to evaluate outside the particular conceptual framework within which they are proposed. Third, categories can easily take on a unitary quality. Yet most of these categories can be fractionated further as analysis proceeds. Fourth, categories are easily assumed to be independent of one another. This assumption is frequently contradicted by empirical evidence. Each of these four points is illustrated in the literature on motivation ("drive") (e.g., Hinde, 1959; Fentress, 1976).

These points can be extrapolated to still more perplexing problems. For example, how separate are such popular categories as perception, motivation, learning, and so forth? To what extent do the boundaries that separate one category from another shift in time (Fentress, 1978)?

Bindra's various postulates are logical in the abstract sense. But are they also biological? Certainly the postulate of changing processes at early stages of a complex and poorly understood temporal network of events can yield a change in observed output. That is logic, as any pencil and paper exercise will clearly demonstrate.

But how strong are the supporting biological (or behavioral) data? Bindra argues against the idea of preexisting responses, but how are we to take this? The potentiality for exhibiting certain responses and not others exists for any species or individual at some level, although one must presumably agree that relevant circuitry not activated at a given moment may fail to "exist" in a full functional sense. This is obvious logic.

More directly to the point is Bindra's assertion that "current perceptions" necessarily dictate the final form of any observed motor act. Yet there are data for both vertebrate and invertebrate species indicating that reliable and complex patterns of motor output can be maintained in the absence of normal sensory input (e.g., Fentress, 1973; Doty, 1976; Hoyle, 1976). The advantage of these studies is that sensory channels can be defined and manipulated without ambiguity. Of course, they do not (logically) address

fully the more complex issues raised by Bindra. Yet they do bear upon his arguments.

One aspect of Bindra's approach that is particularly important is his recognition of dynamic regulation of behavior at several processing stages. This is a clear advance over static models in which control elements are viewed independently from their spatiotemporal surround. The latter models are exercises in pseudoprecision.

An analogous pseudoprecision can arise, however, when a large number of variables are postulated and then connected by equations (or arrows). Surprising experimental data are too easily accounted for by the addition, subtraction, or other alteration in connections between abstractly defined "boxes." The intrinsic defining characteristics of the boxes themselves become immune to modification; that is, their basic properties are assumed.

The essential question here is, what observations would alter the defining characteristics of "pexgos" as opposed to the relations between "pexgos" and other processing dimensions? What types of observation would lead one to abandon the pexo construct as opposed to adding, subtracting, or otherwise modifying rules of connectivity among "pexgos," "motivational states," "valences," and so forth?

Bertrand Russell is said to have argued that if given more than three variables ("postulates") and all possible rules of connectivity between them, he could (logically) account for all but the most complex manifestations of nature. While Bindra clearly has proposed an advance over previous S-R and response-reinforcement models (which are blatantly inadequate), I wonder whether the thinking he uses remains rooted in the basic form of logic from which these models were derived. I would be interested to see Bindra address such issues further.

I suspect that future models in behavioral biology will have to take a much more relativistic stance, in which separated dimensions of behavioral expression are recognized as convenient yet arbitrary abstractions whose refinement will depend upon the logic of defining these dimensions in part as a function of their rules of relationship and data that are explicitly biological as well as logical.

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### by John P. Flynn

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**Sensory vs. motor effects of brain stimulation.** In arguing against the idea that motivation influences behavior by a direct effect on responses, and in favor of a primary effect on the perceptual processes, Bindra presents as critical evidence experiments involving electrical stimulation at "motivational sites" in the brain.

Movements elicited by electrical stimulation of the brain are flexible, and adjustments to the changing situation do occur. Nonetheless, Bindra's exclusion of direct effects on response is too sweeping, since there are motor responses determined by stimulation at attack sites in the brain. In our work, the paw with which a cat strikes a rat is exclusively contralateral to the side of the brain stimulated, even though the cat may be greatly handicapped in the use of that paw (Flynn, Edwards, & Bandler, 1971). A cat in which the pyramidal tract innervating the cells corresponding to the contralateral paw is severed, and the opposite side is intact, continues to use the partially denervated limb, even though an intact limb is available. The influence of stimulation upon motor systems involved in striking was demonstrated by elec-

trophysiological experiments on the forelimb biceps motoneurons (Edwards & Flynn, 1972)

Electrical stimulation at attack sites in the brain influences the effectiveness of certain environmental stimuli in eliciting behavior, but the influence is not limited, as Bindra suggests, to the sensory sphere. Many of the effects observed in our laboratory have been of a reflex nature, and could be explained in terms of influence on either the motor or the sensory side of the arc. However, a study of a reflex involving placing the paw on an object implicates the sensory side of the arc. The receptive field for the elicitation of the reflex increases in size, as the intensity of stimulation is increased, in accord with the sensory dermatomes rather than in accord with the motor innervation (Bandler & Flynn, 1972). It should be noted that the vigor of the motor response increases with intensity.

Another example of the influence of hypothalamic stimulation on sensory mechanisms is provided in an experiment by Beagley and Holley (1972). Deprived rats learned to press a bar to obtain food. These rats, which could also be induced to press a bar for food and to eat by stimulation of the hypothalamus, were trained to press only while signal lights were on. Each eye received light solely from a single small bulb mounted on the top of the rat's head. When both lights were on, the rat pressed the bar. If a single light was on, the rat pressed the bar when the contralateral eye was illuminated, and not while the ipsilateral eye was illuminated. Shifting to stimulation of the opposite side of the brain produced the same phenomenon. This response was a learned response; its occurrence depended on illumination of the contralateral eye.

The basic question that Bindra raises of the flexibility and adjustment of the motor responses to present stimuli probably has to be answered in terms of the breadth of motor activities available to the naturally induced state, as opposed to the limited number available in the electrically induced state. Differences in terms of the number of sensory routes by which an act may be brought about appear in our data. The state-dependent reflex of mouth opening can be elicited by both visual and tactile stimuli when some sites are electrically stimulated. At other sites, only tactile stimuli are effective. When a natural state is induced probably the whole gamut of responses is available.

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by C. R. Gallistel

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**The irrelevance of past pleasure.** The S-R conception has indeed proved to be an unsatisfactory analysis of learned behavior. The continuing commitment to such a view is an impediment to attempts to find a physiological basis for learning. Many such attempts hinge on finding a suitable model system, a system that displays the crucial features of learning and is amenable to neurophysiological experimentation (e.g., Berger, Alger, & Thompson, 1976; Mpistos & Collins, 1975; Norman, Buchwald, Villablanca, 1977). If one's conception of the crucial features of learning is erroneous, then these attempts are doomed at the outset. One is looking for a square model of a round phenomenon.

Does Bindra's alternative satisfactorily explain behavior in traditional instrumental paradigms? I am not sure, because it is not clear how it deals with two traditional objections to the explanation of instrumental learning in terms of incentive or the remembrance of past pleasure. The first objection is that the appetitive phase begins (almost by definition) in the absence of the assumed-to-be-pleasurable goal stimuli. One must ask what incites and directs the appetitive phase. It cannot be the memory of the pleasure afforded by the goal or the memory (record of correlations, etc.) of what leads

to the goal. These are with the animal always, but the animal only occasionally pursues the goal. The rat, in a familiar but large and complex environment, only goes to the food source 5-8 times a night (personal observation). The knowledge of the environment that enables the rat to go directly to the food must be there even during the period when the rat chooses not to go there. This knowledge (or, in neuropsychological terms, this engram) cannot, therefore, explain why the rat does or does not choose to go there. Bindra's way out seems tortured. He argues that certain organismic states determine whether or not a given incentive will actually incite. In order for food to create a "central motive state of feeding" the animal must already be hungry (see, second paragraph of section on Spontaneous Activity). The "hungeriness" of the animal, which antedates and selectively makes possible the appetite-whetting effect of the food, carries so much of the explanatory burden that one wonders why the focus on incentive

The second objection to the incentive theory is that the pleasantness or unpleasantness of a stimulus is contingent on the animal's state. The sweet that is pleasant when one is hungry is neutral or unpleasant when one is sated (Cabanac, 1971). It would be more adaptive to have an animal's behavior governed by a present need for, say, salt and oriented by a memory of where salt has been found than to have behavior both impelled and oriented by whether the salt was unpleasant or aversive when found. The animal may have been hypernatremic when the salt-lick was found and the salt therefore hedonically neutral (Kriechbaum & Wolf, 1968). If the memory of recent pleasure or displeasure were decisive, then those of us who rise from the Thanksgiving table nauseated by the sight of turkey should be ambivalent at the prospect of a Christmas turkey. In fact, however, although my memory of the displeasure brought on by surfeit is still fresh as I write this, I am hungry right now and would welcome that noble bird. Latent learning studies show that much the same is true of animals: A stimulus that had negative hedonic significance when last experienced becomes the goal toward which action tends when the animal's needs have changed (Stricker & Wilson, 1970).

Incentive theories assume that pleasure begets desire; Cabanac's (1971) work shows the reverse is often true: pleasure consists in experiencing what one desires. The philosopher Ryle (1969) makes the same point. Animals often pursue past pleasures and avoid past displeasures only insofar as the present desire (need, specific drive) matches the past desire. Incentive, therefore, often cannot explain desire, and the effect of a specific drive on the use made of memory remains to be explained.

To the extent Bindra's formulation can allay these criticisms, it may be useful in analyzing the behavior of the rat in traditional instrumental paradigms. However, it seems not to apply to many learning phenomena. Consider, for example, the nestling bunting, which learns the center of rotation of the night sky and months later uses this knowledge to help orient its maiden migratory flying (Emlen, 1970). Consider the rat in Olton's (1976) 8-arm radial maze. It notes the arms it has sampled, and carefully avoids them in order to sample other arms, even though it has received and eaten food in the already sampled arms. Consider the female digger wasp. She makes a stereotyped reconnaissance flight over her egg-containing burrow, during which she notes its location vis-à-vis near and far landmarks. Subsequently, when she has found prey for the hatched larvae to eat, she homes on her burrow from a hundred or more meters away. She homes successfully even when she is captured during her journey and released again at a different point of the compass (van Beusekom, 1948; van Iersel & van den Assem, 1965; Thorpe, 1950). Consider the chimpanzee, which watches food hidden at a dozen different spots and when released moves systematically from spot to spot gathering the food it saw hidden (Menzel, 1973 and this Commentary). In all these adaptive behaviors, learning plays a crucial role, yet I do not see that Bindra's formulation applies to them. The pleasure the rat may derive from eating in a sampled arm does not explain why or how it avoids that arm until it has sampled the others. The pleasure the nestling may experience in watching the stars does not explain how or why it abstracts the center of rotation; nor does it explain the use the bird makes of the knowledge gained from its juvenile astronomy. Can finding the center of rotation be thought of as learning a correlation? The notion of learned correlation does not explain why animals record only selected aspects of the information derivable from experience. Perception and memory are more differentiated than is implied by the notion of stimulus-stimulus correlations. Correlation is too vague. Nor does the notion of incentive explain the diverse ways in which the record of what past experience has taught is used to govern present behavior. I think

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we will have to be more specific about exactly what is recorded in the engrams and how the system uses the engram to control behavior. The only cognitive-map theorist who has been sufficiently explicit so that one could build a working model is Deutsch (1960).

However, Deutsch's ingenious and explicit model cannot be applied to some of the examples cited above. The examples were deliberately chosen to suggest the heterogeneity of learning phenomena. A unified theory of learning may be a pot at the end of a rainbow. What we may find in searching for that pot is that the principles of learning depend both upon the species and upon the type of problem that the learning helps to overcome. There may be no general principles of learning, only problem-specific principles (Rozin & Kalat, 1971). We must content ourselves for the foreseeable future with much less sweeping models. We should attempt to formulate the principles of learning underlying, say, homing behavior in diverse species and diverse contexts. Here, the logical structure of the problem may have forced a limited class of solutions upon the innumerable species whose lifestyle requires homing. We should not expect that the principles underlying the learning that makes homing possible will apply to the learning that makes possible other kinds of behavior (bird-song dialects, diet selection, language, etc.). Bindra's formulation takes no account of problem specificity.

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**Misconceptions about the origins of behavior.** Bindra addresses issues that we used to debate earnestly as undergraduates in the late 1950s. Do organisms learn relations between stimuli, or between stimuli and responses? Is what an organism perceives at the time of learning more important than what the organism is doing? Is behavior the result of the history of its reinforcement, or of the motivational and incentive-producing

properties of current stimuli? What is learning, anyway? Who knows? Who cares?

Bindra cares because he wants to be able to elucidate the "principles and mechanisms of adaptive behavior." That was our aim in the 1950s. We got nowhere, and the only concrete result of the whole exercise was the generation in many of us of revulsion for a kind of theorizing about the nervous system that had much of the obscure unreality of scholastic philosophy and little of its human interest.

Had Bindra been our guide to this netherworld of hypothetical neural constructs we might not have been so blighted. His presentation is well-organized and pulls no punches. He provides a broad historical context (more than half the cited material was published before 1962), a magisterial sweep of problems of behavior ranging from auto-shaping to the acquisition of knowledge, and promise of substantial progress if his view of things prevails. My overwhelming sense on reading his article was nevertheless one of *déjà vu*, chased by a conviction that speculation of this kind about the nervous system has hardly advanced.

To be fair, this lack of advance is part of Bindra's point. Bindra believes that conceptualization about behavior has been sidetracked by the hegemony of what he describes as the "response-reinforcement framework."

What Bindra means, I think, is, first, that there has been too much emphasis on the experimenters' view of what organisms are doing, and not enough on the organisms' view of what experimenters are doing – too much emphasis on action and reaction, in other words, and not enough on perception.

Second, Bindra wants to promote the incentive properties of hedonic stimuli such as food and electric shock at the expense of the reinforcing properties of these stimuli. This means, as far as I can ascertain, that for Bindra the important aspect of the delivery of food to a hungry animal in a particular situation is not the strengthening of the behavior that happens to precede the presentation of food, so that this particular behavior is more likely to occur when the situation recurs. It is, rather, the ability of the food to infuse the situation with the power to induce anticipation of food in the animal.

To buttress his description of essentials, Bindra reaches into the organism's brain where he postulates many "central representations" of stimuli and behavior, and a neural condition known as a "central motive state." Proxies for stimuli and their perception are given the acronym "pexgo," from presently excited gnostic organization. Proxies for behavior are called "act-assemblies."

At the core of Bindra's treatise is the assertion that learned behavior arises because a pexgo of a particular stimulus aspect of a situation generates a pexgo of food presentation (or whatever hedonic stimulus was last noticed by the animal in conjunction with the stimulus), which generates the same central motive state as food presentation itself, which together with "the detailed sensory-spatial features of the situation" produces the response.

In a rare appeal to data, Bindra adduces studies of the effects of electrically stimulating the hypothalamic-limbic "motivational sites" of the brain via implanted electrodes. Animals thus aroused do not emit behavior that is specific to each stimulated site. The site appears to determine only the general direction of the behavior. The specific actions depend very much on the particular situation. These observations are provided as evidence for a central motive state. Evidence for other neural constructs and relationships is not given. The lack is admitted in the case of the critical "mechanism by which a certain pexgo is translated into a particular act," but it is not clear to this commentator that the other postulates can be given much substance. Even the cited evidence for the central motive state should be a cause for cynicism. Probing the brain in 1978 is still very much a hit-and-miss affair. In 1966, when the last of the data Bindra adduces were gathered, electrical stimulation of the brain was conducted with the same order of finesse as that of an inquisitive child exploring the functioning of a computer with a screwdriver.

Stripping Bindra's analysis of its neurophysiological speculation, it seems to me that what is being proposed is a two-level mechanism for the determination of behavior. At the more general level, a "situation" determines the broad features of behavior, according to the experience of the organism in the situation. More locally, behavior is determined by "detailed sensory-spatial features of the situation."

Likewise, behaviorists who emphasize behavior have been flexible in the units of their investigation – not a bad strategy for a scientist, when it is remembered that Charles Darwin in his *The Origin of Species* did not even

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consider the problem of speciation. Behavior selected for analysis has ranged from general categories such as school attendance to very particular acts such as pressing a bar within a certain force band. Implicit in this eclecticism is the assumption that the relationships between reinforcing stimuli, behavior, and stimuli that come to occasion behavior hold for many levels of analysis. The situation of "being at home after breakfast" occasions "preparation for school" in a child with a probability that depends on the child's experience of consequences of behavior emitted in that situation. The emission of parts of the preparation, such as the details of tying a shoelace, is similarly dependent on the history of consequences of very local acts occurring during the action of very local stimuli. Viewed in this way the two frameworks are hardly different.

Bindra asserts two differences between the frameworks, even aside from differences in assumed or implied neurophysiological mechanisms. He claims that situations in which hedonic stimuli occur can acquire significance for the organism even when no specific response is reinforced, whereas the response-reinforcement framework does not allow this. Even apart from the impossibility of determining that no specific response is reinforced, this difference is really no difference at all, because most scientists who emphasize behavior allow that stimuli can acquire significance when they are paired with significant stimuli (e.g., Staddon, 1972).

The second asserted difference concerns the flexibility of the behavior that occurs in a situation in which a hedonic stimulus has previously been experienced. Bindra's formulation of the actual production of such behavior is vague, but depends on the concept of an "eliciting stimulus complex" that, via the previously mentioned pexgos and act-assemblies, gives rise to a specific act lasting less than half a second with a probability that depends on the relative "motivational valence" of the complex. Flexibility is assured, according to Bindra, because longer instances of behavior are "put together fleetingly from act-assemblies for a particular occasion," and because "an old sequence can be interrupted and changed within a fraction of a second" according to "moment-to-moment changes in the current sensory inflow and the central influence from contingency organizations, as well as central motive states" allowing "substitutability of acts" and "motor equivalence at the behavioral level!" Clearly this mechanism is as flexible as anyone could want; so flexible, in fact, as to almost obviate utility, although it should also be noted that a certain rigidity of relationship between eliciting stimulus complexes and specific acts is supposed, which, undiluted by further refinement of the mechanism, could impose severe restraints upon an organism's repertoire. All this is contrasted with the response-reinforcement framework, which Bindra says provides only for repetition of previous behavior and therefore cannot encompass flexibility. This false characterization requires more substantial rebuttal, for here we may be right at the heart of what, from his title, should be the focus of Bindra's argument, namely the production of adaptive behavior.

Leaving aside for the moment the difficult problem of defining "adaptive" (which Bindra ignores), it is reasonable to assert that an adaptive system can be of two forms. Either it is so programmed as to anticipate every change in the environment, and to behave appropriately, or it embodies mechanisms for variation in output and for selection by the environment of appropriate variants (Gilbert, 1972). Obviously a single system can embody both forms.

The evolution of organisms provides a good example of a largely unprogrammed system, depending profoundly on variation and selection, but with constraints on the former imposed by the chemistry of the substrate. Behavior change may be an example of a mixed system, with phylogeny providing certain programmed responses and important constraints on both variation and selection. The so-called response-reinforcement framework almost precisely parallels these system requirements. The emphasis has been on selection, that is, reinforcement, but variation has not been ignored.

Variation in behavior, which is the source of adaptivity, seems to be intrinsic to the business of response reproduction, as much as phenotypic variation is intrinsic to the reproduction of organisms. The fact that a reinforced response may not be duplicated precisely provides a rich source of "new" behavior that can, in turn, be selected. Segal (1972), in her comprehensive discussion of the provenance of instrumental behavior, describes many other sources of new behavior, including induction by deprivation and by releasing stimuli, and what she calls "emotional induction," namely "operations that are insufficient to guarantee the occurrence of any particular topography, but that set the stage for a certain range of stimuli to be effective in inducing or releasing a delimited range of topographies."

This last process sounds suspiciously like Bindra's description of the role of the central motive state. The difference is important, however. Segal's analysis of how adaptive behavior occurs is well within the response-reinforcement framework and, as such, makes no appeal to neural fictions.

A salient feature of Bindra's article is the absence of any discussion of the relation between ontogeny and phylogeny in the production of behavior, notwithstanding the inclusion of the term "adaptive" in the title. That is partly why the article sometimes seem unreal, for phylogeny is the stuff of adaptation. Bindra should be contrasted with Herrnstein (1977a), who has also launched a recent attack on the response-reinforcement framework, but did so from a biological perspective, that is, one that recognizes that instrumental behavior has two kinds of provenance. Herrnstein, like Bindra, emphasized what he regards as shortcomings in the treatment of motivational variables. Skinner (1977) replied by further invoking phylogeny, which Herrnstein (1977b) described as an unnecessary strategy. The interchange is rich, and is mentioned here only to suggest the poverty of an analysis that ignores evolution.

"Adaptive" is, in any case, a loaded term that should be avoided. It not only implies ends, thereby incurring problems of teleology in its use, but it also suggests hierarchies of ends, thereby raising considerations of metaphysics and morality. Many actions (e.g., suicide while fertile) may not be adaptive from the point of view of preserving the individual or even the species, but presumably they conform to the usual relationships that determine behavior. In discussing flexibility, Bindra seems to confuse adaptivity with creativity. The former, if it means anything at all, refers to appropriate adjustment to a changed environment, which is a process entirely within the ambit of the response-reinforcement framework. The latter term refers to useful new behavior, the organized production of which will probably defy analysis for a very long time whatever framework is espoused.

In short, Bindra's article provides no good reason for completely discarding the "response-reinforcement framework," as Bindra would have us do. Indeed, often his alternative seems so similar as to cause wonder as to why differences should be emphasized. Two differences nevertheless remain conspicuous. Bindra's "perceptual-motivational" alternative invokes much speculation about neurophysiology and little about phylogeny. Behaviorists interested in behavior have developed much interest in the latter and maintain little in the former. Which strategy is more appropriate to the changing circumstances of our knowledge remains debatable.

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*With reinforcement in trouble, what about motivation? – Bindra's alternative* This is a very thoughtful discussion of problems with which Bindra has been concerned for many years, as evidenced by his many previous discussions, some book length (1959, 1976). It is good to have this accessible version of his matured theory.

In order to comment briefly on something that deserves more extensive discussion, I shall consider three major thrusts of his argument: first, that the response-reinforcement position has become outmoded; second, that the perceptual-motivational alternative is sound from a psychological point of view; third, that the theory can gain support from neurophysiology.

*Critique of response-reinforcement* In this Bindra is on firm ground and in

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good company, for even many of those who were earlier strongly committed to the reinforcement position have since preferred to modify their views I suppose that the chief holdouts are those most strongly committed to the orthodoxies of the Skinnerian position, but this standpoint, despite its obvious successes in the management of performances under many circumstances, has long been characterized as an engineering achievement rather than as a contribution to theory, (Verplanck, 1954)

*The perceptual-motivational alternative as a psychological theory* The alternative theory has to be viewed in the context of the popular cognitive or information-processing theories with which it competes There are many overlaps, such as learning to pay attention to relevant information sources, remembering the contingencies, and so on It would take more space than I have to make detailed comparisons It is probably fair to say that the information-processing theories have been a little weak on the motivational-affective aspects, but developments such as those of Rescorla (1969) on fear conditioning do not seem too remote from those of Bindra Abelson (1963) early made a distinction between "hot" and "cold" cognition, with the idea that "hot" cognitions are emotionally and motivationally loaded, whereas "cold" cognitions are very intellectual Most information-processing models tend to fall on the "cold" side

Even so, Bindra has tended to remain pretty close to drive notions by emphasizing stimuli associated with hunger, thirst, and the like in his descriptions of the affective or motivational components This may be due to his preoccupation with animal learning, because that is where the response-reinforcement theory held sway In human learning, language is important (as Pavlov noted in his "second signal system"), and there is more to be said about enduring intense emotions and moods as they affect human learning and decision processes (Janis & Mann, 1977)

The treatment of these topics is a thoughtful one, and in characterizing the paper briefly I have overlooked much of its richness in detail

*The neurophysiological interpretation of the alternative.* Most of us are fully convinced that mental events are rooted in the anatomy and physiology of the nervous system We are not equally convinced about the fruitfulness of speculations concerning the relationships between psychological findings and neural events Obviously there are well-established facts at both levels that have not yet been coordinated, and these separate knowledges have scientific status in their own right That is, facts such as visual acuity, or color vision, do not require for their validation knowledge of the retina or the central connections of visual mechanisms; correspondingly, the courses of nerve fibers in the optic chiasma, or from the lateral geniculate bodies to the cortex can be mapped without necessary recourse to psychology It is a happier state when the facts are brought into coordination, as when the fineness of visual acuity and the coarseness of the cellular mosaic of the retina are brought together through a knowledge of the synthesis provided by small eye movements; then we are more comfortable with the resulting "explanation," which means that the related facts are coordinated without contradiction Unfortunately, it is easier to draw the illustration from perception than from learning

There is the danger of making use of the C N S as a "conceptual nervous system," as Skinner once defined it I am a little uneasy about calling central perceptual representations pexgos, and then treating them as substantive, for example," perception involves the generation of a pexo – spatially and temporally ordered activation of certain structural gnostic elements or pexo components The gnostic elements are gnostic-neurons and gnostic-assemblies" Although unintended, this sounds a little too much like an "en-gram" as the basis for memory Of course memory must somehow be represented, and pexgos must somehow be represented, but the representation as described seems to me a little cavalier The important search that Bindra recommends, however, is not for these hypothesized elements, but for the locus of the motivational influences on learning, in which the central roles of perception and knowledge displace the prior emphasis upon response This is a desirable search pattern

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### **by Stewart H. Hulse**

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*The problem of serial order in adaptive behavior: why not some formal cognitive structure.* There are many things to commend the approach outlined in this article Some suggest to me things of the past or directions for further development other than those Bindra has proposed, however For one, he has pushed theoretically important structures underlying behavior upstream and inside the organism, which is certainly where they belong We have had peripheral S-R reinforcement models for too long But he seems far more successful in dealing with certain aspects of this process than with others I like his discussion of the relationship between eliciting stimuli and pexgos, but these ideas were clearly foreshadowed by Lawrence (1963) in his concept of the "stimulus-as-coded" The concept of an act-assembly as a short-duration building block for overt behavior subject to rapid and flexible modification is also useful. But it conjures the problems inherent in a micro-molar analysis of behavior (Logan, 1960), and it fails to solve the problem so masterfully expressed by Lashley (1951) of how syntactic structures of whatever kind can be so rapidly produced in behavior, e.g., the pianist's arpeggio, all of which do call for the concept of some pre-programmed integrating schema for the overt production of muscle movement Taub's (1976) work with deafferented primates raises similar questions about the central programming of motor control [see Roland et al., this issue] Granted that Bindra's system is probably sufficiently flexible to handle these matters, the problem may be that it is almost too flexible Much behavior, once underway, is not easily modified by eliciting stimuli, and some of it requires longer than the half-second that Bindra postulates as a temporal boundary for an act Any golfer will testify that a swing, once underway, is virtually impossible to stop given some unusual "extraneous" eliciting stimulus In fact, I wonder if Bindra has adequately explored the realm of preprogrammed motor responses with respect to their modifiability – or lack of it

A more general, and perhaps more important, concern has to do with Bindra's postulation of a linear, chain-like organization for behavior One of the great contributions of models of human information processing has been to move away from linear, list-generated models and to explore the advantages of other organizations for behavior, such as hierarchies We have recently been trying with some success (Hulse, in press; Hulse & Dorsky, 1977) to apply principles derived from hierarchically based models to the manner in which animals learn serial patterns of stimulus events The end product in overt behavior is a linear organization of responses in time, but the underlying formal structure to which the animal must respond is best described by models based in general on hierarchical arrangements of relations among stimulus events General discussions of this approach, and some of the problems it presents, may be found in Restle (1970) and Jones (1976). Bindra comes close, perhaps, to this idea in his concept of "contingency organizations" and their nested relation, but I fail to detect that he has in mind anything more than a linear structure in which overlapping contingencies vary in strength and so excite successive components in an overt response system after the fashion of a response chain Perhaps I misinterpret, but the problem is of sufficient importance, I think, that it might warrant further exploration within Bindra's system Dewey (1898) identified the problem to some extent in his criticism of the concept of the reflex arc; Lashley (1951) certainly reemphasized it; and the matter remains of significant importance for new theories of adaptive behavior that we develop today

By and large, however, Bindra has been organizing and reconceptualizing a vast amount of information, a task that few are willing to undertake today He is to be commended and, if anything, encouraged to go even farther afield in restructuring the way we think about models for adaptive behavior in animal learning

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**Some comparisons of Bindra's theory with a situation-act-outcome system** Bindra's development of an alternative to response-reinforcement theories of behavior is of special interest to anyone who, like myself, has undertaken a similar task (Irwin, 1958, 1961, 1966, 1971, 1974). Like Tolman (1932 *op. cit.*) and some more recent cognitive theorists Bindra and I believe that instrumental behavior depends upon the organism's cognitions of contingencies relating environmental events to each other and to its behavior, and upon differential values of events to the organism. What invites comparison of our theories is their marked divergence in program, theory, and metatheoretical presuppositions, of which a few instances are discussed below.

**Theory and metatheory** First, Bindra aims to provide hypothetical neural mechanisms adequate to account for intelligent behavior. His rather informal system, as I understand it, can be described as atomistic and connectionistic, the input elements being neural representations of perceptual features combined into wholes called "pexgos," and the output elements being "movements" that are constituents of unitary all-or-none "acts" that in turn are combined into "responses." The various combining operations take place in intricately organized neural structures called "gnostic assemblies" and "act assemblies." Pexgos are interconnected by "associations" (as in his Table 1) that represent "learned contingencies" between successive events. Because pexgos and acts are fleeting and never twice identical, they must be constructed afresh upon each occurrence by the gnostic and act assemblies. These structures and interconnections are required to accomplish remarkable feats of creative organization, and I confess that I do not see clearly how they do it; even a highly simplified mathematical or computer model might help here.

My moderately formalized system takes a wholly different path. It attempts to construct psychological concepts from purely behavioral evidence by means of three primitive terms, "situation," "act," and "outcome," and certain discrete-trial binary-choice procedures, called standard experiments. With this equipment, SAO theory (named from the initials of its primitives) claims to provide empirical criteria of three fundamental psychological processes, viz., "discrimination," "preference," and "act-outcome expectancy." Additional processes are defined upon this basis, such as "detection," "desire," "aversion," and "neutrality," and an empirical distinction is made between "intentional" and "nonintentional" behavior. Although SAO theory rests upon purely behavioral conceptions, it welcomes psychophysical and psychophysiological hypotheses. Bindra's theory (Bindra, 1976 *op. cit.*) is boldly fleshed out with specific hypothetical neural mechanisms designed to account for a multitude of phenomena, to which these comments cannot begin to do justice. SAO theory, on the other hand, has proceeded cautiously, and in its present stage of development is an incomplete skeleton that suggests and organizes a variety of hypotheses that have been developed rather opportunistically to show how the system works.

Second, relatively few explicitly psychological terms are embedded in Bindra's system; the main instances are perception and certain hedonic or affective notions called variously pleasure, pain, pleasantness, unpleasantness, discomfort, valence, appetite, aversion, and the like, which are not defined nor given behavioral criteria. This rather extreme informality raises

the question whether the theory is committed to psychological concepts at all. Perception, for example, enters the theory only as that which is represented by pexgos, which in turn are purely neural organizations; this suggests that perception might be dispensed with altogether, without damage to the theory. SAO theory, on the other hand, explicitly denies the identity of psychological phenomena with whatever physiological states and processes may be associated thereto (Irwin, 1971, p. 107). I fear that, for Bindra, perception, pleasure, pain, and the like are "private experiences" (Bindra, 1976 *op. cit.*) in sharp contrast to SAO theory's attempt to objectify such notions, as illustrated by its treatment of "consciousness" (Irwin, 1971, pp. 107-108).

Third, according to Bindra, explanation is the successful reduction of a phenomenon to a more "molecular" level. SAO theory takes a phenomenon to be explained if it is deducible from a generalization or law, whether or not the generalization involves physiology.

Fourth, informality in the theory may have heuristic advantages in enabling the theorist to extend the scope of his ideas freely over a broad field and perhaps thereby discover unexpected regions of applicability. Bindra is, I think, well served in this respect. At the same time, risks of unnoticed inconsistency or circularity are increased. As an apparent instance of the latter point, consider the statement that certain stimuli "have the property of reliably producing appetitive or aversive reactions and being affectively pleasing or discomforting," where it is doubtful that the terms of the conjunction are independently definable in Bindra's system. A similar difficulty occurs with his claim that "the most general principle determining response production" is that "an animal will move toward an ES that is of greater appetitive motivational valence or of lesser aversive motivational valence than other ESs in the situation at that time."

**Motivation** Bindra and SAO theory treat motivation in much the same way, if one makes rather generous allowance for differences in language and emphasis upon the roles of stimuli. His "incentive stimuli" and the "desired" and "aversive" events of SAO theory overlap greatly, if not completely. It is true that such events as "being a member of a prestigious group" and "being thrown into jail" are possible desired or aversive events in SAO theory; I would be happy to know what Bindra would say about them, since they are hardly stimuli. Bindra's "central motive states" are conceived of as physiological, whereas SAO's "preferences" are behavioral dispositions that are defined relative to standard preference experiments; in practice however, I see little difference between them. The theories agree that motivation is not defined by tendencies to perform actions that are specific to particular motives. It therefore seems odd to find reference in Bindra's Figure 1 to a central motive state of "eating," aside from Epstein and Teitelbaum's (1961) demonstration that rats can sensitively regulate their food-intake by pressing a bar for intragastric injections of food without either the ordinary "consumatory acts" of eating or the usual tastes, smells, and sensations associated with those acts, can bar-pressing be eating?

**Cognitions of contingencies** Act-outcome expectancies, complementary pairs of which represent contingencies between acts and outcomes, are basic to SAO theory (Irwin, 1966, 1971), and event-event expectancies, of which acts are not elements, have recently been given significant roles (Irwin, 1974), although their behavioral criteria remain to be established. In Bindra's theory, "learned correlations" among eliciting stimuli (ESs) are crucial, but correlations between acts and subsequent stimuli, although acknowledged, are much less prominent. For example, in the lower half of Bindra's Table 1, both kinds of contingency could arise, but only the stimulus-stimulus relation enters the diagram. Examples of learned correlations strikingly represent the organism as a passive spectator of events programmed by an experimenter (Bindra, 1976 *op. cit.*, Table 7.1, p. 146). It is extremely difficult to understand how the fine-tuned temporal organization of Bindra's "overlapping and nested" contingencies among events can survive the organism's concurrent behavior; obviously, whether one ES does or does not occur after another often depends altogether upon how the organism behaves in the interval. The difficulty would be compounded when the organism's own behavior interacts with that of others in social situations, prey-predator relations, and the like.

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**Signal learning and response learning.** I would like to comment on the most radical feature of Bindra's thesis: the claim that the sole basis for learned modifications of behavior is the learning of correlations between eliciting stimulus complexes (ESs). Although Bindra writes "There is little doubt that animals learn the correlations between their actions and the consequences of those actions . . ." he nevertheless believes that this learning is not a determinant of performance. It is important to understand that Bindra's proposal is not merely to reject some specific version of how response-outcome learning might work, but to reject the general idea that response-outcome learning, however conceived, has a function.

In my view Bindra's arguments for denying a role to response-outcome learning are not compelling and his attempt to substitute universal causation by elicitation is mistaken.

Bindra's argument against assigning any role to response-outcome learning begins by citing phenomena such as motor equivalence and sensory preconditioning, which cannot readily be explained by the theory that response-outcome learning is the sole basis for learned behavior. But no matter how interpreted, these phenomena do not imply the absence of a role for response-outcome learning when a reinforcer is made to depend on a specific form or property of responding.

The critical part of Bindra's argument is his contention that there is a special difficulty in conceiving how the learning of a response-outcome relation can be translated into the production of a response – a difficulty somehow avoided in the translation of learned stimulus-stimulus relations into responses. Is it especially difficult to conceive of a translation from response-outcome learning? Consider a possible account in cognitive language. The occurrences of certain response-outcome sequences (perhaps those involving surprising outcomes) are represented in memory and stored in relation to the stimulus conditions in which they occurred. When the situation recurs the representations may be retrieved. If on the occasion of the retrieval the outcome has a positive valence, a control center calls for the execution of that motor program that corresponds to the stored representation of the response. A more detailed, and more physiologically oriented, model for translating response-outcome learning into action has been described by Milner (1970, pp. 405-6). It is not my intention to defend a particular scheme for translating a representation of response-outcome relations into action but only to argue that the problem is no more baffling than the problem of translating a representation of stimulus events into action.

Another objection offered by Bindra is that response-outcome learning provides no way of accounting for the first appearance of the response. I am sure Bindra would agree, however, that we are rarely in a position to identify an external eliciting stimulus. In the absence of evidence to the contrary the most plausible assumption would appear to be that the immediate cause of a response sometimes lies in internal processes and sometimes in external stimuli. No advantage is gained by denying the possibility of internally initiated responses.

Consider now Bindra's own account of response production. As the result of the contingent presentation of ESs the first presented ES, or signaling stimulus, acquires incentive value or motivational valence. The principle of response production is that "an animal will move toward an ES that is of greater appetitive motivational valence or of lesser aversive motivational valence than other ESs in that situation at that time." From this statement it appears that Bindra considers the autoshaping experiment to be the prototype of response production. It is of course true that animals sometimes approach the signaling stimulus as though it were the appetitive object being signaled. But, as is evident from a scattered literature (e.g., Zener, 1937; Stepien and Stepien, 1965; Konorski, 1967; Kupalov, 1969), a signal will often initiate ac-

tion toward the food site in advance of food delivery. In Bindra's theory actions directed toward the food site would be explained by supposing that the signaling stimulus renders fixed features of the environment conditionally excitatory (see also Moore, 1973). Movement in any direction would be viewed as the product of a hypothetical profile of valences attaching to features of the environment. But until the principles for the transfer of valence from the CS to other features of the environment are formulated we will not have an explanation of response production in the classical conditioning, or signaling, experiment.

The most difficult problem for Bindra's theory of response production is to account for the shaping of responses, and response properties, through response-contingent reinforcement. He suggests that response-contingent reinforcement has its effect because it ensures that the animal will be observing whatever stimulus elicits the response just prior to the presentation of the reinforcing stimulus. Although in shaping the true contingency is with respect to the response, the animal behaves, according to Bindra, as though the contingency were with respect to a specific feature of the external environment. One might say that the animal is always a victim of a sensory superstition.

In order to hold this view one must believe that for every shapable response or property there is a specific eliciting stimulus or set of eliciting stimuli. This is hard to accept for shapable responses and response properties without spatial orientation. These would include responses such as repeated circling and properties such as rate, interresponse times, and the force of response.

For the sake of exploring Bindra's argument further let us accept that each time the response specified by the shaping contingency is made the animal observes an environmental feature. To fix ideas, suppose a pigeon has been shaped to move its head rapidly back and forth between two spots. According to Bindra, it is being pulled from side to side by oscillating valences of the two spots. But the spots are not in fact being turned on and off, and they are not the cause of a behavior; rather, the behavior causes the spots to be seen one after the other. "Looking at spots one after the other" is in fact a response and for that reason the shaped performance cannot be assigned to purely external causation.

There are many attractive features of Bindra's theory but I see no reason to abandon the commonsense view that animals are sufficiently flexible to learn that outcomes are dependent on responding in certain ways as well as to learn that outcomes may depend only on external signals.

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**Decision rules, decision rules.** Two general orientations are possible for evaluating Bindra's stimulating article. One is to operate within the framework of his theoretical speculations and assess them in relation to the available evidence. The other is to view his efforts within an historical-methodological perspective to understand the decision rules that are employed to evaluate the theory's explanatory power as well as to judge its future potential.

The former approach is impossible because of limitations of space, and admittedly, of my knowledge. A shift of interest from general problems of theories of animal learning (Kendler, 1959) to specific issues of human

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developmental changes in discrimination-shift and classificatory behavior (Kendler & Kendler, 1975) precludes my serving as an expert in analyzing all the empirical issues encompassed by Bindra's conception. In spite of this drawback, some benefit may accrue from uncovering certain methodological decisions and attitudes implicit in Bindra's conception.

Bindra exhibits a cavalier willingness to interpret certain complex phenomena as simple refutations of "the traditional response-reinforcement principle." For example, he offers the phenomenon of latent learning as being inconsistent with the "response-reinforcement principle." In support of his conclusions he cites Kimble's box score (1961 *op. cit.*), which lists more studies finding latent learning than those that did not ignore it in this box score, however, is the most informative of all the California-type latent learning studies (Kanner, 1954). Kanner tried twice to replicate the original California latent-learning studies (Blodgett, 1929; Tolman & Honzik, 1930) without success. In his third attempt, he was successful because he employed guillotine instead of swinging doors, and detained both the rewarded and unrewarded (latent learning) subjects in the end box for a lengthy time period. It is important to note that when transfer tests were employed to determine whether the latent learning subjects learned as much as the food-rewarded animals, the results definitely supported a negative answer. Cognitive theories of animal learning, or Bindra's perceptual-motivation model, would be hard put to explain these results.

I am neither implying that a "response-reinforcement" model can explain all latent learning data nor am I trying to resurrect the latent learning controversy. My point is that the same decision rules should be employed to judge evidence against one's own theory as against an opposing theory. I would defy any theory, including Bindra's model, to offer a detailed interpretation of latent learning studies that would meet a stringent criterion of explanation. It should be noted that the decision rule to employ a "box-score method" to resolve theoretical issues represents a pathetic commentary on the status of psychological theorizing.

Another decision rule implicit in Bindra's paper is that the "reinforcement-model" is a frozen theoretical entity. For example, he argues that the drive stimulus idea is untenable because it has been found that rats can learn two opposed responses under simultaneous hunger and thirst drives (Kendler, 1946). Although this study was designed to test Hull's (1943 *op. cit.*) drive stimulus explanation of the findings (Hull, 1933; Leeper, 1935) that rats are capable of acquiring differential reactions based solely on internal conditions, the negative results obtained do not automatically make "inadequate" the notion of a drive stimulus. The point is that whenever a theorist obtains results that are inconsistent with his hypothesis, he need not reject his general theory or any basic concept within it. He has the option to modify his theoretical assumptions so that the anomalous results can be explained. This was done (Kendler, 1946 *op. cit.*) without dispensing with the drive stimulus concept. Much subsequent data (e.g., Levine, 1953) have supported the theoretical usefulness of the drive stimulus concept. The significant methodological point is that a theory cannot be rejected on the basis of results of so-called crucial experiments because such studies are never really crucial. We must recognize that the "response-reinforcement" model, like other formulations, is more of an ongoing research program than a frozen theoretical entity (Lakatos, 1970). Evaluations of research programs, which in the strict sense can never be final, depend upon whether the program is progressive in the sense of enlarging its empirical realm especially by predicting novel results and/or by becoming more precise, or whether the program is degenerating in the sense of continually adding ad hoc assumptions to account for a mounting number of anomalous results.

Bindra's conception of the core assumptions of the "response-reinforcement" model differs from mine (Kendler & Spence, 1971). He maintains such a formulation is rigidly committed to the acquisitions of specific responses and thus is incapable of dealing with facts of behavioral flexibility. A historical view suggests otherwise; the early emphasis on stimulus-response associations was in part due to the methodological concern of meeting the standards of natural science objectivity: S-R language forces its user to think in terms of manipulable experimental variables and observable responses. But the neobehavioristic research program is not necessarily committed to a rigid response reproduction view; theoretical mechanisms can be, and have been, developed to explain behavioral variability. The significant strategic issue is at what point in the "theoretical bridge" between the independent and dependent variable should the nature of the response be specified. A single-unit S-R model specifies the nature of the response relatively early

along the bridge, while cognitive theories aspire to do this rather late. The danger of the former position is that the model cannot cope with facts of behavioral flexibility, while the risk of the latter orientation is that the behavioral estimate may not be generated, leaving the subject "buried in thought." Bindra believes he has solved this problem by proposing a model that in essence makes the common-sense distinction between *thought* and *action*. This may prove to be a giant step forward, but we will know this only when his "perceptual-motivational model" matures beyond its present stage of development which Bindra describes "...as a fresh starting point and avenue of approach rather than as a formal theory." I would be more optimistic about its ultimate success if it were not so strongly committed to one kind of learning. It may be that the response-reinforcement model is an appropriate representation of some kinds of learning (Kendler & Kendler, 1975).

Perhaps the greatest obstacles to the potential success of Bindra's model are the possible explanatory limitations of behavioral theories. Behavioral theories have been notably unsuccessful in resolving their fundamental controversies, for example, one-trial versus incremental learning, serial versus parallel processing, the necessity of postulating different memory stores, latent learning, and so forth. Perhaps behavior is too much of an open system to allow definitive tests of the basic assumptions of its theories. Perhaps this is why *all* general behavioral theories have ultimately degenerated. One hope that Bindra would probably share is that the truth about these fundamental behavioral assumptions can be ascertained when they are *rigorously* translated into testable neurophysiological assumptions. There is, however, no guarantee that such translations will be possible. We shall see!

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*Pexgo: a plausible construct in need of data.* Here is a brave new approach to the problem of how behavior is evoked, coordinated, and strengthened. The central question is whether the pexgo, and its fellow-travelers the act-assembly and eliciting stimulus, carry their weight in explanatory power, or whether they just add to the load of intervening constructs that currently burden experimental psychology.

An explanation is achieved, some say, when the student stops asking questions – when the bedrock of prior assumptions is finally tapped. The "generic nature of the concepts 'stimulus' and 'response'" explained the "stimulus and response equivocation paradoxes" for many of us. But if the operant was defined by the three-term contingency, how could we get by so long by operationally defining one of the terms as a switch closure, ignoring

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the other terms and their interaction with it? "Operant" was a promissory note, requiring that we fill in the details of response induction, elicitation, and constraint by the eliciting stimulus context. But it seemed easier to study schedule effects, letting the lever or key winnow the wheat of operant responses from the chaff of interim behavior and terminal behaviors of deviant topography. This mechanized sorting technology has saved us much labor and much thought. But it left hidden in the box much that was important, behaviors that can no longer be ignored.

Bindra's constructs are not necessary for the analysis of most of the operant conditioning and animal behavior studies that have appeared in our literature; it would be a mistake to infer that they are therefore not necessary for the study of animal behavior. Because inductive empiricism has lagged, speculative theorizing must lead, and unfortunately must do it with little in the way of a data base to guide it. Tolman's "vicarious trial and error" responses were never adequately investigated, nor was his "searching for the stimulus," nor was Skinner/Estes' stimulus sampling theory. The important role of feedback stimuli in the free food phenomenon, the powerful and asymmetric observing response effect, the ubiquity of sign-tracking – these phenomena underscore the creative role that stimulus effects can have in the control of behavior. Discriminative stimuli do not just turn operants on and off.

Skinner's treatment of seeing as behavior, subject to manipulation by deprivation, discriminative and reinforcing operations is no less radical than Bindra's discussion of the pexgo. But what did "seeing" do for Skinner? It was often a "precurrent" behavior, effective because it brought the organism into contact with stimuli that released desired behaviors. Skinner readily moves responses inside the skin of the organism; why does he insist on keeping stimuli outside? In urging that "seeing does not imply something seen" he denigrates the utility of "sensations, images, and theory congeries." But once inside an organism, how do we tell the difference between emancipated seeing and a sight? He quotes "a recent book" that contained the sentence "A system of emancipated ideas temporarily seizes control of the brain" and says "The facts are as well described by saying a system of responses is temporarily pre-potent. In either case we have still to ask 'Why?'" (Skinner, 1953, *op cit*.)

Although Bindra's "ideas" are tied closely to the environment and to one another and although they do not have to seize control, for they have always had it, we still have to ask "Why?" Why is his account to be preferred? As Bindra notes, the proposed concepts "can serve as the elements of several different types of new framework." His own construction, then, must be seen as an arrangement of the explanatory elements that is only incompletely constrained by the scant data. Indeed, the constructs themselves – perception, knowledge, and motivation – are still so vague that they are more sentiments than elements.

Why, for instance, can acts take no longer than half a second, necessitating an "act-assembly" to keep coordinated movements straight? Although we are dealing with a conceptual nervous system, we can still be parsimonious in detail. Let our acts be of various lengths, synthesized out of smaller acts. This retains all the flexibility of the act-assembler. Perhaps too much, for not all "old sequences can be interrupted and changed within a fraction of a second", try picking up a tune in the middle.

Why, in Figure 1C, does not the central representation of food, generated by the light, elicit the approaching and eating behaviors, as it did in 1B? Why is control of responding transferred to the pexgo of light, instead of the light's becoming a more certain elicitor of the food pexgo, which would continue to control behavior? The way it stands, one wonders why a light pexgo should elicit gnawing, and so forth – unless it is a foodlike light pexgo, an unnecessary redundancy. I suspect the problem is one of graphics rather than of theory. One struggles for ways to represent the blending of stimulus control and the veering directions it imparts to behaviors, with two dimensional monochromatic media being little better than linear discourse.

Whereas I wholeheartedly endorse Bindra's stand on incentive-motivation, spatio-temporal valence gradients, and the interplay between CNSs and environmental control, I remain neutral on his Perceptual-Motivational Alternative. The positive valence of its novelty is balanced by the attraction of Skinner's response orientation – I feel more in control when I think of seeing as behavior than when I think of pexgos vying every few centiseconds for the control of my next move. Neither alternative is forced by the data. But Bindra had more data at his fingers when he erected his framework, leaving it more commodious, and no one has yet started pouring concrete

Let's see now, if I take the arrows at the bottom of Figure 1, and bend them around, so that

by M. J. Klingsporn

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**On explanation, familiarity, and response sequencing.** In his paper Bindra continues the development of his position that incentive-anticipation is the prototype of all learning. Since the paper is a bit brief I found it necessary to include under my considerations elaborations given by Bindra in his 1968 and 1974 *Psychological Review* articles as well as in his book *A Theory of Intelligent Behavior*.

The theory has shown considerable progress in its development over the years. Basically, I find myself strongly in support of those aspects of the theory developed earlier, but not quite so comfortable with the more recent development of his "pexgo" concept. Let me first sketch what I perceive to be the major solid features of the theory, adding an occasional "Amen," and then I shall raise some questions regarding some of what I suspect are more minor features that may be still in a state of development flux.

First some general comments. The theory is atomistic in that it assumes that both gnostic assemblies (percepts) and act assemblies (directed motor movement) can be decomposed into fundamental and irreducible components consisting of gnostic neurons and motor neurons. Furthermore it is reductionistic, both the atoms and the larger functional organizations being putative physiological entities. In these respects the theory is similar to Hebb's (1949). Previous attempts to articulate physiological concepts with psychological concepts rich enough to explain complex behavior have been premature. Perhaps the time is ripe now, perhaps it never will be. At any rate this commentator is not qualified to judge this aspect of the theory. The theory attempts and succeeds, up to a point, to be a truly explanatory theory. Descriptions are not meaningful unless they are tied together by explanatory constructs [see also Brainerd, this volume, next issue, forthcoming]. Bindra is to be applauded here. Finally, the theory is "monistic" in that learning is assumed to be a single process, all learning involves the construction of gnostic assemblies, and these assemblies function both in knowing (the 'g' in pexgo stands for gnostic) and in behaving. No schemata intervene to store memories or to account for recognition via some matching process.

Of course Bindra makes allowances for the recognition process. Sensory features can presumably be activated apart from the development of gnostic assemblies. This event would result in one's failing to identify what one has encountered. When the stimulus activates a pexgo, then one recognizes the stimulus object. Thus the gnostic assembly nicely handles recognition. However, it is not clear that it will allow for distinctions of familiarity in the recognition process. Not only do I recognize a particular object as a plate, but I am aware that it is the same plate I saw five minutes ago, which was given to me by a friend two weeks ago. While Bindra makes some provisions for pexgos differing with respect to intensity of arousal, it is not clear that this single dimension makes sufficient provisions for the complex distinctions one can make with respect to familiarity.

Similar questions can be raised regarding motor involvement. I know how to throw a ball; I can claim that I intend to throw the ball five minutes from now; I can be "set" to throw it upon command, or I can, in an emergency, throw the ball without conscious preparation. It is not clear that the motor readiness associated with a pexgo is such as to allow for all these distinctions. I realize that Bindra has been primarily concerned with accounting for sub-human animal data, which attenuates the above criticisms somewhat. Nevertheless, the theory, to be complete, must ultimately come to grips with these issues.

I am a bit uncomfortable with Bindra's hypothesis that integrated sequences of responses are neither chained together nor organized in hierarchies as the TOTES of Miller, Galanter, and Pribram (see Pribram, this *Commentary*), but seem to be multiply linked conglomerates. An example helps clarify this issue. Suppose a rat is conditioned to run a maze with choice points, A, B, and C in that order. The central motive state of hunger together with the sight of A arouses the components of the pexgo pA, which makes ready response A which, as response A unfolds, introduces stimuli from B, which makes ready response B, etc., pA is also correlated with pC as well as pB, and all of them with the anticipated food, so that if leg B of the maze were removed, the organism would be able to complete the final leg without somehow simulating the running of B as the chaining hypothesis would seem

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to require So far, so good! Now suppose we sequence responses in the same location For example, suppose levers A, B, and C must be pressed *in order* in an operant box If short circuiting is possible, why wouldn't it occur in this situation to abort the normal sequence of responses leading to response C immediately? The levers are close enough so that to see one alone is nearly impossible If an initial intensity differential leads to response A, the feedback from response A could augment pB etc, but then how could one ever master a sequence wherein the "same" response occurs more than once? For example, the sequence A-B-A-C requires response A to augment pB on one occasion and pC on the other Similar difficulties obtain in the temporal double-alternation response as well as the detour problem

The rejection of the notion of a reinforcement event strengthening the association of stimuli to responses has been brilliantly defended by Bindra on previous occasions The difficulty one has in accounting for motor equivalence with such a theory leads this commentator to prefer an S-S theory Of course, one could define a "response class" as all responses that affect the environment the same way, as the operant folk are prone to do However, if one opts for an explanatory theory, rather than a description of control techniques, one must inquire as to the nature of such functional equivalence Functionally equivalent responses require concomitant representations of the corresponding stimulus configuration such as schemata, or Bindra's pexgo, to provide a basis for such functionally equivalent response classes

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by Frank A. Logan

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**Intelligent vs. stupid behavior, or learning concepts vs. responses.** Because Bindra's article ranges from very broad conceptual issues to rather specific analytic details, and many of the latter draw importantly on his other recent publications, I shall confine my comments to the general orientation of his approach as reflected in this article Bindra has quite clearly characterized the several related approaches to the fundamental issue of "What is learned?" In this regard, he aligns himself with those who focus on the purposive variability and adaptive flexibility of behavior For example, Tolman believed that behavior can never be understood in terms of specific responses; the behavioral component of a means-end-readiness or a sign-gestalt-expectation is a performance that may encompass a wide diversity of specific responses adapted to the prevailing circumstances Tolman was joined in this position by Lashley, whose rats successfully traversed mazes in spite of extensive cortical damage that severely impaired motor performance as initially practiced To such theorists, the obvious implication is that organisms know a great deal more than at least appears to be implied by traditional S-R conceptions

Historically, theorists of this persuasion have played the role of critics of S-R theory, with paltry little in the way of explicit theory susceptible to attack Although much of Bindra's case rests on the presumed inadequacy of the response-reinforcement approach, he has proposed at least the rudiments of an alternative formulation that may go beyond merely identifying problem areas However, in this article at least, Bindra does more by way of telling us how he thinks about topics ranging from instrumental acts to observational learning than he does in providing a specific theory amenable to experimental analysis

For my part, I have been driven in the diametrically opposite direction My view can be traced to Guthrie, who contended that his cats did not learn the experimenter's concept of the act of escaping from a puzzle box, but rather learned the very particular movements that happened to eventuate in escape I have extended this thesis (in the reference cited by Bindra) to include the quantitative as well as the topographical features of a response My own thesis is much stronger than that organisms can learn particular speeds and amplitudes when the conditions of reinforcement require finely tuned performance; they inevitably and automatically do learn the micromolecular features of behavior the very way it is practiced

It would be easy but fruitless to argue the case on the basis of examples I am confident that for every instance of intelligent behavior in which, for example, a chimp assembled two sticks to make a longer tool, there is a contrasting instance of stupid behavior in which, for example, a chicken

pranced helplessly behind the barrier of an umweg problem And I side with Hull in contending that an adequate theory must equally account for maladaptive behavior as for adaptive behavior

My preference is to begin with the fine-grained details of responses and to build these up to intelligent behavior through response generalization and acquired equivalence Bindra's preference is to begin with perceptual organization and attribute stupid behavior to restricted environmental stimulation and limiting conditions of motivation My belief is that perceptual organization is a necessary feature of an adequate general theory of behavior, but it remains to be seen whether Bindra, or others of similar orientation, can sustain the thesis that perception alone is sufficient My hunch is that Bindra's further elaboration of the notion of act-assemblies will soften the edges between our positions

by N. J. Mackintosh

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**Limits on reinterpreting instrumental conditioning in terms of classical conditioning.** There is much to admire, and much with which I agree, in Bindra's paper We should, I think, accept, far more than has been generally accepted, that the changes in behavior an experimenter observes in a conditioning experiment are a consequence of his subject's learning relationships between stimuli and reinforcers Responses once thought to be paradigm cases of operants, strengthened or weakened by their consequences in accordance with the law of effect, now appear to depend largely on stimulus-reinforcer contingencies Pigeons peck illuminated response keys, not so much because the response of pecking is strengthened by the subsequent delivery of food, but rather because the sight of the key is associated with food Wherever a supposedly instrumental response moves the subject in space, as when a rat runs down an alley into a goal-box, or jumps from one compartment to another of a shuttle-box, the instrumental contingencies of reinforcement (food is contingent on running, or escape from shock on jumping) necessarily produce correlations between external stimuli and changes in reinforcement It is probable that these latter correlations are the more important: the rat learns that the goal-box is associated with food, or one compartment of the shuttle-box with shock, and appropriate approach and avoidance behavior is established in exactly the sort of way Bindra has proposed

So far, so good Bindra is, in effect, arguing that classical conditioning is more important than has generally been recognised and that a process of classical conditioning will explain many instances of supposedly instrumental conditioning The question remains whether a complete takeover bid is feasible Not all instrumental responses, as Bindra notes, move the subject in space It is true that a rat must be in a particular location in order to press a lever protruding from one wall of an experimental chamber, but the fact that he is in that location does not guarantee that he will press it We can grant that the contingencies of an instrumental experiment will ensure that only a highly restricted set of stimuli (pressure on and contact with the lever in addition to a close view of it) will be correlated with food; it remains a matter of some speculation whether these "finer perceptual differentiations" are sufficient to explain the skilled performance of the well-trained rat Moreover many instrumental responses need not be defined in relation to external stimuli at all A dog may be trained to flex his foreleg to obtain food or avoid shock, when these responses have no other effect on the external environment Experimental intervention may remove all, or most, external stimuli that normally accompany the performance of an instrumental response, as when pigeons are required to peck a key in a totally darkened chamber, or monkeys to press a lever that they can neither see nor feel In all of these cases, instrumental responding is maintained or can be established *de novo*, but it is difficult to see how such instrumental learning can be reduced to "stimulus discrimination learning" in the ordinary sense of that term It seems more plausible to suppose that animals have learned in this sort of case (as in perhaps others) a correlation between their responding and its consequences I agree with Bindra that the ability to learn such a relationship does not in and of itself explain why the animal's behavior changes I equally agree that William James' (1890 *op. cit.*) ideomotor theory is not the most plausible explanation of voluntary action: thinking of a response is not the same as executing it What we need, however, is a fully developed theory of instrumental performance I am not convinced that we can get rid of the problem by assimilating all instrumental learning to classical conditioning

## Commentary/ Bindra: A perceptual-motivational view of adaptive behavior

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**Reinforcement is dead; long live reinforcement theories.** Because I think of myself as a reinforcement theorist, I read with great interest articles claiming that the concept of reinforcement should be put to rest. In making such a suggestion, Bindra links the term "reinforcement" with a series of highly questionable positions. These include: 1) the idea that very specific stimuli are connected to very specific responses through the process of reinforcement – a notion that cannot account for the "adaptiveness" or "flexibility" of motor behavior, 2) the belief that all behaviors can be readily modified by operant conditioning procedures, a belief that is at odds with many findings concerning "instinctive" or "species-specific" behavior patterns (e.g., Bolles, 1970 and this Commentary; Tinbergen, 1951), and 3) the position that reinforcement is a necessary ingredient for all learning, a position that cannot account for phenomena such as latent learning, sensory preconditioning, and observational learning. Whereas some reinforcement theorists may have held these views in the past, I contend that they are not prevalent today. Before discarding the concept of reinforcement, we should at least examine its current meaning.

Unfortunately, use of the term has been anything but consistent. Textbooks usually define reinforcement as the presentation of some stimulus (a reinforcer) following a response, which increases the strength of that response. This definition is ambiguous, for reinforcement might refer to either the procedure of following responses with certain stimuli or to the effect, the increase in the strength or probability of a response. Both usages appear in the literature. Furthermore, psychologists have suggested that reinforcement can occur in the absence of any external stimulus, because a behavior may produce interoceptive stimuli that are themselves rewarding (Skinner, 1971). To make matters worse, the term is not invoked just when there is a change in behavior. It is also applied to behaviors in a "steady state," where an organism has become fully acquainted with the procedure and its behavior has stabilized (Herrnstein, 1970). Thus "reinforcement" may refer to a procedure for presenting stimuli, or it may involve neither a procedure nor an observable stimulus. The term is used when a behavior increases, or when it is maintained at some stable level. Because the term can mean so many things, it is not surprising that misunderstandings frequently arise between its advocates and its critics.

This does not mean that reinforcement theorists can make no important contributions to the study of behavior. On the contrary, their subject matter is central to a science of behavior – the factors that determine an organism's choice of behaviors and the pattern of these behaviors through time. In discussing this problem of "response-production," Bindra suggests that "the form and frequency of an instrumental action are determined, not so much by the incentive value of a particular stimulus in the situation, but by the relative valence of the various situational stimuli and by the pattern of their distribution in space and time." Bindra's position is similar to my own view that different behaviors (or more precisely, the stimuli resulting from different behaviors) have different subjective values for the organism, and a behavior's relative duration will depend on its current relative value to the organism (Mazur, 1975, 1977). Other reinforcement theorists have also suggested that the probability, duration, or strength of a behavior is determined by its value to the organism (Baum & Rachlin, 1969; Cohen, 1975; Killeen, 1972 and also see this Commentary; Miller, 1976; Premack, 1965, 1971; Rachlin, 1971). Thus at least among some researchers, the search for the determinants of behavior has proceeded from externally-presented stimuli to the subjective value of these stimuli for the organism. This change is one that Bindra would probably approve of, yet the term "reinforcement" has remained and it is often used as almost a synonym for "subjective value."

If this hypothetical construct of "subjective value" is to have any predictive worth, it must retain as close a connection as possible to observable events. Bindra makes no concrete predictions about the three-way relationship between E, pE, and R, but this relationship must often be straightforward. Many studies have found simple relationships between such factors as the rate, duration, or delay of reinforcer presentations and the rate, duration, or intensity of instrumental behaviors (de Villiers, 1977). Thus, in many cases, the experimenter need not explicitly consider the "subjective values" of the alternative behaviors because measurable aspects of the external stimuli (reinforcers) are sufficient to predict a subject's behavior quite accurately. This experimental strategy by no means implies that the researcher believes

that no representation of the stimulus exists in the organism's nervous system. Some such representation must exist for every stimulus influencing the creature's behavior, and in some cases it may be a very distorted reflection of the external stimulus.

I suggest that although many modern reinforcement theorists study the relationships between stimuli (reinforcers) and responses, their implicit assumption is that Es do not control behavior directly, but only via pEs, the organism's subjective representations of stimuli and their values. The orderliness of the data (de Villiers & Herrnstein, 1976; Navarick & Fantino, 1976 and see also Navarick, this Commentary) suggests that their research strategies are not foolhardy, but their use of the term "reinforcement" subjects their writings to ambiguities and misconceptions. Perhaps Bindra is correct when he says that the term should be abandoned; if not, researchers may never be able to straighten out all the confusion. Researchers might use the terms "hedonic stimulus" or "contingent stimulus" when referring to an external stimulus and "subjective value" or "valence" when discussing the organism's internal representation of the importance of that stimulus.

The terminology of behavior analysis is of secondary importance, for the research of reinforcement theorists will continue in any case. The orderly relationships between reinforcers and responses cannot be ignored. The significance of this research will be a matter for future assessment. I, for one, am very optimistic about its importance for the understanding of behavior.

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**Reinforcement theory: an experimenter-oriented rather than a subject-oriented philosophy.** I have always found myself in basic agreement with Professor Bindra's writings, and the present article is no exception. However, the more experimental data I see cited in favor of perceptual-motivational alternatives to response-reinforcement, the more I wonder whether they are necessary here. Quite possibly logical or philosophical considerations alone would suffice to settle this issue.

The key assumption behind response-reinforcement (and behavioristic theories in general) is that events are best described from the experimenter's

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rather than the subject's point of view. If one accepts this assumption without question, it does seem plausible that not only "reinforcers" (objects) but also "reinforcement" and other psychological processes can ultimately be defined in terms of the operations and procedures the experimenter uses; that the subject's behavior can similarly be defined; that the effects of an object or event upon behavior often seem "automatic"; that accounting for the variance of a subject's behavior is equivalent to explaining it; and that to ask what correspondence, if any, exists between the experimenter's point of view and the subject's point of view is unnecessary.

If I were studying the behavior of rocks I might accept these assumptions; but it seems to me that for most psychologists (including at least some behaviorists) even a complete and highly parsimonious description from the point of view of an "objective" experimenter remains totally trivial unless it can somehow be transformed into statements that make sense from alternative points of view, most particularly that of one's subject. In other words, we ask different questions of living beings than we would ask about rocks; and if anyone doubts this, I ask him to look at the titles of any journal in psychology or animal behavior he cares to examine. If this makes psychology dualistic, so be it.

Let me give a concrete example of what I mean. Imagine that, like René Descartes when he first got the idea for his analytical geometry, I found that I could completely and parsimoniously describe the movements of a fly on a ceiling by noting, at any given time, its distance (and the distance of its various body parts) from the walls of the room. Would this constitute an adequate psychological or behavioral description? For a Cartesian geometer or Newtonian physicist who was content to ask no questions about the fly that he would not ask about any other object or "point in space," it obviously might. But even here some serious problems remain. My description thus far is not an "unbiased" view of reality; it is, rather, a statement about what the world might look like if the front-left corner of the room were one's zero point, and the walls of the room were one's frame of reference, and space were empty, homogeneous, and isotropic, and distance relationships were transitive, and all our measurements were made with absolute precision. But neither I nor any other living being (only some fictitious and idealized "standard observer" into whose shoes I have been trying to place myself) really sees the world in this fashion. In brief, the very axioms of Cartesian geometry and classical mathematics are *psychophysical assumptions* which are known from the outset to be arbitrary, freely chosen and freely modifiable if this will better suit our purposes as students of nature. The principal justification for such descriptions in psychology is not that they are "true" but that they are convenient, reliable and extremely simple – at least in principle – and they are widely accepted by other experimenters as a standard of comparison against which we can contrast other, more interesting descriptions.

By what criteria, then, can we decide on the best description? My own answer to this question (at least in the present context) is that we do not know until we have first of all discovered what "axioms" and what metrics are appropriate from the point of view of one's principal subject – which in the present case I shall take to be the fly rather than myself. And this inevitably gives rise to psychophysical and perceptual questions about the fly, including (for example): What does the fly's zero point seem to be? What is its frame of reference (if not the walls of the room)? To what degree and in what senses is its space non-empty, nonhomogeneous and anisotropic? To what degree are distance relationships transitive for it? Does the fly's system remained fixed and constant over time; and if not, what rules govern the changes that do occur? Until such questions have been answered, any talk about having measured the fly's behavior or the features of its world should be construed in a nonpsychological and provisional sense.

To carry the argument one step further: Functional classifications of objects (for example, terms such as reinforcer, food, fear object, sex object) are no more definable from the point of view of the experimenter alone than are the foregoing spatial concepts, for they have no meaning without reference to a live and perceiving subject, and there is probably no object whose effects are invariant across time, across contexts, across species, and across individual subjects. To accurately predict that a particular object will function as a reinforcer, the experimenter must obviously first of all know something about his subject's abilities and predilections, and only after this can he rely upon his knowledge of what is in the "object as such." Thus, for example, before I would assume that a banana actually will function as a reinforcer, I would assure that the subject is a monkey (say) rather than a cat, that he knows or is capable of discovering that this object is out there in the environ-

ment, that he will detect its similarity to other objects he has encountered on previous occasions and thus recognize its potential value for himself, and so on. Furthermore, before I can be said to have explained how this object functions as a reinforcer, it seems to me that I am obliged to explain how the subject can know and do these things, and not simply take his knowledge as well as my own for granted. In short, the same psychophysical considerations apply to all objects and events in general, and in this sense the distinction between "pure psychophysics" and the "psychophysics of reinforcement" collapses, at least if one is trying to discover the point of view of one's subject. How does the subject (as well as the experimenter) distinguish between "pure, neutral psychophysical stimuli" and "impure" ones, or classify objects, if not by perception? Here is where response-reinforcement theory tends to become either silent or circular; and this is why I see no real alternative to a subject-oriented perceptual-motivational theory of some sort. Professor Bindra's theory seems a bit more experimenter-oriented than it needs to be; but it is certainly a step in the right direction.

A subject-oriented approach does not, incidentally, imply that one really can get out of one's own skin and literally describe the world in precisely the way it would look from the point of view of a monkey or a fly. It implies only that insofar as we do utilize subjective information and ask subject-oriented questions, we should not pretend to do otherwise. For that matter, however, can we as objectivists really place ourselves in the shoes of a "standard observer" whose perceptual and motivational organization is also quite non-human? In either case we are obviously indulging in metaphors; and I fail to see how the former sort of metaphor is any more harmful than the latter.

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*Learning theory, a will-o-the-wisp?* It is extremely difficult – I am tempted to say impossible – to prepare, in brief or even extended scope, a critique of Bindra's paper that is incisive and all-inclusive. His paper, I feel, is paradoxical because the realities with which it purports to deal are paradoxical.

Some twenty years ago, when I was preparing a two-volume treatise on the psychology of learning that I then believed represented a substantial, if not epochal advance in learning theory, I recall the following comment by a colleague: "There has been a long succession of learning experiments which, at least in the eyes of the experimenters, provided definitive support for some specific conception of the learning process, but which would be discredited by later research quite as effectively as earlier theories had been." Of the appearance and decline of differing conceptions of the learning process there seems to be no end; and despite Bindra's evident conviction that he now, at long last, has at least a "tentative" hold on the real thing, it seems unlikely that his postulations will have exceptional stability or durability.

At the outset of Bindra's paper one is reminded of E. C. Tolman's book, *Purposive Behavior in Rats and Men* (1932 *op. cit.*) in which the latter writer, even then, was likewise in revolt against the "traditional response-reinforcement principle." Tolman held that, in the course of learning, living organisms do not require stereotyped responses but, instead, "cognitive maps," "that this leads to that," and that performance is thereafter determined by these cognitions, and what Bindra calls "incentive-motivation," that is, what it is that an organism wants at a particular point in time and space. But although Tolman's work was an effective blow to Watsonian Behaviorism, it did not prevent the emergence, in B. F. Skinner's book, *The Behavior of Organisms* (1938, *op. cit.*) of a form of Behaviorism that was destined to be even more far-reaching and influential than that of J. B. Watson ever was.

Common sense and common experience favor the view that learning involves specific cognitions rather than specific behaviors. For example, I recently needed to do a number of errands, and in preparation I made a list of the things I wanted to do (so I would not forget any of them), and then I mapped out or planned, "in my head," the most efficient sequence or route for doing these things I had never before performed this particular sequence of acts, and probably never shall again, hence giving the lie to the view that what we learn is a particular sequence or course of actions. But everyone knows, and from time immemorial has known, how to make creative and novel plans of this kind, so where do we go from here?

Such knowledge and behavior involve a mystery: How does physical energy emitted by stimuli in one's environment or generated in one's own body get translated into conscious processes? It would appear that Bindra

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thinks he has solved the "mind-body" problem or the paradox of psycho-physical interaction; at least he does not refer to it as an obstacle. He uses both physicalistic and mentalistic terminology – as we all do in common speech; but there is a staggering perplexity here, when one stops to think of it, and Bindra does not even discuss it. So far as I can see, Bindra has not dispelled this ancient source of perplexity, and I personally doubt that anyone ever shall [see Roland et al., this issue; Haugeland, next issue].

Behaviorism is an attempt to develop an admittedly simplistic (if one is honest about the matter) conception of behavior change that is based entirely upon merely observing correlations between events (behavior and reinforcement) in the physical world, and Bindra admits that this "framework" has certain "strengths," recognized by Skinner and innumerable others for thousands of years before him; and Bindra does not, I believe, demonstrate that his framework is stronger or more useful, although certainly conceptually more intricate. And, of course, the behaviorists are left with the embarrassment of explaining what they mean by "observing."

The Bindra paper is hard reading. One problem is that certain "technical" terms are not defined or only weakly so; and phrases such as "modulating the strength of pexgoes of eliciting stimuli" and "overlapping and nested correlations between stimulus events" are exceedingly hard to grasp. In such instances, it is often difficult to discriminate between nonsense and profundity. I confess that this was a difficulty that I frequently experienced in reading, and rereading, this paper.

It is certainly true that recently there has been a widespread upswing in the use of cognitive and perceptual language on the part of learning theorists, thus representing a strident deviation from the language of Skinnerian Behaviorism; but, as we have seen, Tolman and associates staged a similar revolt against Watsonian Behaviorism nearly half a century ago, and that had immediate but not lasting effects. Will the present renaissance of cognitive psychology (with which I have a certain sympathy) be any more enduring? The trouble is, as I have earlier suggested, that in psychology we seem to be vexed with certain paradoxes that, because of an apparent and perhaps understandable inability of the mind (or organism) to fully understand itself, are doggedly persistent.

**by Edward J. Murray**

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**Implications for clinical behavior modification.** One of the most important developments of the last few decades in the area of mental health has been the emergence of clinical behavior modification. Based on the idea that behavioral disturbances are learned rather than the product of a medical disease, the behavior modification movement promised a whole new technology based on principles developed in scientific psychological laboratories. Yet, in spite of some exciting practical applications, the behavior modification movement today is in a state of ideological crisis and theoretical confusion (e.g., Lazarus, 1977).

The ideological crisis centers around the realization that the principles of learning on which most clinical behavior modification procedures were based are simply not adequate to explain the phenomena encountered by clinicians attempting to solve practical problems. To a large extent, the principles of learning that have failed the practicing behavior modifier are those included by Bindra in the response-reinforcement framework that has dominated the behavioral sciences for so long a time. The theoretical issues raised by Bindra about direct response strengthening, behavioral flexibility, perceptual learning, and cognitive processes are paralleled by practical difficulties in clinical behavior modification (e.g., Murray and Jacobson, 1978).

A great many behavior modification programs based on the response-reinforcement theory have been established for disturbed children in family and school settings, for institutionalized juvenile delinquents, for hospitalized psychiatric patients, and other specialized populations. Although these programs start out successfully, they frequently begin to run into serious difficulties and limitations. Too many events are difficult to explain in terms of direct reinforcement. Successfully reinforced adaptive behavior often fails to generalize from the home to the school. In fact, children sometimes get better in one situation and worse in another. When adequate control groups are utilized, changes attributed to reinforcement frequently turn out to be due to normal developmental processes, the attention shown to the child, or more complex changes in the social systems involved.

As clinicians depart from the conceptual constraints of the response-reinforcement position, the behavior modification procedures may become more effective. For example, many programs start out modifying behavior with very concrete reinforcers such as candy, tokens, and even money. Since this method of behavioral control departs from social reality, concrete reinforcers are "faded out" in favor of the natural contingencies in the environment that involve social approval, reciprocal support, self-reinforcement standards, and so on. Of course, these natural contingencies were there all along so what the concrete reinforcement procedure seems to have done is focus attention on the way the social environment works.

Going even further, it seems that many reinforcement programs in clinical settings operate as somewhat awkward means of teaching patients about social contingencies, about the nested correlations between informational and hedonic stimuli and between responses and stimuli. Except for special groups such as the severely retarded, this information can be taught in more effective ways dependent on perception rather than response shaping. Thus, most progressive behavior modification programs now emphasize observation of models, vicarious conditioning, verbal instructions, dramatic demonstrations, symbolic rehearsal, and group discussion. In other words, the focus of behavior modification has shifted to the transmission of information.

It is interesting to note that the importance of observational learning in clinical situations was resisted for a considerable period of time because of the adherence of early behavior modifiers to the response-reinforcement position. Attempts were made to derive such observational learning from response-reinforcement principles reminiscent of the latent learning controversy. It took a great deal to break through this conceptual logjam and open up the newer technology. The perceptual emphasis of Bindra's approach is definitely consistent with these developments in clinical behavior modification.

The problems of flexibility in behavior that Bindra addresses have also been problems in clinical behavior modification. Again, the practical difficulty seems related to the response-reinforcement concept. Early attempts at behavior modification emphasized the conditioning or extinction of specific responses. Such treatment tended to have limited success. One example is systematic desensitization for phobic stimuli. Originally the idea was to replace the fear response with something like a relaxation response to the specific fearful stimulus. This procedure turned out to be of limited clinical usefulness. A more effective procedure is to teach relaxation as a coping response to use in a variety of fearful situations. Even more powerful is teaching the patient a number of coping responses including assertiveness, self-statements, fantasies, and strategies for application to many situations, even those not foreseen by the clinician. The change is from dealing with specific responses to teaching general principles.

The motivational aspect of Bindra's theory also bears on very recent developments in clinical behavior modification. One of the earlier theoretical embarrassments of behavior therapy was that eliminating one small problem, such as a phobia, often had a positive effect on many other problems in the person's life. For example, teaching a somewhat timid boy to swim seemed to enhance his self-esteem beyond ordinary expectation. So, too, a juvenile delinquent earning tokens for work with an automated learning machine sometimes changes his entire attitude. The process is similar to the expansive mood after receiving a pleasant letter described by Bindra. The effect of such hedonic events is not simply reinforcing but emotional! The individual does not just increase the frequency of the most contiguous response but changes the perception of the entire environment. Perhaps even more important the person changes his perception of himself. The emphasis today in clinical modification is also on the person's perception of his efficacy and competence.

In conclusion, Bindra's theory of perception-motivation is a welcome alternative to the response-reinforcement theory with respect to its implications for clinical behavior modification. Rather than abandoning the learning approach for a sheer empiricism, turning to a better conception of learning such as Bindra's perception-motivation theory, may provide a meaningful foundation for behavior modification procedures.

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**A framework with no foundation: comments on Bindra's perceptual-motivation theory of response production** Throughout Southern California, housing units in various stages of construction dot the landscape. The stages of construction eventually become familiar to any casual observer. After the lot is cleared, a concrete slab is poured for the foundation. Then, a wooden frame is bolted to the foundation and the skeleton of the house takes shape. In later stages of construction, wooden sheets are added, followed by roofing and siding materials. Although the units differ in size and style, they all incorporate a skeletal frame that has been firmly anchored to a concrete foundation. This is apparently a fundamental principle of housing construction, and one would hope that it would some day become a principle of theory construction in the behavioral sciences. Professor Bindra proposes a framework for the study of adaptive behavior that, lacking a foundation, is not likely to bear the weight of experimental evidence.

The theory is built on the concept of "motor equivalence"; a given response, "defined in terms of a certain outcome," may be effected at different times by different muscles. The concept of motor equivalence, or behavioral "flexibility," is said to be inconsistent with the behavioristic view that organisms learn specific stimulus-response associations. Whatever the validity of that inference, the concept of motor equivalence is not a proper foundation for theory building, as it is an assumption, not a concrete fact. Recent developments in operant psychology underscore the tentative nature of the concept. It is well known that Skinner's concept of the response is a generic one. Occasionally, a rat presses a bar with the left paw and occasionally it presses the bar with the right paw, but the effects of these different movements upon the environment are the same; a switch closes. The justification for ignoring differences in movements and defining the response as a switch closure is the orderliness of functional relations obtained between independent variables and behavioral units so defined. But "orderliness" is very much in the eye of the beholder; a function on a cumulative record may appear orderly and smooth at a distance of five feet from the eye, but variable and choppy at a distance of five inches. Although a molar definition of the response has proven useful in studies on a wide variety of subjects, molecular perspectives on response definition are receiving increasing attention as investigators look more closely at temporal and sequential patterns of responding. For example, investigations of choice behavior in pigeons suggest that in some circumstances, a sequence of pecks on the left and right keys can function as a unit (Shimp, 1976). Because the criteria for defining behavioral units are still under development, any view regarding the equivalence of movements must be regarded as tentative. We shall be able to address the issue of motor equivalence only when we can give a definitive reply to the question, "What is a response?"

In addition to being premature, the proposed framework seems much too vague to serve any useful purpose in guiding research efforts toward a resolution of the issues which Bindra has raised. Nowhere in the paper do I find clear operational definitions of the central concepts of "act," "act-assembly," "sensory representation," and "pexgo." Even on the theoretical level, a major gap exists. Bindra rejects the behavioristic view that organisms learn stimulus-response associations. He also rejects the cognitive view that organisms learn to "expect" reinforcements on the grounds that no mechanism is specified by which expectations are translated into actions. What mechanism is proposed to fill this void? There is a vague suggestion that "pexgos" and "act-assemblies" develop under the influence of the same "eliciting stimulus complexes," but what is the nature of this influence? How is it different from an association? These fundamental issues are not addressed in the paper, nor is there any clear indication of how we might go about seeking a resolution. None of the directions for future research discussed at the end of the paper point directly to the problem.

In sum, the proposed framework is premature. It is vague. It is confusing. It testifies to the advisability of postponing the construction of theoretical structures until they can be anchored to firm empirical foundations.

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**Feedback systems invite tautological constructs.** Bindra's article has fine, well-reasoned objectives, and I agree with most of them. Regrettably, the article's model-building does not speed the paper toward attainment of these objectives, but away from them. The otherwise excellent contribution begins to drift as soon as I read that perception initiates behavior because of "a pexgo involving the activation of several gnostic-neurons and gnostic-assemblies." A pexgo is defined earlier as "The momentary central representation that underlies the perception of a stimulus." pexgo is an acronym for presently excited gnostic organization."

Before proceeding further, I must extract what I consider to be the two major ideas in Bindra's formulation (I am more confident of my having pinpointed the first than I am of the second). *First*: The traditional loop-concept of learning and performance (motivation → cue → response → reinforcement, with the last component returning to influence the first) must be expanded to include a couple of key neglected facts. These are: a) The functional character of each of the four respective components *changes*, as response follows response. b) Each of these changing components contributes to the organism's overall, momentary *perception* (not simply discrimination) of what it must do in order to obtain reinforcement, and therefore to its overall incentive to produce the appropriate (or adaptive) response. *Second*: None of the constructs (e.g., programs, schemata, templates, perceptual traces) offered by today's mutually opposing camps of motor outflow (essentially, Lashley's functional assembly position) and motor inflow (James's kinesthetic feedback position), is sufficient to account for an organism's continually changing *reorganization* of perception and subsequent action [See Roland, this issue].

Although both these ideas make substantive contributions, I am left uneasy by their being connected to an assumed central representation involving the activation of gnostic-neurons and gnostic-assemblies. In what way am I to distinguish a gnostic-neuron from a non-gnostic-neuron? Is even my attempt at distinction a meaningful one? I get the feeling that I am being seduced into having faith in a modernized, dynamic version of an engram (the long sought-for physical or chemical analog of a learned response), even though I know full well that such is not Bindra's objective. Nor am I reassured upon reading the following statement in the fifth (and last) of the list of research implications: "The elucidation of the neural relations of perceptual structures with motivational structures on the one hand and with act-organization structures (pre-motor areas) on the other thus becomes an important task for physiological psychology." Of course, Bindra is properly objecting to wire models of the brain. So did Lashley, but he did not get very far with the engram (Robinson, 1973, pp. 88-101). So did Kohler, but he did not do much better with psychophysical isomorphism (Köhler, 1969, pp. 63-93). Notwithstanding, both Lashley and Köhler made significant contributions to experimental methodology and to science. Let us therefore attend to the article's four other implications for research. I shall comment on each, as I abstract them:

1. We should learn more about eliciting stimuli. I agree, and – in fact – have made some modest contributions toward that end (Notterman and Mintz, 1965, chaps. 1, 4, 6, 7; Notterman, Filion, and Mandriota, 1971).

2. We should expand the number of options available to an organism in a standard experimental situation, so that we can study "the relative valence of the various situational stimuli." "This idea has obvious implications for therapeutic behavior modifications in man" [see Murray, this Commentary]. I happen to agree, but more importantly, so do clinical psychologists (Levine and Fasnacht, 1974), theoreticians of abnormal psychology (Krasner, 1976), and psychoanalysts (Moore, 1974). And they do so in the absence of pexgos, or even superegos.

3. We should construct laboratory environments in such a manner as to reinforce an organism for observing how well eliciting stimuli predict the

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occurrence of incentive stimuli We are enjoined to do so because " learning by observation of models (copying, imitation, etc ) need no longer be considered as a special form of learning, but as one in which the demonstrator model plays a part in making the novice or learner observe the critical stimuli and their relations" (Bindra, 1974 *op cit* ) In both of the key studies cited in the reference, learning by modeling groups was superior to that of controls However, so was the learning by merely observational groups (i e . non-modeling, but looking-on in one manner or another, without active reproduction of the demonstrator's behavior) Groesbeck and Duerfeldt (who did one of the cited experiments) remark: "If exposure to the learning situation without a demonstrator provides the same results as exposure with a demonstrator, there is probably a better term to explain the phenomenon than 'observational learning' A simple construct such as stimulus enhancement would be more parsimonious" (Groesbeck and Duerfeldt, 1971, pp 41-43)

4 We should " study the development of the ability to abstract knowledge of causal relations – what has been called 'development of causality' – (which) is from the present viewpoint an aspect of the general problem of learning the correlations we observe in our environments " I wish that space had allowed Bindra to make contact with others who have expressed similar concerns (for example, Kant's "apperception," Piaget's "conception," and Harry Stack Sullivan's "protaxic, parataxic, and syntactic" cognitive modes of organizing experience)

In sum, I respect Bindra's objectives However, I am uncomfortable with the article's constructs For any construct to be considered as contributing to an effect, it must be specifiable independently of that effect Otherwise, we have sheer tautology This basic principle is especially germane when we examine feedback systems (regardless of whether they are response-reinforcement or perceptual-motivational), which – precisely because of their circularity – almost invite tautological explanations

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**Stimulus theory and response theory: their complementarity and neurobehavioral basis.** In this paper, as in his previous work, Bindra makes a case for a "perceptual (incentive)-motivational" alternative to "response-reinforcement" as the guide for behavior Although I am in sympathy with many of Bindra's aims, I take serious exception to some of the logic by which they are carried through Most of my objections concern a failure to take into consideration the rich body of knowledge about the nervous system and its relationship to behavior This knowledge makes possible clarification of concepts that in their current dress tend only to obfuscate and produce unproductive controversy

Bindra points out that response-reinforcement theory emphasizes what an organism is doing, while the incentive-motivation theory emphasizes what the animal is perceiving But is this really a fundamental difference? As Bindra points out later in the paper, perception is dependent on attention

Reinforcement theorists might suggest that "attention" involves overt or implicit "looking" or "listening," and so forth, that responses become differentially reinforced

The problem with any attempt to bring conceptual order into a controversy such as the one addressed by Bindra is that the opposing views must first accept the definitions of their opponents Thus Bindra clearly recounts Skinner's definition of a response as a certain measurable environmental outcome He then correctly points out that this circumvents the problem of motor equivalence But he also goes on to say that "while this circumvention of the problem of motor equivalence may have 'emancipated' psychology from a neurological straightjacket, it did so only by evading the fundamental problem of how adaptive behavior is put together"; and he dismisses the entire problem by stating that "it [this definition of response] sacrificed explanation to the empirical goal of enunciating descriptive principles "

Although this description applies to operant behaviorism, it need not be that way In ch 12 and 13 of *Languages of the Brain* (1971), I show how experiments on the precentral classical motor cortex of primates can be interpreted to demonstrate the encoding of the environmental consequences of behavior Others such as Bernstein (1967) and his followers in the Soviet Union (Gel'fand et al , 1971) and Turvey (1973; see also Fowler and Turvey, this issue) and his colleagues in the United States have explored in detail how coordinate structures are developed and how these incorporate the features of the environment necessary to carrying out an action For us, an act is therefore more than a sequence of muscle contractions or movements – a conception of "act" that differs from Bindra's and was necessitated by the results of neurobehavioral and behavioral experiments Our conceptions fully account for motor equivalence and can encompass the response-reinforcement theory of guides on behavior But note: substantively, by making essential the environmental consequences of behavior rather than its muscular contractions, the sensory aspect of motor control is emphasized In fact, I once dubbed the motor cortex of the brain the *sensory cortex for action* (Pribram et al , 1956), a view shared by Penfield (1956) and Dusser de Barenne and McCulloch (1938) Does not a "response" so considered provide its own "incentive-motivation," which is, of course, "reinforcing"?

Neurophysiology and neuropsychology can take us even further The control of muscle contraction is in the main effected by way of a servomechanism, a feedback mechanism in which the control is exercised on the muscle receptor rather than on contractile muscle itself [see also Roland, this issue] Movement is thus regulated according to principles akin to those operating homeostatic mechanisms (and not those that make of the motor cortex a piano keyboard) Miller, Galanter, and Pribram (1960, ch 2) detail the revision of the conception of reflex behavior that this entails (making a reflex-arc concept untenable) and suggest in the remainder of the volume the consequences for psychology necessitated by this revision We use the term "Image," while Bindra uses "presently excited gnostic organization" or "pexgo" – but are not the conceptions identical? Images are formed of tests between sensory input and a representation of prior experience (genetic or individual); pexgos are "momentary representations that underlie [i e , are excited by the input to produce] the perception of a stimulus "

My point is that I believe "response-reinforcement" theory and "incentive-motivation" theory to be complementary views of the selfsame process and that neurobehavioral analysis and cognitive (e g , information and image processing) theory can clarify this complementarity "Incentive-motivation" is a theory couched in stimulus language "Response-reinforcement" is a theory couched in response language Since stimulus and response mutually imply each other, one of the terms has to take precedence, and the fault of stimulus-response psychologies was that they attempted to make a chain out of a mutual implication (Estes, 1959; Pribram, 1971; Skinner, 1976) Bindra continues this fault when he pits response-reinforcement theory against incentive-motivation theory Logically, the two theories ought to imply each other mutually, and in the above paragraphs I have attempted to show that substantively as well as logically they do

If the two theoretical frames are in fact complementary, each should have strengths (explanatory powers) that the other lacks Read in this light, Bindra's paper makes many excellent points that warrant his claim to a "new approach" Response-reinforcement theory and practice has provided a low yield of explanation and even of lawful relationships My own remedy has been initially to enrich response theory in terms of a computer metaphor that could, with a reasonable amount of work, be turned into a neurophysiological model (Miller, Galanter, & Pribram, 1960; Pribram, 1960) Then I turned to

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enriching stimulus theory by recourse to optical information (i.e., image processing in terms of holography that has been, with a reasonable amount of work, turned into a neurophysiological model)

Bindra has chosen to dispose of response theory up to now. But "pexgo" is a cognitive structure and as such is a response organization. Bindra may well have to devote more and more of his theorizing to these pexgos - how do they create "motivational states"? How are eliciting stimuli correlated with incentive stimuli? And how do "viscerosomatic" and "transactional pexgos" get together, and these with "instrumental pexgos"? Or do they? And what about "act assemblies" and "gnostic-assemblies"? These are problems in cognitive neurophysiology that probably will take any investigator away from specifications of the stimulus responsible for incentive-motivation.

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**Behavioral evolution and individual development.** After my initial timidity to comment on a theoretical article that sets out to revise our thinking about response-reinforcement, which I consider the province of learning theorists with whom I have not had contact since my graduate school days, I realized that, to paraphrase G. B. Shaw, reinforcement is much too important a concept to be left in the hands of learning theorists alone. Like many concepts of whose history I am ignorant, reinforcement derives historically from evolutionary theory, as Bindra makes clear by quoting Bain and Spencer, so that its recent entry into ethological thinking is in fact a reentry. That ethologists are exploring "natural reinforcers" of learning as an aspect of the current emphasis on "constraints on learning" indicates that the concept has received a fresh source of energy that is likely to sustain it for some time. There can be little doubt that ethologists have sensed the underlying analogy between natural selection as a "reinforcer" of adaptive behavior traits in phylogeny and selection by reinforcing incentives of stimulus-response contingencies as the basis of individual adaptation in ontogeny. A paradigm that seemingly links two closely related disciplines, namely, behavioral evolution and individual development, is not likely to give way even to Bindra's strong and intelligent arguments, whose effect would be to upset the entire concept of reinforcement. The author, I am sure, is aware of this, so let us move on.

My comments will be confined to the main issue to which Bindra addresses himself: the failure of reinforcement theory to deal adequately with

the problem of flexibility in responses, which the present theoretical alternative is intended to remedy. Flexibility of responses and the adaptive nature of this flexibility is achieved by two processes, according to Bindra. The first is exemplified by viewing motor responses not as fixed integrated patterns that are triggered by sensory stimuli but as responses created anew each time a pattern of stimulation recurs; the effect, therefore, is that responses are able to vary flexibly according to variations in the pattern of incoming stimulation. There has been much evidence around for a long time supporting this view of how motor responses are formed, but we cannot ignore contrary evidence (e.g., Fentress, 1972; Gray and Lissman, 1946; von Holst, 1935; etc.) that some motor responses do have the character of triggered responses: relatively simple and unpatterned stimuli produce complex patterned responses (e.g., swimming movements, forelimb grooming, etc.). It is not incompatible with Bindra's views to consider that there must be a range of flexibility-inflexibility with respect to sensory-motor relationships and the organization of motor responses.

The second basis of flexibility of motor responses, according to Bindra, is based upon his views concerning the locus of learned associations. As long as the learned association is regarded as being between a stimulus and a response, it is difficult to see how substitute responses, leading to flexibility, could replace the original learned response. By shifting the locus of learning to sensory-sensory relationships and having them feed into motivational systems before the response is determined, he has opened the possibility that a range of alternative responses is available, depending upon the perceptual-motivational processes that are aroused. It is evident that sensory patterning of motor responses, the first basis for response flexibility, and perceptual-motivational determination of motor responses are closely interdependent processes for achieving flexibility of motor responses.

However, the problem of response flexibility, now shifted from the motor response side of behavior to the perceptual and motivational sides, still requires to be dealt with, and it is here that I find that difficulties arise. Of course, by proposing a multiplicity of determining factors for the eventual motor responses, Bindra already provides a basis for flexibility, since different combinations of these factors will result in a corresponding variety of responses. The motivational component ensures that this variety of responses will be focused and organized in relation to the incentive.

But how do perceptual and motivational processes themselves gain flexibility, or is flexibility an inherent property of these processes? If so, in what does the flexibility reside? It is here, I believe, that a developmental analysis is required to trace the ontogeny of flexibility in perceptual-motivational processes and their relationship to motor processes. The work of Bateson on visual perceptual development in chicks would contribute to such an analysis by showing that the chick builds its perception of the parent from multiple single instances in which she is viewed from slightly different angles, and that the chick actively seeks these different views of her. Our studies on home orientation in kittens have shown an even earlier stage of perceptual development, before vision, in which orientation to the home is based upon the combined use of thermotactile and olfactory stimuli in a complex pattern. In many species of altricial mammals, the beginning of olfactory-based responsiveness introduces the earliest release from the reflex-like determination of responses by thermotactile stimuli, which may, in fact, be one of the earliest forms of flexibility during development.

During early development among altricial mammals one can see even more clearly than in mature animals how sensory stimuli provide the basis for motivational processes at the same time that they elicit and guide responses by means of which the newborn reaches the incentive. At this early age, however, this dual role of sensory stimulation may result in lack of flexibility, since alternative motor patterns would have little chance of emerging. How is this situation altered during development such that while maintaining the same basic relationship between perceptually-based motivational processes and motor responses, nevertheless, flexibility arises within the system?

These statements are not to be viewed as criticisms of Bindra's theory but rather as suggestions for research and thinking that need to be done if the theory is to gain the strength it will need to dislodge reinforcement theory from its paradigmatic position.

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**Bindra's theory from the perspective of human motivation: unit size, stimulus centering, and the value of neural theory.** For someone whose main concern has been human motivation, I find it a surprise to find response-reinforcement theory treated as just now "finally ending" Tolman and Lewin clearly dominate the theoretical climate in current theories of human motivation. Nowadays I even find myself playing devil's advocate for the S-R view because present students find it difficult to believe that the approach could once have been taken seriously and that intelligent men could have really expected it to succeed. At the same time, it must be admitted that there are still residues of the Hullian era influencing theories of human motivation, and even of human cognition. Apparently Bindra finds these residues still more noteworthy in the field of animal behavior, and he is in a better position than I to judge the need for a review of the shortcomings of the reinforcement view.

This commentary will consist mainly of raising questions concerning Bindra's account of motivation, questions about things that he does not explicitly deal with in his paper. Raising these questions is not meant to imply that they cannot be answered, but rather it is hoped that the questions will serve as "eliciting stimuli" for further explanations of the assumptions underlying the theory.

Some characteristics of the general behavioristic position still seem to be retained in Bindra's account, and my first questions concern the reasons for retaining these assumptions. One of these presuppositions is the desirability of reducing behavior to small units as opposed to working with large behavioral units, as is common in studies of human motivation. A very important characteristic of Hull's approach was the reductionist assumption that all behavior can be reduced to component S-R units and that the explanation of complex behavior involves the same assumptions as those of the single S-R unit. This reductionist assumption was, of course, common to all behavioristic theories, otherwise there is no justification for the amount of effort and money spent on investigations of the white rat. In contrast, students of human motivation have given up the hope of finding a reductionist solution and are taking human behavior in the common-sense large units in which it is found - going shopping, typing this paper, giving a lecture. Behavior may still be broken down into units for specific purposes, but there is no assumption that there is a single set of natural "atoms" to which everything can be reduced. This is one of the important ways in which the influence of Lewin is much stronger than that of Hull. While Hull (1943 *op. cit.*) spoke of "molar" units, this merely meant that he was not concerned with the ultimate reduction to physiological terms and his unit was as small as it could be without switching to physiological concepts.

On this issue of reductionism and the appropriate units for a theory of behavior, I find Bindra very close to Hull. His "act" is about the same size as Hull's "molar" response - in the mature animal, the act takes "no more than half a second." Hull took great pains to spell out the manner in which complex behavior was put together out of the basic response units - this was a major concern of the theory and the reason for the introduction of such concepts as the  $r_{G-S}$  mechanism (Hull, 1952 *op. cit.*). In other words, Hull recognized the difficulties with simple chaining of S-R units and tried to provide for more Gestalt-like organization of large behavioral units. In fact, when I play my devil's advocate role, one of the advantages to be cited for the Hullian view is its specific and detailed explanations for complex behavior. In this regard it was more specific and more testable than the "cognitive" views.

While Bindra shifts from an S-R account to an S-S account, there are still important similarities to Hull. The "S-S" terminology is really a misleading usage imposed long ago by the dominant S-R theory. Clearly S's are not associated with each other. Either one S is connected or associated with a

cognitive phenomenon such as an expectation, "idea" or "pexgo," or else no stimuli are involved at all, only two cognitive phenomena. Bindra's pexgos, contingency organizations, expectancies, and so on, seem functionally to be very similar to the ideas of classical associationism. The S-S terminology is really a way of avoiding the use of forbidden experiential terms such as ideas, perceptions, or expectations, while actually introducing these concepts into the theory. Nevertheless, the S-S shorthand has become so widespread it can be useful, as long as we do not take it literally. After all, Hull's  $r_{G-S}$  mechanism - the fractional antedating goal response - was a device for including anticipation and expectancy in a response-oriented theory. Bindra's account seems to be just as atomistic except that he chains S-S units instead of S-R units. This does provide for greater flexibility on the motor side, but it is still an atomistic account very close to classical associationism. Bindra does not deal explicitly with the problem of elementarism, and neither his article nor his book (1976 *op. cit.*) makes reference to Lewin or other current writers on human motivation such as McClelland, Atkinson, or Weiner. Yet if a theory is expected to have general significance not only for the white rat but also for man, it is precisely this kind of fundamental question that must be faced. We therefore need to know Bindra's justification for his form of atomism and how he expects this to form a basis for treating human motivation. (For a more extended discussion of the problem of reductionism in action and motivation, see Ryan, 1970.)

Another similarity between Bindra's account and that of behaviorism is generally that stimuli play a dominant role in both, and my second question has to do with the justification for this emphasis. The concern with eliciting stimuli is very reminiscent of Watson and Hull. Yet there are clear instances in human activity and even in animal behavior in which the immediate situation is irrelevant or plays only a minor role. Often it is not even certain that there are internal stimuli involved, unless we want to destroy the usefulness of the term completely by speaking of "central stimuli." In both this article and in his 1976 book, Bindra seems to claim that there is no truly spontaneous behavior in the sense that there are always eliciting stimuli, if we look for them. It could be argued that many of the most important problems of motivation, especially in the human organism, are cases in which there are no important eliciting stimuli. To deny the importance of stimuli in such cases does not mean that we deny that there are causes or determinants of the behavior. Much of human behavior is intentional (it is *determined* by intentions), but where are the eliciting stimuli that are crucial to such behavior (Ryan, 1970)? In short, an adequate motivational theory cannot be built upon S's, even "eliciting" S's or on S-S's alone. To be sure, it is an advance to recognize that stimuli operate by way of perceptions. Motivation, however, can be independent of perception. Bindra recognizes this when he discusses planning, but really provides no theory of planning beyond the notion that S-S units or pexgos can be chained to form "contingency organizations." Clearly, a theory of planning or of intentional behavior requires more than such a promissory note. My question here, then, is a very broad one: What kind of *specific* contribution can the theory be expected to make to the analysis of intentional behavior, planning, or other behavior that is or seems to be independent of any eliciting stimuli? I do not ask for final validated answers, but only for examples of the kind of specific answers that could conceivably come out of this kind of approach.

My third general question has to do with the value of introducing physiological mechanisms as well as "molar" constructs such as stimuli, responses, expectancies, or other "psychological" constructs. Bindra takes for granted that such physiological theorizing contributes significantly to the total system. I, however, am skeptical of the value of such theorizing at the present stage. In order to specify my question more clearly, it is necessary to explain the bases of my skepticism. First, I am concerned with the contribution of neural theory to the total motivation theory as a way of explaining behavior. There is a clear choice here in that it is conceivable that a *complete* explanatory theory of behavior can be constructed without reference to the nervous system. After all, Hull's system was such a system and it did not fail because of any lack of theorizing about neural function; it failed because it did not make satisfactory testable predictions about animal or human behavior. The reason for introducing neural constructs, then, is not that they are necessary for a complete theory, but because they may make some contribution to the explanatory success of the theory. Such theorizing might serve several functions:

1. If the neural model is based upon physiological evidence, it can serve to correct psychological theorizing that is based upon either explicit or tacit

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assumptions concerning the nature of neural function. Thus current neurophysiological findings make the simple switchboard concepts of the S-R psychologists implausible or at least highly oversimplified. With regard to this function, my impression, perhaps unjustified, is that Bindra's neural model is not based upon substantial physiological evidence. Instead, it seems to be a construct derived primarily from psychological facts and theories.

2 If the theory is largely conjectural and based only loosely upon *physiological* evidence, it may suggest new approaches to psychological theorizing, or it may make more plausible theories that seem to be contrary to previous general assumptions about the nervous system. Thus Hebb (1949) performed the great service of showing that a drastically different conception of neural function could be formulated and could be used to free our psychological theorizing from tacit assumptions about the general operation of the central nervous system. The nature of Bindra's model does not seem to change the nature of the psychological theory. In fact, the general theory seems to be very close to Tolman's. The properties of the "pexo" seem to be derived from current cognitive theories that both perceptions and memories are *produced* rather than being patterns called up intact from some sort of store. That is, the theories are based upon psychological evidence and the pexo, in turn, is based upon these theories. Moreover, the theories are still subject to considerable debate, which becomes important to our next point. The present question is how the pexo construct has modified the psychological theory, and I have been unable to find such modifications.

The above possibilities were concerned with the possible contributions of neural modeling to psychological theory. Another possible function is to make suggestions to neurophysiologists to aid in the pursuit of their own problems. Even if we had a complete and completely successful psychological theory at the "molar" level, there are still those who are curious about the neurological mechanisms underlying the psychological events. While we as psychologists do not need to be concerned with this problem, and may even be better off not getting involved in it, this does not preclude others from being concerned with the problem. For this purpose it could be useful to translate aspects of the psychological theory into a neural model. In the long run, neural research based upon the theory could feed back to correct, expand, or help to substantiate the psychological theory (but only in the long, probably very long, run, and the validity of the theory can be checked more rapidly by purely psychological research). It must be emphasized that this function of modeling is dependent for its success upon the plausibility of the theory as an account of the psychological facts. A premature model, based upon unsubstantiated theory, runs the risk of wasting the time of the neurophysiologist as well as of the psychologist.

We are therefore brought back to the question of the validity and usefulness of the psychological theory. Until that is established, the neural model is of doubtful value. For this reason, my first two questions concerning the elementarism of the psychological theory and the emphasis upon eliciting stimuli are of primary importance. The value of the neural model depends in a critical way upon the validity of the psychological aspects of the theory.

Because of the shortness of the space available, I have concentrated upon the questions that trouble me and have had to skip my many agreements with the article. The questions that I have raised are perennial questions that constantly trouble psychological theorists, and I am merely trying to provoke Bindra into further work on these questions. I must emphasize, however, that I am in strong agreement with the general "cognitive" approach that is exemplified there and support him fully on the advantages of the S-S over the S-R reinforcement view. I am sure that the article will be stimulating and influential in our pursuit of motivational theory.

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**A death too soon reported.** There is no end to the announcements of the recent or impending death of behavior theory. Bindra tries to make his arguments stronger by dating behavior theory's beginnings further back than is usually the custom, referring to Spencer (1872-3). The more typical procedure is to blame Skinner for the sins of Watson. Apparently, these critics believe that age alone is a sign of the lack of validity of a theory or an approach of study. In any case, history hangs heavily on those of us who continue to consider ourselves behaviorists.

Bindra raises two sets of issues beyond the simple one of age: The first one talks of empirical findings and criticisms made by ethologists and cognitive psychologists about some particular forms of behavior. The second set of issues relates to the question of what kind of theory is best for psychology, at least at our present level of knowledge and procedural expertise.

Let us look first at the criticisms of the ethologists (I have responded to the arguments by cognitive psychologists elsewhere [Salzinger, 1973] and will therefore not discuss them here.) The words "biological constraints" loom large in this area. We are persistently confronted with findings a little bit akin to those made public by anthropologists when they used to confound us with yet another different approach to life and living. The ethologists thus have been showing how different animals are, and how futile the search for universality must therefore be, when one wishes to talk of general laws of learning. Moreover, having shown an instance of learning that seems to be different for a particular animal or for a particular response in the same animal, they immediately reach for their burial tools.

To be sure, the phenomenon of autoshaping has inspired a large number of clarifying experiments. It has by no means, however, elicited a uniform reaction of "disaster." Thus Herrnstein (1977) believes that behavior controlled in the manner of autoshaping can best be explained in terms of a concept of some behaviors reinforcing themselves; Skinner (1977), in response to Herrnstein's article, discussed autoshaping by attributing its special effects to the phylogenetic character of some responses; he pointed out that responses may be phylogenetic in character at one time (such as being investigatory under some conditions) and operant under others (such as when a reinforcer is made contingent on the same response). Finally, Schwartz and Gamzu (1977) say, based on their review of the literature, that no new laws need to be constructed to explain the data on autoshaping; they show that the interaction of the principles of operant and respondent conditioning explain this phenomenon.

The last point relates to the second set of issues raised by Bindra. Essentially, Bindra believes not only that a psychological theory must satisfy the criterion of consistency of behavioral data, but also that its descriptions must lead directly to a "reasonable" physiological substrate. The question is whether any behavioral theory can be validated by the flimsy notion of the physiological underpinnings of behavior. Is it useful to conjecture about the nature of the physiological correlates of the behavioral processes, or is it more useful to worry about the consistency of the scheme that explains the behavior at its own level? Surely the new findings in the behavioral literature which appear so contradictory to Bindra must be resolved at a behavioral level before they can be used for elucidating the physiological level or before the physiological level can elucidate them. This is where the controversy really exists: not in what the data say but rather, whether what the data say should be reconciled at the behavioral or at the physiological level. Furthermore, the physiological level is reconciled, not in actual physiological findings, but in physiological deductions several stages removed from actual experiments. It is here where I believe that Bindra will lead us astray - away from the laboratory and into the never-never land of physiological speculation and into a slower, more lingering version of the death of behaviorism that he prematurely reports.

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Skinner, B. F. Herrnstein and the evolution of behaviorism. *American Psychologist*, 1977, 32:1006-1012.

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**Omnipotent pexgos and the goddess parsimony.** I concur with Bindra's criticism of an S-R-reinforcement psychology, but, instead of expanding on this theme, I would like to discuss a few points in the explanatory part of the present article and his recent book (Bindra 1976 op. cit.) His alternative to the S-R approach is an updated S-S learning theory. In this conceptual framework the responses appear to be a by-product of "pexgos," i.e., hypothetical dynamic neural representations of perceived or imagined environmental stimuli. A brief summary of this version of the S-S theory will help to clarify my comments: (1) some stimuli are so-called eliciting stimuli (SEs) and will elicit responses whenever they are perceived or imagined with sufficient intensity; and, (2) some stimuli possess positive hedonic value and will, because of that, enhance (prime) the pexgos of other stimuli (for instance SEs) if the latter become conditioned stimuli (CSs) predicting the occurrence of the "hedonic" rewards.

These two tenets are applied to the case of discriminated instrumental responses in the following way: (3) Instrumental CS (i.e., a Skinnerian S<sup>D</sup>) becomes a signal of reward (i.e., a Pavlovian food CS), and this endows it with the property of an "incentive motivational" agent. Incentive motivational excitation leads to the recreation of the proper pexgos of eliciting stimuli of the instrumental response, which in turn excite the "assembly of the motor act."

It is not clear to me why the "multipurpose movements" (a term borrowed from Lorenz 1973) that constitute the bulk of instrumental behavior and do not have any definite, strong ties with the environmental stimuli, should be elicited by "primed pexgos" of external stimuli. The voluntary character of most instrumental responses derives from the fact that they are freed from the tyranny of eliciting stimuli and are accessible to several motivational systems. The weaker the reflexive bond of a particular motor act with its SE the better its instrumental conditionability. The purely perceptual S-S theory could include such responses by postulating kinesthetic pexgos and assuming that they have a weak eliciting function, revealed only in conjunction with motivational excitation. This would bring us close to Pavlov (1932, 1936) and Konorski (1967, 1974).

If the instrumental responses were really elicited by the pexgos of their implicit SEs, then the "transactional" responses with their strong SEs would be the best candidates in the organism's motor repertoire for instrumental conditioning. Studies on scratch and yawn reflexes (Jankowska and Solysik 1960, Solysik 1975) have shown that, although there is a certain degree of quasi-instrumental conditioning of these responses, it differs in some important ways from the "voluntary" (locomotor or manipulatory) type of instrumental learning. First, these instrumental transactional reflexes seem to retain their involuntary character (Thorndike 1911, Kupalov 1948, Yakovleva 1952, Fedorov 1955). Secondly, they are unstable and tend to "degenerate" (Thorndike's term [1911 op. cit.]). Thirdly, they become contaminated with imitations of clearly voluntary character, described by different authors as a "so-called voluntary" (Yakovleva 1952, Fedorov 1955) or "sham" form of a particular reflex pattern (Jankowska and Solysik 1960). Fourthly, the instrumental conditioning of a true form of scratching (Fedorov called it involuntary or subcortical) was explained (with some evidence for it) by the learned sensitization of the receptive field for its SE (Jankowska and Solysik 1960). We have coined a term "operant sensitization" (Solysik 1975) that seems quite compatible with the "SE-pexo priming," although we postulated a different mechanism, namely, a positive gating in the afferent pathway. Kupalov (1948) and Fedorov (1955) were even closer to Bindra's position in hypothesizing direct facilitation of the cortical "points" representing the SEs of these involuntary responses and ascribing the facilitation to the food signalling function of these SEs (incentive motivation?).

The important point is that, whatever their mechanism, such "instrumental" responses differ considerably from instrumental responses of non-reflexive character, and their handicaps can be attributed to the fact that they possess, or are bound by, the eliciting stimuli. Obviously, the priming of the pexgos of SEs, although possible, does not constitute an efficient mechanism of learning.

Finally, concerning the "incentive" role of food signals, it was Konorski

who recognized long ago (Miller and Konorski 1928, Konorski and Miller 1930) that S's and food signalling CSs are very different conditioned stimuli, both in their motivational value (S<sup>D</sup> elicits approach, striving behavior, while CS inhibits it), and in their signalling (informational) role: food CS heralds an obligatory occurrence of food, while the S<sup>D</sup> signals only conditional availability of food, depending on the instrumental response. These views were later confirmed (Solysik et al. 1976), and I proposed a new mechanism for incentive motivation (1975). That food CSs are poor elicitors (motivators) of instrumental responses was found in several studies (Azrin and Hake 1969, Meltzer and Brahlé 1970, Miczek and Grossman 1971, Kelly 1973, LoLordo et al. 1974, Harvey 1977), and a good separation of food signalling and instrumentally "motogenic" functions was demonstrated (Ellison and Konorski 1964, Miyata and Solysik 1971).

In summary, one gets the impression that the adherence to a single principle of S-S learning sacrifices heuristic flexibility implicit in a more open approach that accepts more than one mode of plasticity in organisms' neurobehavioral machinery.

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**Bindra's perceptual-motivational theory and social behaviorism's emotional-motivational theory: separatism exemplified.** Bindra's perceptual-motivational theory has many similarities with the motivational theory of social behaviorism, which is called emotional-motivational theory or, in abbreviation, the A-R-D theory of motivation, with the initials standing for the emotional (or attitudinal), reinforcing, and incentive (or directive) functions of motivational stimuli. The fact that Bindra makes no reference to this motivational theory indicates that this is a case of separatism. It is important to draw attention to the relationships between the two similar theories. The scholar in this area needs this information for there are important elaborations and bodies of knowledge involved in each that should be known. It is also important to consider this as an example of a more general separatism in psychology, a separatism that impedes the advance of unified knowledge.

This commentary will concern the similarities of the two theories of motivation in rectifying the present separatism. To begin, A-R-D theory was developed in the context of the failure of the classical learning theories to treat adequately various topics and questions. For example, Skinner's operant approach entirely separates classical (emotional) conditioning and instrumental conditioning, failing to indicate the interrelations involved. A-R-D theory began (see Staats, 1963) by indicating that stimuli that have elicited emotional responses would, because of this, also have reinforcing properties (thereby independently defining the reinforcement value of stimuli). Bindra utilizes the same basic principle, also referring to Bentham, Hobbes, and Freud as examples of the common-sense confusion of pleasure and pain (positive and negative emotional responding) with reinforcement, topics that social behaviorism has treated much more extensively (see Staats, 1963, Chapter 14, especially pp. 513-523).

A-R-D theory states that there are stimuli that elicit emotional responses in the organism on a biological basis. These responses are of a positive or negative sort. Positive emotional responses can have peripheral manifestations that differ from each other, as well as a common central process. This is true of negative emotional responses as well. The stimuli that elicit pleasure or pain in the individual can serve to reward or punish, as the case may be.

One central process in the formation of the individual's emotional-motivational system (the stimuli that have emotional-motivational functions for the individual) involves how formerly neutral stimuli come to elicit emotional responses in the individual. A-R-D theory says the process is basically classical conditioning (Staats, 1963, 1968, 1975), although the circumstances may differ widely, and conditioning may take place on a linguistic or cognitive basis. Bindra has the same principle in his theory (see his second principle). In his terms, central representation of the emotional state is connected to the representation (pexgo) of the formerly neutral stimulus.

To continue, a crucial aspect of A-R-D theory is the indication of how emotion-eliciting stimuli perform motivational functions. One motivational function involves what stimuli will reward or punish the organism; a second involves the incentive or directive power such stimuli will have on behavior. Unlike other theories, A-R-D theory explains the incentive function, that is why the organism strives to obtain positive emotional stimuli and strives to avoid negative emotional stimuli. This incentive function comes about through learning. When the organism approaches a positive emotional stimulus, he is reinforced by contact with the stimulus since the stimulus has reinforcement value. As a consequence, he learns specifically to approach that stimulus. But he also learns generally to make approach responses when being stimulated to experience a positive emotional response and to make avoidance responses in the opposite case.

The important thing is that the organism has thousands of such experiences. He thus learns many approach and avoidance instrumental responses under the control of the stimulus produced by the appropriate

emotional response. This process has been described fully (Staats, 1963, 1968, 1975) and depicted graphically to show that there are many stimuli that elicit an emotional response in the organism and the emotional response can mediate any of a number of instrumental responses (see, for example, Staats, 1975, p. 110). Bindra has these principles and mechanisms in his theory, at least in part, describing the "many-to-one mapping of eliciting stimulus complexes to acts," as well as the fact that the one central motive state tends to elicit many different instrumental responses. Bindra describes the class of instrumental (and transactional) responses, as well as the putting together of act-assemblies. As in A-R-D theory, there is an additional principle involved to the effect that other situational stimuli, aside from the emotional (A-R-D) stimulus, determine which of the class of approach or avoidance responses will occur. It is central that while Bindra indicates that the motivational state has tendencies to elicit a large class of approach or avoidance instrumental responses, he does not indicate why, seemingly assuming that unlearned mechanisms are involved.

To conclude this brief description, once the organism has acquired the mechanism by which the positive or negative emotional response will tend to elicit a large class of approach or avoidance instrumental responses, that mechanism then provides the means by which the organism will immediately respond to new stimuli without prior instrumental training. "[A]ny stimulus that . . . comes to elicit an emotional response, will by virtue of eliciting the emotional response also tend to elicit all of the approach responses the individual has already learned" (Staats, 1975, p. 111). This is one of the bases for the flexibility that Bindra refers to repeatedly, and he states the principle involved clearly in a manner that is the same as in A-R-D theory.

A number of additional principles and analyses are the same in the two theories; only some of these can be mentioned here. For example, (1) like Bindra A-R-D theory (Staats, 1968, p. 45; 1975, pp. 114-116) presents the principle of the relative strengths of emotional stimuli, and ties the principle in with deprivation-satiation (see, for example, Staats and Warren, 1974); (2) A-R-D ties the motivational theory in with physiological mechanisms (see Staats, 1975, Ch. 15) relating the motivational properties of stimuli to the limbic system (Staats, 1975, pp. 536-537) as does Bindra; (3) A-R-D theory treats the topic of motivation in terms of external stimulation and internal emotional conditions as does Bindra; (4) A-R-D theory (see, for example, Staats, 1968, pp. 50-53) has repeatedly indicated that, as Bindra puts it, the "environmental stimulus that generates a motivational state can also serve as the goal stimulus to which the action is directed"; (5) A-R-D theory has been used to explain animal learning phenomena such as autoshaping and transfer of control (see Staats and Warren, 1974); (6) A-R-D theory has always included focal treatment of classical conditioning, as does Bindra.

Bindra indicates that it is important that motivational theory extend to the human level. The motivational theory of social behaviorism does this extensively - in social psychology, personality theory, clinical psychology, psychometrics, the social sciences, and so on - and the scholar of motivation needs to know this. The weakness of the present day pre-paradigmatic separatism of psychology is shown in the present example, where two highly relevant theories have remained separated. If unified theory is to be sought in the study of behavior, we must begin to bridge the many separatistic gaps that are now existent in our field, including the present one.

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**Perception as response.** In his recent paper, Bindra claims to offer a new and constructive approach to the traditional S-R principle of learning;

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however, I regretfully did not find his presentation particularly illuminating Insofar as this *particular* manuscript is concerned, nothing new has been offered; it has all been said before, as far as I can tell, by theorists such as Lashley, Bartlett, Tolman, and Hebb, to name a few

It may be an injustice to the concepts Bindra is trying to elucidate, but my charge was to evaluate this particular paper as it is presented; it seems to me that the reader should not be required to read his previous book to appreciate and understand the points raised in the present article

Let me begin my commentary with some of the basic points that Bindra discusses. He argues that the response-reinforcement hypothesis assumes that behavior-modification produced in a given stimulus situation (SS) is dependent on the contiguity between what the animal was *doing* at the time of presentation of some hedonic stimulus. The incentive-motivation hypothesis assumes that behavior modification is dependent on contiguity with what the animal was *perceiving* at the time of the hedonic stimulus presentation. The point that I wish to make here is that perception is a response, albeit a very complex one, involving many sensory and motor systems; i.e., perception often involves the *total* organism, not just a particular sensory system! If we accept that perception is indeed a response to various energies impinging on organisms undergoing various changes in state at different times, then the principal distinction between Bindra's position and the S-R position he decryes becomes blurred. He should clearly define "perception" and "response" and carefully delineate the differences between them. If he believes that "perception is simply a passive reflection of reality," and not a response of the organism, he should say so

At the "neural level," the distinction Bindra tries to make between his and the S-R position is vague—he suggests that there occurs a "selective strengthening of *some sort* of neural connection between the central representation [of a *perceived* stimulus—which as I have said is a response] and the representation of the hedonic stimulus [italics added]" This is not all that different from the "typical" S-R position itself, and, like it, relies primarily on hypostatization as a weak form of explanation. As Bindra himself states that "the central motive (which determines the incentive value of stimuli) *determines* what response will emerge." While this may not be quite as "mechanistic" as the S-R notion of response chaining, it still remains a very deterministic (albeit superficial) approach to explaining motivation, learning and memory, and at the neural-systems level of analysis it has no particular heuristic or explanatory value

At this point, I want to make it clear that I generally agree with Bindra that the S-R approach is limited and does not deal realistically with what we know about brain functions and behavior—there is no argument here. I contend, however, that Bindra's "theoretical" treatise provides no further understanding of brain functions although his paper does serve to reiterate what others have already claimed (Pribram, 1971; see also this Commentary)

Let me deal with some more of the specific points he raises. I have already discussed the question of whether "stimuli" are perceived, i.e., "out there," or rather a creation and therefore actually a response of the organism to energy impingements (Bartley, 1958). Second, is "knowledge of the environment represented in the brain" as Bindra suggests? Does the brain passively reflect "input from sensory inflow" or have we here a case of the reification of handy metaphors to replace descriptions of the complex events taking place in the CNS that permit the organism to "perceive," that is, to "create" its environment (see Von Uexküll, 1957, for example). Bindra argues forcefully that many factors determine an organism's "response repertoire"—yet he falls back on the naive and highly mechanistic notion of "gnostic neurons" controlling and determining the behavior of the organism. This implies that there are specific "command neurons," (cf. Kupfermann and Weiss, this volume) papal cells, grandfather cells or some such, which somehow "know" the true nature of the "real" world because, supposedly, they have a "window on it" and can "process information" which accurately reflects reality. There is *no* evidence for this hypothesis; it is simply a belief based on inference and speculation concerning the "meaning" of firing patterns of single neurons (action potentials, IPSPs, EPSPs, etc.) in the brain in response to activation of sensory cells perturbed by energy impingements. To make the logical jump from perturbation of these units to "knowledge" of the environment in all its richness and complexity is an act of faith, which we may, or may not, wish to indulge in—the elegant work of Hubel & Wiesel notwithstanding!

Sometimes the most effective way to evaluate a theoretical position is to "dejargonize" it and then see if anything new or innovative emerges. A

theoretical offering should be just as valuable, or even more so, if it can be innovative and offer new principles in simple language

Permit me to apply this translation of a few of the "principles" proposed by Bindra. For example he states that:

a. "There is no such thing as the 'pexgo' of a certain object; the same object is likely to generate different pexgoes on different occasions."

*Trans.* Objects in the environment may be perceived differently at different times depending upon the subject's motivation, arousal and qualities of the object itself

b. "It should be clear that the main functional outcome of the perception of an event, E1, in a given, familiar situation, is the anticipatory priming of pexgo components of events (E2, E3, etc.) correlated with or predicted with E1 in that situation. Thus, the perception of subsequent (predicted) events is facilitated by the earlier (predictive) events."

*Trans.* Organisms have selective attention tuned by previous experience. One response (perception) may lead to another. In other words, organisms think!

c. "If some ESs have some appetitive or aversive incentive properties, and the animal is in a relevant organismic condition, then it will continue to act in relation to those ESs."

*Trans.* What an animal does depends on its needs!

d. "arranging a response-incentive contingency is the best way so far discovered for ensuring the animal will observe the critical stimulus features [ES] which must enter into the stimulus-incentive contingency [ES:ES'] for producing the specified response"

*Trans.* Rewarding an animal practically guarantees that it will pay attention to subsequent presentations of stimuli deemed important by the experimenter in a test situation

In general, each of the points offered by Bindra can be re-defined in simpler terms. When this is done, it is difficult to find anything that has not been suggested, in one form or another by Thorndike (1911 *op. cit.*), Guthrie (1957) or other, more contemporary learning theorists. Certainly, with respect to understanding brain functions, Bindra's suggestions offer little, if any, explanation of the specific neuronal mechanisms that underlie or mediate learning and memory. Proposing that there are "gnostic" neurons that control "cognition" is also hypostatization; it simply states that since we observe organisms learning, there *must* be something responsible, i.e., there is learning because there is learning! There are few, if any, new and direct hypotheses that can be experimentally verified and this is perhaps the major weakness of Bindra's presentation

Bindra's comments on *flexibility* represent the best part of his paper. He is probably right in stating that there is "no continuous existence of a response in the brain"; that each is a fresh construction depending upon many organismic and environmental factors. This means that there are many ways to achieve a given goal and that often, but not always, responses can be substituted, one for another, in achieving a given end. While Bindra is correct in this observation, it should be noted that Lashley repeatedly stressed this point in his earlier writings, and there are few who would doubt this contention today. The main point is that we still are in the dark as to the physiological mechanisms that control much of our behavior. Strictly deterministic and Cartesian models of neural functions and/or behavior in whatever language they are couched are inadequate to account for the complex phenomena we call cognition and memory, whether they are based on the relationship between stimuli and response(s) or between stimuli themselves. Lashley (1960), Bartlett (1932), Tolman (1948 *op. cit.*, 1949), and others have recognized this long ago and perhaps a careful re-reading of their ideas and concepts may now be in order

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**Skinner, Maslow, and common sense** Bindra's paper has two essential purposes: (1) to show why and how the basic tenets of the response-reinforcement framework are invalid, and (2) to propose a framework that he believes will do the job (of accounting for adaptable behavior) better. For the reasons he advances, plus many others advanced by other persons, I agree with his first purpose and, therefore, will concentrate on the second purpose in the rest of this commentary.

According to Bindra's abstract, "An act is produced when its act assembly is activated by a pexo (perceptual representation) of a certain eliciting stimulus complex (ES)." At different points in the paper, Bindra defines a pexo as (a) a perceptual representation; (b) momentary central representation that underlies the perception of a stimulus; and (c) a presently excited gnostic organization. I do not believe that I will do great injustice to the concept if I use the first of these. He defines an act assembly as, "a neural organization that temporarily orders the excitation of certain movement commands." I shall use the word act here since it seems obvious from his statement quoted above that there is presently no way to tell them apart.

Bindra further says that, "When certain eliciting stimuli are correlated with incentive stimuli, they acquire motivational properties that serve to strengthen the pexos generated by those eliciting stimuli and thereby increase the probability of activation of the corresponding act assemblies." Thus motivation influences response production by "modulating the strength of pexos of eliciting stimuli for the succession of acts that comprise a response." In another part of the paper Bindra states that what the animal learns is correlations between stimulus events - SS.

Finally, Bindra states that "the production of any act is determined jointly by the animal's current organic condition, motivational state, and sensory inflow from within its body and from the environment."

To me, all this boils down to the following: an animal has an organic condition (he is hungry). He becomes motivated (incentive eliciting stimuli exist both within and without his body). He has pexos that are correlated with other pexos (he has learned what leads to what). These lead to acts. Bindra remarks that "the mechanism by which a certain pexo is translated into a particular act is not known" - I do not know it either. The reason the animal stops acting in a given direction is not specified in this paper.

In 1948, Tolman published his, "Cognitive Maps in Rats and Men" (Tolman, 1948 *op. cit.*) It is so well known that I will not try to abstract it here. Suffice it to say that he was proposing a cognitive alternative to the response-reinforcement view of learning and that his system was composed of correlations between stimuli (the animal learns what leads to what). He, like Bindra, felt that there were perceptual representations in the animal's brain (cognitive maps? pexos?) and that the system was more complicated than was thought by the response-reinforcement theorists. I can see no real difference (except for language) between what Tolman said in 1948 and what cognitive theorists (including Bindra) are saying now. Thus, I do not believe that significant progress has been made in this field of theory in the last twenty-nine years.

Does this mean that they were giants in those days and "they just don't make them like that any more?" In my opinion, the answer to that question is a resounding "NO." I believe that if you had been walking down the street of a large city in 1900 (or earlier) and had asked a reasonably well-educated person, "How is adaptive behavior learned?" he would have given essentially the same answer (again in different language) that was given by Tolman in 1948 (*op. cit.*) This answer, of course, was not satisfactory to many psychologists of the twenties through the forties and it is not satisfactory now. The theory is too vague; it leads directly to too few operational definitions; it is not specific enough; it is not testable enough; it does not predict. Therefore, we had psychologists trying to give us more scientific theories and those, being more testable, have been tested and found wanting.

I am saying, then, that we began with a common sense view of how adap-

tive learning took place. We replaced that with a more scientific, testable view. We found that that view did not work. We are now back with the common sense view. It is probable that the common sense view is correct. I, for one, certainly feel much more comfortable with it than with any other view. However, it would seem that we need a great deal more data (maybe especially physiological data) before we can do anything with that view.

I do not believe that Skinner (*op. cit.*) has been telling us that theory is not important, any more than Maslow has been telling us that we should not do controlled experiments with the "higher" human motives. Maslow has been telling us to learn as much as we can, in any way we can, about the "higher" human motives and this may lead us to be able to do controlled experiments with them. Skinner, I believe, is telling us that at present we do not know enough to build the kind of theories we want and need. I think he is telling us to learn as much as we can about learned behavior - to build a technology - and that might lead us into the kind of theories we want and need. Our penchant for theory is sometimes hard to believe. We even have mathematical model theories when we have no idea of what to put into them.

Maybe it is time we listened to people like Skinner and Maslow.

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**Another minor revision, or the disregard for control theory and the analysis of inductive feedback systems.** Many who have in the past taken issue with the dogmatic simplifications and distortions perpetrated in the name of Behaviorism will tend to agree with Bindra's introductory declaration that "the basic tenets of the response-reinforcement framework are invalid" and that "the difficulties cannot be simply patched up with minor revisions." Reading on, however, it soon becomes clear that different critics of that framework may have quite different ideas as to which basic tenets are invalid and what major changes would have to be made in our view of behavior in order to arrive at a more plausible explanatory framework or model.

Bindra, clearly, is struggling to get away from the primitive assumption that behavior is determined in a direct, linear cause-effect way by stimuli. Yet he undermines his efforts and minimizes his chances of success by perpetuating a terminology that inevitably brings with it much of the traditional behaviorist conceptualization. Learning, he postulates, consists in the forming of an association between a perceptual stimulus  $S_n$  and the hedonic stimulus  $S^H$ , and this learned association then manifests itself because  $S_n$  comes to generate "the same motivation (central motive state cms) as is normally generated by the hedonic stimulus ( $S^H$ ) itself." And, he adds, "this central motive state, in combination with the detailed sensory-spatial features of the situation, then determines what response will emerge." Contrary to S-R theory (according to which a response is "emitted" under certain stimulus conditions because it was linked to one or more of the stimuli by means of reinforcement), Bindra proposes that the organism does not have a repertoire of response behaviors, but that each individual response "is a fresh construction dependent on the motivational state and the pexo (i.e., presently excited gnostic organization) generated by the prevailing stimulus situation."

I would suggest that the word "motivation" is being used here in two rather different senses and that this is misleading because, in conjunction with the term "hedonic stimulus," it makes it all but impossible to focus on the crucial connection between motivation and satisfaction. On the one hand, a hedonic stimulus is said to generate motivation (and to transmit this capacity to other stimuli by association), and this motivation No. 1 leads to the construction of a response behavior that, on the basis of "structural gnostic elements" (structured, presumably, by prior experience), is expected to lead to some kind of consumption (or avoidance) and hence to satisfaction. On the other hand, there is the organism's general motivational state, that is to say, motivation No. 2, which refers to the kind of consuming the organism wants or is driven to look for at the moment (e.g., hunger). The indiscriminate use of "motivation" for both these items hides the fact that when Bindra talks about "detailed sensory-spatial features of the situation" determining "what response will emerge," or "an environmental stimulus complex that is critical for the production of a certain act," he is making the tacit assumption that the organism, in its prior experience, has established that, given certain "sensory-spatial features" or "environmental stimulus complexes," certain ways of acting are likely to lead to particular forms of satisfaction. In other

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words, if we are discussing "learning" (and not primary reflexes), the central representation of the visual stimulus I call "apple" becomes a hedonic stimulus only after I have at least once (a) bitten into an apple, (b) liked the result, and (c) in some way recorded that connection. If from this we form the hypothesis that the satisfaction derived from biting into an apple generates a specific kind of association between the apple-stimulus and the act of biting, we get a rather widely applicable principle or model that, of course, is really no more or no less than the well-known principle of inductive inference. As Maturana (1970) put it: "The living system, due to its circular organization, is an inductive system and functions always in a predictive manner: what occurred once will occur again." For the inductivist, the association between stimulus and response is, of course, not causal but *instrumental* in that it becomes an operative link if, and only if, the organism wants or needs the kind of satisfaction that was experienced in conjunction with that stimulus and that act (i.e., when there is motivation No. 2).

Though Bindra says "there is little doubt that animals learn the correlations between their actions and the consequences of those actions," he maintains that these correlations in no way explain how specific actions get produced. This difficulty, I would suggest, arises only if one assumes that the sensory signals out of which an organism builds up its "central representation" of a perceptual stimulus or stimulus configuration (e.g., an object-concept of apple) are data that are altogether different in kind or dimension from the proprioceptive signals out of which the organism builds up central representations of its motor acts [see Roland, this issue]. From a cyberneticist's point of view, they are all *input* (Powers, 1973). That is to say, they are all *proximal*, and the problem of making combinations with distal stimuli simply cannot arise for any organism, because there is no such thing as a distal stimulus in an organism's experience. (Only an observer can meaningfully distinguish "distal" and "proximal" relative to an observed organism.) Also, there is a considerable body of evidence from work with infants that indicates that the representation of objects, as items *independent* of the infant's motor activity involved in touching or grasping them, is only gradually and laboriously developed and externalized (Piaget, 1937) [see also Brainerd, this volume, next issue, forthcoming].

The main problem Bindra sees in the formulation of "specific principles of response production" springs from two sources. First, his rigid and rather arbitrary dichotomy between fixed "motor programs" that last half a second or less, and "long sequences of acts" for which, he says, "there is no lasting 'memory' basis." Since he considers the short acts to be rigidly fixed and stimulus dependent, he needs an altogether different mechanism for the construction of "act-assemblies." Second (and this, I believe, is due also to his profound involvement in the traditional behaviorist conceptualization), he completely disregards the principles of control theory and hierarchical feedback systems (Craik, 1966; MacKay, 1966; McFarland, 1971), which, ever since the pioneering work of Craik and Wiener in the 1940s, have given us not only theoretical but also functioning models of goal directed motor programs and act-assemblies that operate on the basis of continuous, situation-specific adaptation.

In short, from this reader's point of view, Bindra's proposal is still an attempt at "minor revision" that does not get away from the traditional tenet that living organisms are passive receivers of stimuli to which they react according to pseudo-mechanical principles. A detailed analysis of what is covered by the term "gnostic organization" would, I believe, reveal the indispensability of induction and of inductively built-up representations of desirable and undesirable states and events that can then serve as goals in the assembly of adaptively modifiable behaviors.

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**Neural mechanisms and Occam's razor.** Many readers will be highly prepared to respond favorably to Bindra's paper; it is in part a sermon to the converted on the shortcomings of classical S-R association theories of learning. Everyone who has watched unrestrained animals has stories to illustrate the fact that laboratory animals frequently act as if they were more highly motivated to gain information about their environment than to reduce homeostatic drives; for example, I recently observed that mice that had lost 30 per cent of their initial body weight during two days of water deprivation spent much more time exploring than drinking in an unfamiliar enclosure where water was freely available. More formal grounds for skepticism regarding S-R theories were provided to many in graduate school where one was entertained with recitals of the many deficiencies of S-R learning theories, of defects like S-R theorists' inability to provide a plausible account of motor equivalence, latent learning or sensory preconditioning.

There are thus experiential and theoretic reasons why the reader will be sympathetic to a theory like Bindra's that emphasizes sensory-sensory correlations and deemphasizes specific sensory-motor linkages, as seems necessary to deal, for instance, with the phenomenon of motor equivalence.

Bindra does more than this. He also presents in considerable detail a model for associative learning in terms of changes in sensory, perceptual, and motivational processes. In addition, he presents theoretical arguments to show that research in six areas in behavioral and neural science is desirable to assess the validity of his theoretic model.

I have, however, two critical comments on Bindra's paper. First, it is unfortunate that Bindra has chosen to call responses like eating, gnawing, licking, and biting (Figure 1) "consummatory" acts. Responses that are consummatory in their appropriate context may appear as displacement or instrumental responses in other contexts. The execution of consummatory acts is sometimes neither necessary nor sufficient to terminate a behavioral sequence. Bindra's use of the term consummatory response is particularly paradoxical since some ethologists have brought forth evidence that exposure to consummatory stimuli is more important than execution of the consummatory response in punctuating sequences of behavior.

Second, I suspect it will be a long time before definite tests of the neurological mechanisms for learning implicit in Bindra's theory can be made; it is unlikely that the black boxes and their functions in the theory will be spatially segregated in the central nervous system. It is, however, likely that behavioral phenomena recalcitrant to explanation in terms of an S-S model will appear. Consider the compensation to distorted sensory input that occurs in mammals. Human beings adapt rather quickly to prisms that distort their visual input, but only if they are allowed to be active while wearing the distorting lenses; adequate recalibration observed. With what is the altered visual input associated to permit compensation for the distorting lenses? With proprioceptive feedback from the muscles? Or with some central command center for the motor system? It is rather difficult to obtain disjunctive results from human subjects, but Taub's (*op. cit.*, Hulse, this *Commentary*) studies of monkeys subjected to virtually total destruction of the proprioceptive nerves from the limbs and trunk show that a very high degree of motor control may survive elimination of proprioceptive information from the body [see Roland et al., this issue]. It thus seems possible to hold a modified S-R explanation of compensation for distorted sensory inputs (Held, 1968).

I do not allude to the experiments on compensation to distorted sensory information to impugn Bindra's theory in respect to the phenomena he attempts to explain, but only to raise the possibility that natural selection need not have acted parsimoniously in respect to learning mechanisms. Conceivably, there are different mechanisms for different forms of learning and both S-S and S-R may apply to specific behavior domains.

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**Bindra's theory: some successes and precursors.** Over the past ten years, Bindra has been developing a cognitive-motivational theory of learning. The

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present paper describes the current status of this important approach to the study of learning and behavior. The central tenets of Bindra's theory are that an organism forms an internal representation of repeated external stimuli and that particular motivational and stimulus conditions combine to produce specific autonomic and somatic reactions.

These two tenets are closely allied with the fundamental distinction between learning and performance. Speaking to this distinction, Bindra first holds that learning involves the elaboration of stimulus-stimulus associations; he explicitly excludes response-stimulus association as a factor influencing behavior change. Secondly, Bindra holds that indirect activation of a central motivational state by a signalling stimulus provokes particular autonomic and skeletal reactions in relation to the response-eliciting properties of the external signal. The emphasis that Bindra places on response elicitation counters the frequent criticism of cognitive learning theories that such notions leave the organism behaviorally rooted and "buried in thought."

Clearly, any theory that disavows the importance of response-stimulus association in behavior modification represents a dramatic departure from prevailing contemporary thought. Such a radical reformulation seems indicated by an extensive series of experiments on the conditioning of directed locomotor and manipulative behaviors using the stimulus pairing operations of Pavlovian conditioning (Hearst & Jenkins, 1974 *op. cit.*, see also Jenkins, this Commentary).

As originally reported by Brown and Jenkins (1968), hungry pigeons learn to approach and peck at a small key whose illumination signals the response-independent delivery of food. Such "sign-tracking" seems quite difficult to explain on the basis of response-stimulus association. First, the original response most contiguous with food delivery is not the one that ultimately becomes most prominent. Initially, the pigeon turns toward and looks at the key light signal. Later, it begins to spend more time in the vicinity of the key. Finally, it comes reliably to approach and peck the lighted key. These progressive changes in responding to the lighted key cannot be readily accounted for by response-stimulus association; however, the changes can be explained by Bindra's more flexible formulation, according to which the conditioned response "is a fresh construction each time it occurs," depending upon "the momentary effectiveness of the various eliciting stimuli in the situation." Second, vigorous pecking of the key light signal arises and persists even when pecks prevent grain delivery (Williams & Williams, 1969); contrary to the premise of response-stimulus association, but consistent with Bindra's stimulus-stimulus association tenet.

Historically, stimulus-stimulus association is connected with the classical research of Pavlov (1927) and with the presumably limited number of effector systems that are modifiable by stimulus pairing operations. Of course, Pavlov never felt that the conditioning principles that he discovered applied only to glandular and visceral reactions, and research on sign-tracking certainly supports Pavlov's original suppositions as to the generality of his conditioning principles. Another historical misconception surrounds the degree to which behaviors conditioned with Pavlov's procedure are mere "replicas" of unconditioned responses. A careful reading of Pavlov's (1927, p. 22) conception of "stimulus substitution" reveals that such substitutability applies only to artificial conditioned stimuli (such as lights and buzzers) standing for natural conditioned stimuli (such as the appearance and odor of food). The diverse motor and secretory reactions to conditioned stimuli that Pavlov and others (Zener, 1937) observed seem quite consistent with Bindra's account of conditioning and performance in which "the new learned response is determined by the type of motivational state generated by the conditioned stimulus and by its detailed stimulus features in relation to the total situation." Bindra's theory also accommodates the important finding in sign-tracking that approach and pecking emerge to localized visual stimuli even when the reinforcing stimulus elicits neither of these behaviors (Wasserman, 1973; Woodruff & Williams, 1976). And his account nicely embraces the finding that approach-with-contact arises to a localized stimulus paired with an appetitive reinforcer, whereas escape-avoidance arises to the same signal when unpaired with an appetitive reinforcer (Wasserman, Franklin, & Hearst, 1974), for according to this view, the most general principle determining response production seems to be that "an animal will move toward an eliciting stimulus that is of greater appetitive motivational valence or of lesser aversive motivational valence than other eliciting stimuli in that situation at that time."

Anticipating that others will criticize Bindra's theory for the difficulty it cur-

rently has in explaining response differentiation (Notterman & Mintz, 1965; see also Notterman, this Commentary), learned helplessness (Maier, Seligman, & Solomon, 1969), and the instrumentalization of autonomic behaviors (Miller, 1969), let me conclude with some historical remarks pertinent to the inclusion of instrumental conditioning within the domain of stimulus-stimulus association.

Although not acknowledged by Bindra, this suggestion was made several years ago by Birch and Bitterman (1949; see also Bitterman, this Commentary). These authors, too, laid great stress on only one form of learning, which they termed sensory integration - "a process of afferent modification which depends primarily upon contiguity of stimulation and which can occur in the absence of need-reduction" (p. 305). How does this approach deal with instrumental conditioning? "In the Skinner box, for example, the rat experiences food only in the vicinity of the lever. As the lever and other local stimuli acquire food-properties, the animal orients to these stimuli and the only responses which appear are those which they elicit. The critical features of the stimulus situation may in this way be differentiated out of the mass, and the range of behavior accordingly restricted" (p. 306). This earlier construction is then quite similar to Bindra's without the trappings of incentive motivation.

Finally, although Skinner's classical (1938 *op. cit.*) concept of the operant explicitly omitted reference to eliciting environmental stimuli, his earlier (1935) distinction between operants and respondents relied, in part, on the nature of supporting environmental stimuli. Respondents "require no external point of reference in their elicitation or description," whereas operants "require points of reference for their elicitation which are not supplied by the organism itself, but by the stimulus" (p. 68). Bindra expects no important functional difference between learning under these different stimulus conditions, and research in sign-tracking supports this expectation.

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**Behavioral flexibility in learning situations: adaptive or adaptable behavior?** The issues to which Bindra's paper is addressed concern the mechanisms of adaptive action by laboratory animals in typical learning

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situations Although the paper is entitled, "How Adaptive Behavior is Produced," it might be more semantically appropriate to entitle it "How Adaptable Behavior is Possible." The term "adaptive" usually refers to biological fitness arising from selection processes; more specifically, the selection processes operate upon survival and success in reproduction and social organization rather than on learning ability. It seems to me that Bindra is dealing with the processes that enable laboratory animals to show behavioral change when the situation seems to demand it. In that respect, Bindra is concerned with the behavioral adaptability and flexibility of individual organisms rather than with adaptive behavior *per se*. Learning may be a mode of adaptation, but it is only one of many alternate modes. For this reason, I wonder whether Bindra's perceptual-motivational framework is appropriate for "the study of behavior at all levels" as tentatively promised. However, in dealing with an explanation of "animal behavior in typical learning experiments," Bindra's framework is most impressive.

More specifically, this paper deals with the explanation of what Bindra (1976 *op cit.*) has called "intelligent behavior – the types of purposive, foresighted, and innovative actions that characterize the biological adaptation of man and related animals." The approach deals with the psychology of learning from the perspective of the "general process" rather than the "adaptive-evolutionary" paradigm. Bindra is more concerned with the mechanisms of intelligent behavior common to all mammals than he is with the special adaptations required for "real-world learning" (Rozin, 1977). The Darwinian adaptation-evolutionary viewpoint emphasizes adaptation and selection for phenotypes that deal most effectively with environmental problems. Much of real-world learning consists of specialized adaptations; examples include food selection (Barker, Best, and Domjam, 1977), species recognition (Brown, 1975) and song-learning in birds (Marler, 1970). I raise the issue of paradigms not as a criticism of the goal that Bindra is attempting to achieve but as an attempt to clarify the perspective of his particular framework.

It appears that Bindra's framework can account for many aspects of learning that have eluded the traditional learning framework. Problems of motor equivalence and behavioral flexibility are adequately handled in Bindra's framework. I think that his framework can also deal with the phenomenon of "exposure learning" suggested by Sluckin (1964). In that respect, this framework can deal with many features of imprinting in the laboratory setting, and may thus suggest some mechanisms to account for a phenomenon usually studied by researchers interested in specialized adaptations. However, I am not sure how Bindra would explain the results of the Krieckhaus (1970) and Krieckhaus and Wolf (1968) demonstrations of the rat's "innate recognition" of sodium despite the fact that prior associations or correlations between the stimulus (sodium that is ingested) and the need (the state of sodium depletion) were minimal or nonexistent.

Bindra has developed a model of behavior change that prescribes the necessary elements and the rules governing their operation in the system. Within this framework, predictions can be made about the adaptable behavior of mature, adult mammals in specified situations. I would be interested to see Bindra's discussion of developmental aspects of learning in future papers. The developmental emphasis of Bindra's (1959) earlier conception of motivation has not been emphasized in his present treatment.

I would also like to see how Bindra's framework would deal with individual differences in learning ability. I have recently summarized the data on infantile stimulation and appetitive learning and concluded that performance differences between handled and nonhandled rats are not solely due to learning (Wong and Wong, 1978). When learning differences appear to be evident, the outcome is due to emotionality differences. Although handled rats seem to be more efficient than nonhandled rats in coping with environmental contingencies in the laboratory, their superiority does not appear to be due to differences in contingency organizations of the sort discussed in Bindra's paper. How would Bindra conceptualize the effects of environmental enrichment on the facilitation of problem-solving skills in mammals? How would he deal with emotional reactivity differences and their effects on performance?

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*In defense of descriptive behaviorism, or theories of learning still aren't necessary.* Bindra offers what he calls a new framework for the explanation of adaptive behavior, one based upon perceptual learning and the motivational role of stimuli and not another version of a response-reinforcement model. According to Bindra, a new approach is needed because reinforcement theories of a descriptive nature, such as Skinner's, or those of a more hypothetically structured kind, such as Hull's, contain major weaknesses. The tenets of a hypothetical reinforcement model that Bindra questions are that learning depends upon contiguity between a response and a reinforcer, that learning can be implicitly or explicitly attributed to the selective strengthening of a connection between neurological representations of a stimulus situation and a response, and that evoking a given neural stimulus representation gives rise only to the previously connected response.

The problems that Bindra sees with an empirical or descriptive reinforcement principle are that learning sometimes can occur without the reinforcement of any specific response, sometimes even without the occurrence of a response at all, that some forms of responding, including instinctive behavior, remain unanalyzed, and that some contemporary researchers are unsatisfied with the adequacy of a simple reinforcement account for all the details of responding, even within the framework of an operant conditioning experiment. But Bindra also identifies a more fundamental flaw in a descriptive reinforcement principle, which is that such an approach offers no real explanation of how adaptive behavior is put together at all.

Before considering Bindra's alternative model, it is important to separate his epistemological criticism of a descriptive reinforcement principle, that it offers no valid explanation of adaptive behavior, from the more empirical one, i.e. that it is somehow incomplete or inaccurate. Presumably a continuing strategy of empiricism could come to grips with the facts Bindra cites as evidence against the descriptive account by stating more or different functional relationships between environmental variables and the various behavioral events under question, such as adjunctive behavior and so forth. As a matter of fact, the current criticism of Skinner's operant behaviorism, which suggests that the approach depends upon a single and all-inclusive reinforcement principle, is simply wrong (Skinner, 1977). But even if such criticism were correct, one could conceivably alter or expand the number of functional relationships within the system without invoking the central nervous system. In other words, Bindra's criticism of current reinforcement theories, empirical and hypothetical, does not necessarily imply that the only improvement lies in the development of a different hypothetical-neurological theory; one could instead look for more, or more accurate, descriptive statements of environmental-behavioral relationships.

At this point, we reach an intellectual impasse. To Bindra and many other learning theorists, an improved or expanded network of descriptive principles still would not provide the kind of explanatory theory they are seeking (Staddon, 1973). To some behaviorists, however, the description of functional relationships relating manipulable or controllable environmental events to observable behavioral outcomes does constitute explanation, and in fact the most useful kind of explanation of behavior that psychology can provide (Baum, 1974; Skinner, 1953, Ch III, *op cit.*). And Bindra is correct, too, in saying that such functional relationships do not imply any within-organism biological or neurological interconnections. Reinforcement is no

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more than a name for a class of behavioral control methods that collectively depend upon the manipulation of consequences for their effect, a set of instructions to scientists about how to control behavior (Catania, 1973 and this *Commentary*; Skinner, 1969, *op cit*, Ch 6; Wood, 1976) That this class may be large or small or potentially replaceable by more effective procedures is another issue This kind of an approach to scientific explanation is not unique to Skinner and other operant behaviorists, of course; it also was the strategy recommended by Bacon, and for many of the same reasons (Kaufman, 1968) It emphasizes control, not contemplation; engineering, not hypothesizing It is an approach to science that by its very nature leads directly to social application, just as it was intended to in *New Atlantis* or *Walden Two*

An alternative strategy, which emphasizes formal theory construction and research designed to test hypotheses, has been followed many times, too, particularly late in the development of certain fields such as physics However, such success as this approach has had has led some philosophers and scientists to identify the hypothetico-deductive approach as the scientific method, with the unfortunate result that newer sciences in the behavioral and social fields have tended to adopt it almost without question Skinner (1956) argues that this emphasis on theory construction and hypothesis testing fails to recognize and give appropriate consideration to the earlier stages in the development of physical sciences where research necessarily focuses upon experimental control and fact-finding In asking whether theories of learning were even necessary in psychology, Skinner (1950, *op cit*) opposed any attempt to explain behavioral facts "which appeals to events taking place somewhere else, at some other level of observation, described in different terms, and measured, if at all, in different dimensions," and specifically listed mentalistic, physiological, and conceptual-neurological theories as examples He argued instead for research designed to relate orderly behavioral changes to manipulable environmental variables, an approach that would become appropriately theoretical only with the development of a formal system of minimal terms to represent comprehensively the collected descriptive relationships

In any event, whether one attempts to relate behavioral facts in terms of conceptual theories that depend upon explanatory models in other dimensions (such as, for example, pexgos), or simply attempts to systematize the data at the observational level, the facts one works with must be firmly established We have seen in the last several years the emergence of data that apparently test the reinforcement account but, as I have argued above, a descriptive reinforcement principle is essentially unassailable; appropriately understood, it is not a theory or model at all, but rather a method of behavioral control And, as even Bindra noted, it is a method that has worked quite successfully outside the laboratory as well as within it The question now is whether these contrary data need a new conceptual-neurological explanation, as Bindra offers, or whether closer attention to strictly environmental variables can result in satisfactory descriptive accounts for their occurrence? It is certainly possible to reevaluate empirically the adequacy of the reinforcement principle without positing gnostic cell assemblies (Honig & Staddon, 1977)

The important behavior to consider in choosing between pursuing research intended to identify functional relationships and research that tests hypothetical-neurological models seems to me to be that of the scientists and not the subjects And without prejudicing the argument by using the term "reinforcement" as descriptive (explanatory) of the events of the last few decades of experimental psychology, one still sees formal learning theory as being about where it was in the 1940s, at least for all practical purposes, regardless of whether cognitive terms are now beginning to appear in some formulations This situation can be contrasted with the exploding ramifications of operant or descriptive behaviorism, where today, for example, virtually every school teacher can incorrectly use the term "negative reinforcement" If, as Guttman (1977) commented, a single reinforcement principle is almost commonsensically inadequate, wherein lies the strength of the notion? It is, quite simply, that reinforcement is a behavioral control method, a way of dealing with behavior, and not just, or even, an explanatory model It is applicability, not theoretical refinement, that has virtually enthroned the reinforcement principle in behavioral psychology

How then to react to Bindra's new model? I suggest that replacing one hypothetical-neurological account of adaptive behavior with another is as likely to be useful as is strengthening a house of cards by calling for a new deck It may appear to some academic learning theorists that emphasizing

perceptual-motivational constructs instead of stimulus-response ones, whether couched in neurological terms or not, is a big shift in intellectual focus Perhaps it is, but unless such a revision produces concrete methodological improvement in the control of behavior, the change is illusory If one seeks to dethrone reinforcement, look for better ways to control behavior, not "explain" it

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## Author's Response

by Dalbir Bindra

### A behavioristic, cognitive-motivational, neuropsychological approach to explaining behavior.

The commentaries prompted by my paper vary greatly in tone, from the kind and supportive to the rather pungent and grudging, yet they are remarkably similar in the issues they raise for discussion. This makes it easy to concentrate on the substantive points and to deal with the commentaries en bloc instead of singly. My task is also made easier by the fact that frequently a question raised by one commentator has been answered by another; I trust the commentators will read one another.

### What is new?

According to a proverb often quoted in academic circles, a person goes through three stages on the way to accepting a new theoretical position: (1) "This is nonsense." (2) "There is nothing new in this." (3) "I have been saying this all along." While each of these three attitudes is well represented in the above commentaries, the second is perhaps the most common. The usual form it takes is the statement that "what Bindra proposes is in essence the same as . . . (insert someone else's earlier proposal)." A problem with judgments of sameness is that they depend on the momentary purposes of the observer; any two things can be called "same" or "different" depending on the aspect under consideration and the required level of discrimination; that is, on the criterion of sameness. The concepts of atom (Democritus), monad (Kepler), and wave-particles (Schrödinger) are the same if one considers them merely as examples of atomism, but they are different if one considers the theories of matter of which each is a

part – the theoretical framework or “paradigm” within which each was elaborated. Clearly, it *can be* correct to say that the three concepts of matter are the same if one selects a lax rather than a stringent criterion of sameness.

So Bitterman can be correct in saying that Bindra’s proposal is the same as Mowrer’s if he chooses to compare the two formulations on the importance both ascribe to motivation and to ignore their differences on such points as the nature of motivation (central neural state versus feedback from peripheral visceral commotion) and the determination of the initial response (determination by motivational state and eliciting stimuli versus scanning of response repertoire). But Mowrer and Teevan, with at least equal justification, say that Bindra follows in the footsteps of Tolman, and Wasserman says that Bindra’s position resembles that of Pavlov (1927) and of Birch and Bitterman (1949)! Similarly, Bolles is not wrong in saying “Bindra is a Guthrian,” nor Logan in saying that he (Logan), not Bindra, is a Guthrian. Nor is Stein’s “It’s all been said before by Lashley, Bartlett, Hebb . . .” necessarily a hyperbole. None of these statements is really untrue – or wholly true.

This historical scholarship raises no useful issues whose discussion would advance our understanding of adaptive behavior. Certainly, the types of similarities explored by the commentators would make important components of a lesson in the historical continuity of scientific ideas, as well as a reminder of how slow the progress in understanding can be. But it is doubtful whether such historical considerations have any useful part to play in the evaluation of the validity of a framework *as given*.

The task that the commentators and I share jointly is that of examining the validity of what I have, to give it a convenient label, called a “perceptual-motivational” framework, as it stands today. Since science has no absolute scale of validity (i.e., absolute truth is not known), we can only evaluate the validity of any given framework relative to that of other current frameworks that bear on the explanation of the same phenomena. The most meaningful comparison frameworks for the present task are, of course, those of response-reinforcement and cognitivism. [See Haugeland et al., next issue.] One criterion especially relevant to the evaluation of a broad framework is the extent to which workers in other areas have found it useful or developed parallel concepts. In this regard I am gratified to find support from Rosenblatt, representing ethology, Staats, representing social psychology, and Murray, representing clinical psychology.

#### **What of operant behaviorism and systems modeling?**

One subclass of the response-reinforcement frameworks is Skinner’s operant (or descriptive) behaviorism. Skinner and his school profess no interest in explaining behavior, that is, discovering how adaptive responses are produced or by what underlying processes a response is put together. Operant behaviorism limits itself to empirical analysis and aims at formulating exact descriptive principles of behavior and reaping the benefits of behavior control that such knowledge makes possible. Since Skinner and his followers have chosen not to get involved in the problem of explaining behavior, my discussion of *how* adaptive behavior is produced has no critical bearing on what they are trying to achieve.

Catania and Wood recognize this. Catania observes, correctly, that my paper has little to do with the “framework of the contemporary experimental analysis of behavior” and that my passing (historical) references to Skinner are quite peripheral to the objective of my paper. Epstein, Mazur, Navarick and Salzinger have apparently missed this point. These commentators have come out in a fighting defense of operant behaviorism – and of Skinner! No such defense is needed. Operant behaviorism is unassailable as a technology – and a useful technology – of behavioral control; and Skinner (even undefended) will remain one

of the loftiest figures of our discipline. As I pointed out in my paper, the operant behaviorist’s view of the task of psychology is not wrong; it is only limited in that it disregards the basic scientific problem of explaining behavior in terms of underlying processes. Knowledge of underlying processes implies the capability of control, but control may be achieved without that knowledge.

In the domain of the current cognitive frameworks there is also a subclass not directly concerned with explanation. These may be described as “systems-modeling” frameworks. These frameworks, though cognitive in the sense that they consider knowledge to be relatively independent of responses, are described in the language of engineering systems, information-processing and artificial-intelligence. It should be evident that modeling cognitive processes in terms of engineering functions (e.g., storage, filter, feedback, feedforward, scanner, subroutine, retrieval) is not the same as searching for the underlying processes themselves. The engineering functions in the systems are analogical descriptions, not descriptions (more or less hypothetical) of the real underlying processes. Thus, like operant technology, cognitive systems-modeling frameworks likewise fail to aim at explanation. [See Pylyshyn et al., this issue.]

Dennett and von Glaserfeld have overlooked this resemblance between operant behaviorism and systems-modeling frameworks of cognition. While agreeing with me that operant behaviorism evades the problem of explanation, they nevertheless advocate systems models, failing to see that the actual brain processes underlying cognition are not the same as the engineering models our brains can formulate. Craik, whom von Glaserfeld mentions approvingly, knew this well. Craik wrote “. . . we want to know how our brains work – not to explain other things on the basis of the fact that they work” (Craik, 1966, p. 75, *op. cit.* in von Glaserfeld, this Commentary). Dennett’s design engineer may be able to put together certain intelligent functions, but that will not tell us how the *brain* produces intelligent actions, consciousness, or intentionality.

Thus, the perceptual-motivational framework I have proposed is in competition with neither operant behaviorism nor systems models of cognition; those are altogether different enterprises. My framework is in competition with those other reinforcement and cognitive frameworks and formulations that address themselves to the problem of explaining adaptive behavior in terms of the real (brain) processes that produce it.

It is a source of some confusion that operant behaviorists portray themselves as the sole custodians of behavioristic psychology, and systems modelers of cognitive functions seem to regard themselves as the exclusive executors of cognitive psychology. This narrow view of behavioristic and cognitive psychology results in my being regarded by Salzinger as anti-behavioristic and by Dennett as not cognitive enough. From their perspectives, these commentators see operant behaviorism and systems-modeling frameworks as two opposing and jointly exhaustive approaches in psychology. They have not understood the behavioristic and cognitive movements in psychology. These are neither contradictory nor do they cover the whole domain of the discipline. One can indeed consider me to be, like Hunter, Tolman, and Lashley, a cognitive psychologist in that I believe the processes representing knowledge of the world to be somewhat independent of sensory-inflow and motor-outflow processes and to have a life of their own not rigidly tied to particular responses. And like these theorists, I am also a behavioristic psychologist in the sense that I believe the processes underlying the phenomena of intelligent behavior and consciousness (whether we call the processes mental, neural, or something else) to be discoverable only by dispassionate study of, and careful speculation based upon, behavioral data; there is no direct introspective window on the mind. Cognition and behavioristics can go hand in hand.

### Why the neurologizing?

It is a fair guess that all the commentators are monists and believe that the development of the unified view of nature to which science aspires would require the phenomena of the mind (intelligent behavior and consciousness) to be explained in or translatable into neural terms. Thus, it would be surprising if most scientists in the fields of the behavioral and brain sciences did not look upon a neuropsychological bridge between the two domains as a goal – if not the eventual destiny – of the two fields of investigation. From this perspective, the genuine explanation of a phenomenon at the behavioral level is achieved only when it is shown, at least in principle, how it is translatable into the phenomena at the next lower level of analysis (i.e., neurological). [See Haugeland et al., next issue.]

It was this orientation that prompted me several years ago to look carefully at the newer findings in the neurosciences and to try to formulate a coherent set of neuropsychological constructs for explaining the varied phenomena of adaptive behavior – from instincts to remembering and problem solving. The result was my monograph, *A Theory of Intelligent Behavior* (1976), in which I explain how the evidence from neurophysiology, clinical neurology, and behavioral investigations dictates the postulation of some such concepts as pexgo, act-assembly, contingency organization, central motive state, and the like. In the paper now under commentary it was not possible to explain the reasons for these new constructs, and their exact meaning and implications may not have been fully understood by some commentators. I apologize for this, and am grateful to those who, like Booth, Hilgard, Klingsporn, and Soltysik were able to examine the monograph as well.

Pribram says that in developing my theoretical scheme I have failed to take into consideration existing knowledge about the nervous system. This is obviously not the case (Bindra, 1976, chs. 2, 3, and 4). What is true is that the constructs I have formulated from my reading of the neural and behavioral evidence are different from those that Pribram (1971, this Commentary) has suggested. In particular, the ways in which he and I handle motor organization, perception, and attention are quite different, and I have explicitly pointed out the shortcomings of his constructs. One shortcoming, from my viewpoint, is that he has not made a distinction between the experienced percept or image on the one hand and some earlier process like pexgo, which, together with different subsequent processes, might be the basis of a percept, an image, a memory, or a discriminative response. I think it might be profitable to discuss the relative merits of Pribram's neuropsychological constructs and mine, but his supposition that I have ignored the brain is wrong.

There are those who see little point to what they call "neurological speculations." Catania, Dennett, Gilbert, Hilgard, Ryan, Salzinger, & Warren have, each in his own way, questioned the value of neuropsychological constructs. The main concerns seem to be that such constructs are somewhat ad hoc, insufficiently constrained, with too many degrees of freedom, too tautological and "flimsy" to serve as useful "underpinnings" of behavior, or that they are premature. These views, I submit (and the point has been made before, Hebb, 1977), are characteristic of those who look at the work of neurosciences from afar. Those who are not themselves involved in examining neurophysiological findings or choosing between alternative types of constructs on the basis of available neurophysiological evidence are likely to be oblivious of the constraints that neurophysiological evidence places on neuropsychological constructs; understandably, they tend to look upon such unfamiliar constructs as ad hoc or tautological. It should also be noted that just because a construct is derived only from behavioral evidence (e.g., short-term storage, reinforcement, association, drive) does not necessarily make it less vague or tautological than neuropsychological constructs. Since the latter are subject to

dual constraints (of neurophysiology and behavior), they have the possibility of becoming even more "firm" and meaningful than purely behavioral constructs. The prematurity argument is vitiated by the fact that description and explanation often go hand in hand, and to wait for impeccable description before starting a search for explanation is to wait forever. Further, as Booth and Killeen have pertinently noted, even the most empirical of concepts of operant behaviorists (e.g., stimulus, response) have some hidden tautological or theoretical base that, though it may be more familiar or implicit, is not necessarily more valid.

A general antipathy is evident in the commentaries to constructs that have some neural base or reference. My concepts of pexgo, act-assembly, gnostic-assembly, etc., have formed easy targets for derision and obloquy. The basis of serious criticism in most cases does not lie in considerations of the usefulness of the construct but in the general sentiment that either a new word is being used for something that already has a suitable name (e.g., pexgo for percept) or that the neural ring of the new word makes it appear to refer to a brain reality that is only speculative. The criticism is misguided. Pexgo is not the same thing as percept, and the reason for introducing the concept of pexgo is that such a construct is needed to separate a percept (the experience) from an underlying process. At an earlier time, distinctions were introduced to separate sensation from perception and impression, idea from image, memory from imagination, action from intention, and so on. The enterprise of refining experience and explanatory ideas has been going on for a long time and will continue. The question to ask in each case is whether the new construct refers to some entity, experienced or hypothetical, that is required for completing a description or an explanation. The question of the source of the name for the construct (emotion, quark, eidolon, etc.) is a separate issue; the name by itself does not, or should not, make a construct more or less useful. In the case of pexgo, the need for the construct became evident while considering what is known about the psychology and neurology of perception, and the name (presently excited gnostic organization) reflects this. Pexgo is not meant to replace either percept or hypercomplex cells, but it is meant to replace "template"; pexgo is needed to deal with the uniqueness of each perceptual experience. To criticize this construct because it is derived from two types of evidence or constraints (psychological and neurophysiological) instead of only the psychological is absurd and, theoretically, suicidal. The issue is the need for or value of a concept, not whether it has a mentalistic or a neural ring to it.

For an avowed antidualist, Booth's stand on neurologizing is puzzling. He hopes that psychology and neurophysiology will be married but is against their mixing; apparently, he would like them to stay in pristine isolation until the magic moment of wedlock, when, at once, all will be well. My view is that to be ideally united – married or not – they must grow into each other, and this requires constant give and take and "dressing one up as the other" to see how far the two bodies have become one. The unity of science will not be achieved by perfecting different bodies of knowledge in isolation, but by trying continually to link them and perfect them together. Attempts to picture what perception and meaning might be in neural terms have clarified the psychologist's confusion over whether perception without meaning is possible (yes, it is). And attempts to neurologize (McGill and Russian style) about language have guided neuroanatomical investigations. Booth has missed the point that advances in science come from many diverse and unexpected sources, and there is little point in putting a ban on neurologizing (or anything else) on the basis of a presupposition about what is or is not "the right approach" to the advancement of knowledge.

### What is incentive motivation?

Most students of behavior now seem to accept the idea that hedonic stimuli ("reinforcers" or "incentives") can generate (in-

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centive) motivation and that the latter can influence the production of responses. The question that remains an issue is whether hedonic stimuli, apart from generating incentive-motivation, also have response-reinforcing properties (i.e., capability of strengthening S-R linkages). Several commentators (e.g., Bolles, Hilgard, Rosenblatt) appear willing to consider the motivational function of hedonic stimuli to be an important one in response production, but none (with the possible exception of Booth and Wasserman) are willing to accept my position that all the behavioral effects of hedonic stimuli are of motivational origin and that, therefore, hedonic stimuli serve no response-reinforcing function, in the sense of strengthening S-R linkages.

As I pointed out in my paper, the distinction between the response-reinforcement and incentive-motivational roles of hedonic stimuli has not been an easy one to understand. It is not surprising, therefore, that the remarks of a few commentators show some confusion on this point. Gallistel and von Glaserfeld, for example, talk about the connection between "motivation" or "drive" and "satisfaction" or "pleasure" (thereby reviving the Hullian concept of drive-reinforcer connection) and then wonder about the need for the concept of incentive-motivation. They miss the point that the incentive-motivation concept has been designed to replace both the drive and the reinforcer (or satiety) concepts, and is not meant to be synonymous with either. They view "motivation" (drive) as something that determines "the kind of consuming the organism . . . is driven to look for." But it is known that drives (e.g., "hunger" or the organismic condition produced by food deprivation), in the absence of environmental (unconditioned or conditioned) incentive stimulation, fail to produce goal-directed responses (see Bindra, 1968, 1976, Bolles, 1972; *op. cit.*). Thus the factor that is critical in the organization of goal-directed responses, what the animal "is driven to look for," must be something other than the organismic state per se. The essential point of the incentive-motivation concept is that it is the inherent or acquired hedonic (affective) properties of certain (incentive) stimuli that generate the type of motivational factor, *central motive state* (cms), required for producing goal-directed responses. The generation of a cms is influenced by organismic conditions, but the goal that is pursued is determined by the cms. Gallistel and von Glaserfeld's idea that internal "motivation" first determines what satisfactions are desired, and that some system (or homunculus) then matches the satisfactions desired to appropriate memory or action, amounts to only a statement of the problem. The framework I have outlined includes a possible mechanism by which particular central motive states might influence the production of goal-directed responses. Several other points raised by Gallistel and von Glaserfeld, as well as by Booth, Bitterman, Catania, Ryan, and Wong, have been dealt with in some detail in earlier statements (Bindra, 1968, 1969, 1974, 1976, *op. cit.*).

Pribram's remark that response-reinforcement and incentive-motivation hypotheses concerning the role of hedonic stimuli in behavior are "complementary views of the self-same process" also points to some confusion about what the distinction between the two views really is. Pribram notes that the response-reinforcement hypothesis is couched in response terms and the incentive-motivation hypothesis in stimulus terms, and then, observing that both stimulus and response are implied in response production, concludes that the two hypotheses refer to the same underlying processes. Though the hedonic consequences of adaptive responses are achieved by both stimulus-related and response-related processes, it does make a difference whether the mechanism of such responses involves the linking of hedonic stimuli to responses or to stimuli. The two postulated mechanisms are quite distinct and would involve different neural circuitry. Pribram may be correct in the sense that both mechanisms may operate, but he is clearly wrong in suggesting that they are complementary aspects of the same underlying processes.

Ryan has asked how I would deal with the problem of human

motivation. Hilgard's point about "cold" and "hot" cognitions provides me with a way to indicate a general line of reply. In my scheme the priming of perceptual representations (pexgos), on which the response ultimately depends, arises from two sources: priming from knowledge or cognitive structures (contingency organizations) and priming from motivational states. Since the latter frequently depends on the former (conditioned motivation), whereas the former (cognitive priming) need not depend on the generation of any specific motivational state, it is reasonable to distinguish between "cold" and "hot" cognitions. I agree with Hilgard that human actions, decisions, and so forth, may be made mainly based on cold cognitions (see Bindra, 1976, *op. cit.*, Chs. 13 and 14), but I am inclined to think that when motivation (affection, emotions) is involved in human actions, the motivational arousal remains dependent on organismic conditions even when the conditioned incentive signals belong to the cool, second signalling system, or language.

### **How does motivation influence response?**

Flynn has examined my proposal that motivation (cms) determines response production by influencing the latter's perceptual aspects rather than by a direct control over motor outflow. He cites evidence showing that hypothalamic stimulation can have an effect on sensory-perceptual mechanisms; this supports my view. However, Flynn considers too sweeping my denial of a direct motivational influence on motor outflow. The difference of opinion here may be more apparent than real.

Two problems make a clear answer difficult. One is that the facilitation of an observed response (whether a reflex, movement, or consummatory action) by a motivational intervention does not by itself tell us whether the locus of the facilitatory influence lies in sensory structures, motor structures, or both. A related problem is the lack of agreement over what structures are to be called motor: Sperry (1952) once remarked that producing motor outflow is what the brain is for; whereas Pribram calls the motor cortex the "sensory cortex for action"! I have taken the conservative position that only sites whose stimulation leads reliably to the production of particular movements ("movement commands") are to be called motor (see Kupfermann & Weiss, this issue). The points of origin of such movement commands ("sensory-motor coordination organizations") receive topographically differentiated input from current sensory inflow (exteroceptive, postural, proprioceptive), as well as from perceptual and motivational structures. The question then is whether input from motivational structures is on presynaptic afferent neurons or on postsynaptic movement commands. I am inclined to think that all extrinsic neural influences at this level are likely to be on presynaptic processes; that is why I say there is no direct motivational (or cognitive) influence on motor outflow.

Flynn's argument that there may be a direct motivational influence on motor outflow rests on his findings that (a) stimulation at certain hypothalamic sites can facilitate *reflexive* components of attack, and (b) that this facilitation effect is seen only when the reflex is elicited on the side contralateral to that of hypothalamic stimulation. However, though neat and important, these demonstrations do not unequivocally answer the question of where in the reflex arc the motivational influence operates: They only show that the neural projections involved are highly specific topographically. Neither the reflex nature of the responses nor the lateral specificity of motivational influence dictates that the effect is on the postsynaptic movement commands. I believe that an examination of my account of sensory-motor coordination organizations (Bindra, 1976, *op. cit.*, Ch. 3) would clarify the context of my proposition that motivation does not influence motor outflow directly and would show that the difference between Flynn's position and mine may be a difference only of terminology.

Booth's idea that "any perceptible bodily state" can serve as a

partial determiner of response output is intriguing but a bit obscure. I agree with Booth's (1977) proposition that conditioning plays an important part in satiety and appetite; in this case the mechanism of conditioning is readily understandable. But the mechanism by which a bodily state (what I call "organismic condition") may determine response output is not obvious. One question is whether a given organismic condition (e.g., a hormonal change or gastric distension) influences motor outflow by itself, as a cue, or by producing a change (through its interactions with some incentive stimulation) in a central motive state. Another question is whether the behavior induced by a certain organismic condition can per se be meaningfully said to be goal directed in a way that is relevant to *that* organismic condition (see Bindra, 1968, *op. cit.*). It would be good to get further elaboration of Booth's ideas on these points.

### One type of learning or more?

The phrase "type of learning" is ambiguous. As shown in Table 1, *type* could refer to either the nature of linkage (associationistic or cognitive) or to the nature of things that are linked, that is, the locus of learned change (sensory-sensory, sensory-motor, or motor-motor). Traditionally, *type* has meant the latter, and arguments have involved the question of the "locus of linkage."

Commentators who have addressed this question - Bitterman, Black, Booth, Bolles, Hilgard, Jenkins, Kendler, Mackintosh, Mowrer, Soltysik, Wasserman - seem agreed that the formation of sensory-sensory linkages is important in learning and that such linkages, in the absence of the formation of new sensory-motor linkages, may be sufficient to account for a good many phenomena, including certain instrumental learning phenomena. This is a big step forward from the situation in the 1950s. However, with the possible exception of Booth, Soltysik, and Wasserman, these commentators are unwilling to abandon

the idea that sensory-motor linkages are necessary or that they play an important part in some types of learning phenomena. The general sentiment seems to be that it is "safer" ("nature may not have been parsimonious," "nothing is gained by denying S-R learning," etc.) to recognize both sensory-sensory and sensory-motor types of learning. This kind of gratuitous eclecticism is not sufficiently compelling to counteract the strong reasons (given in my paper) that make the idea of learning sensory-motor (S-R) linkages implausible.

Logan remains perhaps the only commentator who hopes to find the answer to the problems of learning and cognition wholly in terms of S-R learning. His approach is to concentrate on the micromolar features of behavior; He hopes then to show how these ultimate S-R elements can combine to yield intelligent behavior. Curiously, his path may land him very close to my position, for he is likely to be led to inquire as to the sensory and other determiners or eliciting conditions of each individual movement of which an animal is capable (see Notterman). And once the problem of all the different ways in which a movement may be produced - all the different functions it may serve - is posed, the untenability of learned S-R connections will, I hope, become apparent. Although Logan continues to work within the response-reinforcement framework, I think his empirical work may contribute a good deal to the elaboration of the perceptual-motivational framework.

Table 1 shows that most other commentators accept (at least) two types of learning, in the sense of two *loci* of linkage (sensory-sensory and sensory-motor). I differ from them in accepting two types of learning in the sense of two types of linkage *processes*, one associationistic, involving fairly direct connections, and the other cognitive, involving complex contingency organizations representing stimulus correlations. In my monograph (Bindra, 1976, *op. cit.*, Chs. 4 and 5), I have made the point that the processes underlying the development of (sensory-sensory) gnostic-assemblies, and (motor-motor) act-assemblies are different from those involved in the development of contingency organizations. In my paper I emphasized only S-S correlation learning (involving contingency organizations) because I was concerned mainly with the type of mammalian learning phenomena that have traditionally interested "learning theorists" and those concerned with animal behavior.

Warren has raised the problem of compensation to distorted sensory input (e.g., distorting visual input by prisms) and has suggested the possibility that there may exist two quite distinct domains of learning, each involving different processes. As explained above, I agree with this, in the sense of two types of linkages. Associationistic sensory-sensory linkages may be involved in the compensation phenomena, as well as in the acquisition of initial perceptual motor coordinations. I am also ready to entertain the possibility that in this type of direct-connection learning new sensory-motor linkages may be formed, but even though experience of movement appears necessary for compensation, and proprioceptive feedback (see Roland et al., this issue) appears irrelevant, I wonder whether the postulation of sensory-motor linkages will help explain the compensation phenomena. One reason for my doubt is that the movements required for achieving compensation need not be those of the limb involved in the test for compensation. I am also uncertain whether Pribram's idea of movement control by homeostatic mechanisms would be helpful here. Nor am I satisfied with my own account (Bindra, 1976, *op. cit.*, Ch. 3) of the compensation phenomena, and I welcome further discussion of it.

Table 1. Commentators' apparent preference for types of learning, defined by nature and locus of presumed linkage.

Nature of linkage	Locus of linkage		
	Sensory-sensory <sup>a</sup>	Sensory-motor	Motor-motor
<i>Associative:</i>			
Contiguity learning involving simple mediation (e.g., direct connections)	Bindra Jenkins Kendler Soltysik Warren	Black Catania Gilbert Jenkins Kendler Logan Notterman Soltysik Warren	Bindra Pribram
<i>Cognitive:</i>			
Correlation learning involving complex mediation (e.g., contingency organizations)	Bindra Bitterman Black Bolles Jenkins Mackintosh Mowrer Pribram Wasserman	Bolles Mackintosh Pribram	

<sup>a</sup>In the case of what is called response-outcome learning, the linkage is presumed to be between the sensory representation (percept or idea) of R (i.e.,  $S^R$ ) and the sensory consequences of another stimulus ( $S^X$ ), hence this is treated as sensory-sensory learning.

### Is the ideomotor concept tenable?

Black, Gilbert, Jenkins, and Soltysik have the misgiving that my account leaves the production of an instrumental response tied to environmental eliciting stimuli and their central representations (plexos), and thus negates the apparent internal or voluntary

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control of responding. Jenkins and Soltysik suggest that inner control, such as what might be based on memory representation of a kinesthetic pexo of a response (image or idea of a response), would save response production from the "tyranny of eliciting stimuli." It is not clear why they regard pexgos of environmental stimuli (e.g., visual pexgos) as exercising "external" control and pexgos of responses (e.g., kinesthetic pexgos) as exercising "internal" control, for in fact both types are equally central, and the sensory input on which each is based is equally peripheral. Quite apart from this terminological point, there are three reasons why I have found this concept of ideomotor action, subscribed to in various ways by James, Konorski (*op. cit.*), and Pavlov, to be unacceptable.

First, what Soltysik calls the tyranny of control by external stimuli may also just as legitimately be called the necessary guiding role of such stimuli in directing the animal's actions away from unexpected obstructions and toward particular environmental targets. If a response is to be efficient in terms of an environmental outcome, then it must remain, at least partly, under the control of either environmental stimuli themselves (early in training) or the priming of their pexgos (later in training). Ignoring this role of the pexgos of external stimuli would result in inefficient or misdirected action.

Second, no movement can have an address (i.e., be directed to an environmental target) without the integration of postural information, proprioceptive information from the responding limb(s), and information about the location (relative to the moving limb) of the external target. Elsewhere I have explained how these three sources of current sensory information may be integrated to produce an environmentally directed movement (Bindra, 1976, *op. cit.*, Chap. 3). Jenkins and Soltysik say that some responses are internally induced whereas others are externally elicited, but this is inconsistent with the obvious requirement of the integration of diverse (including external) sources of information in the production of *any* movement. Without such integration, a movement produced by a kinesthetic pexo would remain undirected and rigidly inflexible, and the tyranny of kinesthetic-pexo control would match the tyranny of eliciting stimuli. Both external control and internal control can be tyrannical if they are envisaged as the sole determiners of a response. In my scheme, this potential tyranny of rigid control is eliminated by ascribing a role to motivation in response production (in the determination of the form of the response) and by assuming that a response as such has no permanent representation (kinesthetic or other) but is constructed afresh each time it occurs by the unique combination of activated contingency organizations and primed pexgos, with the current sensory inflow, exteroceptive and interoceptive, always playing a necessary (though not necessarily decisive) role. The necessity of recognizing such dynamic control finds support in the commentaries of Booth and Fentress.

Third, perhaps the most serious shortcoming of the kinesthetic-pexo hypothesis is that it leaves unclear how – by what mechanism – the kinesthetic pexo leads to the production of the corresponding movement. As Mackintosh puts it, thinking of a response is not the same as executing it. The fact that instrumental responses can be performed, and perhaps acquired, even when the responding limb has been deafferented (see Bindra, 1976, *op. cit.*, Ch. 4), also makes it implausible that the ideomotor hypothesis can provide a workable explanation of instrumental responding.

Soltysik has raised another objection. This concerns the results of attempts at instrumental training of reflexive "transactional" acts, such as scratching. He argues that since such acts have strong eliciting stimuli, my account would make them readily trainable, say, when followed by food, whereas in fact such training is difficult and the animal's behavior frequently shows certain peculiarities. In attempts to train grooming in the rat, I have also observed the types of behavioral peculiarities noted by Soltysik. Consider training to scratch. If a dog happens to scratch

at a given moment, it must be assumed, in my scheme, that a certain motivational and eliciting stimulus (say, some minor tickle on the abdomen) has made the dog scratch a particular spot on its abdomen. The ES (tickle) would by itself be ineffective if the animal were in some other motivational state, such as fear, just as contact stimuli of food would not make the animal eat if it were at the moment afraid or sated. Now, suppose we arrange a training procedure such that as soon as the dog happens to scratch, the experimenter gives it its favorite food. If the animal eats the food, we would expect that the ES (tickle) would acquire the same motivational properties as those of food. We should then expect that the tickle stimulus (which "normally" elicits scratching) would now create a motivational state of eating, so that the response that emerges should be different from the one produced when the same ES appears in a different (noneating) motivational state. In other words, the initially pure acts of scratching would become motivationally "contaminated," to use Soltysik's term. This change in motivational state produced through the experimental procedure is sufficient reason for the relatively unstable and peculiar scratching shown by the animal following "instrumental training." Such motivational confusion is probably also a factor in the phenomenon of lack of "preparedness" for certain types of instrumental training (e.g., Shettleworth, 1975, *op. cit.*); M. J. Morgan (personal communication) has elaborated on this motivational interpretation of the preparedness concept. Can the ideomotor concept deal with these findings?

A related point raised by Soltysik is that, in apparent contradiction to my view, conditioned stimuli (CSs), when presented in an instrumental situation, have properties quite different from the properties of discriminative stimuli ( $S^D$ s) in instrumental training. I have already dealt with this question (Bindra, 1974, 1976, *op. cit.*), and shown, in terms of my formulation, why appetitive CSs often have suppressive effects on appetitive instrumental responses, whereas aversive CSs typically have facilitatory effects on aversive instrumental responses. Again Soltysik does not say how such findings might be approached in terms of the ideomotor concept.

### **How are skills acquired?**

The main question raised by Black, Jenkins, and Mackintosh concerns instrumental training. They seem to find it inconceivable that the shaping of arbitrary responses by the instrumental procedure can be accounted for without ascribing some role to "response-outcome learning." Note that they are willing to drop the response-reinforcement principle as such, but-maintain that the learning of the relation between (pexo or percept of) a response and (pexo of) an outcome is necessary for the production of instrumental learning and that (pexgos of) eliciting stimuli are not.

Jenkins' argument is twofold. He first argues that by combining the idea of response-outcome learning with certain cognitive concepts, it may be possible to deal with the problem of how the learning of a relation between the percept of a response and the percept of an outcome (reinforcement) may be translated into the production of a particular response. But in arguing this he invokes a motor program, its storage, and its retrieval – ideas that I have shown in an earlier section of this reply and elsewhere (Bindra, 1976, *op. cit.*, Chs. 4, 11, 12) to be utterly untenable. He realizes that the response-outcome learning hypothesis as presently developed cannot deal with response production adequately, but he looks hopefully to some cognitive demon to do the trick. Since he does not offer any specific new hypothesis as an alternative to my account of response production in terms of eliciting stimuli, his conviction that response-production could be explained in terms of response-outcome alone becomes a proclamation of faith rather than a debatable hypothesis.

Jenkins then questions the adequacy of my account. Can we identify eliciting stimuli? Yes, at least in a statistical way, we can and do: Every time we arrange an instrumental training experiment with a new species, we construct the experimental environment in a way that would provide the eliciting stimulus configuration for the "operant" we want to train (see Bindra, 1976, *op. cit.*, Ch. 3, for further discussion of the concept of eliciting stimulus). Why, in an autoshraping experiment, does a signal (CS) sometimes initiate an action toward the reinforcement site (say, food receptacle) rather than toward itself? It is because the momentary motivational valence of a site (CS site or food site) is determined by the imminence of the unconditioned stimulus (e.g., food) predicted by that site at a given moment, and the exact response that emerges depends on the location of the animal at the time of CS onset and the average interval between the CS and reinforcement. Even in the case of instrumental training (e.g., lever-pressing for food), a rat initially continues to go to the food receptacle *before* going to the lever (because the motivational valence of the food receptacle is relatively high); it is only when the rat learns that the lever-receptacle sequence brings the food more quickly than the receptacle-lever sequence that the relative motivational values of the two sites change in favor of the lever. Elsewhere I have discussed several specific examples of response determination by the changing relative motivational valence of different sites (Bindra, 1976, *op. cit.*, Chaps. 11, 12, and 13).

But the crux of the matter in the problem of instrumental learning lies in the shaping of skills involving no apparent spatial response differentiation, that is, responses such as head turning, leg-lifting, and chain pulling. Black has summarized the concerns of many by challenging me to give an account, in terms of my formulation, of the acquisition of a leg-lifting response. Perhaps the first point to note is that, although it is easy to train a leg-lifting response by the classical-conditioning procedure (e.g., tone followed by a brief electric shock to the limb), it is rather difficult to train such a response by the normal instrumental method (i.e., waiting for the leg-lifting response to occur "spontaneously" and then "reinforcing" it). The Russian and Polish studies in which a leg-lifting response is trained instrumentally leave little doubt that such responses are difficult to train without considerable intervention by the experimenter (pushing or pulling the leg, etc.); success is slow and uncertain, and performance is frequently unstable and erratic.

In Black's example, the response as defined has two components,  $R_1$  and  $R_2$ . If each of the components requires its own eliciting stimulus,  $S_1$  and  $S_2$ , then how can the response occur in the absence of the actual eliciting stimuli? The difficulty Black has in interpreting the production of such skilled actions consisting of two or more components may arise because he has ignored two concepts that are important in my account. One is the idea that any given training situation consists of several stimulus events,  $S_1$ ,  $S_2$ ,  $S_3$ , and so on, each usually occurring at a particular point in a fairly stable sequence. Therefore not only does a correlation exist between one critical eliciting stimulus and the incentive stimulus, but several successive stimulus events bear overlapping and nested correlations with the incentive stimulus; it is the contingency organizations representing these correlations that determine the sequential production of the successive components of a multicomponent response. The second concept Black has ignored is that of predicted imminence, which determines that the priming of plexus of various eliciting stimuli reaches a critical level at a particular point in time in relation to the sequence of eliciting stimuli. With these two concepts it becomes possible to show how a long sequence of response components could be produced in proper order by the sequential priming of plexus components of the eliciting stimuli that were originally (during training) necessary for the acquisition of the skill (see Bindra, 1976, *op. cit.*, Chs. 7 and 11).

How plausible my account of skilled actions turns out to be in the light of further studies remains to be seen. What can be said

now is that, given the various fundamental difficulties with the response-reinforcement and response-outcome learning (ideomotor-action) hypotheses, my classical-conditioning approach to instrumental learning appears to be the most promising at this time. I am pleased to see that Booth and Wasserman are also inclined to support this view. It would be useful if Black, Jenkins, and others were to suggest some new ideas about instrumental training to provide a real challenge to the framework I have proposed.

### **Is phylogeny relevant?**

Those who work within the response-reinforcement framework have of course been revising and improving their formulations to deal with criticism and new findings. Some have recognized that the learning of certain types of responses, such as sign-tracking (Hearst & Jenkins, 1974, *op. cit.*) and interim activities (Staddon & Simmelhag, 1971, *op. cit.*) may not follow the response-reinforcement principle. Their position is that the response-reinforcement principle, as it is currently understood, is adequate for dealing with certain types of learning, but that other principles may have to be formulated for other types of learning phenomena. Rather than questioning the response-reinforcement principle, they assign to it a more limited role in behavior. Others are beginning to consider several newly discovered phenomena of behavior ("preparedness," species differences, "biological constraints," etc.) and to examine what modifications *their* study might require in the response-reinforcement framework. Thus, Herrnstein (1977, *op. cit.* by Gilbert), in response to the thrust of the currently popular "biological context" and ethological analyses of behavior, has suggested that greater attention should be paid to the particularities of species, stimuli, responses, drives, and reinforcers in the analysis of behavior. But he too fails to offer any revision that would address the fundamental difficulties of the response-reinforcement framework (what he calls "behaviorism"). Rather, he is content to specify how these particularities "feed into the quantitative laws of behavior," that is, the laws of response reinforcement. Herrnstein's suggestions may make the response-reinforcement framework more appealing to some, but they do not in any way come to grips with the basic inadequacy of the response-reinforcement principle for explaining how adaptive behavior, flexible and fluent, is actually put together.

Gilbert recognizes one inadequacy and tries to deal with it within the response-reinforcement framework. The problem is that of "the origins of behavior" – the first occurrence of operants before they can be reinforced. Two sources of "new behavior," he says, following Segal (1972 cited by Gilbert), are "releasing stimuli" and "emotional induction." Of course, releasing stimuli are what I have specifically considered as a subset of eliciting stimuli (the critical determiners of particular response components) and emotional induction corresponds to my assumption that motivation enters into determining the form of any action that emerges at a given moment. Realizing that in suggesting this modification to the response-reinforcement framework he has come precariously close to ideas that form an important part of my own account of how adaptive behavior is produced, Gilbert backtracks and simply asserts that his account is "well within the response-reinforcement framework." Without saying exactly how releasing stimuli and emotional induction are to be fitted into his framework, he changes the subject.

Gilbert's second tack is to focus on biology – but his biology would exclude the brain! He points out, correctly, that the secret of the adaptive mechanisms of behavior lies in evolution or phylogeny. Next, he appeals, implicitly, to the currently fashionable analogy between evolution and learning and suggests that response variation, like individual variation of the members of a species, is "intrinsic to the reproduction of

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organisms," and that response reinforcement, like survival success in evolution, is the basis of selective advantage gained by certain responses. The origin of different and new operants must then lie in the processes that produce variation in response output. Well, what are these variation-producing processes?

Instead of addressing this question (which may bring him back to releasing stimuli and emotional induction), Gilbert falls back on phylogeny and the inherent ubiquity of phenotypic variation as a biological fact. Just as many biologists do not concern themselves with the mechanisms underlying genetic mutations (observed as phenotypic variation), so also Gilbert does not wish to address this question of mechanisms, and is content with the study of the principles that give selective advantage to particular operants. Clearly, this whole exercise of appealing to phylogeny has been used as a way of evading the central question of the real processes that determine the (initial) production of responses and response variation. There is perhaps some justification for biologists to be hesitant about trying to discover the processes that have led, in the remote past, to the particular genetic variations necessary for the emergence of a given species. But processes that produce response variation are not buried in the remote past; they can be investigated directly if one chooses to do so. This brings me back, full circle, to the statement in my paper that operant behaviorists have elected not to address the problem of explaining behavior - of discovering the real mechanisms that produce it.

The origin of the evolution-learning analogy lies in the writings of Herbert Spencer. It is unfortunate that this infelicitous analogy has been taken up so seriously by many present-day psychologists who, perhaps in response to the writings of ethologists, have "gone biological." Although early ethologists were inclined to think that a certain behavior could be said to have been explained if it could be considered as "adaptive" in terms of the (presumed) history of the species, most present-day ethologists are seeking genuine explanations of behavior in terms of its immediate causation. I hope that the current fad of going biological does not lead psychologists into the ethological thinking of the 1950s and thus distract them from seeking real explanations in terms of real processes. (Several issues related to phylogeny and behavior were discussed at a symposium on human ethology held recently at Bad Homburg. A volume reporting the proceedings of the symposium is now in preparation.)

### **Motor programs or constructed responses?**

Catania, Fentress, Gilbert, Hulse, Killeen, Klingsporn, and Rosenblatt indicate I need to explain further why I oppose the idea that preorganized motor programs are the basis of what we observe as long and unified responses, such as traversing a runway, pressing a lever, and typing a word or phrase. The problem is to explain how such responses show both flexibility (spontaneous adjustment) and fluency (coherence, speed, stability, etc). The dilemma is this: flexibility requires that the response be interruptible and changeable in the course of execution, and thus that the acquired functional segments of motor outflow be small and discrete, while fluency requires that the response be coherent and ballistic, and thus that the acquired functional segments of motor outflow be large and unified. Traditionally, both the response-reinforcement and the cognitive frameworks have concentrated on fluency and thus postulated the development of large (long), unified functional segments, called "motor programs" and "schemata." However, it is evident that a motor segment that functions as a unit can be large only at the expense of flexibility. If a head turn to the right is part of a unitary motor program of walking toward the right, then responses requiring other combinations involving a right turn of the head (e.g., to nod at someone) will be interfered with. Simi-

larly, if a stimulus display, CON, always triggers a central schema corresponding to CONCUBINE, then the perception or reading of CON, CONE, and CONUNDRUM will be interfered with. On the basis of such considerations, as well as in view of evidence from patients with certain motor disorders (Bindra, 1976, chs. 3 and 4), I have proposed that the structural motor organizations that develop through individual experience (act-assemblies) represent rather small (about 500 msec) segments of behavior (acts), and that a response as a whole is constructed from these small segments. Such small segments of motor outflow (act-assemblies) would provide fluency at the level of act components of a response, while making it possible for a response as a whole (lever-pressing or maze-running or reading a word) to be constructed afresh, flexibly, as different preexisting act-assemblies are called into play by momentary circumstances.

The fact that I discard the idea of motor program in favor of fresh response construction has led Hulse to assume that I subscribe to some sort of peripheral chaining idea as the basis of response production. This is not so. I have specifically rejected the idea of chaining in all its various forms (including central chaining) on the basis of the same evidence (deafferentiation studies, etc.) that Hulse and Klingsporn cite (Bindra, 1976, ch. 4). And like them I believe that an explanation of flexible and fluent responding must be found in terms of central organization. My point of departure is that even the types of central organization that Lashley (1951) and Miller, Galanter, and Pribram (1960, this Commentary) have suggested are inadequate. Their concepts of central preorganized scanning systems or hierarchical TOTES are not sufficiently distinguishable from the central chaining concept; and merely calling the central organization a "flexible motor program" is to beg the question (Bindra, 1976, pp. 256-58). I have no objection to using the term *program* to describe the central organization that produces a fluent yet flexible response. What I maintain is that such a program could not be a *motor* program or a structural "preprogrammed" one; it must be a program that gets constructed as it is being executed and has only a fleeting functional existence. Such a program would involve a continuous process of moment-to-moment selective activation of act-assemblies by perceptual-cognitive-motivational processes. It is the act-assembly that functions as a unit and can be triggered as a whole; a response is not triggered but is always constructed afresh and involves the successive activation of a large variety of act-assemblies. The main point is that (central) act-assemblies are not linked to each other to form a continuous motor program; each act-assembly has to be triggered independently by its own antecedent "selection" processes, thereby maintaining flexibility.

Rosenblatt has pointed out that some behaviors made up of fairly long chains of acts nevertheless give the appearance of being triggered rather than constructed. In support of this, he cites, the remarkable work of Fentress and his colleagues [*op. cit.* and this Commentary, *q.v.*], showing that a rather long chain of grooming acts in the mouse, once started, may be completed even in the absence of the normal consequences of grooming acts (contact of the grooming limb with the face). A point to note is that actions such as grooming (and swimming in the fish) are repetitive and species-typical, and it is likely that we are dealing here with some special mechanisms by which one act creates the condition for triggering the same act (circular reflex) or the next act in the series (chain reflex). Such mechanisms have been demonstrated in scratching and "walking" in the decerebrate dog and in ingestion and swallowing in the normal animal. So Fentress's examples may not be contrary to the proposition that each organized unit of motor outflow in itself represents only a brief segment of behavior, though these may be repeated for a long time due to special reactivation or sequencing mechanisms. Nonetheless, I agree with Rosenblatt that the sensory-motor relations relevant to different movements must vary in their "openness" or flexibility (from spinal reflexes to the "higher reflexes" of the brain).

**Falsifiability or where do we go from here?**

Five commentators, Bitterman, Hilgard, Irwin, Kendler, and Mowrer, were among the stalwarts of the 1940s and 1950s who shaped learning theory and helped generate the spirit of optimism about the prospect of genuine understanding of the mind, animal and human, normal and abnormal. It is dismaying to find that now three of them appear to have given up hope – have thrown in the sponge. Bitterman says that no theory, connectionist or nonconnectionist, holds promise of a solution. Kendler, now apparently more concerned with rectitude (being fair) in the decision rules for assessing evidence than with leaping to bold conclusions that appear to lie on the truthful track, remarks, "Perhaps behavior is too much of an open system to allow definitive tests of the basic assumptions of its theories." And Mowrer, calling the mind-body problem the paradox of psychophysical interaction, doubts that anyone ever shall "dispel this ancient source of perplexity." Their dissatisfaction with the present state of our knowledge is understandable. What is dismaying is that they no longer appear willing even to suggest what, from their vantage points, may appear to be profitable new approaches to follow. I hope this pessimistic "drop out" sentiment is not contagious. Certainly Hilgard and Irwin have remained steadfast and realistic, patiently assessing the gains, however small, but keeping in full view all that remains to be done.

Booth, Irwin, Killeen, and Menzel are clearly optimistic, or at least hopeful, about continued progress in the understanding of behavior. The source of their optimism seems to lie in the current attempts at developing new frameworks for the study of behavior rather than merely new hypotheses within old frameworks. Though they all point to shortcomings in the framework I have proposed, it is gratifying to me that they think that the framework may hold promise for making some advance toward understanding behavior.

But on what grounds do we decide that one framework is better, more promising, than another? Kuhn has noted that at some point in time the ad hoc assumptions of a framework (paradigm) become so numerous and tedious that the structure becomes shaky and begins to give way to other, competing frameworks. Menzel talks about the appropriateness of axioms and matrices as bases for choice. Fentress would rather be guided by biology than by logic. Killeen looks for a balance between inductive empiricism and speculative theorizing. Ryan asks for a proper, molar unit of analysis. My view is that the relative validity of a framework is best decided by how consistent or compatible it is with other related domains of knowledge (e.g., neurosciences, ethology, developmental biology) and how far it integrates existing knowledge from diverse fields of research into a unified scheme. What all these ideas point to is that the choice between alternative frameworks is a matter that involves wider *Weltanschauung* or philosophical considerations than does the choice between more specific hypotheses about explaining particular phenomena or findings. Falsifiability, in the narrow sense in which this term is commonly used, may be sufficient for choosing between alternative hypotheses, but it has little to do with choosing between alternative frameworks.

This point is not understood by all the commentators. Several – Bitterman, Bolles, Dennett, Logan, and Navarick among them – have said or hinted that a weakness of what I propose is that it is not explicit or specific enough to be falsifiable and that, therefore, it is of little value. What must be a rather nar-

row reading of Popper (1959) seems to have made many psychologists regard falsifiability in the sense of goodness of fit with data as the primary, if not the sole, criterion of the validity in science. Such an attitude can do as much harm as a complete neglect of Popper's message. Falsifiability by data has its place in science, but not a supreme, overriding position. If science is the enterprise of building a unified view of nature, then the soundness of a theoretical framework in terms of its compatibility with other domains of knowledge is as important as the explicitness and testability of specific hypotheses within the framework.

The competition between frameworks, then, must go on in terms of considerations of overall plausibility rather than only in terms of specificity and falsifiability. A framework is not a finished theory but a working domain to be gradually "filled in" by more specific propositions whose truth or falsity can be more directly and simply determined. It is this process of filling in that eventually brings into the open weaknesses of a framework – weaknesses that may form a basis for new, improved frameworks. Competition between frameworks thus goes on and becomes more acute and specific as the frameworks are made more specific and exact. The choice between alternative frameworks at any given time is not reached merely by accumulating data and falsifying particular hypotheses; hundreds of hypotheses may be confirmed within a framework that is basically wrong (e.g., the drive-reduction hypothesis of reinforcement or the retrieval hypothesis of recall). But, in the end, no framework gets fully refuted; it only gets bypassed or superseded because some other framework begins to make more sense.

**Conclusion**

It should be clear that the general approach implied in the proposed perceptual-motivational framework is *behavioristic* in the sense that the phenomena to be explained are described in behavioral terms and the primacy of behavioral data in postulating and testing explanatory constructs is assumed. The approach is also cognitive-motivational in its assumption that knowledge representations (contingency organizations) and affective processes (central motive states) are not rigidly tied to particular responses but determine the latter through the modulation of perceptual processes. Finally, the approach is neuropsychological in the sense that it postulates explanatory constructs that are compatible with contemporary understanding in the relevant neurosciences.

I regret that owing to space and time limitations I have been unable to respond to several other interesting questions raised in the thoughtful commentaries that so many colleagues were generous enough to make on my paper.

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