Reprinted from:
HANDBOOK OF PERCEPTION, VOL. I
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New York San Francisco London

Chapter 11

THE LEARNING TRADITION

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By the "learning tradition" we may understand those historical continuities of psychological research and theory which have expressly sought to describe, systematize, and explain how past experience influences present behavior. Not included are studies in which learning principles are presupposed as an adjunct to accounting for something else. The "tradition" actually comprises several lines of development which arose more or less independently about a century ago and have largely preserved their distinctive identities to this day. None of these have explicitly addressed perceptual issues. In fact, learning theory has been home ground for psychology's behaviorist turn against uncritical acceptance of mentalistic concepts, and in frequent moments of extremity has been wont to repudiate all internal processes alleged by common sense to mediate between stimulation and behavior. Even so, any sector of psychology in which stimulus reception is a critical factor must perforce reckon with perceptual issues in one guise or another. In this review, I shall (1) chart the main channels along which learning theory has developed, (2) sketch the logic of its inherent perceptual concerns, and (3) note the more salient points at which these concerns have surfaced.

I. THE HISTORICAL STRUCTURE OF LEARNING THEORY

Modern approaches to learning have a fourfold origin in the late nineteenth century dawn of scientific psychology, corresponding to the still familiar labels "acquisition of skills," "verbal learning and memory," "classical (Pavlovian) conditioning," and "instrumental (operant) conditioning." The last two, in an uneasy union ("conditioning theory") eventually dominated by the latter, soon gave rise to behavior theory, most importantly of the Hullian, Tolmanian, and Skinnerian varieties. On the fringe of these main traditions lies work on concept formation and naturalistic/comparative accounts of animal behavior, while within recent times mathematical modeling has established a distinctive albeit still actively evolving outlook on learning issues.

A. Acquisition of Skills

As understood by ordinary language, "learning" is what one gets from education, namely, cognitive knowledge and skills (cf. any standard dictionary). Technical psychology has never seriously studied the acquisition of knowledge, though educational and phenomenalistic psychologists have toyed with it, and the current renaissance of cognitive psychology should soon alleviate this deficiency. On the other hand, the very earliest stirrings of learning research addressed the manner in which practice improves proficiency at a skillful task. Observations on transfer of training across body regions date back to the 1850's (Woodworth, 1938, p. 181), while Bryan and Harter's (1897) classic study of telegraphic-skill learning established the definitive character of this tradition. Subsequent decades saw a flurry of studies on a variety of sensorimotor skills, settling down by the 1940's to an emphasis on tracking tasks and other complex eye/hand coordinations. (For access to the history of this movement, see Fitts, 1964; Irion, 1966.) What most distinguishes "skills acquisition" from other learning traditions is the ordinary language holism of its conceptual paradigms: The dependent variable is a person's *ability* to achieve a difficult goal, as measured by the merit of his success at this on occasions when he is presumably trying to excel. And learning of an ability is construed to occur simply through *practice,* that is, by repeated doings of the relevant actions. This tradition has thus remained tied to the ordinary language view of complex psychological functioning as a purposeful flexing of mental muscles, strengthened through exercise. Technical research on skills quickly recognized that these are integrations of manifold subprocesses, and has sought to identify significant parameters of practice (notably, in its temporal pacing, intertask and part/whole transfer efficiencies, and, most recently, fine details of outcome feedback; see Bilodeau, 1969). But the specific mechanisms responsible for skillful performance have remained largely tenuous conjecture built on theoretical ideas developed elsewhere in learning theory.

B. Verbal Learning and Memory

Since the main dependent variable in Ebbinghaus's (1885) monumental creation of the verbal learning tradition was accuracy of recall, i.e., success at a memory task, he could easily have construed his results in terms that would have pioneered the learning of abilities. Or, with greater emphasis upon the cognitive aspects of memory, he might have founded an experimental psychology of knowledge. Instead, Ebbinghaus's familiarity with classical philosophic analyses of mental events induced him to view memory as a sequence of ideational elements evocatively linked by associations. The recall task he invented, serial reproduction of nonsense-syllable lists, was admirably suited to this interpretation. And when Calkins' (1894) paired-associates modification of the technique apparently gave direct experimental access to the strengths of individual item bonds, research on the acquisition and loss of rote verbal associations was off and running in a narrow-gauge rut whose nineteenth century outlook endured without essential modification until the late 1950's. Unlike the other main learning traditions, however, verbal learning has recently undergone a profound metamorphosis in keeping with psychology's new cognitive turn. Concerns for information storage, meaning, imagery, linguistic structure, and their like have become ascendant, and the long regency of elementwise associations as the area's key explanatory mechanism has nearly run its course. (See especially Tulving & Donaldson, 1972.) Having never really abandoned human mental life to begin with (except to rename ideas "words" and then conflate the afferent, central, and efferent embodiments of these), the verbal learning tradition needed only a fresh look at where it had been all along to find itself in the vanguard of psychology's return to the inner organism.

C. Classical (Pavlovian) Conditioning and Instrumental (Operant) Conditioning

Under these labels I mean to contrast not so much empirical stimulus-pairing versus response-rewarding conditioning paradigms as the distinctive, persistent outlooks on learning theory which have respectively emphasized these particular training procedures. The origins of classical conditioning in the work of Pavlov, Bechterev, and their father-figure Sechenov needs no recounting here (see Kimble, 1961, Ch. 1; Razran, 1965a). What is perhaps not so evident from latter-day integrative surveys of learning is the distance—both conceptual and historical—between this and the instrumental conditioning tradition initiated by Thorndike (1898) and later powerfully enhanced by Skinner (1938). Classical conditioning is the legacy of psychology's medical/physiological root and has been persistently physiotropic in stance. Stimuli and responses are paradigmatically conceived as proximal events, i.e., minimally patterned bursts of action in localized regions of the organism's sensorimotor surface,* and its main explanatory construct relating input to output is the reflex arc construed as neural pathway (Sherrington, 1906).

In contrast, the instrumental conditioning tradition arose in naturalistic accounts of animal behavior, notably the inspiration of Thorndike's early work by Morgan's (1894) attempted deanthropomorphizing of animal psychology), and has consistently maintained the "distal focus" (Brunswik, 1952) dictated by this origin. Paradigmatically, its stimuli are molar features of the organsm's external surround. Responses are molar changes in his environment or his relation thereto defined indifferently as to how he brings these about. And those states of the organism held responsible for S–R regularities are conceived functionally in terms of what they do rather than what they may physiologically be. As part of the behaviorist movement's general decline, instrumental conditioning has of late shown more than a trace of senescence. But in its day it had greater integrative breadth than perhaps any other sector of research psychology, investigating not merely the parameters of behavior modification through reward and punishment but also the determinants of stimulus properties other than response evocation, notably drive and reinforcement effects. Often its concern was less for learning as such than for disclosing the nature of what gets learned (cf. the varieties of "latent learning" and "transposition" experiments). In contrast, though classical conditioning has pointed toward an intriguing intricacy of detail (see Razran, 1957), it has never really *gone* anywhere except at the

* It is clear from Razran (1965b) that this verges upon caricature. Even so, *qua* caricature, I think it is fair enough.

hands of neurophysiologists whose studies of evocative relationships between activity patterns in localized brain regions, classically conditioned or otherwise, have begun to achieve impressive sophistication (e.g., John, 1967; Konorski, 1967; Pribram, 1971). As important as the latter development promises to be, it lies beyond the scope of this chapter.

D. Behavior Theory

Behavior theory has been the speculative phase of conditioning research, devising explanatory constructs and directions of inquiry for the latter's empirical work even while proposing to account for all organismic (or at least mammalian) behavior in those same terms. The earliest behavior theories, commencing with Watson (1914), had a classical conditioning orientation which persisted into Hull's (1943, Ch. 3) professed conception of stimulation as a pulse of energy transduced by a localized sense receptor. By the 1930's, however, the increasing dominance of empirical conditioning research by Thorndikian paradigms assimilated a similarly burgeoning awareness of motivational phenomena to produce the three great universal systems of Hull (1943, 1952), Skinner (1938, 1953), Tolman (1932, 1959), and their respective disciples. The universality of these resides in that each tried in its own distinctive way to formulate explicit principles according to which molar behavior is jointly determined by present stimulation, past experience, and motivational factors, thus covering in intent, if not necessarily in accurate detail, all primary categories of psychological events' ultimate sources other than constitutional determinants. Since much of the nomic force of Skinnerian theory is covert in its definitions of terms, the gross extent to which its implications outrun its data base, including implicit denial of many complex empirical phenomena alleged if poorly documented by commonsense psychology,* has remained largely invisible to its parti-

* For example, suppose in a Skinnerian variant of Type-3 latent learning (MacCorquodale & Meehl, 1954, p. 209) that (A) an organism o receives water reward for operant response R at a time when he is so water-satiated that no increment in his rate of R-emission then occurs. What will happen to o's R-rate if he now becomes motivated by (B_1) water deprivation or (B_2) frigid ambient temperature? According to the standard Skinnerian conception of "reinforcing stimulus," receipt of water is nonreinforcing to o in Phase A and should hence produce no increment in his R-rate under either B_1 or B_2. Even were the notion of reinforcer broadened to admit "latent" reinforcements, moreover, it would still remain to develop an account under which the latent reinforcement in Phase A might bring R under the control of water deprivation but not of temperature extremities. The important point here is not that the operant conditioning perspective is incapable of such conceptual development, but that its radical-empiricist profestations of scientific purity have to date been a mask for grotesquely overgeneralized oversimplifications.

sans. Such selective awareness, combined with the enormous technical power of operant research methodology, may well be why this special branch of the behavioristic tradition is still flourishing even if cultishly ingrown. Tolmanian expectancy theory, on the other hand, never really became respectable except as counterpoint to *S–R* theory, and faded from contention as *S–R* mediational mechanisms seemingly took over its distinctive predictions.* Meanwhile Hullian *S–R* theory grew with the yeoman assistance of Spence, Mowrer, Neal Miller, and numerous lesser lights to dominate not just behavior theory but virtually the whole of American psychology from the 1930's until its death around 1960 from attrition of dedication.†

Even as outline the foregoing does insufficient justice to the history of learning research. Comparative/naturalistic approaches to adaptive animal behavior, especially the Ethology movement (see Hess, 1962) and studies of animal intelligence (e.g., Maier & Schneirla, 1935), have exploited concepts and paradigms‡ not always readily assimilable to conditioning theory. Work on concept acquisition (classically Heidbreder, 1924, 1947; Hull, 1920; Vygotsky, 1934; for recent surveys, see Bourne, 1966; Pikas, 1966), though often interpreted in *S–R* terms, has had a sufficiently distinctive past and increasingly active present to warrant recognition as a minor tradition of its own. (Some might wish to make a similar claim for work on problem solving, but I would argue that the latter is not properly classified as "learning.") Though largely suppressed until quite recently by an inhospitable zeitgeist, a "trace" theory of memory importantly more powerful than association formation§ has been persistently voiced, albeit never effectively developed, by the Gestalt movement (see Koffka, 1935; Köhler, 1929).

*Tolmanian theory's fatal error was to remain so commonsensically intuitive for so long. By the time MacCorquodale and Meehl (1954) gave it a precision comparable to Hullian theory, the latter had already cornered the market among whatever psychologists had not already turned elsewhere for gratification for their cognitive yearnings.

† Although the failure of extant *S–R* theory to yield unequivocal predictions in specific applications (for reasons indicated in Section II) had become increasingly fretful to some, this should have been merely a goad to further development of this perspective. However, yearnings to get on with central mediators (proscribed by the *S–R* outlook though not by behavior theory in general) had been thwarted for too long. Inner events were elsewhere becoming respectable again, and although its creation in the late 1950's of incentive-motivation mechanisms (see Bolles, 1967, Ch. 12; Rozeboom, 1970, pp. 109–119 and 130–136) amply proved *S–R* theory to be still capable of major innovations, few any longer cared.

‡ E.g., delayed-response and double-alternation tasks.

§ Despite the burgeoning of trace notions in recent memory models, the formal differences between memory traces and associations still remain largely unappreciated. For a brief explication thereof, see Rozeboom (1969).

And most significant of all, the mathematical-models approach to learning born in 1950 as statistical learning theory (see Neimark & Estes, 1967) has increasingly come of age, most recently joining forces with fresh thinking in the verbal learning and concept formation traditions to mount the most progressive thrusts of current learning research (e.g., Norman, 1970). Even so, to the extent there exists an identifiable "learning" outlook in the history of psychology, it is characterized by the peripheralistic associationism embodied in the main traditions sketched above and epitomized by Hullian *S–R* theory.

II. PERCEPTUAL COMMITMENTS OF LEARNING THEORY

Psychology's behaviorist era expired in the late 1950's; we may choose 1960 as a convenient year from which to date the contemporary cognitive period. The visibility of perceptual concerns in learning theory depends greatly upon which side of this divide is examined. For insofar as some of behavioral research's old vitality has persisted to the present, its main post-1960 investment has been precisely in problems of "stimulus selection" which would have been taboo or at least *outré* previously. The vocabulary of perception is still rare in these studies, however, and even were it not one might well wonder what in the data justifies such talk. The foremost task of this chapter, therefore, is to indicate what, in a theory of learned behavior, can fairly be construed as a perceptual concern.

The variables which constitute a behavior system such as an environment-coupled organism can usefully be classified according to whether their values represent *states* or *process stages* (Rozeboom, 1965, p. 340ff). Process stages are the system's moment-to-moment fluctuating activities, notably stimulus reception, behavior, and the internal episodes such as perceiving and thinking whose variation as a function of input mediates the effect of stimulation upon concurrent behavior. In contrast, an organism's state properties—habits, preferences, and all others of the sort commonly thought of as "dispositions"—are relatively stable attributes which determine the parameters of the organism's process regularities at a given moment but are not themselves a function of input on that occasion even though they may well depend importantly upon the organism's past process history. For example, organism *o*'s strength of Hullian habit ${}_{S_i}H_{R_j}$ at time t governs the probability or vigor with which o performs R_j at t in response to stimulus S_i, but is not affected by whether or not o is in fact stimulated by S_i at t even though an experience involving S_i at t will generaly modify *o*'s subsequent ${}_{S_i}H_{R_j}$-strength.

Since "learning" comprises changes in an organism's state properties induced by experience (Rozeboom, 1965, p. 343), any account of learning perforce rests upon some view of what *are* the state variables which characterize real-life behavior systems—habits or expectancies, reflexes or rules, drives or wants, etc.—and by what deterministic or stochastic laws these control the organism's response to his environment. Such a theory may undertake perceptual commitments in two ways, one obvious and voluntary, the other neither.

The obvious way is to postulate a class of process variables, intervening causally between input and output, which are described in a way more like what common sense says about percepts than about memories, desires, or other central mediators distinguished from perception by classic mentalistic psychology. Since above all percepts are prima facie veridical inner representations of concurrent externality, a conjectured stage of central mediation is *manifestly perceptual* if it is described in terms of features $\{s_i\}$ which stand in some sort of logical correspondence to features $\{S_i\}$ of the organism's environment or his relation thereto and are such that when *o*'s process activity contains s_i, it is highly probable that the corresponding external S_i, or another rather like it, is present in *o*'s surround. For example, if S_i is the color *red,* the corresponding perceptual feature s_i might be described as "red-seeing," or "attending to red," or "detecting red," or more abstractly "s_{red}" in contrast to overt stimulus "S_{red}," and so on for various styles of notation or terminology in which an internal process is characterized in terms of an environmental feature considered to be its primary elicitor.

But what if one's learning theory does not acknowledge any mediational process meeting the above criterion—or even if it does, what empirical grounds could there be for such an *S–R* heresy? It turns out that a special logical property of natural behavior systems induces a perceptual phase in any realistic theory thereof, namely, that so far as we have any reason to believe, *an organism's input receptivity is descriptively unbounded.* By this I mean that we cannot plausibly assume that any given description of *o*'s environment at time *t* includes or entails all features thereof which have some effect on *o* at *t*. From this it follows, by an argument impractical to develop here,* that a theory which allows *o*'s value of a state or process variable **X** to be partially (probabilistically) predictable at *t* from finite information about the input to *o* at *t*, even though **X**'s stimulus determinants are descriptively unbounded, must conceive of the environment as affecting **X** through a class $\{S_i\}$ of potential stimulus features—call them *action units*

* See my "The logic of unboundedly reactive systems," in preparation.

with respect to **X** for o at t—which have the following properties: (a) The number of **X**-wise action units (i.e., stimulus features relevant to **X**) which can be simultaneously present to o at t has no fixed upper limit. (b) Each action unit S_i actually present to o at t generates a corresponding **X**-strength *tendency* (or distribution of tendencies over the alternative strengths possible for **X**) whose value for o at t is determined by the state parameters which modulate S_i's effect on **X**. (c) The actual strength (or probability distribution over alternative strengths) of **X** for o at t is a concatenation (i.e., quasi-additive function) of all the **X**-strength tendencies variously evoked by action units present to o at t.* If this abstract and highly compressed sketch of stimulus action is not immediately clear, no matter: the essential point is that a realistic behavior theory must treat the impinging environment as an indefinitely large set of stimulus components (though perhaps not logically simple ones—see Section III, J, below), each of which exacts its own behavioral influence independently of the other stimulus contributions to this. Moreover, the set of *all* environmental features conceivable by us as logically possible action units for a given organism will generally be much larger than for one reason or another it makes sense to admit as his actual action units. But if a behavior theory grants o fewer action units at t than are definable features of his environment, and especially if the theory allows the set of action units for o at t to be specified by parameters which are lawfully dependent upon antecedents of their own, it thereby adopts views on *stimulus selection* which are tantamount to a stand on perceptual issues.

Action-unit restrictions intrude into accounts of learning in two main ways. One is the theorist's presumptions, usually implicit, about what stimulus features could possibly be psychologically efficacious. For example, should we allow even in principle that the stimulus-object property of *being either-square-or-soft,* or *being non-red,* or *having remained motionless for the last 30 minutes,* or *having once been admired by a descendent of*

* This definition (more precisely, definition sketch) of "action unit" does not preclude the possibility that the distribution of **X**-tendency strengths generated by an action unit S_i is uniformly null as a result of null parameters in the $S_i \rightarrow \mathbf{X}$ function. E.g., a stimulus feature S_i might be perceived by o at t even though, through insufficient prior conditioning of habits in which it is the stimulus term, S_i has no effect on o's behavior at that time. It is also important to note that the set of action units for o at t relative to one system variable need not be the same as the action units for o at t relative to another. In particular, a stimulus feature which interacts with state property H_j to influence process **X** need not be all or part of an action unit for modifying H_j itself; while conversely, input features which affect H_j need not affect **X** at the process level. In somewhat different terms, this point has already been emphasized by Berlyne (1970, p. 31) and Lovejoy (1968, p. 15).

Benjamin Franklin can be an action unit for o at t? A priori intuitions on this score—which tend in practice to be unreasonably constrictive*—are obviated, however, by a second and more legitimate delimitation of action units, namely, by empirical diagnosis. For any general theory of how an organism's response to a present composite of action units is governed by his past experience with these or related stimulus features will inevitably fail dismally to account for the local input/output covariations observable within a brief period of a given organism's history unless the stimulus features admitted as action units for this particular o at this time are a select subset of those which must be acknowledged to account for behavior in other organisms or even in this same o at other times. Since the details of this point are rather complex, I will try to convey its essence through a brief, highly simplified example in the next three paragraphs which the reader may omit if preferred.

Suppose that our theory of learning postulates (a) that if stimulus feature S is an action unit for o at t, then for each possible behavior R, o has some strength (possibly zero) of a habit variable $S \rightarrow R$ such that occurrence of S in o's input at t interacts with $S \rightarrow R$ to produce an R-doing tendency whose strength is proportional to the strength of $S \rightarrow R$ for o at t; (b) that o's probability of actually doing R at t is an increasing symmetric function of all his variously aroused R-doing tendencies at t; and (c) that increases in o's $S \rightarrow R$ strength are caused by, and only by, experiences in which doing R while receiving S-featured input is followed by reward.† Consider, now, a set $\{T_{ij}\}$ of possible stimulation totalities which are identical in all respects except the size of a contained rectangle, the rectangle in T_{ij} being i inches tall and j inches wide. (For simplicity, I will speak as though each T_{ij} is nothing but this triangle.) For a given behavior R, we can estimate the probability of o's doing R at t in response to each T_{ij} by observing within a series of test trials commencing at t the proportion of T_{ij}-trials on which o does R.‡ If we give an o whose initial probability to R-doing is (for sim-

* Thus Taylor (1964, p. 131ff.) scoffs at a suggestion by Restle that the property, *having been reinforced on the last trial,* could in itself be a stimulus feature of the sort to which responses can be conditioned. But while it is indeed contrary to common sense that such properties are perceivable, this is more an intuitive acknowledgment that we do not in fact ordinarily perceive them than it is reason to hold that we cannot do so. Similarly, the failure of some psychologists to grant action-unit status to feature-absences is a demonstrable source of needlessly quirky theory (see Section III, J, below).

† Precise details of this learning principle, which when properly formulated subsumes extinction as well as acquisition, are not required here. The interested reader can find its specific Hullian version (which Hull himself never quite made completely explicit) in Rozeboom, 1970, p. 110ff.

‡ In practice, of course, empirical determination of response probabilities is nastily

plicity) the same to all the different rectangles $\{T_{ij}\}$ some experiences in which R is reinforced to a particular one of these, say $i = 4$, $j = 3$, what will be the shape of o's resultant R-response gradient over $\{T_{ij}\}$? According to our simplistic model, o's post-training probability of R-doing given stimulus totality T_{ij} should be an increasing function of the number of features common to a 4-inch by 3-inch and i-inch by j-inch rectangle which are action units for o at this time. Examples of such features by which training on T_{43} might transfer to other rectangles are (1) being 4 inches tall, (2) being 3 inches wide, (3) being both 4 inches tall and 3 inches wide, (4) having a height-to-width ratio of 1.25, (5) being at least 2 inches tall, (6) not being 5 inches wide, (7) etc. Each of these stimulus properties, if an action unit for o, contributes a distinctive R-tendency pattern to o's post-training response gradient over $\{T_{ij}\}$. Thus, if for simplicity we assume that tendency strengths come in just two grades, "some" versus "none," feature (1) as action unit contributes some R-evoking tendency to T_{ij} if $i = 4$ regardless of j, and none if $i \neq 4$; feature (2) contributes some R-tendency to T_{ij} just if $j = 3$; (3) contributes some only if both $i = 4$ and $j = 3$, i.e., only to the training stimulus; (4) contributes some to just those rectangles whose heights are 1.25 times their widths; and so on. According to the model, o's post-training R-doing probability gradient over $\{T_{ij}\}$ is a composite of these tendency patterns for just those training-stimulus features which are action units for o at this time; hence from our empirical determination of the former we can diagnose what the latter must be. For example, if $Pr(R|T_{ij})$ is equally low when $i \neq 4$ and $j \neq 3$, equally medium when $i = 4$ and $j \neq 3$ or $i \neq 4$ and $j = 3$, and high when $i = 4$ and $j = 3$, we can infer that the action units in T_{43} for o are (1), (2), and perhaps (3), whereas if $Pr(R|T_{ij})$ is high at $i = 4$ and $j = 3$, equally medium when $i = 1.25 \times j$ for $j \neq 3$, and equally low otherwise, our inference is that they are just (3) and (4).

In practice, diagnosing action units from pretraining/post-training response-surface comparisons, i.e., empirical generalization gradients, is usually complicated by inclusion in one's learning theory of some principle of *similarity induction*. According to such a principle, the more that two stimuli are alike, the more strongly does a state or process effect of one on o at t tend also to be produced by the other as well. (Some such principle is inescapable if the theory is to avoid unnatural discontinuities.) For example, a more realistic version of the present learning model would include one or both of the following postulates: ($G1$) Reinforcement of re-

contaminated by the learning which occurs during the test trials. Except for a nod of appreciation to operant conditioning methodology for having vastly enhanced our technical prowess at such diagnoses, we may ignore such complications here.

sponse R to action unit S_i strengthens habit $S_j \to R$ by an amount which is an increasing function of the similarity between S_i and S_j. ($G2$) Stimulation by action unit S_i interacts with habit $S_j \to R$ to evoke an R-doing tendency which is an increasing function of both the strength of $S_j \to R$ and the similarity between S_i and S_j.* The notion of stimulus "similarity" is sufficiently vague that it readily becomes a theoretical construct characterized in part by organism-specific parameters (e.g., in Hullian theory, a *jnd* metric). So treated, "similarity" is a relation between input features as received rather than as presented and thus constitutes the theory's version of a *perceptual resemblance* factor.

Moreover, once one has acknowledged that a given feature of the environment may or may not be psychologically efficacious for a particular organism, little motive remains to require that action-unit status be all-or-none. Instead, a sophisticated behavior theory will admit a class of receptivity or "salience" parameters such that the effect of feature S_i upon system variable $\mathbf{X}$ for o at t is modulated by the degree of $\mathbf{X}$-wise salience S_i has for o at t. With empirical generalization gradients simultaneously reflecting both salience and similarity induction parameters, however, the former no longer afford *simple* diagnosis of the latter. For the degree of generalization between two stimulus complexes T_i and T_j can be due either to the salience of features which T_i and T_j have in common, to the perceptual resemblance between salient features wherein T_i and T_j differ, or to some combination of both. Considering the extensive redundancy between these two mechanisms, it is not surprising that studies of generalization seldom discriminate clearly between them.

In brief, then, a behavior theory which allows every one of the infinitely many logically distinguishable properties of an organism's environment to acquire stimulus control over his behavior in the fashion envisioned by its learning postulates would yield predictive absurdities. And once the theory admits as behaviorally consequential input only a parametrically adjustable portion of the organism's stimulus surround, it thereby acknowledges a reception selectivity which is tantamount to a perceptual stage of input processing even if perception in the strictest cognitive sense may be only a special case of this. The primary empirical phenomena which constrain a theory's treatment of input selection, moreover, are the patterns by which behavior established in one environment generalizes to another. Precisely

* Few accounts of stimulus generalization are articulate enough to distinguish similarity induction in learning from similarity induction in performance, i.e. type-$G1$ versus type-$G2$ inductions, or for that matter, generalization based on similarity induction from generalization due to shared action units. Hullian theory, however, was reasonably (though not completely) clear that "primary stimulus generalization" was a principle of type $G1$ rather than $G2$.

how generalization gradients are to be so interpreted depends greatly on the specifics of the theory in question. Even so, an important principle which obtains for all is that *if two complex stimulus alternatives T_i and T_j contain the same action units (i.e., features with nonzero salience) for o at t with respect to state or process variable* **X**, *then T_i and T_j are* **X**-*wise equivalent for o at t, i.e., it makes no difference for* **X** *which of the two o receives.* Conversely, if $\{T_i\}$ is a class of complex stimulus alternatives sharing a common property *S*, and all members of $\{T_i\}$ are **X**-wise equivalent for *o* at *t*, then it is likely that the only action units with respect to **X** for *o* at *t* contained in any member of $\{T_i\}$ are features entailed by *S*, especially if shifts in the **X**-effect of any one T_i generalizes completely to the others. The most explicit perceptual concerns of recent learning research have in fact centered upon just such equivalence classes.

III. LEARNING-THEORETIC ACKNOWLEDGMENTS OF PERCEPTION

Fragmentary and cryptic as they usually are, learning-theoretic encroachments upon perception do not submit to tidy organization or concisely comprehensive summary. My best efforts in this regard suggest the following gridwork of overlapping hits and near misses.

A. Perceptual Lip Service

Explicit acknowledgement of perceptual mediators by leading learning theorists has been largely vacuous, an occasional genuflection to this concept's stature elsewhere in psychology without, however, finding any distinctive role for it in the theorist's own system. Thus for Neal Miller (1959, p. 242ff.), percepts are just internal responses to which other responses can in turn be conditioned; and with a de-emphasis on centrality the same is true for Skinner (1953, pp. 140 and 275ff.). Guthrie (1959, p. 165ff.) insisted that learning theory must describe input in "perceptual terms" insomuch as only stimuli which are "meaningful" to the organism get conditioned to responses, but said nothing about the nature of such meaningfulness, how some stimuli get that way, or even how we diagnose this condition. For early Tolman (1932, p. 137), percepts were expectancies activated wholly by present input rather than through previously established means-ends-readinesses; later, Tolman (1959) spoke of perceptions merely as internal counterparts to external stimuli without equating them with expectancies or for that matter giving them any particular work to do, and suggested (1959, p. 114) that they can be detected by noting which changes in the organism's environment leaves his responding thereto undisrupted. This would amount to searching out equivalence classes of inputs, Tolman's assumption presumably being that the members of such an equivalence class would have their perceived features in common. It is important to appreciate, however, that two different stimulus configurations to which *o* has the same response probabilities are by no means certain to be perceptually alike to *o*; it is also possible that they have had the same past reinforcement contingencies for *o* despite a lack of shared perceptual features.

B. Sensory Integration

A more significant move to analyze perception, as a received phenomenon, in learning-theoretic terms lies in the proposal [e.g. Hilgard, 1948, p. 332; Sheffield, 1961 (quoted in Hilgard & Bower, 1966, p. 98ff.)] that perception consists in, or at least involves, evocation by a stimulus *S* of the total sensory complex previously aroused in *o* by a larger stimulus ensemble S^* containing *S*, the power of *S* to do this deriving from sensory-sensory associations acquired through past contiguity of the afferent processes respectively elicited by *S* and the other elements in S^*. Osgood (1957) has proposed a behavior-theoretically advanced version of this notion which melds not merely sensory components but efferent "meanings" as well, the latter being covert cue-producing responses ("representations") reliably elicited by the stimuli whose meanings they are. For Osgood, one perceives, say, an apple when some apple-produced cue (which one doesn't particularly matter) releases the whole congeries of multiple-modalitied sensations (visual, tactile, gustatory, etc.) and representational responses which have become evocatively interconnected through one's past sensorimotor transactions with apples. This of course merely updates a centuries-old tradition in mental philosophy (cf. Boring, 1942, pp. 5–9 and 14–18; Brett, 1965, esp. pp. 120 and 389), and has the considerable merit of providing for the thing-constancy and aura of expectations which seem to typify perception. There is, however, something amiss in seeking the essence of percepts in their manner of arousal rather than the character of the processes aroused. If, by experiential association, cue *S* evokes the same sensory or sensorimotor complex *P* previously aroused by a more inclusive stimulus compound S^*, it seems perverse to class *P* as perceptual when evoked by *S* but not by S^*. But if *P* is a percept even when evoked by a stimulus sufficient for this without learning (e.g., S^*), then sensory integration cannot be definitive thereof. And of course neither does the sensory-integration notion distinguish perception from mnemonic recall or other forms of postperceptual ideation. (Even so, see p. 236 below.)

C. Distal versus Proximal Stimulation

A learning theory which takes the organism's distal environment to be the initial stage of input can scarcely deny that events upon and within *o*'s sensory surface ("proximal" stimulation) mediate the former's psychological import for *o* (see, e.g., Hull, 1943, Ch. 3). Several perceptual issues turn on this distal/proximal distinction. For one, do not the receptor processes evoked by *o*'s distal environment qualify as "perceptual" by my first criterion in Section II, above? Prima facie, they fail at this through insufficient object-constancy, i.e., distal and proximal features do not seem to be even remotely in one-one correspondence (cf. variation in the retinal outline projected by an object of fixed shape). Actually, there is reason to think that *o*'s perceiving of a distal feature *S* must be mediated by a distinctive proximal-stimulus *pattern* whose presence/absence correlates with the environmental presence/absence of *S* at least as highly as does occurrence/nonoccurrence of a central percept of *S* (cf. Gibson, 1950; also see Rozeboom, 1972a, p. 324, on rotation of axes in Brunswikian proximal-cue space). However, the learning literature has never seriously contemplated the stimulus potential of proximal patterns. Instead, there has been a historically persistent behavior-theoretic tension over the proper locusing of stimuli: Should input be defined in environmental terms, as comes most naturally to empirical studies of learning, or in terms of physiologist-approved sensory signals which, though causally closer to output, show negligible correlations with response measures when they are empirically accessible at all. The "generic" conception of stimulus so well stated by Skinner (1938, p. 33ff.) has by now fairly well carried the day, namely, that a "stimulus," properly construed, is a class of input events whose members (which can be at any distality distance from *o*) are interchangeable for *o* in their demonstrable effect on an appropriately defined response variable —in short, whatever stimulus descriptions yield the tidiest empirical regularities.* For a behavior theory which seeks to identify its stimuli in this way, the defining feature of such an equivalence class is prima facie

* Skinner's treatment of this matter, which I have heavily compressed and paraphrased, is essential reading for anyone seriously interested in the logic of "stimulus" and "response" concepts. Unfortunately, Skinner does not make very clear what are to be taken as the "events"—distal versus proximal, repeatable configurations versus dated occurrences (cf. MacCorquodale & Meehl, 1954, p. 220), stimulation totalities versus augmentable complexes—which compose these classes.

an action unit in the sense discussed above, and hence an implicit diagnosis by that theory of what the organism perceives. But which input features are "stimuli" for *o* at *t* is then an empirical fact about *o* at *t*, not just an arbitrary choice of units by the theorist, a fact which raises the important questions (*a*) Why is *o* at *t* responsive to this particular set of input features rather than to some alternative set?, and (*b*) When these stimuli are distal, what mechanism can produce so profound an equivalence among the diverse proximal input manifolds through which the distal feature alternatively exerts its effect? These are puzzles which learning theory has as yet scarcely considered, much less solved.

D. Orienting Behavior

When distal events are taken to be the first stage of input, *o*'s generalization patterns at *t* are due at least in part to parameters of *o*'s sensory-surface functioning. An obvious case in point is sensory capacity: whatever the objective differences between two environments, they must belong to the same equivalence class for any organism whose receptors cannot react differentially to them. For learning researchers (unlike comparative psychologists), individual differences in sensory capacity have been mainly a nuisance complication which help explain why certain promising experiments came to grief with the subjects used. Neither has learning theory paid much attention to transient reception changes of the satiation sort, probably because learning data tend to average out such effects. An appreciable body of conditioning literature has, however, recognized that organisms have an important degree of efferent control over their own sensory function. Most of this material, under the title "orienting reflex" or "orienting reaction," is Pavlovian (e.g., Lynn, 1966; Sokolov, 1963; Voronin *et al.*, 1958); but the instrumental conditioning and mathematical model traditions have also given heed to learned "observing responses" (e.g., Atkinson, 1961; Wyckoff, 1952; Zeiler & Wyckoff, 1961) whose only direct reinforcement is a stimulus change affording increased accuracy of a subsequent discrimination response. As most recent writers on the topic have noted, orientation reactions and their reception-modifying kin (see Lynn, 1966, p. 6ff.) subsume a multiplicity of mechanisms, of which Pavlov's original "investigatory reflex," positioning the sense organs to maximize pickup from a novel stimulus, is merely one. The major issues which weave through these seem to me to be the following: (*a*) *Receptor positioning versus receptor tuning.* Most easily comprehended of orienting behaviors are movements which affect the environment's receptor impingements. However, there is also increasing evidence (Sokolov, 1963; see also

Bruner, 1957) that the receptor's reaction to that impingement is itself under some central control. Whatever may be the nature of this still-obscure tuning process, it is surely similar enough to more central perceptual mechanisms that an adequate theory of the former will do much to clarify the latter as well. (*b*) *Feature specificity*. The behavioral import of orienting reactions to an environment T lies in how these affect the salience and similarity-induction parameters of T's assorted features for o at t. When distal features S_i and S_j of T are spatially well separated or emphasize different sense modalities (e.g., an object's color and temperature), orientations which enhance reception of one generally degrade it for the other. But to what extent can orientations accomplish such differential selectivity when S_i and S_j act simultaneously upon the same receptors? Is there, for example, any way for positioning to help o perceive a rectangle in terms of shape and size rather than height and width, e.g., *squat and small* versus *moderately wide but very short*? And while poor orientation can obviously blur the input difference between, say, two complex tones, to what extent and in what way following optimal ear placement might orientation further decrease the similarity between the percepts aroused by these?* The point here is that while orienting behavior clearly provides for sharpening or attenuating reception from stimulus *regions*, its contribution to specific *feature* selection is problematic. (*c*) *Perceptual preconditions of orientation*. The very operation of an orienting reaction generally requires some perceptual processing of the preorientation input. Thus a Pavlovian investigatory reflex requires registering some input feature as "unfamilar" and, if the resulting orientation is to be properly directed, locating it within a spatial reference frame. When explanations of perceptual phenomena are sought in orienting behavior, there is a good chance that the account presupposes much of what it professes to explain.

E. Vigilance

The term "vigilance" alludes to whatever is involved in the human performance decrements generally found to occur with increasing duration of

* I speak of "similarity between percepts" rather than "perceived similarity" because in principle these are quite different things. Perceived similarity between distal features is perception of a supposedly objective relation between these which may or may not be closely correlated with the similarity between the internal representations of those features. Hence an adjustment which decreases the similarity between percepts need not alter how similar the corresponding external features are perceived to be. Insufficient appreciation of this distinction is, I think, an important contributor to the confusion which widely persists on the difference between generalization and discrimination (see p. 230, below).

effort on repetitious sensory tasks such as signal monitoring (see Buchner & McGrath, 1963; Davies & Tune, 1970; Mackworth, 1968). The topic is at best marginal to this chapter, for it has entered learning theory only as a minor consideration in the skills-acquisition tradition. Even so, it deserves passing mention in that insofar as vigilance is to be analyzed "in terms of emission of observing responses" (Jerison & Pickett, 1963, p. 219), it points up the prospect that orienting behavior is but an executive phase of a more complex, centrally integrated system for self-regulation of input. Moreover, the concepts of "expectancy" and "habituation" (Sections III, F, G) have figured prominently in discussions of vigilance, albeit more as presuppositions than as targets of analysis. The perceptual issues of orienting behavior cited above are equally relevant to vigilance theory.

F. Expectancy

Theoretical controversies in perception and learning find an important intersection, though with rather different emphases, in the concept of "expectancy." For perception theory, expectancies have been problematic mainly in regard to how they may help enrich bare sensory givens into full-bodied percepts (see "sensory integration," above) and bias reception sensitivity to specific input features. Learning theorists, on the other hand, have worried whether such blatantly mentalistic entities as expectancies, i.e., central "ideas" of stimuli previously contingent in experience upon their present elicitors,* could be countenanced by an objective science of psychology. The main behaviorist sponsor of expectancies was of course Tolman (1932, 1959), whose theorizing however remained unconscionably intuitive until MacCorquodale and Meehl (1954) toughened it for him far too late in the day to reclaim its fair share of behavior-theoretic respect. Meanwhile, Hullian *S–R* partisans had (*a*) engaged Tolmanian theory in a series of inconclusive "latent learning" contests (see MacCorquodale & Meehl, 1954, pp. 199–213); (*b*) argued in some quarters that the theoretical point at issue was empirically vacuous (Kendler, 1952); and (*c*) conjectured mediation-response mechanisms to explain purported phenomena of the expectancy sort. [The inconsistency between moves (*b*) and (*c*) does not seem to have occasioned much embarrassment.] I have discussed the logic of S–R expectancy surrogates at some length elsewhere (Rozeboom, 1970, pp. 103–109, 118–123, and 130–136). Their trick is to replace an

* The additional refinements of commonsense expectancies, specifically, as anticipations of the future in contrast to other varieties of ideation, have never been recognized by behavior theory, nor to my knowledge by modern perceptual theory, either.

unmediated sensory-sensory association $s_i \rightarrow s_j$, where s_i and s_j are the respective ideational correlates of external stimuli S_i and S_j, with an *S–R* association $S_i \rightarrow r_j$ in which r_j is a hypothesized mediation response productive of sensory feedback s_j' more or less functionally equivalent to S_j. Although these *S–R* expectancy surrogates work very badly if at all, and there never has been the slightest evidence to favor them over ideational expectancies, hard-core behavior theory eventually (post-1950) came to tolerate any and all speculations about internal arousal sequences so long as these feigned a mediation-response embodiment. Unfortunately, there has been little empirical effort to determine in detail how expectancies operate, even though behavioral paradigms for doing so with quantitative precision now exist (Rozeboom, 1958). In humans, these show that expectancies can indeed occur in considerable strength, but also that neither *S–S* nor *S–R* associations adequately characterize their nature (Rozeboom, 1967). Thus the ebb of behavior-theoretic resistance to expectancies should not be taken as uncritical support for traditional views thereof.

G. Habituation and Inhibition

An extraordinarily pervasive phenomenon at all organizational levels from firing of single neurones to linguistic and existentialistic meaning (cf. semantic satiation and *ennui*) is that exercising a psychological function creates a temporary decrement in the ease with which that process can be reactivated, such decrements being to some extent cumulative under repetitive arousal (Thompson & Spencer, 1966; also see Ratner, 1970; Razran, 1971 Ch. 3). Moreover, the considerable duration of such habituation in some instances (notably, investigatory responses and defense reactions) and the ease with which it can often be set aside (disinhibition) by a shift in context strongly suggests that habituation is not just an inherent fatigue of the process activated, but also involves some extrinsive suppressive control which can be conditioned to and modified by other processes. Few topics in learning theory have had such a schizoid history as this one. Pavlov (1927, 1928) extensively researched autonomic inhibition, but had little evident impact in this outside of Russian reflexology. The verbal-learning tradition spoke much of "inhibition" as a paraphrase for "negative transfer," but analyzed the phenomena so labeled in terms of associative competition (interference) and unlearning (cf. Underwood & Ekstrand, 1966) without appeal to inhibitory mechanisms as such. The skills-acquisition tradition had abundant need for inhibitional concepts to account for reminiscence, distribution-of-practice, and work-decrement phenomena (see McGeoch & Irion, 1952), but borrowed Hull's (1943, Ch. 16) postulated mechanisms of reactive inhibition (I_R) and conditioned inhibition ($_SI_R$) rather than exploiting its indigenous theory in this regard (see Irion, 1966, p. 19ff.). Ironically, Hull seems to have introduced these concepts mainly because he mistakenly (see Rozeboom, 1970, p. 116f.) thought them necessary to explain extinction, and failed to reconcile them with the rest of his system (see Gleitman, Nachmias, & Neisser, 1954). Even so, there is considerable evidence (see Boakes & Halliday, 1972; also Hearst, 1969; Jenkins, 1965; Kalish, 1969 p. 270ff.; Rescorla, 1969) that response inhibition is not merely a genuine phenomenon but abides by essentially the same conditioning principles that govern response activation. Moreover, while the nonPavlovian learning literature has restricted its view of the inhibitable to response processes, only the inertia of a dying *S–R* outlook blocks extension of inhibition theory to sensory suppressions as well. Consequently, just as expectancies provide a mechanism for selective sensory enhancement of input features, so may conditioned arousal of the inhibitory counterpart of a sensory process—a negative expectancy—be conjectured to suppress perceptual vividness of a received input feature (a mechanism for *inattention*) and perhaps also assist in perception of negative features, i.e., noticing respects in which the environment is lacking.*

H. Generalization and Discrimination

Learning theorists have never been able to decide exactly how to relate the concepts of "generalization" and "discrimination" (cf. Brown, 1965). Both terms have been variously applied to (*a*) the difference in strength with which, for *o* at *t*, two stimuli S_i and S_j respectively elicit a given process *R*, (*b*) the extent to which shifts in the *R*-evocativeness of S_i for *o* at *t* transfer to S_j, and (*c*) how distinct are *o*'s central representations of S_i and S_j (see footnote, p. 227). On the whole, "generalization" has been most commonly understood as (*b*), while "discrimination" focuses upon (*a*) and (*c*), the last presumably determining *o*'s capacity to discriminate in sense (*a*). Senses (*a*)–(*c*) are in decreasing order of empirical determinability; moreover, importantly unlike (*c*), (*a*) and (*b*) do not require that S_i and S_j be action units for *o* at *t*. Behavioral research on generalization and discrimination has expanded remarkably during the past two decades (cf. Gilbert & Sutherland, 1969; Kalish, 1969; Riley, 1968; Mostofsky, 1965; Terrace, 1966), with discrimination having received

* The relation between positive and negative expectancies envisioned here is analogous—and likely much more than just analogy—to the relation between positive and negative visual afterimages, and to the mutually cancellative "efference copy" and "reafference copy" of motor action (von Holst, 1954).

much the greater share of the action. No brief summary of this area's complexities can aspire to more than travesty, but its key concerns may be condensed as (1) the extent to which the sense-(*b*) generalization between two stimuli results from their sense-(*c*) discriminability or differential tuning of *o*'s sensitivity to components of those stimuli, and (2) how far sense-(*b*) generalization of excitation and inhibition or some theory of feature selection can explain the development of discrimination in sense (*a*). Next to feature selection (see "Attention," below), most relevant of this for perception theory is the generalization/discriminability issue: Under what conditions if any do manifest changes in generalization between S_i and S_j for *o* reflect corresponding changes in the similarity between *o*'s percepts of S_i and S_j, and by what mechanisms might these resemblance changes occur? Much data have accrued to show that discrimination training on two stimuli S_i and S_j (i.e., reinforcing a response to one while extinguishing it to the other) decreases generalization along multiple stimulus dimensions, most evidently those on which S_i and S_j differ but also to some extent ones on which they are alike* (e.g., Arnoult, 1957; Honig, 1969; Kalish, 1969, p. 222ff.; Thomas *et al.*, 1970; Warren & McGonigle, 1969). Conversely, there is also some evidence that equivalence training on S_i and S_j (i.e, reinforcing both or extinguishing both to the same response) increases generalization at least between S_i and S_j themselves and perhaps between other stimuli as well (e.g., Honig, 1969). Behavior-theoretic interpretations of such learned generalization phenomena have taken two main forms, incremental (mediational) and decremental.† The former, a close kin of "sensory integration" theory (Section III, B, above), is the classic *S–R* approach: Under equivalence training, external stimuli S_i and S_j, with respective internal counterparts s_i and s_j, presumably become conditioned to a common response r_m whose sensory feedback s_m brings it about that the respective sensory consequences of S_i and S_j are no longer just s_i and s_j but the more similar $s_i + s_m$ and $s_j + s_m$ (Hull, 1939; Miller & Dollard, 1941, p. 74f.). Conversely, discrimination training supposedly attaches distinctively different mediation-response feedbacks s_m and s_n to S_i and S_j, respec-

* The concept of stimulus "dimension" has been treated confusingly in this literature through insufficient appreciation that entities located in a multidimensional space must logically have a position on *each* dimension of that space, not on just some of them, even if some values on those dimensions are anomolous. Thus when *o* is trained to discriminate a blank stimulus display from one containing a horizontal bar, it is misleading to say—as has become common—that the blank is not on the bar-tilt dimension at all; for *having no bar* is simply another alternative to features in the non-exhaustive set {*containing a bar tilted x degrees*}.

† Views of perceptual learning developed outside of behavior theory divide in this very same way (see Tighe & Tighe, 1966).

tively, thus revising the contrast of their sensory consequences from s_i versus s_j to $s_i + s_m$ versus $s_j + s_n$—an "acquired distinctiveness of cues" (Lawrence, 1949; Miller & Dollard, 1941, p. 73). However, despite the undeniable reality of incremental effects in, e.g., semantic generalization (cf. Feather, 1965) and verbal mediation (cf. Jenkins, 1963), it is doubtful that they suffice for learning of *sharp* discriminations and equivalences (cf. Estes, 1970, p. 171ff.; Rozeboom, 1970, p. 127ff.), or for the general steepening of generalization gradients by discrimination training (see especially Terrace, 1966, p. 307ff.). Some mechanism of the decremental sort seems also needed. In the latter, S_i and S_j are treated as complexes of features or elements, some but not all of which are common to both. Discrimination training on S_i versus S_j is then held to delete their shared elements from behavioral effectiveness (or equivalence training to delete their distinctive ones). How to achieve such differential cue neutralization, however, remains a theoretical puzzle; for, Hull's last-gasp proposals to this effect notwithstanding (Hull, 1952, p. 64ff.), orthodox *S–R* principles cannot achieve it (Rozeboom, 1970, p. 127) unless the unwanted stimulus components can be suppressed by orienting behavior. From its earliest days, the mathematical models approach to learning has simply postulated, without attempting deeper explanation, that irrelevant stimulus elements become ineffectual (see Bush & Mosteller, 1951, and numerous later papers collected in Neimark & Estes, 1967), and esentially the same is true of more recent "attention" theories which presume reduced selection of irrelevant cues. It is, of course, only sound science to diagnose a system's functions before conjecturing mechanisms which account for them; but despite its good intentions, learning theory cannot yet authoritatively advise perception theorists how, beyond receptor orientations, organisms manage to enhance the distinctiveness of elementwise overlapping complex inputs. Even more profoundly lacking at present are nonincremental theories of how stimulus elements themselves become differentiated, i.e., how similarity induction between action units can be decreased.

I. Attention

Although problems of stimulus selection had been tugging at the sleeve of behavior theory ever since the continuity/discontinuity controversy of the 1930's (see Kimble, 1961, p. 128ff.; Riley, 1968, p. 118ff.), it took a convergence of American mathematical modeling and the British outlook on input processing (e.g., Broadbent, 1958) to coalesce assorted strands of conditioning research into what, during the mid-1960's, became an explosion of attention to attention. The evidence is now overwhelming that the degree to which a stimulus feature S_i sensorily present to *o* at *t* has the **X**-wise effect on *o* which, learning-theoretically, it ought to—i.e., the degree to which S_i has action-unit salience for *o*'s value of **X** at *t*—is a local parameter whose empirical determinants, moreover, are to some small extent becoming identified (Egeth, 1967: Honig, 1969; Jenkins & Sainsbury, 1969; Kamin, 1969; Lovejoy, 1968; Mackintosh, 1965; Mostofsky, 1970; Sutherland & Mackintosh, 1971; Thomas, 1969, 1970; Trabasso & Bower, 1968; Wagner, 1969a, 1969b; Warren & McGonigle, 1969). Of especial importance for perception theory in this development are (*a*) its working of behavioral indicants of stimulus salience into a nonphenomenological data base for attention theory, and (*b*) its present emphasis upon stimulus selection by *dimensions* rather than by specific features. That is, the currently dominant conjecture is that when *o* is set at *t* to attend α and disregard β, α and β are *sets* of feature alternatives (e.g., *color* and *shape* rather than just *red* and *square*) such that *o* will be affected (unaffected) by whatever value of dimension α (β) holds for *o*'s input at that time even if that value falls outside the region of *o*'s past experience on that input dimension. If this thesis holds up even limitedly,* the concept of stimulus "dimension" will assume a psychonomic significance far weightier than its casual treatment to date now enables it to bear. Moreover, if stimulus selection really does turn out to parse pretty much by dimensions, it is surely not the case that all axes of the infinitely many different ways to dimensionalize stimulus space (e.g., height and width versus shape and size of stimulus objects) are equally available to *o*'s attentive mechanisms at t. The really big perception-theoretic payoff of behavioral attention theory will come when the latter begins to account for which stimulus dimensions *o* *can* attend to at *t*.

J. Stimulus-Configurational Phenomena

In Section II, I claimed that the probability with which a stimulus complex *T* elicits response *R* in *o* at *t* must be a concatenative function of the *R*-tendency strengths individually evoked by those features of *T* which are action units for *o* at *t*. Logically necessary or not, this has in any event been a standing assumption of learning theorists. Consequently, some have found the fact that an organism can be trained to make the same response *R* to each of two stimulus features S_1 and S_2 when these are presented separately, yet to withhold *R* when S_1 and S_2 occur together, inexplicable unless the

* The feature-positive versus feature-negative data of Jenkins and Sainsbury (1969) make clear that the two values of a binary dimension (feature present versus feature absent) do not capture attention with equal strength.

sensory effects of concurrent input features are so modified by "afferent interaction" among them that if S_1 unaccompanied by S_2 is received as s_1, and S_2 alone as s_2, S_1 and S_2 together are received as $s_1' + s_2'$, where s_1' differs from s_1 and s_2' from s_2. (Hull, 1943, Ch. 13; Razran, 1965b). On the face of it, a principle of afferent interaction should have major perceptual implications; however, not only is the theory of this unworkable,* it has been motivated by the gratuitous presumption that logically complex environmental features cannot themselves be action units. Actually, it is not at all behavior-theoretically anomalous to suppose that *o* can learn to do *R* in response to complex features *S_1-but-not-S_2* and *S_2-but-not-S_1*, to inhibit *R* in response to *S_1-and-S_2* and *neither-S_1-nor-S_2*, and that simple features S_1 and S_2 meanwhile lose *R*-wise action-unit status for *o* through whatever mechanism attenuates the salience of irrelevant cues. That such complex stimulus "patterns" can function as cues in their own right has, in fact, become a recent mathematical modeling orthodoxy (e.g., Atkinson & Estes, 1963, p. 239ff; Estes & Hopkins, 1961).

More profound in its theoretical implications is the configural phenomenon of relational responding wherein, e.g., given a choice between stimulus object *A* and another object *B* to which *A* stands in relation ϕ (larger than, darker than, etc.), *o*'s preference is strongly for *A* even though he has previously been reinforced for choosing *B* over another object *C* to which *B* is ϕ-related and has never previously encountered *A* (see, e.g., Hebert & Krantz, 1965; Reese, 1968). Despite the efforts of Spence, the main *S–R* spokesman on this point, to explain such "transpositional" phenomena as an artifact of approach and approach-inhibition tendencies generalized to new situations by similarity induction from nonrelational stimulus features of the training situation,† it is hard not to conclude that relational responding is generally real. That the class of perceivable stimulus features includes relations is not the deep issue here; most crucially at stake is some behavior-

* If this interaction principle were taken seriously, the enormous complexity of de facto stimulation totalities would result in no stimulus feature ever having the same sensory consequence on more than one occasion—whence the effects of past experience on present responding would have to derive from a principle of similarity induction sufficiently broad to undercut the sharp discriminability between s_1 and s_1' presumed by this hypothesis.

† For a comprehensive summary and discussion of this approach, see Reese (1968, p. 273ff.) Spence's model has to cook its generalization parameters just right if it is to obtain transposition, and cannot pretend to explain many forms of relational behavior (e.g., oddity selection and matching-to-sample); but a more penetrating criticism is that the model is not even logically coherent. The response supposedly being generalized is *approach,* but approach to what? (The only answer which even begins to make sense is "approach to the eliciting stimulus," but that works only if no more than one input feature can be evocatively effective at a time.)

theoretic recognition of perception's *propositional* composition (cf. O'Neil, 1958). When *o* chooses square *A* over square *B* by virtue of having learned to select the larger of two co-present squares, the stimulus complex which elicits this behavior is not just an unstructured aggregate of concurrent features, say *square-A* + *square-B* + *larger-than,* in which the relation is merely another isolated term. Rather, these features must occur in *o*'s input as parts of an integrated whole from which *o* can determine that *A,* not *B,* is the proper choice, i.e., a percept of *A's-being-a-larger-square-than-B* as distinct from *B's-being-a-larger-square-than-A.** Once one is attuned to appreciate it, the afferent importance of propositional structure (i.e., what distinguishes a grammatically well-formed sentence or sentential clause from a mere list of its constituent terms) is evident even in simultaneous discrimination between nonrelational features: If *o* learns at a T-maze choice point to pass through the gate marked with a circle and avoid the one with a triangle, his input must be something like *circle-on-left-gate, triangle-on-right-gate* rather than just *circle, triangle, left-gate, right-gate.*

In short, the responding of organisms to complex stimulus configurations makes clear that the set of action units received by *o* at *t* from his environment cannot adequately be described by a simple list of feature elements; at least some action units have a complex formal structure involving predication and probably other logical operations (e.g., negation) as well. Unfortunately, no mainstream approach to learning has yet evolved a theoretical framework within which the behavioral role of this structure can be expressed. Even so, recent efforts to interpret concept-attainment phenomena as a learning of "principles" or "rules" (e.g., Gagné, 1966; Hunt, 1962; cf. also Rozeboom, 1972b, p. 66f.) foreshadow the type of theory needed here.

K. Et Cetera

We may conclude by noting with even greater brevity than before a residuum of cryptoperceptual issues in learning theory. *Transfer of training* (cf. Ellis, 1965), which is the skills-acquisition and verbal-learning version of the concept of generalization, has an implicit concern for stimulus-selection principles through which different tasks are psychologically similar. The verbal-learning distinction between *nominal stimuli* and *functional stimuli* (cf. Underwood, 1963) emphasizes that the cues which actually

* See Rozeboom (1960, 1969). Alternatively, *o* might perceive, say, *A is a middle-sized square, B is a small square,* and from there infer *A is a larger square than B;* but either way, propositional structure plays an essential role in determining the input's consequences.

affect *o* at *t* are an *o*-determined selection from those to which *o* is exposed. Whatever else may be involved, the *intentional versus incidental learning* contrast (cf. McLaughlin, 1965) reflects an influence of cognitive set in human input processing. Finally, an exceptionally important notion which first appeared in surprisingly modern form at the very outset of skills-acquisition research (Bryan & Harter, 1899) and has figured in a variety of recent verbal-learning themes, notably *stimulus integration, response learning, meaningfulness and familiarity,* and *coding* (e.g., Goss, 1963; Mandler, 1954, 1967; Tulving & Madigan, 1970, p. 461ff; Underwood & Schulz, 1960), is that repeated activation of a complex sensory or sensorimotor process eventually gives it a *unitary* character which it initially lacks. This unitization is shown by various increased efficiencies in, e.g., reaction time and association formation, but perhaps most striking is the lessened demands of a unitized process on memory span and immediate recall (Melton, 1963; Miller, 1956). It does not seem inappropriate to think of complex but unitized processes as "concepts" in a sense envisioning an essential similarity between linguistic meaning units and comparable organizations wherein sensory or motor components dominate. Very likely, stimulus reception does not become full-blooded cognitive perception as we know it until rather high levels of afferent unitization are achieved. Were accounts of "sensory integration" (Section III B, above) to emphasize *integration,* rather than just the learned joint activation of sensory elements, and to seek insight into its still enigmatic psychonomic character, these might yet constitute the most satisfactory theory of perception now available.

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