An Examination of the Treatment of Stimulus Patterning in Professor Hull's *Principles of Behavior*

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In his *Principles of Behavior* (1943) Professor Hull has made a noteworthy attempt to derive the well-known empirical phenomena of stimulus patterning from more 'primary' principles in the general theory of behavior developed in that book. In the light of the continuing controversy among theorists as to whether such phenomena are reducible to more general principles or must be taken as primitive in a logical system of behavior, a successful effort in this direction would constitute an advance. Of course it is possible to develop alternative deductive systems eventuating in the same theorems but beginning with different selections of the primitive propositions, such that in one system of behavior configurational laws would appear as axioms while in another they would be derived. Perhaps a better formulation of the controversial point would be whether a set of nonconfigurational postulates can be found such that the empirical laws of patterning may be derived as theorems. It seems to the writer, however, that the basic issue is not whether configurational propositions enter at all, since it would be cumbersome and unnecessary to attempt to exclude them in the description of the stimulus side. Rather, the controversy is over the question 'Must postulates regarding the reactivity of the organism include the discrimination of stimulus configurations?' The present writer is wholly sympathetic to a reductive attempt and is without bias in regard to the sanctity of the 'configuration'; but it does not appear that Professor Hull's attempt can be called really successful unless the notion of 'reduction' is stretched to an unwholesome degree in order to bring his treatment under that description. The present paper will attempt briefly to defend this statement.

In the first place it is necessary to inquire in very general terms what such a reduction can be reasonably expected to achieve. What problem does the theorist face in such an attempt, and of what would any genuine 'solution' have to consist?

In a note at the conclusion of his chapter on the patterning of stimulus compounds, Professor Hull says:

"*Gestalt* writers frequently leave the impression that an adequate derivation of the reaction of organisms to stimulus configurations is *a priori* impossible from behavioristic or nonconsciousness principles. The position of the present work is that such a derivation is not only possible, but relatively simple and straightforward. Moreover, the preceding pages have presented a number of such deductions, thereby showing the *Gestalt a priori* claims to be mistaken. Meanwhile it remains to be seen whether *Gestalt Theorie* can itself mediate comparable deductions. Clearly, no dispute exists as to the genuineness or the importance of the patterning of stimulus compounds; the difference of opinion concerns, rather, the logical question of whether stimulus patterning is a primary or a secondary principle" (Hull, 1943, pp. 379-380).

It is necessary to distinguish the question whether 'conscious' principles are required to arrive at such a derivation, from the different question whether configurational premises are required. It is clear, I think, that one can include thoroughly 'configurational' laws in his primary propositions regarding behavior without making any reference to consciousness. For example, one may reinforce an organism when it makes a certain response in the presence of a discriminative stimulus consisting of three red lights in a horizontal row, but not if it responds to the 'same' three lights when the middle one is raised so as to form the 'pattern of a triangle.' One must recognize that aside from the discriminative capacities of the organism involved, the two stimulus situations differ objectively from the standpoint of the physicist as well as the psychologist. It is of course a mistake to speak as if the physical stimulations were identical and the only difference lay in the pattern-building propensities of the animal. It is logically impossible that any stimulus equivalence or non-equivalence which could be considered at all in a science of behavior would involve physically common (or disparate) properties on the stimulus side whose complete description would transcend the power of contemporary mathematical analysis, although it is already clear that such a description will often be of a high order of complexity. The point I wish to stress is that the physical description of the stimulating field can include what amount to configurational descriptions, and that hence configurational laws can be stated of which behavior is the dependent variable, without any recourse to 'consciousness.' It may be found impossible to accomplish a reduction to basic behavioral laws in which these configurational judgments do not appear, and yet it would remain possible to retain a thoroughly behavioral definition of the subject-matter. In the case described above, for example, it might turn out that one could not give an adequate description of the discriminative behavior of a dog so trained unless the defining property of the discriminative stimulus class was one of 'triangularity.' There is no reason to consider such an eventuality as disconcerting, since the specification of a 'triangle' is just as much a possibility for the mathematical description of the physical stimulation as, say, that of a sinusoidal function for a pure tone, or a decreased frequency for a light wave. It is not my contention that such irreducibility will ultimately be found, but merely that if it should be found it would constitute no real threat to a consistently behavioral formulation of the material. In what follows I shall take the complete independence of the 'configurational' and 'consciousness' requirements for granted, and confine my discussion to the former.

The problem with which we are presented is that of deducing the well established empirical laws in which configurational words and sentences appear, from a set of postulates of a more general character in which they do not. The first thing that seems apparent from this formulation is that the derivation of such a special case as that of Woodbury's dog (Hull, 1943, p. 351) can hardly be said to substantiate Professor Hull's assertion that he has shown the Gestalt claims to be 'mistaken.' It is not made clear in Professor Hull's chapter how he intends to pass from the case of reinforcing a response to two auditory stimuli separately but

extinguishing it in the presence of their combination, to the more important and typical case of discriminating 'patterns' in the visual field and the like. I do not mean to deny *a priori* a continuity among these cases, but it would seem that the successful derivation of the empirical findings for the more commonly mentioned case ought to be carried through before one can say that the Gestalt claims are 'mistaken.'

However, let us ignore this criticism and assume that whatever success attends the analysis of Woodbury's experiment may be tentatively generalized for patterning phenomena collectively. Even so it is hard to see wherein any genuine 'reduction' has been accomplished. The case of patterning with which Professor Hull is concerned is the relatively simple one in which two conditioned stimuli receive a different evocation strength when presented together from what might be expected knowing their strengths separately. Thus one conditions a response to a buzzer and also to a tone, and then presents both stimuli at once. That the strength of the response to the combination is less than what would be predicted had the two component strengths been simply added, is called 'spontaneous patterning.' It is further known that by reinforcing a response in the presence of the two stimuli when presented separately and never reinforcing it in the presence of the combination, the strength of the response in the presence of the combination may be made very low in spite of the maintenance of the same response to the separate components at very high strength. This is referred to as 'patterning by differential reinforcement.' The problem is to derive these findings from more 'primary' conditioning principles previously stated in the book.

There is of course no experimental or *a priori* reason to expect the habit strengths in question to simply 'add,' any more than there is for expecting them to combine in any other arbitrary fashion. I mention this only because there is a tendency to think of the fact that a dog may salivate to a light or to a buzzer but not to both at once as being somehow 'unreasonable' and surprising, although I do not attribute this psychology to Professor Hull. Directed forces do not simply 'add,' temperatures of mixed fluids do not simply 'add,' and there is no good reason for expecting that topographically overlapping habits should do so.

The general sequence of inferences which Professor Hull employs in order to derive the patterning effect in question from simpler principles is simple and straightforward, as he says. He makes use chiefly of two of his postulates, the postulate which states that "afferent neural impulses interact… in such a way as to change each other…" (Postulate 2), and the postulate which states and quantifies stimulus generalization (Postulate 5), together with his Major Corollary I which describes the manner of addition of habit strengths involving the same reaction but diverse stimuli (Hull, 1943, p. 199). In essence what he says is that the presentation of a second stimulus brings about an alteration in the afferent neural impulse of the first, and conversely. Since it is actually the afferent impulse and not the physical stimulus itself which has been conditioned, this change results in a decrease of the effective habit strength because of the existence of a stimulus generalization gradient previously postulated. In the case of patterning by differ-

ential reinforcement, use is made of the same kind of reasoning, except that here is invoked also the gradient associated with the incomplete generalization of inhibition.

Now the objection here offered to this procedure is not that it involves any statements one would care to deny, but that it is very little more than a restatement of the empirical phenomenon of patterning combined with a non-contributory localization of the processes in the nervous system. I am unable to see how Professor Hull is really saying anything more than "all of the stimuli presented exert some influence upon the animal, and if this influence is great enough it will show itself by a difference in the overt behavior. The chief locus of this influence is the animal's nervous system." The manner in which this 'interaction' takes place is not described or even hinted at. The neurophysiological laws are not stated, nor are the more exact loci given. For two visual stimuli the interaction can be readily thought of as retinal from the structural possibilities available, whereas between a tone and a light one cannot conceive of their 'interacting' until they have ceased to be 'afferent' impulses at all. The fact that the term 'afferent' does not really imply anything as to the locus of the interaction is made clear by Professor Hull in a more recent article (1945) on this subject. Unless one is able to state something in greater detail about the 'molecular' processes actually concerned in mediating the behavioral facts described, it seems to me that we have here an example of what Skinner (1938, p. 421) has referred to as the use of C.N.S. to mean 'conceptual nervous system.' The desired quantitative properties of the explanatory nervous system are constructed wholly from the properties of the behavior itself rather than from an independent investigation in the study of neurophysiology. It is not that the neural interaction hypothesis is to be denied, or even seriously questioned; it is rather that it has nothing to recommend it over the already admitted fact that the presentation of another stimulus often makes a difference in the behavior of the organism. It is of course no objection to a deductive-empirical system to point out that the theorems are contained in the postulates, inasmuch as some fallacies must have been committed in getting to the theorems if they were not so contained. But there is, it seems to me, a great difference between the way in which the laws of gases are 'implicit' in the dynamical theory of gases, and the way in which patterning is 'implicit' in Professor Hull's postulates. If the reduction of patterning to other principles is to be viewed as a genuine one, it must do more than reiterate the configurational facts as behaviorally observed and then assign the underlying processes to neural tissue. It is hard to see how Professor Hull's treatment does any more than this, except as regards the application of Perkins' equation to the material. It is in the use of this equation that one passes from a mere statement of the fact of interaction to a quantitative law regarding it, even though the data given in Professor Hull's book are wholly hypothetical cases. I shall examine this application of Perkins' equation in a moment.

Suppose one took the thoroughly positivistic attitude that the simplest way to handle this case is to include 'membership' (Skinner, 1938, p. 171) as a way in

which stimuli may differ. Thus a composite stimulus $S_aS_bS_c$ has 'membership' in common with another $S_aS_gS_h$ and to that extent one can expect some generalization to occur between them. The amount of generalization that will occur depends upon specific parameters admittedly not given immediately by this formulation, but in this respect Professor Hull's treatment is no better off since the specific parameters of his gradients of excitation and inhibition and the specific amounts of afferent neural interaction that occur are unknown and given purely hypothetic values in his illustrative examples.

'Membership' can even be made a special case of the stimulus generalization gradient, although such treatment does violence to our usual way of thinking. Consider a stimulus compound S_aS_b and another compound $S_{a'}S_b$ in which the prime indicates a change in intensity. Then on the basis of Postulate 5 (Hull, 1943, p. 199) alone one does not expect the generalization of excitation (or inhibition) from one of these to the other to be complete. Professor Hull (1943, p. 186) includes a curve showing the gradient of stimulus generalization for intensity in one particular experimental situation. The greater is the change in intensity represented by the prime, the smaller the generalization from the one compound to the other. If one were determined to be very parsimonious in the number of postulates, he might try to avoid the afferent neural interaction principles in favor of Postulate 5 alone, emphasizing that there is no special reason for introducing a theoretical discontinuity at the particular point where $S_{a'}$ falls below the threshold and the organism is then being effectively stimulated by S_b 'alone.' I do not advocate this formulation but merely wish to indicate its possibility and suggest that it does no violence to the facts. Including 'membership' as a special case of stimulus generalization enables one to make the same inferences as Professor Hull. Here, just as in his case, the postulate is not very useful until the specific quantitative properties of the gradients involved are known. But in a general way it is clear that one would expect the generalization from tone to tone plus buzzer to be incomplete, on the basis of the stimulus generalization gradient alone. Similarly one would expect the generalization from buzzer alone to tone plus buzzer to be incomplete in the complementary case. Hence the combination of tone plus buzzer evokes the incompletely generalized strength of two habits (with the same response topography) and one should expect the result to be less than the simple sum of the two habit strengths separately.

The preceding remarks do not treat of an important aspect of Professor Hull's chapter, namely the fact that he does more than merely attempt to elucidate how stimulus patterning can occur *at all.* In addition to this, he gives an account of the hypothetical quantitative relationships that obtain. It is readily conceded that if this quantitative relationship had been correctly deduced, it would have constituted a genuine contribution to analysis. For while the fact of patterning would be expected upon grounds given in Postulate 5, this postulate merely refers to the 'difference' as determining the degree of decrement along the generalized continuum, and the detailed quantitative properties of this 'difference' are not given. In the more recent article previously cited, Professor Hull (1945) neglects for

simplicity the exact manner of addition of the generalized potentials, devoting his attention more to the possible mathematical form of the afferent neural interaction gradients themselves. No comment on his proposed function can be made here since Professor Hull suggests it as a mere possibility and as yet no empirical evidence exists for evaluating its adequacy. The more precise treatment he refers to as being in the book is the one under present consideration. Presumably they are to be determined separately for the various cases that arise, such as the one-stimulus case of altered pitch, the two-stimulus case in which the intensity of one member of the pair is increased or decreased, and the sub-case of this latter in which the decreased intensity lowers the one stimulus of the 'patterned' pair below the threshold—the chief case under consideration in Professor Hull's chapter. If this quantification were successful, the chapter on stimulus patterning would be of the greatest importance even though the preceding arguments were accepted as correct. Unfortunately the quantification as it stands can hardly be of great value because its derivation contains a serious mathematical limitation upon the use of the equations proposed.

The equation used by Professor Hull (1939) is the so-called 'Perkins equation,' which was originally derived by Perkins for use in the paper on 'The problem of stimulus equivalence in behavior theory'. That derivation is expressed in terms of the summation of excitatory potentials for the same reaction conditioned to two different points on the *same* stimulus continuum, but the required assumption of 'physiological addition' can be readily restated for the case of two conditioned stimuli on different continua. In the original paper the required assumption reads:

"Let it also be assumed that overlapping generalization gradients combine in the same manner to produce a joint excitatory potential as would two sets of repetitions each sufficient to produce one of the generalized excitatory potentials in question" (Hull, 1939, p. 20).

That is to say, instead of adding the actual excitatory potentials of the components to obtain the resultant, it is required to add the reinforcements given to the two components and apply the resulting number in the basic formula, which is a simple positive growth function. Due to the non-linearity of the dependence of excitatory potential upon *n,* this result is smaller than would be obtained by adding the original excitatory potentials in question directly.[1]

The variation on this assumption which extends to the case for diverse stimulus continua, in which the only common experimental variable is the response itself, is stated in Professor Hull's Major Corollary I, as follows:

"All effective habit tendencies to a given reaction, whether positive or negative, which are active at a given time summate according to the positive growth principle exactly as

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[1] There is a confusing combination of typographical errors in the original article (Hull, 1939) with regard to Perkins' equation. On page 21, equation (8) should read

$$E_{r_1+r_2} = E_{r_1} + E_{r_2} - \frac{1}{A} E_{r_1} E_{r_2}.$$

would the reinforcements which would be required to produce each" (Hull, 1943, p. 199).

This postulate is expressed more succinctly in mathematical form by 'Day's equation' (Hull, 1943, p. 200), of which the Perkins' equation (as modified to apply to different stimulus continua) is easily seen to be a special case. The derivation of Day's equation is not given in Professor Hull's book and for purposes of criticism it must be given here.   Day's equation reads

$$_{ss}H_r = \Sigma_1 - \frac{\Sigma_2}{M} + \frac{\Sigma_3}{M^2} - \cdots$$
$$+ (-1)^{n-1} \frac{\Sigma_n}{M^{n-1}}$$

where Σ_1 is the simple sum of the excitatory potentials to be combined, Σ_2 is the sum of the products obtained by multiplying all of the excitatory potentials involved in all possible combinations two at a time, Σ_3 is the sum of all possible products of such potentials taken three at a time, and so on. M is here the physiological limit of the learning process, in other words the asymptote which the growth functions in question progressively approximate with succeeding reinforcements. The proof of this equation is as follows:

Let a response R be conditioned to a set of stimuli S_1, S_2, S_3, … S_k not excluding the case in which these stimuli belong to different continua or even different modalities. The usual objection that the response will be somewhat different depending upon which is involved need not detain us, since we are concerned with a quantitative treatment of certain selected (relevant) properties which define the class 'R' and do not propose to include in one equation the organism's 'behavior as a whole.' Then if y_j is the habit strength conditioned to the stimulus j, the entire set of habit strengths can be arrayed as follows, in accordance with Postulate 4 which states the dependence of a single habit's strength upon the number of reinforcements as a simple positive growth function:

$$y_1 = M(1 - e^{-iN_1})$$
$$y_2 = M(1 - e^{-iN_2})$$
$$\vdots \qquad\qquad\qquad\qquad (A)$$
$$y_k = M(1 - e^{-iN_k}).$$

The mathematical statement of Major Corollary I is clear directly from the verbal form, since we have merely to consider a single positive growth function in which the reinforcements-variable is the sum of the values of N necessary to yield the component habit strengths involved. Thus, if y_t is the total habit strength to the compound,

$$y_t = M\big(1 - e^{-i(N_1+N_2+N_3+\cdots+N_k)}\big)$$
$$= M\big(1 - e^{-iN_1}e^{-iN_2}e^{-iN_3}\cdots e^{-iN_k}\big).$$

If we return to the equations (A) we can express all of the negative exponential terms in terms of the component habit strengths, which is what we wish the final equation to do. Substituting these values in the above,

$$y_t = M\left[1 - \left(1 - \frac{y_1}{M}\right)\left(1 - \frac{y_2}{M}\right)\right.$$
$$\left.\times \left(1 - \frac{y_3}{M}\right) \cdots \left(1 - \frac{y_k}{M}\right)\right]$$
$$= M\left[1 - \overset{k}{\underset{1}{P}}\left(1 - \frac{y_j}{M}\right)\right]$$

where $\overset{k}{\underset{1}{P}}$ indicates the product of all the terms of form $\left(1 - \frac{y_j}{M}\right).$

It is shown easily by mathematical induction that an alternative algebraic form for the total product of such a set of complements $(1 - x_1)$, $(1 - x_2)$, $(1 - x_3)$ ... $(1 - x_m)$ is given by

$$\overset{m}{\underset{1}{P}}(1 - x_j)$$
$$= 1 - \Sigma x_j + \Sigma x_i x_j - \Sigma x_i x_j x_k$$
$$+ \cdots + (-1)^m \Sigma x_i x_j x_k \cdots x_m.$$

Taking the x's in the above general algebraic identity as the terms $\frac{y_j}{M}$ and substituting, we have

$$y_t = \Sigma y_j - \frac{\Sigma y_i y_j}{M} + \frac{\Sigma y_i y_j y_k}{M^2} - \cdots$$
$$+ (-1)^{m-1} \frac{\Sigma y_1 y_2 y_3 \cdots y_m}{M^{m-1}}$$

which is the form given as Day's equation. When only two habits are concerned, this reduces to the Perkins' equation published in the earlier paper.

An examination of the preceding considerations shows immediately a serious defect in Professor Hull's application of the formula, such as to invalidate the quantitative treatment given to the problem of stimulus patterning, for the vast majority of concrete cases. It is required in the derivation that the parameters M and i should be identical in value for all of the component habits to be summed. Any alternative derivation differing in minor algebraic details could evidently not escape this restriction as long as it makes use of Major Corollary I as stated in Equation (1). That is to say, both the asymptote of the growth function expressing strength as a function of number of reinforcements, and the negative exponential constant i which expresses the *rate* of growth (and thus *F,* the fractional growth increment per reinforcement) must be quantitatively identical for all of the habits to which the equation is to be applied. Thus if we condition a dog to salivate to a tone and also to a buzzer or a light, both of these conditioned responses must approach the same final state of strength after maximal reinforcement, and both must grow at the same rate. Now it is pointed out earlier in Professor Hull's book (1943, p. 207) that this is actually very rarely the case, and he spends over two pages discussing the various factors which may be employed to account for this difference, such as the finding of Pavlov that different modalities do not condition

equally strongly, that the intensity of the conditioned stimulus makes a difference, and so on. If we take any actual case of stimulus patterning, even in the controlled conditions of the laboratory, the Perkins' equation can be of no use to us in most cases because its derivation requires two assumptions about the growth of habits conditioned to different stimuli which are almost certain to be false.

In Professor Hull's chapter on the patterning of stimulus compounds, he makes use of several numerical examples which involve mathematical contradictions because of this fact. For example, he says:

"Let it be assumed that by the Pavlovian technique a simultaneous stimulus compound of two components (S_1 and S_2) is conditioned to a reaction (R) strongly enough for S_1 to command a habit strength of 40 habs and S_2 to command a habit strength of 60 habs" (Hull, 1943, p. 356).

Now if the simultaneous conditioning of the two habits has resulted in the acquisition of these two different habit strengths, we know that either the constant M or F, or both, must be different in the equations for their respective rates and asymptotes of growth. Yet after setting the drive at unity, Professor Hull goes on to say,

"Now, by the physiological summation of these two excitatory potentials, we have

$$Q' = {}_{S_1 + S_2}E_R = 40 + 60 - \frac{40 \times 60}{100}"$$

making use of the Perkins' equation, the derivation of which depends upon a denial of the assumption just preceding.[2] This criticism applies without modification to the other quantitative examples given in the chapter. It probably explains in part at least the fact that if one applies the Perkins' equation to the data presented in Chapter XIII on 'Compound Conditioned Stimuli' (Hull, 1943, pp. 209-212) in such a way as to solve *backwards* for M and then applies the resultant value to the other cases given, the agreement with these other results is not encouraging in the least.

In summary, my contentions are: (1) The general fact of stimulus patterning (whether spontaneous or produced by differential reinforcement) can be parsimoniously included under the general case of the generalization gradient and requires no special treatment to account for its mere occurrence; (2) the attempt to give it a special treatment by making use of the afferent neural interaction hypothesis is in effect simply restating the behavioral finding; (3) the real problem presented by the fact of patterning is the quantification of the laws of interaction and generalization to make concrete prediction possible, which Professor Hull has been unable to do because of the very limited applicability of the Perkins' equation. This is not to be in the least construed to imply a deprecation of the effort

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[2] If one abandons the effort to derive Perkins' equation from Corollary I and merely assumes such an additive law to hold, he is still confined by the appearance of M in the equation itself, in addition to the fact that in the absence of any direct empirical evidence regarding it, there seems no good reason left for assuming such a relation.

nor an expectancy on the writer's part that such laws may not be found. As Leeper has pointed out in his review of Professor Hull's book, some of them have already been found by the Gestalt psychologists, but not as explicitly and quantitatively formulated as could be desired. Whether a function involving the summation (including 'physiological summation') of stimulus components is the kind of law which will turn out to be most adequate is not clear. There does not seem to be any strong evidence in Professor Hull's book that it will. His experimental material on this point is confined to a set of data on the conditioned galvanic skin response to which the Perkins' equation is not applied (and if applied does not work), plus the setting up of a patterned reaction to two buzzers of different pitch which is not quantified at all by the Perkins' equation but only by the patterning index. It is not inconceivable that because of the specificity of generalization gradients as regards different kinds of stimulus material, the most that will be achieved in the way of a *general* formulation will not be much more specific than what is now available in the writings of the Gestalt psychologists. It would be disappointing to many if this should turn out to be the case, but it is of the greatest importance that those interested in analysis should not mislead themselves, despite the best of intentions, into supposing that they have advanced beyond such a level when they have not in fact done so.

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