ON THE REFORMULATION OF INHIBITION IN HULL'S SYSTEM

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Among the least satisfactory elements of Hull's behavior system is his formulation of inhibition. As a result, there have been several attempts in recent years to reformulate Hull's theory with respect to the inhibition variables in the equation for effective reaction potential $(_{s}\overline{E}_{R})$. The present paper critically examines these reformulations in the light of relevant experimental evidence. The conclusions to which this examination leads are that these reformulations have not been an improvement over Hull and that this kind of reformulation itself is a futile approach to the problem of improving Hulliantype learning theory.

In all versions of his theory Hull (1943, 1951, 1952) formulated "effective reaction potential" $({}_{S}\overline{E}_{R})$ as being essentially a function of "drive" (D) and "habit strength" $({}_{S}H_{R})$, related multiplicatively (i.e., $D \times_{S}H_{R})$, minus "reactive inhibition" (I_{R}) and "conditioned inhibition" $({}_{S}I_{R})$, related additively (i.e., $I_{R}+{}_{S}I_{R})$. Thus:

$$_{S}\overline{E}_{R} = (D \times_{S} H_{R}) - (I_{R} + _{S} I_{R})$$

Most of the attempts to reformulate Hull's equation have been the result of logical, or at times merely verbal, rather than empirical considerations. For example, Hilgard's (1956, p. 139) criticism is directed at the fact that Hull did not carry out the logical implications of his statement that I_R is a "negative drive state." As such, I_R logically should subtract from D (i.e., $D-I_R$) and, like D, should interact multiplicatively with habit strength (i.e., $I_R \times_S H_R$). Hilgard also suggests that, since ${}_S I_R$ is a negative habit, it should interact multiplicatively with I_R . Thus, Hilgard's proposed reformulation of the equation for net reaction potential results in the following:

$$_{S}\overline{E}_{R} = \left[(D - I_{R}) \times_{S} H_{R} \right] - (I_{R} \times_{S} I_{R})$$

This new formulation seems to be more consistent with some of Hull's own statements about the nature of these intervening variables, but Hilgard avoids trouble by not attempting to relate this formulation to empirical findings.

Similarly, Iwahara (1957) carries Hull's characterization of I_R as a negative drive and ${}_{S}I_{R}$ as a negative habit to what may seem the logical conclusion in terms of the internal consistency of Hull's theory-that the relationship between drives and habits is always multiplicative and never additive. Iwahara then goes a step further to regard ${}_{S}I_{R}$ as a conditioned or secondary negative drive, with I_R being the primary negative drive. From this it follows that the product of $I_R \times_S I_R$ should subtract from positive drive, D, and should also multiply ${}_{s}H_{R}$. Symbolically,

 $s\overline{E}_{R} = sH_{R} \times [D - (I_{R} \times sI_{R})]$

or, in expanded form,

 $_{S}\overline{E}_{R} = (_{S}H_{R} \times D) - (_{S}H_{R} \times I_{R} \times _{S}I_{R})$

Osgood (1953, p. 379) states that Hull need not have postulated ${}_{S}I_{R}$ at all, since it might have been derived from other postulates in the system. If ${}_{S}I_{R}$ is nothing other than negative habit strength or the habit of not responding (reinforced by the dissipation of I_R), it would seem logical to subtract ${}_{S}I_{R}$ directly from ${}_{S}H_{R}$. This is the formulation Osgood has proposed (p. 349).

More recently, Jones (1958) has incorporated the foregoing suggestions in his revision of Hull's equation. The Jones version, which combines the properties of the other revisions (except Iwahara's ${}_{S}H_{R} \times {}_{S}I_{R}$) and appears identical to Osgood's suggestion, is as follows:

$$_{S}\overline{E}_{R} = (D - I_{R}) \times (_{S}H_{R} - _{S}I_{R})$$

That this formulation is quite radically different from Hull's is even more obvious when Jones mathematically expands the equation, thus:

$$s\overline{E}_{R} = (D \times_{S} H_{R}) - (I_{R} \times_{S} H_{R}) - (D \times_{S} I_{R}) + (I_{R} \times_{S} I_{R})$$

Jones' formulation has been subscribed to by Eysenck and his coworkers in their attempt to utilize Hullian postulates in developing a theory of personality (Eysenck, 1957; Kendrick, 1958).

Another revision, rather casually suggested by Woodworth and Schlosberg (1954, p. 668), is that inhibition $(I_R \text{ or } sI_R \text{ or both}?)$ should subtract from "incentive motivation" (Hull's K, a function of the amount of reinforcement). Presumably the total inhibitory potential I_R (the sum of I_R+sI_R) subtracts from K, though this point is not clear in the Woodworth and Schlosberg discussion. Their suggestion might be expressed symbolically as follows:

$$_{S}\overline{E}_{R} = (K - I_{R} - _{S}I_{R}) \times D \times _{S}H_{R}$$

The most carefully formulated and empirically anchored modifications of Hull's theory have been those of Spence (1956). His changes in the

inhibition part of the theory are of a fundamentally different nature than the other revisions. He has more or less wiped the slate clean and started anew by redefining inhibition and the independent variables of which it is a function. Spence's extinctive inhibition (I_n) is not a function of the amount of effort or rate of responding, as is Hull's I_R , but is a function only of the number of nonreinforced responses. There is also an oscillatory inhibition (I_{o}) , which is the same as Hull's concept of oscillation $({}_{S}O_{R})$. The inhibition due to delay of reward (I_t) is essentially the same as I_n . The basis of this inhibition is assumed to be the competing responses that are established during the delay period or during extinction. The molar concepts of I_t or I_n simply represent the quantitative effects of these competing responses. Spence's inhibition does not interact with other intervening variables but only subtracts from the reaction potential. In this last respect his formulation is essentially no different from Hull's. It might be asked why D, if it is regarded as an energizer of all responses in the organism's repertoire, should not interact with inhibition as Spence conceives of it, that is, as consisting of interfering or compet-In this respect ing responses. Spence's theory of extinction is not unlike Guthrie's.

With the exception of Spence, these attempts to reformulate Hull raise a number of crucial questions in common, some of which must be critically examined on the level of theory and methodology and others in terms of empirical evidence. First there are questions of a general theoretical nature which must be considered in relation to any attempt to criticize or reformulate Hull's theory.

1. Is the verbal formulation of

Hull's theory to be taken more seriously than the symbolic and quasi-quantitative formulations, or than the actual empirical relationships which formed the basis for Hull's postulates and which he has held up as examples of the relationships he wished his system to predict?

2. Does the algebraic manipulation of Hull's intervening variables make sense theoretically and psychologically? Are the functions representing their interrelationships "isomorphic" with the rules of simple algebra?

3. Can experiments be designed to determine the exact nature of the intervening variables?

Once one has decided to argue within the Hullian framework a number of questions arise from the attempts at reformulation, the answers to which must depend upon empirical findings.

1. Does ${}_{S}I_{R}$ subtract from ${}_{S}H_{R}$? Are ${}_{S}H_{R}$ and ${}_{S}I_{R}$ both basically the same phenomenon, one merely being positive and the other negative in effect, or do they represent basically different processes?

2. Is there any empirical evidence to support the following formulations?

a. The interaction of $D \times_{s} I_{R}$ (Jones, Osgood)

b. $D-I_R$ (Hilgard, Jones, Osgood)

c. The interaction of ${}_{S}H_{R} \times I_{R}$ (Hilgard, Iwahara, Jones, Osgood)

d. The interaction of ${}_{S}H_{R} \times {}_{S}I_{R}$ (Iwahara)

e. The interaction of $I_R \times_S I_R$, which paradoxically represents an *addition* to reaction potential, the multiplication of two negative quantities making a positive (Hilgard, Iwahara, Jones, Osgood)

f. $K - I_R$ (Woodworth & Schlosberg)

THE LIMITATIONS OF HULL'S THEORY

In offering his revision, Jones (1958) points out that the inhibition aspect of Hull's formula for reaction potential has been criticized by Koch (1954). Koch's criticisms, however, apply equally to Jones' revision as well as to all the others, with the possible exception of Spence. Koch points out that the intervening variables concerning inhibition in Hull's system, particularly ${}_{S}I_{R}$, are not rigorously defined, are not clearly tied to experimental variables, and hence are indeterminate. Because of this, it is impossible to make rigorous experimental tests of Hull's formulations or of the alternative revisions. Cotton (1955) has shown that a literal interpretation of Hull's postulates leads to predictions that differ from the experimental data upon which Hull based the formulation of his postulates in the first place. In short, much of Hull's theory does not even predict the very facts it was expressly devised to predict. This is especially true with regard to the inhibition postulates. None of the revisions of Hull has improved this situation. They have merely rearranged in various ways the same indeterminate variables of Hull's formula for $_{R}\overline{E}_{R}$.

Hull's revisers have followed him in treating his intervening variables, D, ${}_{s}H_{R}$, I_{R} , ${}_{s}I_{R}$, etc., as if they were real, independent quantities whose laws of interaction are isomorphic with the rules of arithmetic and algebra. As we shall see, the manipulation of these hypothetical variables in such fashion can at times lead to absurdity. Hull's intervening variables *are* only intervening variables in the sense which MacCorquodale and Meehl (1948) have assigned to that term, and are defined only in terms of the independent and dependent

variables to which they are tied. The danger arises when Hull's revisers mathematically manipulate the intervening variables without regard for the defining experimental variables which are actually all that give any meaning to the intervening vari-Of course, one of the purables. ported virtues of intervening variables is that they can be mathematically manipulated as independent entities. But once the intervening variable has been properly defined, the question arises as to the nature of the mathematical operations that can suitably be applied to it. It is highly doubtful if the exclusive use of linear algebra by Hull and his revisers is at all suitable. It should be noted that in Hull's own statements (1943) the relationship between experimental variables and intervening variables is usually anything but linear. If the exact form of the functional relationship is not known, performing linear algebraic operations on the intervening variables is practically meaningless. Under these conditions, for example, one cannot prove on the basis of experimental data whether changes in response strength are the result of an additive or a multiplicative relationship between intervening variables. From more fundamental considerations, Hilgard (1958) points out that Hull's intervening variables cannot in their present form be multiplied meaningfully, since they are not in comparable units of measurement. Certainly the least objectionable formula for reaction potential is also the least specific. Consequently it has the least predictive power:

$$s\overline{E}_{R} = f(D, K, sH_{R}, I_{R}, \text{etc.})$$

In view of the facts here noted, great difficulties arise when Hull and his revisers become more explicit about the nature of the relationships between these variables.

Though it would not be in keeping with the spirit of Hull's formal theorizing, some of the problems might be avoided if Hull's formula for $s\overline{E}_R$ were regarded, not as a true mathematical equation, but merely as a kind of shorthand for expressing certain relationships suggested by empirical findings. The arithmetic signs of addition, subtraction, and multiplication in the formula would then not be taken too literally. Thus, E = H - I would not be taken to mean that inhibition subtracts from habit and that when E finally equals zero, the habit has been removed and the organism restored to the same state as before the habit had been The equation merely acquired. states in shorthand form that reaction potential, as inferred from some measure of response strength, decreases as the experimental procedures said to increase habit strength are removed and the conditions said to produce inhibition are applied. The subtraction sign is used here, not in a strict mathematical sense, but only as a shorthand expression for an experimental manipulation. Whether Hull has chosen to add or to multiply various intervening variables most likely has been a result of his attempt primarily to represent known empirical relationships rather than to maintain logical consistency within his theory. He most likely formulated $D \times_{S} H_{R}$, for example, because he believed this interaction of habit and drive represented the experimental evidence. And most probably the reason he did not formulate $D \times_{sI_{R}}$, even though his theory seems to call for this logically, was simply because he found no evidence that suggests an interaction between drive and inhibition.

From the foregoing considerations, probably the ultimate conclusion to which we are forced regarding the attempted revisions of Hull's theory is not so much that these revisions are no improvement over Hull, but that it is futile to attempt to improve upon Hull by mere juggling of his intervening variables. Hullian theory will not be improved by continuing to work with the concepts of drive, habit, inhibition, etc. in exactly the same form they were given by Hull. The very building blocks of the theory, so to speak, are inadequate, and no amount of recombining them in new ways is likely to result in any substantial advance in learning theory.

Reformulations and Empirical Evidence

$_{S}H_{R}-_{S}I_{R}$

While Hull (1943) refers to ${}_{S}I_{R}$ as a "negative habit," there is no indication in his writing that he regards ${}_{S}I_{R}$ as merely negative ${}_{S}H_{R}$. The revisions suggested by Osgood and by Jones are based on the assumption that ${}_{S}H_{R}$ and ${}_{S}I_{R}$ are basically the same phenomenon, $_{S}I_{R}$ merely being the negative counterpart of $_{S}H_{R}$. Thus, if they are the same process but merely opposite in effect, it seems logical that one should subtract from the other. Similarly, if $_{S}H_{R}$ interacts with drive, so should $_{SI_{R}}$. Hull, however, quite clearly did not regard ${}_{s}H_{R}$ and ${}_{s}I_{R}$ as basically one and the same phenomenon, and his reasons are based on experimental evidence that reveals differences between the two. Pavlov (1927) originally pointed out the greater susceptibility of internal inhibition (of which ${}_{S}I_{R}$ is one variety) to external inhibition (i.e., disinhibition) than is the case with the excitatory process corresponding to Hull's $_{S}H_{R}$. That $_{S}I_{R}$ is more labile and sensitive to external influences than is ${}_{S}H_{R}$ suggests that it is not merely the negative counterpart of the same phenomenon. Therefore, Hull is consistent with Pavlov in not subtracting ${}_{S}I_{R}$ directly from ${}_{S}H_{R}$.

Another line of evidence that excitation (conditioning) and inhibition (extinction) are basically different processes is well demonstrated in a series of experiments by Reynolds (1945a, 1945b), which showed that acquisition of a conditioned response is slower for massed than for distributed trials, while the *reverse* relationship holds for extinction. Also a number of studies (Hilgard & Marquis, 1940, p. 119) have shown a *negative* correlation between the speed of conditioning and of extinction.

The issue of whether the generalization gradients of excitation (conditioning) and inhibition (extinction) are the same or different was left undecided by Hull (1943, p. 265). The Bass and Hull (1934) and Hovland (1937) studies referred to by Hull were not adequate to answer this question. Not finding evidence to the contrary, Hull merely assumed that the generalization gradients of excitation and inhibition were the same, which is a convenient assumption in his theory of simple discrimination learning (1943, p. 267) based on the interaction of the gradients of excitation and inhibition. On this point, however, there is now some tentative evidence that seems to contradict Hull's assump-Liberman (1951) found that tion. extinction $({}_{S}I_{R})^{1}$ has broader transfer

¹ In Hull's system, though the entire process of extinction is not explained in terms of only $_{S}I_{R}$, but includes reactive inhibition (I_{R}) as well, once extinction is complete, or after enough time (probably 5 to 10 minutes) has elapsed for the dissipation of I_{R} , extinction is conceived of as solely a function of the relative magnitudes of the positive reaction potential and $_{S}I_{R}$. effects than acquisition $({}_{s}H_{R})$. Also there is some evidence (Razran, 1938) that the stimulus generalization of extinction $({}_{s}I_{R})$ differs from that of excitation $({}_{s}H_{R})$, in that extinction shows greater stimulus generalization; the gradient of its generalization contains fewer steps; the stimulus generalization of extinction, unlike that of acquisition, does not extend to heterogeneous CRs; and generalization of extinction is more affected by drugs than is generalization of conditioning.

The formulation ${}_{s}H_{R} - {}_{s}I_{R}$ seems misleading in view of the fact that successive periods of acquisition and extinction become more rapid and that an organism in which an acquired response has been extinguished is not the same as an organism that had never acquired the response. Razran (1956) has pointed out that in a partially extinguished CR there can be shown the coexistence of two opposing processes, positive and negative. "Even a wholly extinguished CR bears, by all signs, within itself a two-way CR connection" (p. 42). Successive acquisition and extinction may be conceived of as a kind of discrimination learning, in which both ${}_{S}H_{R}$ and ${}_{S}I_{R}$ grow simultaneously, neither one diminishing the other. The cessation of reinforcement becomes a cue, a condi tioned inhibitor, the strength of which increases throughout successive extinction periods (Bullock & Smith, 1953; Perkins & Cacioppo. This kind of discrimination 1950). learning is likely to be a very primitive kind of discrimination not involving symbolic or mediating proc-Tentative evidence for this esses. opinion is found in the experiments on spinal conditioning, which, however, are not yet entirely beyond dispute as examples of true condition-Nevertheless, for what it is ing.

worth, Shurrager and Shurrager (1946) have reported that both conditioning and extinction, measured at a single synapse in a spinal preparation, become faster with successive periods of conditioning and extinction.

Hull (1952, p. 114) also pointed out that the delay CR (the "inhibition of delay" being due to ${}_{S}I_{R}$) is eliminated by certain drugs, for example, caffeine and benzedrine. It is hard to see why the CR itself would not be markedly weakened or eliminated altogether if these drugs affected both ${}_{S}H_{R}$ and ${}_{S}I_{R}$ in the same The CR is strengthened. manner. however, while the period of delay is markedly shortened. Certain drugs thus seem to have opposite effects on $_{s}H_{R}$ and $_{s}I_{R}$, suggesting again that they represent essentially different underlying physiological processes. Skinner's (1938, pp. 412-413) finding that benzedrine and caffeine increase the number of responses to a criterion of extinction lends plausibility to the idea that these drugs have different effects on ${}_{s}H_{R}$ and ${}_{s}I_{R}$. If ${}_{s}H_{R}$ and $_{S}I_{R}$ were the same process, then a drug increasing ${}_{s}H_{R}$ would also increase the inhibitory effect of each nonreinforced response. If this were the case, the unfailing effect of stimulant drugs in increasing the number of responses to extinction could not easily be accounted for. The evidence bearing on this subject, however, is not crucial, in that we do not have evidence regarding the percentage increase in responding during extinction under benzedrine over the operant level (preconditioning response rate) under benzedrine. Also it should be noted that the theoretical problem hinges to some extent upon the hypothesized relationship between excitation (or $_{s}H_{R}$) and inhibition $({}_{S}I_{R})$; that is, whether it is the absolute difference between the

two that matters or the *ratio* (or "balance") between excitation and inhibition. In the Pavlovian system it is the balance or ratio of excitation to inhibition that determines reaction potential. In Hull's system it is the absolute difference between ${}_{S}H_{R}$ (and the variables interacting with it) and I_{R} . A strictly Pavlovian revision of Hull might take the following form:

$${}_{S}\overline{E}_{R} = \log \frac{D \times_{S} H_{R}}{\dot{I}_{R}}$$

Thus it is the balance between excitatory and inhibitory processes that is emphasized and not the absolute difference. In this equation, when the total inhibitory potential (I_R) is equal in strength to $D \times_{S} H_R$, the ratio of $D \times_{S} H_R/I_R$ becomes 1.0, and since log 1.0=0, the effective reaction potential $(s\overline{E}_R)$ will equal zero.

The fact that Eysenck and his coworkers have subscribed to the Jones revision would seem incompatible with Eysenck's (1956) theory concerning the extinction of $_{S}I_{R}$. The extinction of ${}_{S}I_{R}$ is paradoxical and inconsistent with other aspects of Hull's theory and also of Jones' revision. If, as maintained by Jones and by Eysenck, ${}_{S}I_{R}$ is merely negative ${}_{s}H_{R}$, then the mere lack of reinforcement of ${}_{S}I_{R}$ (reinforcement being the dissipation or avoidance of I_R) should not result in a decrease in $_{SI_{R}}$. Lack of reinforcement does not diminish the ${}_{s}H_{R}$ already present, so it should not diminish ${}_{S}I_{R}$ either. The notion that extinction is an active process of an increasing inhibition (I_R) depressing performance $(_{s}E_{R})$ is basic in Hull's system. It, therefore, seems absurd, while remaining in the Hullian framework, to speak of the extinction of inhibition without first postulating a second inhibitory process which depresses the first. Fortunately, there is no experimental evidence at present to suggest that such a complication would be necessary.

$D \times_{s} I_{R}$

In Hull's theory there is no interaction between drive and conditioned inhibition. The $D \times_{S} I_{R}$ interaction, however, is explicit in a number of the revisions. Since $_{S} I_{R}$ is the primary and essential intervening variable accounting for experimental extinction, we may well examine the different predictions generated by Hull and the revisions with respect to the $D \times_{S} I_{R}$ interaction.

According to Hull, since D multiplies only ${}_{S}H_{R}$ and not ${}_{S}I_{R}$, we should predict that certain measures of extinction will be affected by changes in D. With the Hullian formula $D \times_{s} H_{R} - {}_{s} I_{R}$, one can predict that under a high drive level there will be a greater number of responses to extinction (n) than under low drive. The same increment of ${}_{S}I_{R}$ is generated by each response during extinction, regardless of the level of D, while the positive reaction potential $(D \times_{S} H_{R})$ is increased by a higher level of D. Not only does it follow from Hull's formula that a greater number of responses is required for extinction, but extinction curves under high and low D should be parallel. They approach the criterion of extinction with the same slope, but reach it at different points.

The revisions containing the $D \times_{s} I_{R}$ interaction generate predictions that are exactly opposite to the foregoing. If net reaction potential is a resultant of $D \times_{s} H_{R}$ $-D \times_{s} I_{R}$, then every increment of ${}_{s} I_{R}$ will be increased by D to the same degree that ${}_{s} H_{R}$ has been increased. Consequently, there should be the



FIG. 1. The relationships between drive (D), number of trials to extinction (n), and effective reaction potential $(s\bar{E}_R)$ as predicted by Hull's formulation (left) and by Jones's formulation (right).

same number of responses to extinction under high drive as under low drive. Also, the slopes of the extinction curves, as measured by, say, rate of responding, would be different under high and low drive. In other words, the curves would approach the criterion of extinction with *different slopes*, but would reach it at the *same point*.

If the proponents of the $D \times_{s} I_{R}$ formulation object to the foregoing predictions on the grounds that I_R has not been taken into account, let it be pointed out that ${}_{S}I_{R}$ is essential for complete extinction of the response and that extinction can take place with sufficiently spaced trials to prevent the growth of I_R . If, as Hull hypothesized (1943, pp. 300-301), the formation of ${}_{S}I_{R}$ is dependent upon nonresponding being coincident with the dissipation of I_R , extinction could not take place if all I_R had dissipated in the interval between each presentation of the nonreinforced CS. Yet extinction is known to occur even with long intertrial intervals of 24 hours or more, when I_R should supposedly have been completely dissipated (Razran, 1956, p. 43). This, along with the

fact that in all of the revisions an increment of I_R will reduce ${}_{s}E_R$ by the same proportion regardless of the level of D, makes I_R irrelevant to the present argument. (The $D-I_R$ formulation is discussed at a later point.)

There is a considerable amount of experimental evidence bearing on the above predictions. The preponderance of evidence favors the Hullian formula and fails to support the notion of a $D \times_{s} I_{R}$ interaction. Perin (1942), working with rats, found a marked positive relationship between D (degree of hunger) at the time of extinction and the number of responses required for extinction. Brandauer (1953) extinguished bar pressing in rats under three levels of drive (thirst) and found a positive relationship between strength of drive and number of responses during extinction. Even under minimal differences in hunger drive (.5, 1, 2 hours' deprivation) Saltzman and Koch (1948) found highly significant differences in number of responses to extinction in a modified Skinner box. Brown (1956) also found that rats on high drive make more responses during extinction than those on low

drive. Cautela (1956) showed essentially the same relationship for the extinction of a discrimination response. However, he found a slight decrease in n for levels of D beyond 23 hours' deprivation. He attributed this phenomenon to the generalization gradient of the drive stimuli; under the highest levels of D, the drive stimuli were further out on the generalization gradient from the drive conditions under which the original learning had occurred. The energizing and stimulus properties of drive are thus apt to interact in this type of experiment.

In experiments with human subjects, where anxiety has been used as a measure of drive, a similar relationship with extinction has been found. In one study, high anxiety subjects required almost twice the number of trials to extinguish the conditioned eyeblink as did low anxiety subjects (Spence & Farber, 1953). Bitterman and Holtzman (1952) obtained similar results in extinguishing the PGR in high and low anxiety subjects.

Skinner's (1938) early notion of the "reflex reserve" appears to be consistent with the $D \times_{s} I_{R}$ formulation. Skinner believed that the number of responses emitted during extinction was solely a function of the number of previously reinforced responses and the schedule of reinforcement. Thus drive should not affect n, but would affect only the rate of emission of responses. The reflex reserve concept, however, has long since been found unfruitful. While theoretically it is probably not a strictly testable hypothesis, it now at least appears quite incorrect in view of the evidence (Ellson, 1939). Skinner's (1938) original belief that rate of responding, but not the number of responses in extinction (n),

is affected by drive is contradicted by Bullock's (1950) investigation showing a correlation of .61 between rate and n. This positive correlation between response rate and number of responses to extinction would certainly seem inconsistent with a $D \times_{S} I_{R}$ formulation. If drive increases response rate, ${}_{S}I_{R}$ should increase faster under higher drive, each response adding the increment $D \times_{s} I_{R}$, thus leading to more rapid extinction. The evidence is exactly the contrary. Higher drive not only increases the rate of response, but also increases the total number of responses to a criterion of extinction.

The best available evidence also indicates that the slope of the extinction curve is the same under high and low drive, as would be predicted from Hull's theory. Sackett (1939) showed that when the extinction curves of two groups of rats, one group extinguished under 6 hours' hunger drive and the other under 30 hours' drive, are Vincentized, the forms of the two curves are almost identical. The 30-hour group produced more responses to extinction and required more time to extinguish, but the slope of the extinction curve was the same as that of the 6hour group. Barry (1958) trained rats in a running response and extinguished them under high and low drive. The extinction curves were parallel, and when drive was equalized in both groups late in extinction, the curves converged and were identical after three trials. When drive was equal for both groups early in extinction, and then, later in extinction, the groups were run under high and low drive, the extinction curves diverged, and, after three trials, continued almost parallel, as would be predicted from Hull. (The fact that it took three trials, rather than one,

for the curves to converge or diverge after the change in D, however, is somewhat embarrassing to Hull's theory as it is also to the revision.) Both these findings are consistent with the $D \times_{s} H_{R} - {}_{s} I_{R}$ formulation and not with $D \times_{S} H_{R} - D \times_{S} I_{R}$. But these experiments cannot be regarded as at all definitive in view of the finding of Reynolds, Marx, and Henderson (1952) of an interaction between D and the incentive factor K (a function of amount of reward). This interaction plays havoc with any theoretical conclusions drawn from experiments on the effects of drive on extinction in which the incentive factor has not been taken into account. Reynolds et al. (1952) had four groups of rats learn bar pressing under all combinations of high drive -low drive and large reward-small reward. All animals were given extinction trials under equal drive. It was found that

in those learning situations where a relatively large amount of reward is employed for reinforcement, high D animals extinguish more readily than low D animals; and . . . where a relatively small reward is given per reinforcement, low D animals extinguish more readily than high D animals (pp. 41-42).

Hull's theory and its revisions generate conflicting predictions regarding spontaneous recovery. In the Jones (1958) formula, ${}_{S}\overline{E}_{R} = D - I_{R}$) $\times ({}_{s}H_{R} - {}_{s}I_{R})$, spontaneous recovery could occur only if at the end of the first set of extinction trials $D - I_R = 0$. But this formulation would lead to problems, since, if $D-I_R=0$, no habits at all could be activated temporarily until some of the I_R had dissipated, and no behavior of any kind would occur after the end of the first extinction period. We know very well, however, that animals go on behaving in various ways immediately following the extinction of a particular response. But then if we do not wish to assume that $D - I_R$ is equal to zero immediately after the first extinction period, we must assume that ${}_{s}H_{R} - {}_{s}I_{R}$ equals zero, or extinction would not have occurred. Yet if ${}_{S}H_{R} - {}_{S}I_{R}$ were zero, there could be no spontaneous recovery. Conceivably one way out of this dilemma for the Jones revision is to make some assumptions about a reaction threshold which must be exceeded before an overt response is made. Thus, overt extinction could occur before either $D-I_R=0$ or $_{s}H_{R} - _{s}I_{R} = 0$. Spontaneous recovery would then result from the dissipation of I_R , as in Hull's theory. If this were true, one might predict from the Jones revision that there would be very little, if any, spontaneous recovery after extinction under high drive, but greater amounts of spontaneous recovery after extinction under low drive. Since $D - I_R$ would approach the threshold value quickly where D is initially low, there would result an appreciable increase in D, and hence of response strength, with the dissipation of I_R , and spontaneous recovery would result. Under high drive $D-I_R$ would not approach the threshold value as quickly as would ${}_{s}H_{R} - {}_{s}I_{R}$. Thus, since $_{S}H_{R} - _{S}I_{R}$ would be a smaller value after the first extinction, there should be less spontaneous recovery at the beginning of subsequent extinction periods.

Different predictions may be made from Hull and the $D \times_{S} I_{R}$ revision concerning the effect of an increase in drive after extinction is complete. According to Hull's $(D \times_{S} H_{R}) - {}_{S} I_{R}$, an increase in drive after complete extinction should result in further "spontaneous recovery." According to the $D \times ({}_{S}H_{R} - {}_{S}I_{R})$ formulation, once extinction is complete (i.e., $_{S}H_{R} - _{S}I_{R} = 0$), an increase in D should not produce any "spontaneous recovery."

Unfortunately, the experimental evidence bearing on all these predictions is meagre, conflicting, and inconclusive. Hull (1943, p. 249) cites Pavlov's finding that an increase in drive after extinction is complete causes the reappearance of the CR in the presence of the CS. This is, of course, consistent with Hull's formulation, but not with the $D \times_{SI_R}$ formulation. The same phenomenon seems to occur also in instrumental conditioning. Jenkins and Daugherty (1951) extinguished a pecking response in pigeons under three levels of drive. They found that the number of responses in extinction is a function of drive level and that when extinction was relatively complete an increase in drive caused gross recovery of the conditioned behavior. The authors used the term "relatively complete" extinction because the pecking response in pigeons never seems to be completely extinguished. But the recovery of a "relatively extinguished" CR under increased drive is certainly more consistent with $(D \times_{S} H_{R}) - {}_{S}I_{R}$ than The writer with $D \times ({}_{s}H_{R} - {}_{s}I_{R})$. knows of only one study that appears to contradict the finding of Jenkins and Daugherty. Crocetti (1952) found that when rats were "completely" extinguished in a Skinner box, increase in drive did not increase the response rate over the preconditioning response rate under the higher level of drive. (Extinction was considered complete when the response rate became equal to the operant level prior to conditioning.) This finding is, of course, inconsistent with Hull's $(D \times_{S} H_{R}) - {}_{S} I_{R}$. Crocetti did not control for the changes in the drive stimulus (S_D) with in-

creased hunger, and so his finding is not definitive with respect to the present theoretical issue. If we assume that ${}_{S}H_{R}$ and ${}_{S}I_{R}$ are conditioned to S_D as well as to other stimuli, then the changes in S_D from the conditioning trials to the extinction trials or spontaneous recovery trials becomes a crucial point in this type of experiment. Fortunately in an experiment by Lewis and Cotton (1957) the effect of such changes in S_D was taken into account. Three groups of rats were trained in a running response under three levels of drive, viz., 1, 6, and 22 hours' food deprivation. Each group was then divided into three groups which underwent extinction under 1, 6, and 22 hours' drive. Extinction proceeded more rapidly under lower drive, as would be expected from Hull's formulation, but drive level seemed to have no effect on the magnitude of spontaneous recovery, a fact which is inconsistent with $(D \times_{S} H_{R}) - {}_{S} I_{R}$. But the $D \times ({}_{s}H_{R} - {}_{s}I_{R})$ revision cannot comprehend both of these findings either, for with this formulation drive level should have no effect on number of trials to a criterion of extinction. It seems obvious that clarification of the effects of drive on spontaneous recovery must await further experimentation which is specifically designed for this purpose and which takes into account both the energizing and stimulus properties of drive. Some of the lack of consistency and agreement in this area may also be due to interspecies differences and to the use of different measures of response strength. Latency, running time, response rate, and number of trials to extinction are used singly in different studies as measures of response strength even though they are far from being perfectly correlated. Each measure undoubtedly involves certain parameters peculiar to itself. To use only one such measure of response strength and only one species of animal is an inadequate method for testing a precise deduction from a general behavior theory.

In the delayed CR, the inhibition of the response during the period of delay is attributed in the Hullian system to ${}_{S}I_{R}$ (Hull, 1952, p. 114). Consistent with Hull's formulation of $D \times_{S}H_{R} - {}_{S}I_{R}$ is the fact that an increase in *D* lessens or eliminates the period of delay in the CR. The $D \times_{S}I_{R}$ formulation does not accommodate this fact, but leads to an opposite prediction, i.e., an increase in *D* should strengthen the inhibition of delay.

One of the weakest points in Hull's system involves the dependence of $_{S}I_{R}$ upon I_{R} . It is no less troublesome to any of the revisions. (Spence excepted, since his inhibition concept has nothing in common with I_{R} .) It is stated that I_R is generated whenever a response is made, the amount of I_R being a function of the effortfulness of the response, and that I_R rapidly dissipates, accumulating only if responses follow one another in rapid succession. The dissipation of I_R , a "negative drive state," reinforces the habit of not responding, which is ${}_{S}I_{R}$. This hypothesis encounters obvious difficulties. If a response is followed by the dissipation of I_R , this would seem to have all the requirements for reinforcing the response, leading to increased response strength rather than extinction.²

² One can get around this problem, of course, by invoking the gradient of reinforcement. If the time required for the dissipation of I_R is greater than the effective gradient of reinforcement, the foregoing proposition would not hold true. At present there is no basis for arguing the point. While Hull gives 20-30 seconds as the maximum delay between the

Also, subzero extinction would be unlikely if increases in ${}_{S}I_{R}$ were dependent upon *reactive* inhibition (I_R) . And it is almost impossible to explain the extinction of relatively effortless CRs, such as salivation, eyeblink, and the alpha rhythm, when the extinction trials are widely spaced. Pavlov (1927, p. 76) obtained rapid extinction of the salivary CR using only one presentation of the CS per day. Razran (1956, p. 43) has reviewed the evidence that contradicts a theory of extinction based on reactive inhibition. There are even cases where spaced trials have led to more rapid exintction than massed trials (Sheffield, 1950; Stanley, 1952). Kimble (1950) has argued from studies of motor learning that a certain threshold or critical level of I_R must be reached before ${}_{S}I_{R}$ develops. Motor learning experiments have presumably shown that I_R can form without leaving behind any $_{S}I_{R}$. This is inconsistent with extinction based on widely spaced trials. In fact, it does not seem to the writer that the Hullian inhibition postulates, as they have been used in the field of motor learning, represent the same processes found in extinction phenomena. It has been a case of giving the same theoretical labels to basically different processes. The most fundamental difference between $_{S}I_{B}$ in conditioning and in motor learning has to do with the amount of response necessary to produce ${}_{S}I_{R}$. Five or six minutes of pursuit rotor practice seems necessary before ${}_{S}I_{R}$ is in evi-

response and reinforcement if reinforcement is to be effective, the time required for the dissipation of I_R is solely a function of the amount of I_R generated by the response and, therefore, is variable, although the *rate* of dissipation of I_R may not be variable. Perhaps an even simpler way out is the idea that I_R leads to a "resting response" which in turn is reinforced by the dissipation of I_R .



Deviations from Stimulus Reinforced

FIG. 2. Illustrates algebraic summation theory of discrimination. (Effective reaction potential, $s\vec{E}_R$, is a result of subtraction of generalized extinction, sI_R , from generalized conditioning, sH_R . See text for full explanation.)

dence, while only a single conditioned response, such as salivation, PGR, or eyeblink, is evidently sufficient to produce ${}_{S}I_{R}$. Thus it does not seem that the ${}_{S}I_{R}$ invoked in theories of motor learning could be the same ${}_{S}I_{R}$ as that in Hull's theory of conditioning.

It is also held by Hull, and even more explicitly by his revisers, that the amount of ${}_{S}I_{R}$ built up per trial is related to the amount of I_R dissipated, the dissipation acting as reinforcement for the negative habit. $_{S}I_{R}$. But this is inconsistent with Hull's own revision of his theory (Hull, 1951), in which the growth of habit, ${}_{s}H_{R}$, and presumably also of negative habit, ${}_{S}I_{R}$, is a function only of the number of reinforcements and not the *amount* of reinforcement. None of these awkward predicaments has been remedied by the revisions Those revisions inhere reviewed. sisting on the theoretical equivalence of ${}_{s}H_{R}$ and ${}_{s}I_{R}$ as being merely positive and negative habits have retained one of the weakest elements in the Hullian system.

Discrimination learning. If dis-

crimination learning involves an increase in habit strength to the positive stimulus (S^{D}) and an increase in inhibition $(I_R \text{ and } sI_R)$ to the negative stimulus (S^{Δ}) , then the effects of drive on discrimination learning should be highly germane to the plausibility of the $D \times_{s} I_{R}$ formulation. Jones (1958) invokes Spence's (1937) theory of discrimination learning, adapted by Hull, involving the overlapping generalization gradients of ${}_{S}H_{R}$ and ${}_{S}I_{R}$, in support of the $D \times_{S} I_{R}$ part of his revision. This theory is illustrated in Figure 2. The discrimination would be perfect (except for behavioral oscillation) when the net reaction potential resulting from subtracting the generalized ${}_{S}I_{R}$ from the generalized ${}_{S}H_{R}$ is some positive value for S^D and zero for S^{Δ} , as in Figure 2. Jones (1958) argues that, according to Hull's $D \times_{S} H_{R} - {}_{S} I_{R}$, an increase in D should obliterate the learned discrimination. Since some discriminations are not obliterated or even weakened by an increase in D. Jones reasons that ${}_{S}I_{R}$ must also be multiplied by D, so that the increase in $_{S}I_{R}$ will be proportional to the increase of ${}_{S}H_{R}$ when multiplied by D, thereby preserving the discrimination.

Before Jones' argument can be evaluated, some clarification of the Spence-Hull theory of discrimination learning is necessary. In the first place, there is often confusion concerning whether discrimination learning is a matter only of the *relative* strengths of ${}_{s}\overline{E}_{R}$ to the S^{D} and S^{Δ} , or whether the formation of a discrimination requires the reduction of ${}_{s}\overline{E}_{R}$ to the S^{Δ} to zero or at least below the operant level of the response, i.e., below the strength of the response before any conditioning or extinction has occurred. If the former,

then all that would be necessary for discrimination to take place would be that the S^D have greater $_{S}\overline{E}_{R}$ than the S^{Δ}. The ${}_{S}\overline{E}_{R}$ to the S^{Δ} would not necessarily have to undergo some degree of extinction. If this were the case, Jones' use of the Spence-Hull theory of discrimination, as illusstrated in Figure 2, would not be applicable to the present argument concerning the effects of drive on discrimination learning. The evidence, however, strongly suggests that the ${}_{S}\overline{E}_{R}$ to the S^{Δ} must undergo some degree of extinction for discrimination to become nearly perfect. To this extent, at least, the Spence-Hull theory appears to be correct.

For example, Grice (1948) gave one group of rats 200 rewarded trials in responding to a disc 8 centimeters in diameter and gave another group of rats 200 rewarded trials in responding to a 5-centimeter disc. Then both groups were given discrimination training, with the 8centimeter disc as the S^{D} and the 5centimeter disc as the S^{Δ} . The group which had been previously rewarded on the 8-centimeter disc learned the discrimination faster. Now if all that were involved in discrimination were the *relative* response strengths to the S^{D} and S^{Δ} , the 8-centimeter group should have learned to make the discrimination immediately, since response to the S^{D} had already been rewarded on 200 trials, and the response strength to the S^{Δ} resulting from stimulus generalization would have been less than the response strength to the S^{D} . Since the learning curve for the acquisition of the discrimination is very gradual, however, it suggests that extinction of the response to the S^A through nonreinforcement is necessary for the learning of the discrimination.

An even more cogent demonstra-

tion of the necessity for extinction of S^Δ in discrimination learning is an experiment by Fitzwater (1952). Three groups of rats were used: Groups A, B, and C. In preliminary training Group A was run an equal number of times into each of two alleys having differential cues—call them X and Y, respectively. X was always reinforced; Y was never reinforced. Group B was run an equal number of times into each of two alleys having the Cues X and Z. X was always reinforced; Z was never reinforced. Group C was run only into one alley, with Cue X, the same number of times as the other groups. Then discrimination training began, with the animals having to learn to discriminate X as the S^{D} and Y as the S^{Δ} . Group A learned the discrimination most rapidly, while Groups B and C did not differ significantly in speed of learning. The theoretical interpretation of these results is that Group A had already built up inhibition to the S⁴, while Groups B and C had not. Fitzwater concluded that

apparently in a visual discrimination task it is about as important to establish an avoidance habit as an approach habit, and that an appreciable discrimination does not seem to occur if an approach habit is established alone (p. 480).

The terms "approach habit" and "avoidance habit" may be interpreted in the context of the present discussion as excitation (or ${}_{S}H_{R}$) and extinction (${}_{S}I_{R}$), respectively. Thus it is apparent that a decrease in ${}_{S}\overline{E}_{R}$ to the S^A as well as an increase in ${}_{S}\overline{E}_{R}$ to the S^D is necessary for discrimination learning. It is not just a matter of ${}_{S}\overline{E}_{R}$ to the S^D being relatively greater than to the S^A.

Another experiment by Grice (1949) offers further evidence that discrimination depends upon the *extinction* of the response to the S^{Δ} and

not merely a relative difference in response strengths between S^{D} and S⁴. One group of rats was trained in a visual size discrimination with S^{D} and S^{Δ} presented simultaneously, and another group was trained on the same discrimination with S^{D} and S^{Δ} presented successively in random order. Grice found no difference between the "simultaneous" and "successive" groups in the rate of learning the discrimination. In both cases learning apparently consisted of increasing the response strength to the S^{D} and completely extinguishing the response to S^{Δ} . Furthermore it was found that the rats which learned the problem as a pair (i.e., simultaneous presentation) responded differently to the S^{D} and S^{Δ} when they appeared singly, showing that even under simultaneous presentation of the S^{D} and S^{Δ} , the response to the S^{Δ} had undergone extinction.

It is not maintained that complete extinction of the response to S^{Δ} is necessary. Extinction is a relative matter and is probably best measured, not in relation to some theoretical "absolute zero," but in relation to the "operant level" or probability of occurrence of the particular response before extinction trials have been assumed to take place. In the Grice (1949) experiment there was a decrease in latency of response to S^{D} and an increase in latency of response to S^{Δ} , whether the two stimuli were presented simultaneously or successively. ${}_{S}\overline{E}_{R}$ to the S^{Δ}, as measured by latency, was considerably less at the end of discrimination training than at the beginning. In fact, extinction of response to S^A may play a greater role in discrimination learning than does the strengthening of the response to S^{D} . Webb (1950) trained rats to jump to a black-white discrimination until it

was well learned. When, after training, only the S^{D} was presented to the rats, the mean latency of their response was 2.0 seconds, which was not significantly less than the pretraining latency. On the other hand, when only the S^{Δ} was presented, the mean latency of response was 80.5 seconds, which may be interpreted as indicating considerable extinction or inhibition of the response to the S^{Δ} . If one defines the zero level of ${}_{S}\overline{E}_{R}$ in the Hull-Spence model in Figure 2 simply as the operant level (i.e., the pretraining latency or probability of responding to the particular stimuli), then this model appears to be quite consistent with the experimental evidence in showing that discrimination depends upon extinction of the response to the S^{Δ} .

This model, however, seems to be deficient in some other respects. Hanson (1957), for example, performed a very careful experiment which led to the conclusion that over-all response strength is not weakened by discrimination training, as would be predicted from the Spence-Hull model. (That is, since the resultant ${}_{S}\overline{E}_{R}$ is the algebraic sum of generalized excitation and inhibition, ${}_{S}\overline{E}_{R}$ to the S^D should be less after discrimination training than it would be in simple conditioning to a single stimulus.) Hanson concluded that

the major result of discrimination training is to bring a large proportion of the responses available in extinction under the control of another range of stimuli, those which do not ordinarily gain control of the response as the result of simple conditioning without differential reinforcement (p. 889).

This conclusion is not compatible with the Spence-Hull theory.

It may be argued that Jones has taken the Spence-Hull diagram (Figure 2) too literally. Very little is

known about the actual shapes of the generalization gradients of $_{S}H_{R}$ and $_{S}I_{R}$, and until a proper metric is worked out, arguments over this point cannot be settled. What little evidence there is, though far from conclusive, suggests that the generalization gradients of excitation and inhibition are probably different in a number of respects (Razran, 1938). Furthermore, the amount of overlap of the gradients of excitation and inhibition will depend on the distance apart of S^{D} and S^{Δ} , and there is reason to believe that the effects of drive on discrimination will interact with the degree of disparity between S^{D} and S^{Δ} (Broadhurst, 1957). We would predict from Hull's $D \times_{s} H_{R}$ $-_{s}I_{R}$ that the farther apart S^D and S^{Δ} are, the less deleterious to the discrimination are the effects of increased drive. This essentially is the Yerkes-Dodson Law (Yerkes & Dodson, 1908), which, in its most general form, states that the optimum motivation for a learning task decreases with increasing difficulty. This relationship between drive and difficulty of discrimination, however, cannot be predicted from the Jones formulation of $D \times ({}_{S}H_{R} - {}_{S}I_{R})$.

Rather than arguing from a highly hypothetical model involving the relative shapes and magnitudes of the generalization gradients of $_{s}H_{R}$ and ${}_{S}I_{R}$, as Jones has done, we can better make predictions concerning the directly observable effects of increased drive on discriminations. What is the effect of drive on the initial learning of a discrimination, and does an increase in drive have a different effect on the learning of easy and difficult discriminations, as determined by time required for learning? What is the effect of change in drive on discriminations that are already established? What effect does a change in drive have on the extinction of a discrimination?

In discrimination learning, since the relative amounts of $_{S}H_{R}$ and ${}_{S}I_{R}$ built up to the S^D and S^A are different, we would expect from the $D \times ({}_{s}H_{R} - {}_{s}I_{R})$ formula that an increase in D would always have a facilitative effect on learning a discrimination. The degree of facilitation would depend upon the degree of difference between S^{D} and S^{Δ} . If we assumed considerable overlapping of generalization gradients, then there would be relatively little effect of an increase in D. If the discrimination were easy, increases in D should improve the discrimination, since the relatively greater ${}_{s}H_{R}$ to the S_D and the relatively greater ${}_{S}I_{R}$ to the S^{Δ} would both he multiplied by D. In no case should discrimination be weakened by an increase in D.

On the other hand, if we assume that response to S^{Δ} must undergo extinction for a discrimination to be learned, Hull's formula $D \times_{S} H_{R} - {}_{S} I_{R}$ leads to quite different predictions, viz., that increase in *D* should weaken difficult discriminations, where one might assume overlap of the stimulus generalization gradients, but would strengthen discriminations in which S^{D} and S^{Δ} are widely separated on the generalization gradient.

What is the evidence? We have already mentioned the Yerkes-Dodson Law, which is possibly consistent with Hull, but certainly not with the $D \times ({}_{S}H_{R} - {}_{S}I_{R})$ formula. Broadhurst (1957) has demonstrated this "law" most effectively, using rats in a brightness discrimination problem and manipulating drive by means of oxygen deprivation. Skinner (1938, p. 188) has observed that it is important in establishing discriminant operant conditioning to keep the hunger drive as constant as possible, for changes in drive disturb the discrimination. More explicitly, Teel (1952) has shown that in selective learning, in which correct responses are reinforced and incorrect responses are nonreinforced or extinguished, rats under high drive (food deprivation) require a greater number of trials to reach a criterion of learning than rats under low drive. One cannot predict these facts from the $D \times ({}_{s}H_{R} - {}_{s}I_{R})$ formula. In fact, just the opposite outcome would be predicted for the Teel experiment. With human subjects, Hilgard, Jones, and Kaplan (1951) found that high anxiety subjects (on Taylor Manifest Anxiety scale) had greater difficulty than low anxiety subjects in forming a discriminatory CR. It is wellestablished that anxious subjects develop simple eyeblink CRs more readily than nonanxious subjects. (This relationship has not been found to hold, however, for autonomic CRs.) Interpreting anxiety as a drive, both sets of findings are consistent with Hull, but not with $D \times ({}_{s}H_{R} - {}_{s}I_{R})$. An experiment by Spence and Farber (1954) found that the difference between high and low anxious subjects in forming a discriminatory response showed up only on the S^{D} but not on the S^{Δ} . That is, D (anxiety) seemed to affect only the CS (i.e., S^{D}) associated with relatively greater ${}_{s}H_{R}$ and not the CS (i.e., S^{Δ}) associated with relatively greater ${}_{S}I_{R}$. Spence interprets this finding as evidence that D interacts with excitation $({}_{S}H_{R})$ but not with inhibition $(_{S}I_{R})$.

In a well-established discrimination, in which S^{D} and S^{A} are relatively far apart on the stimulus generalization gradient, and in which relatively more ${}_{S}H_{R}$ than ${}_{S}I_{R}$ has been built up to S^{D} than to S^{A} , and relatively more ${}_{S}I_{R}$ built up to S^{A}

than to S^{D} , we would predict from $D \times ({}_{s}H_{R} - {}_{s}I_{R})$ an improvement in the discrimination with an increase in drive. That is, the ratio of number of responses to S^{D} to number of responses to S^{Δ} should increase, since response to S^{D} is increased by $D \times_{s} H_{R}$, and inhibition of response to S^{Δ} is increased by $D \times_{S} I_{R}$. Dinsmoor (1952) performed an experiment bearing on this point. A simple discrimination habit was well-established in rats in the Skinner box, with S^{D} being the presence of light and S^{Δ} being total darkness. When D was increased to varying degrees by food deprivation, the *number* of responses per unit of time to both S^{D} and S^{Δ} increased, but the ratio of S^{D} and S^{Δ} responses remained exactly the same at seven different degrees of hunger. In short, the discrimination was not improved by an increase in D. Though Hull's theory is not sufficiently quantified to have precisely predicted the outcome of this particular experiment (because absolute levels of D and ${}_{s}H_{R}$ as well as the ind's between S^{D} and S^{Δ} must be taken into account), at least the result is consistent with the $(D \times_{S} H_{R})$ $-_{s}I_{R}$ formulation.

There is other experimental evidence, however, which suggests that both the Hullian and the revised formulations are inadequate to explain the effects of drive on discrimination learning. A number of studies have found no relationship at all between drive and proficiency in selective learning or solving discrimination problems (Meyer, 1951; Miles, 1959; and a number of doctoral dissertations reported by Spence, Goodrich, & Ross, 1959). Spence et al. (1959) have scrutinized the conflicting findings in this field with a view to discovering the reason for the lack of agreement between various investiga-

tions on the effect of drive on selective learning and discrimination. They arrived at the hypothesis that performance in selective learning (such as learning a black-white discrimination) is independent of drive level when responses to the S^{D} and S^{Δ} are equated, but varies with drive when responses are not equated. They performed a set of experiments which supported this hypothesis. The results are inexplicable in terms of Hull's theory or any of its revisions except that of Spence. These findings suggest that the growth of ${}_{s}H_{R}$ is not a function of number of reinforced responses, as in Hull's system, but is a function merely of the number of responses, whether reinforced or not. The growth of inhibition is a function only of the number of nonreinforced trials. This formulation will account for the major finding of the experiment by Spence et al. (1959). But another aspect of their findings remains inexplicable in terms of any current theory of learning. When responses to S^{D} and S^{Δ} were equated. an increase in drive increased the response strength to both the S^{D} and S^{Δ} . But when the rats were forced to respond twice as often to S^{D} as to S^{Δ} , an increase in drive increased the response strength to S^{D} but decreased response strength to S^{Δ} . Spence et al. concluded that

the results of the two (experiments) are in fundamental disagreement so far as the effects of drive differences on the strength of nonreinforced responses are concerned. It is perhaps obvious that we need to obtain much more knowledge than we now possess concerning the variables affecting the development of response decrement with nonreinforcement. Unfortunately, this problem has been badly neglected in conditioning experiments with the consequence that such an empirically based theory as the present one [i.e., Spence's theory] is weakest in this area (p. 15).

Though the present state of our

knowledge in this area does not permit any definite conclusion regarding the effects of drive on discrimination learning, it appears that no current theory is able to comprehend all the relevant facts now available.

But now let us ask: What happens when a discrimination is extinguished under various levels of drive? Cautela (1956) trained rats in a discrimination under 23 hours' food deprivation and then extinguished the discriminative response under 0, 6, 12, 23, 47, and 71 hours' deprivation. The criterion of extinction was failure to respond to either S^{D} or S^{Δ} within 3 minutes. Many more responses were required for extinction under high drive levels (23, 47, or 71 hours' deprivation) than under low drive (0. 6. or 12 hours). This result can be predicted from $D \times_{s} H_{R} - {}_{s} I_{R}$. On the other hand, it is difficult to see why a change in drive should have any effect on the number of responses to extinction if ${}_{S}H_{R}$ and ${}_{S}I_{R}$ are both increased or decreased proportionately by changes in D, as stated in the revised formula.

$D-I_R$

Since Hull referred to reactive inhibition (I_R) as a "negative drive," he has been accused of logical inconsistency for adding a drive to a habit (i.e., $I_R + {}_{S}I_R$) and the suggested remedy has been the obvious one, viz., to subtract I_R from D. But predictions from this formulation lead to empirical embarrassment. For example, when extinction is carried out under massed trials, and, after a period of rest, there is some spontaneous recover, we must assume, according to the $_{S}\overline{E}_{R} = (D - I_{R}) \times (_{S}H_{R})$ $-_{s}I_{R}$) formulation, that $D-I_{R}=0$ at the end of the first extinction period. For there would be no spontaneous recovery if it were ${}_{S}H_{R} - {}_{S}I_{R}$ that had become equal to zero. Yet, according to Hullian theory (including the revisions), no behavior can occur unless D is greater than zero. And it is known that an animal at the end of extinction is far from being inactive. Only the extinguished CR becomes inactive, while other behavior in the animal's repertoire is immediately evident. Theoretically this could not be so if the drive component in the equation for reaction potential were zero.

Experimental evidence contradicting $D - I_R$ is presented by Hull (1952, p. 50). A rat is trained to press either of two bars in different locations in a Skinner box to obtain food. During extinction the rat alternates its response from one bar to the other. I_R does not have to dissipate before the alternate bar can be pressed. This strongly suggests that I_R must be associated with the particular response, rather than cause a diminution in the total drive state, which in the Hullian system is an amalgam of all the organic needs of the moment and their associated "drive stimuli" $(S_{D}).$

In an experiment highly relevant to this point, Smith and Hay (1954) took advantage of the great sensitivity to changes in drive of rate of responding in the Skinner box. As soon as operant conditioning had led to a stable response rate, a discriminatory stimulus was introduced, the S^{D} always being reinforced, the S^{Δ} never. During the learning of the discrimination, the number of responses to S^{D} increased while the number of responses to S^{Δ} decreased, but the rate of responding remained constant. If the extinction of S^{Δ} had involved $D-I_R$, there should have been the decrease in over-all response rate which is associated with lowered drive. On the other hand, this finding is entirely consistent with Hull's formulation.

$I_R \times_s I_R$

Here we have a formulation which, if the rules of algebra are followed religiously in manipulating Hullian variables, leads to a paradox-a positive addition to reaction potential resulting from the interaction of two inhibitory variables. Tones (1958) even goes on to say that the paradoxical outcome of $I_R \times_S I_R$ increasing ${}_{S}\overline{E}_{R}$ might explain the "ultraparadoxical effect" described by Pavlov (1927). This might be called explanation by clang association.³ It is difficult for the writer to understand why Jones and other revisers have so gratuitously regarded the minus sign as being permanently attached to I_R and ${}_{S}I_R$. Though these quantities are subtracted from positive reaction potential, the negative sign is not necessarily an inherent part of these inhibition variables. Even if I_R and ${}_{s}I_R$ were multiplicatively related, there is no reason why their product could not be subtracted from the positive reaction potential.

The empirical evidence regarding the $I_R \times_{s} I_R$ interaction is far from satisfactory, for there is always an "out" via the possible interaction of all the other intervening variables in the system. But in terms of sheer plausibility—and that is all one can

⁸ The "paradoxical" and "ultraparadoxical" effects observed by Pavlov, in which a weaker intensity of the CS will elicit a CR that had been extinguished to a stronger intensity of the CS, are probably best explained in terms of a generalization gradient on the stimulus intensity dimension. Because of the gradient, extinctive inhibition built up to a CS of one intensity will not be sufficient to inhibit the CR to a CS of a different intensity, even though it be weaker. Or the effect may be explained as disinhibition caused by a "novel" stimulus—novel because the intensity is weaker than that of the original CS. go on at present—it must be said that $I_R \times_S I_R$ is a weak formulation. The only relevant evidence comes from experiments on motor learning, the one area in which there are rather clear-cut operational definitions of what constitutes I_R and $_SI_R$. In general, performance decrement that dissipates during rest is identified with I_R ; the decrement that still remains after rest is identified with $_{SI_R}$.

Duncan (1951) gave two groups of subjects massed and distributed practice on the pursuit rotor. During this 5-minute practice period, the massed group presumably would develop more I_R and hence more ${}_{S}I_R$. Then both groups were allowed 10 minutes of rest, so that at the beginning of the postrest trials, nearly all I_R should have dissipated, leaving the two groups differing only in ${}_{S}I_{R}$. The postrest trials were massed for both groups. Here exist the very conditions which should allow an $I_R \times_{S} I_R$ interaction to show itself. If there were an interaction, the postrest performance curves of the two groups should diverge. In fact, they did not diverge, or converge, but ran exactly parallel throughout the postrest trials, which suggests an additive rather than multiplicative relationship between I_R and ${}_{S}I_R$. There are certain weaknesses and peculiarities in Duncan's study (for example, it could be argued that the 5 minutes' practice was not sufficient to attain the threshold of I_{R} necessary for the development of $_{SI_{R}}$, the evidence for which has been presented by Kimble, 1950); but on the whole, it favors Hull's formulation regarding inhibition more than it favors those formulations which involve $I_R \times_{S} I_R$. Another study by Starkweather and Duncan (1954) was essentially the same as the previous experiment except that the massed

group was given more prerest practice so that performance on the first postrest trial would be the same for both massed and distributed groups. The rest period was 24 hours. Again, when both groups were given massed practice after the rest, their performance curves were approximately parallel, suggesting that there is no interaction between I_R and ${}_{S}I_R$. It is possible to argue from some of the evidence in this study, however, that the presence of ${}_{S}I_R$ was not clearly demonstrated.⁴

Better evidence is presented by Bourne and Archer (1956). Groups trained under massed and distributed practice on the pursuit rotor were given 5 minutes' rest, and then all groups performed under massed conditions. The performance curves converged in the postrest period. But the convergence consisted of the performance of the previously distributed group *reducing* to that of the massed group. If the $I_R \times_S I_R$ formulation were correct, the result should have been just the opposite, with the previously massed group showing an increase up to the level of the distributed group. The prerest practice was more prolonged in this study than in Duncan's, and it can be argued that there was a sufficient amount of ${}_{S}I_{R}$ generated to permit the $I_R \times_{S} I_R$ to show itself. Yet, in another motor learning experiment specially designed to determine if there was an interaction between I_R and ${}_{s}I_{R}$, Bowen, Ross, and Andrews (1956) failed to find any evidence of interaction. So while the evidence is not definitive on this point, the preponderance of it does not favor the

⁴ It seems fairly certain that the concept of ${}_{SI_R}$ invoked to explain decremental phenomena in motor learning could not represent the same process as the ${}_{SI_R}$ involved in experimental extinction.

 $I_R \times_{S} I_R$ formulation. The issue. however, does not seem beyond a clear-cut experimental test. For example, in the Jones revision $D \times_{s} I_{R}$ would always have to be greater than $I_R \times_{S} I_R$, because there can be no performance when D is equal to or less than I_R . If this were true, a person practicing on the pursuit rotor over a long period should finally become unable to perform, since ${}_{S}I_{R}$ would continue to grow and inhibit performance. After I_R had dissipated, $D \times_{S} I_{R}$ would approach or equal $D \times_{s} H_{R}$, and the subject would be unable to perform the pursuit task. Gleitman, Nachmais, and Neisser (1954) were the first to point out this consequence with respect to Hull's As far as the writer formulation. knows, no one has ever found this kind of "extinction" of the pursuit rotor skill. Subjects have been known to practice the pursuit task day after day for months, long after having reached an asymptote for time on target, yet they show no loss of the skill. Hull's formula, on the other hand, can get around this problem. the arguments of Gleitman et al. (1954) notwithstanding. If $_{S}H_{R}$ and $_{SI_{R}}$ both reach an asymptote (Hull, 1951), extinction will have occurred when ${}_{S}I_{R} = D \times {}_{S}H_{R}$. An increase in D will make it possible for $D \times_{S} H_{R}$ to be greater than the symptote of ${}_{S}I_{R}$, so that extinction need never occur if Dremains sufficiently high. Indeed, there instances (Solomon are & Wynne, 1954) of absence of extinction in escape and avoidance training in which the drive is a very strong shock-induced fear reaction.

The unlikely prediction made from Hull's theory by Gleitman et al. (1954) that any response, even though always reinforced, would eventually extinguish if it were repeated often enough was directly tested in experiments by Calvin, Clifford, Clifford, Bolden, and Harvey (1956) and Kendrick (1958). Their studies differ in a few details of experimental procedure. Essentially they ran rats down a long alleyway at the end of which the rats received reinforcement on every trial. After some hundreds of trials (spread over many days) all the rats ceased running down the alley; they would not leave the starting box for a specified period of time designated as the criterion for "complete" extinction. Though this outcome lends support to Hull's theory, other interpretations are certainly possible (see Mowrer, 1960, pp. 426-432; Prokasy, 1960). The results of the Calvin et al. and Kendrick experiments may well be due to peculiarities of the experimental procedure. If not, one should expect "extinction with reinforcement" to occur in many other kinds of performance, such as a rat's bar pressing or a pigeon's pecking in a Skinner box, and in many types of repetitious motor tasks.

One experiment is highly relevant to theoretical predictions regarding the effects of drive on motor learning. Wasserman (1951), using a motor learning task (alphabet printing) found that high motivation resulted in performance which was significantly superior to that of low motivation (in both massed and distributed practice groups), the difference becoming progressively greater as practice continued. The Jones revision would predict just the opposite. Since D must always be greater than I_R , $D \times_S I_R$ would result in greater performance decrement for the highly motivated group. The motivation in this experiment was controlled by the instructions given to the subjects, one group being task-oriented, the other ego-oriented.

$I_R \times_s H_R$

This formulation of an interaction

between reactive inhibition and habit strength implies that the decremental effects on performance caused by the conditions producing I_R (effort and rate of response) will be greater for strong than for weak habits. This is patently incorrect, since it is known that there is a *positive* correlation between number of reinforced responses, of which ${}_{S}H_{R}$ is a function, and the number of responses emitted during extinction. The $I_R \times_{S} H_R$ formulation would predict just the opposite, i.e., a negative correlation between number of reinforcements and number of responses to extinction. This conclusion is not weakened by the fact (for example, Reid, 1953) that in learning to make a discrimination reversal the animals that have had a greater number of prereversal trials learn the reversal more quickly. This phenomenon may be interpreted in terms of the animal's also overlearning the act of making a discrimination (in addition to learning to respond differentially to the S^{D} and S^{Δ}), which facilitates the learning of the reversal.

$_{s}H_{R} \times _{s}I_{R}$

This formulation, derived from Iwahara (1957), is subject to the same criticism just made in the case of $I_R \times_S H_R$. It implies that the stronger the habit, the more quickly it should extinguish, which certainly is not true.

$K - I_R$

The suggestion of Woodworth and Schlosberg (1954), that total inhibition $(I_R = I_R + {}_SI_R)$ be subtracted from incentive motivation, K (a function of amount of reinforcement), seems plausible, in that extinction involves the withdrawal of incentive. Within the total Hullian formulation, however, the Woodworth and Schlosberg suggestion meets with the same difficulties pointed out in the two previous cases. Thus:

$$_{S}\overline{E}_{R} = D \times (K - I_{R} - _{S}I_{R}) \times _{S}H_{R}$$

In expanded form:

$$s\overline{E}_{R} = D \times K - D \times I_{R} - D \times sI_{R}$$
$$\times D \times sH_{R} \times K \times sH_{R}$$
$$-sH_{R} \times I_{R} - sH_{R} - sI_{R}$$

Thus we have again all of the elements that have already been criticized. Spence (1956) has argued, on the basis of experimental findings, that D and K are additive rather than multiplicative as in Hull. But here again the defects of the Woodworth and Schlosberg suggestion of $K - I_R$ are evident.

$$_{S}\overline{E}_{R} = (D + K - I_{R}) \times _{S}H_{R}$$

Expanded:

$${}_{S}\overline{E}_{R} = D \times {}_{S}H_{R} + K \times {}_{S}H_{R} - {}_{S}H_{R} \times \dot{I}_{R}$$

The last term in the expanded formula again meets with the same difficulty pointed out above. It must be concluded that the $K-I_R$ formulation is not an improvement on Hull or Spence.

SUMMARY

Several attempts to reformulate Hull's theory with respect to the inhibition postulates have been criticized. Because of the limitations of both Hull and his revisers in the exact quantification of intervening variables, much of the choice between alternative versions of the theory must be made on the basis of plausibility of congruence with empirical findings rather than of *prediction* of these findings in the rigorous sense of the term. All of the attempted revisions to date, with the possible exception of that of Spence, have serious shortcomings in the light of experimental evidence. They cannot, therefore, be regarded as improvements over Hull's original formulation of reaction potential. Advances will be made, not by the mere algebraic manipulation of Hull's intervening variables—the method that characterizes the present attempts but by the postulation and quantification of new intervening variables, along with the experimental investigation of their interactions.

REFERENCES

- BARRY, H. Effects of strength of drive on learning and extinction. J. exp. Psychol., 1958, 55, 473-481.
- BASS, M. J., & HULL, C. L. The irradiation of tactile conditioned reflex in man. J. comp. Psychol., 1934, 17, 47-65.
- BITTERMAN, M. E., & HOLTZMAN, W. H. Conditioning and extinction of the galvanic skin response as a function of anxiety. J. abnorm. soc. Psychol., 1952, 47, 615-623.
- BOURNE, L. E., JR., & ARCHER, E. J. Time continuously on target as a function of distribution of practice. J. exp. Psychol., 1956, 51, 25-33.
- Bowen, J. H., Ross, S., & ANDREWS, T. G. A note on the interaction of conditioning and reactive inhibition in pursuit tracking. J. gen. Psychol., 1956, 55, 153-162.
- BRANDAUER, C. M. A confirmation of Webb's data concerning the action of irrelevant drives. J. exp. Psychol., 1953, 45, 150-152.
- BROADHURST, P. L. Emotionality and the Yerkes-Dodson law. J. exp. Psychol., 1957, 54, 345-352.
- BROWN, JANET L. The effect of drive on learning with secondary reinforcement. J. comp. physiol. Psychol., 1956, 49, 254-260.
- BULLOCK, D. H. The inter-relationship of operant level, extinction ratio, and reserve. J. exp. Psychol., 1950, 40, 802-804.
- BULLOCK, D. H., & SMITH, W. C. An effect of repeated conditioning-extinction upon operant strength. J. exp. Psychol., 1953, 46, 349-352.
- CALVIN, A. A., CLIFFORD, T., CLIFFORD, B., BOLDEN, L., & HARVEY, J. An experimental validation of conditioned inhibition. *Psychol. Rep.*, 1956, 2, 51-56.
- CAUTELA, J. R. Experimental extinction and drive during extinction in a discrimination habit. J. exp. Psychol., 1956, 51, 299-302.
- COTTON, J. W. On making predictions from Hull's theory. *Psychol. Rev.*, 1955, **67**, 303– 314.
- CROCETTI, C. P. The relation of extinction responding to drive level in the white rat. Unpublished doctoral dissertation, Columbia University, 1952.
- DINSMOOR, J. A. The effect of hunger on discriminated responding. J. abnorm. soc. Psychol., 1952, 47, 67-72.

- DUNCAN, C. P. The effect of unequal amounts of practice on motor learning before and after rest. J. exp. Psychol., 1951, 42, 257-264.
- ELLSON, D. G. The concept of reflex reserve. Psychol. Rev., 1939, 46, 566-575.
- EYSENCK, H. J. "Warm-up" in pursuit rotor learning as a function of the extinction of conditioned inhibition. Acta Psychol., Amst., 1956, 12, 349-370.
- EYSENCK, H. J. The dynamics of anxiety and hysteria. London: Routledge & Kegan Paul, 1957.
- FITZWATER, M. E. The relative effect of reinforcement and nonreinforcement in establishing a form discrimination. J. comp. physiol. Psychol., 1952, 45, 476-481.
- GLEITMAN, H., NACHMAIS, J., & NEISSER, U. The S-R reinforcement theory of extinction. *Psychol. Rev.*, 1954, **61**, 23–33.
- GRICE, G. R. The acquisition of a visual discrimination habit following response to a single stimulus. J. exp. Psychol., 1948, 38, 633-642.
- GRICE, G. R. Visual discrimination learning with simultaneous and successive presentation of stimuli. J. comp. physiol. Psychol., 1949, 42, 365-373.
- HANSON, H. M. Discrimination training effect on stimulus generalization gradient for spectrum stimuli. *Science*, 1957, 125, 888– 889.
- HILGARD, E. R. Theories of learning. (2nd ed.) New York: Appleton-Century-Crofts, 1956.
- HILGARD, E. R. Intervening variables, hypothetical constructs, parameters, and constants. Amer. J. Psychol., 1958, 71, 238-246.
- HILGARD, E. R., JONES, L. V., & KAPLAN, S. J. Conditioned discrimination as related to anxiety. J. exp. Psychol., 1951, 42, 94-99.
- HILGARD, E. R., & MARQUIS, D. M. Conditioning and learning. New York: Appleton-Century-Crofts, 1940.
- HOVLAND, C. I. The generalization of conditioned responses: The sensory generalization of conditioned responses with varying frequencies of tone. J. gen. Psychol., 1937, 17, 125-148.

- HULL, C. L. *Principles of behavior*. New York: Appleton-Century-Crofts, 1943.
- HULL, C. L. Essentials of behavior. New Haven: Yale Univer. Press, 1951.
- HULL, C. L. A behavior system. New Haven: Yale Univer. Press, 1952.
- IWAHARA, S. Hull's concept of inhibition: A revision. *Psychol. Rep.*, 1957, **3**, 9–10.
- JENKINS, W. O., & DAUGHERTY, GEORGETT. Drive and the asymptote of extinction. J. comp. physiol. Psychol., 1951, 44, 372–377.
- JONES, H. G. The status of inhibition in Hull's system: A theoretical revision. *Psychol. Rev.*, 1958, 65, 179–182.
- KENDRICK, D. C. Inhibition with reinforcement (conditioned inhibition). J. exp. Psychol., 1958, 56, 313-318.
- KIMBLE, G. A. Evidence for the role of motivation in determining the amount of reminiscence in pursuit rotor learning. J. exp. Psychol., 1950, 40, 248-253.
- KOCH, S. Clark L. Hull. In W. K. Estes, K. MacCorquodale, P. E. Meehl, C. G. Mueller, W. N. Schoenfeld, & W. S. Verplanck (Eds.), Modern learning theory: A critical analysis of five examples. New York: Appleton-Century-Crofts, 1954. Pp. 1-176.
- LEWIS, D. J., & COTTON, J. W. Learning and performance as a function of drive strength during acquisition and extinction. J. comp. physiol. Psychol., 1957, 50, 189-194.
- LIBERMAN, A. M. A comparison of transfer effects during acquisition and extinction of two instrumental responses. J. exp. Psychol., 1951, 41, 192-198.
- MACCORQUODALE, K., & MEEHL, P. E. On a distinction between hypothetical constructs and intervening variables. *Psychol. Rev.*, 1948, 55, 95-107.
- MEYER, D. R. Food deprivation and discrimination reversal learning of monkeys. J. exp. Psychol., 1951, 41, 10-16.
- MILES, R. C. Discrimination in the squirrel monkey as a function of deprivation and problem difficulty. J. exp. Psychol., 1959, 57, 15-19.
- MOWRER, O. H. Learning theory and behavior. New York: Wiley, 1960.
- OSGOOD, C. E. Method and theory in experimental psychology. New York: Oxford Univer. Press, 1953.
- PAVLOV, I. P. Conditioned reflexes. London: Oxford Univer. Press, 1927.
- PERIN, C. T. Behavior potentiality as a joint function of the amount of training and degree of hunger at the time of extinction. J. exp. Psychol., 1942, 30, 93-113.
- PERKINS, C. C., JR., & CACIOPPO, A. J. The effect of intermittent reinforcement on the change in extinction rate following succes-

sive reconditionings. J. exp. Psychol., 1950, 40, 794-801.

- PROKASY, W. F. Postasymptotic performance decrements during massed reinforcements. *Psychol. Bull.*, 1960, 57, 237-247.
- RAZRAN, G. Extinction re-examined and reanalyzed: A new theory. *Psychol. Rev.*, 1956, 63, 39-52.
- RAZRAN, G. H. S. Transposition of relational responses and generalization of conditioned responses. *Psychol. Rev.*, 1938, 45, 532–538.
- REID, L. S. The development of noncontinuity behavior through continuity learning. J. exp. Psychol., 1953, 46, 107-112.
- REYNOLDS, B. The acquisition of a trace conditioned response as a function of the magnitude of the stimulus trace. J. exp. Psychol., 1945, 35, 15-30. (a)
- REYNOLDS, B. Extinction of trace conditioned responses as a function of the spacing of trials during the asquisition and extinction series. J. exp. Psychol., 1945, 35, 81-95. (b)
- REYNOLDS, B., MARX, M. H., & HENDERSON, R. L. Resistance to extinction as a function of drive-reward interaction. J. comp. physiol. Psychol., 1952, 45, 36-42.
- SACKETT, R. S. The effect of strength of drive at the time of extinction upon resistance to extinction in rats. J. comp. Psychol., 1939, 27, 411-431.
- SALTZMAN, I., & KOCH, S. The effect of low intensities of hunger on the behavior mediated by a habit of maximum strength. J. exp. Psychol., 1948, 38, 347-370.
- SHEFFIELD, VIRGINIA F. Resistance to extinction as a function of the distribution of extinction trials. J. exp. Psychol., 1950, 40, 305-313.
- SHURRAGER, P. S., & SHURRAGER, H. C. Rate of learning measured at a single synapse. J. exp. Psychol., 1946, 36, 347-354.
- SKINNER, B. F. The behavior of organisms. New York: Appleton-Century-Crofts, 1938.
- SMITH, M. H., & HAY, W. J. Rate of response during operant discriminations. J. exp. Psychol., 1954, 48, 259-264.
- SOLOMON, R. L., & WYNNE, L. C. Traumatic avoidance learning: The principles of anxiety conservation and partial irreversibility. *Psychol. Rev.*, 1954, 61, 353-385.
- SPENCE, K. W. The differential response in animals to stimuli varying within a single dimension. *Psychol. Rev.*, 1937, 44, 430-444.
- SPENCE, K. W. Behavior theory and conditioning. New Haven: Yale Univer. Press, 1956.
- SPENCE, K. W., & FARBER, I. E. Conditioning and extinction as a function of anxiety. J. exp. Psychol., 1953, 45, 116-119.
- SPENCE, K. W., & FARBER, I. E. The relation

of anxiety to differential eyelid conditioning. J. exp. Psychol., 1954, 47, 127-134.

- SPENCE, K. W., GOODRICH, K. P., & Ross, L. E. Performance in differential conditioning and discrimination learning as a function of hunger and relative response frequency. J. exp. Psychol., 1959, 58, 8-16.
- STANLEY, W. C. Extinction as a function of the spacing of extinction trials. J. exp. Psychol., 1952, 43, 246-260.
- STARKWEATHER, J. A., & DUNCAN, C. P. A test for conditioned inhibition in motor learning. J. exp. Psychol., 1954, 47, 351– 356.
- TEEL, K. S. Habit strength as a function of motivation during learning. J. comp. physiol. Psychol., 1952, 45, 188-191.

- WASSERMAN, H. N. The effect of motivation and amount of pre-rest practice upon inhibitory potential in motor learning. J. exp. Psychol., 1951, 42, 162-172.
- WEBB, W. B. A test of "relational" vs. "specific stimulus" learning in discrimination problems. J. comp. physiol. Psychol., 1950, 43, 70-72.
- WOODWORTH, R. S., & SCHLOSBERG, H. Experimental psychology. (Rev. ed.) New York: Holt, 1954.
- YERKES, R. M., & DODSON, J. D. The relation of strength of stimulus to rapidity of habit formation. J. comp. Neurol., 1908, 18, 459-482.

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