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THE INFLUENCE OF WORK ON BEHAVIOR

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INTRODUCTION

In recent years it has often been demonstrated that the amount of work (or effort) involved in making a given response influences both the probability of recurrence of that response and the magnitude of the response upon its re-appearance. Interest in this type of phenomenon has, in general, undergone two relatively separate phases. In the first thirty years of this century, the problem of the role of work in the determination of behavior had been considered to be essentially the problem of the work decrement and the fatigue of the working organism. Robinson (73), in his comprehensive review of "the work of the integrated organism," looked upon the problem in that manner. In contrast, within the last fifteen years psychologists have begun to point out some of the inhibitory, motivational, and cue values of work, as well as the fatigue aspects. One way to describe this change in emphasis would be to state that Robinson was studying the work of the already-integrated organism, while the recent workers have been studying the role of work in the process of integration itself. The latter approach has been made more feasible with the development of a large body of knowledge about learning and conditioning. For, with the demonstration of fairly predictable relationships within the phenomena of conditioning and learning, the work or effort variable could be introduced in a controlled manner and its effects could be ascertained as it influenced the course of conditioning or learning. The early success of this type of experimentation is attested by the fact that the effort variable has been included in the theoretical systems of Miller and Dollard (69) and Hull (47); this, in turn, has stimulated more research along these lines. At

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the same time, the studies on fatigue seem to have become fewer in number.

It is not the purpose of this survey to present an exhaustive treatise on the subject of work or effort. Rather, the object is the presentation of a related body of data and theory, much of which has never been treated in a single survey. For the sake of brevity, only experimental and theoretical highlights will be covered. Therefore, there will be many relevant experiments which are omitted; in general, references often will be omitted when the essential relationships involved may be represented by a single reference. Thus this article is in no sense an exhaustive bibliographical effort.

Nor is it our purpose to present a body of data without critical interpretation. Indeed, this review is based upon a definite, though flexible, theoretical framework which will be presented at the outset. Such a procedure leads to admitted oversimplification. But at the present stage of our knowledge about the operation of the effort variable, such a method of presentation will allow us to integrate a large number of experimental findings which at present are, for the most part, unconnected in the literature. It is hoped that the theoretical framework will not be construed as uncompromising bias. The theory merely serves as a convenient and simple pattern on which the data may be arranged. The necessity for such simplification becomes clear when one considers that all responses involve work, and the effort variable thereby is common to behavioral psychology as a whole, to just about every experiment relating stimulus and response.

Theoretical considerations. Nowhere has the role of the effort variable in the determination of behavior received so much attention as in Hull's *Principles of Behavior* (47). Hull makes these assumptions, presented as the three basic postulates of work-inhibition theory:

1. Whenever any reaction is evoked in an organism there is left a condition or state which acts as a primary negative motivation in that it has an innate capacity to produce a cessation of the activity which produced the state (47, p. 278). [This state or condition is given the name "reactive inhibition." Its presence results in a certain reaction decrement, which Hull believes is closely related to decrements which are ordinarily attributed to fatigue. It is to be noted that reactive inhibition is an unobservable, and, in Hull's words, "has the status of a logical construct with all the advantages and disadvantages characteristic of such scientific concepts" (47, p. 278).]

2. The net amount of functioning inhibitory potential resulting from a sequence of reaction evocations is a positively accelerated function of the amount of work (W) involved in the performance of the response in question (47, p. 279). [Hull presents an equation which expresses this relationship. The constant of proportionality for the work variable is actually a parameter which

takes into account the relative work-capacities of the responding muscle groups.]

3. Each amount of inhibitory potential (I_R) diminishes progressively with the passage of time according to a simple decay or negative growth function (47, p. 281).

After making these assumptions, Hull goes on to discuss the various properties of the intervening inhibitory variable. In the first place, Hull believes that the "after-effects of response evocation in the aggregate constitute a negative drive strongly akin to tissue injury or 'pain'" (47, p. 281). Thus, reduction of painful stimulation by means of reduction in activity would be, in reality, a reinforcing state of affairs. As demanded by reinforcement principles, any stimulation present at the time that activity ceased would become conditioned stimulation for cessation of activity. Such decrease in activity, motivated and reinforced by negative effects of responding, can be considered to be a "conditioned resting response," if we may take liberty with Hull's formulation. Thus arises the first corollary:

Stimuli closely associated with the acquisition and accumulation of inhibitory potential (I_R) become conditioned to it in such a way that when such stimuli later precede or occur simultaneously with the stimulus situations otherwise evoking positive reactions these latter excitatory tendencies will be weakened (47, p. 282).

In this way, Hull introduces a new possibility: "that the influence of inhibition on behavior evocation may be controlled by a stimulus" (47, p. 283). Since inhibition derived from responding can be controlled by stimuli, then principles of stimulus generalization will apply to inhibitory potential. The class of inhibition which exhibits such characteristics is called "conditioned inhibitory potential" by Hull. Simple reactive inhibition, which, we remember, is transitory, summates with conditioned inhibition to give us the total inhibitory potential operating at any given time. From such theoretical propositions, Hull is able to derive the law of spontaneous recovery.

Simple reactive inhibition, I_R , is not to be confused with the conditioned inhibitory potential which is actually reinforced by the reduction of I_R . Conditioned inhibition is thought to involve the whole neural receptor-effector mechanism, while reactive inhibition, the primary negative motivating state, is believed to be an effector process. Hull states that such inhibitory potential most likely is dependent upon a substance "resident in the effector organs involved in the response" (47, p. 281). Moreover, such a substance probably will be removed by the blood stream in an amount which will be proportional to the amount

of the substance present at any given time. This is the basis for the assumption that reactive inhibition fades with time according to a negative decay function.

Hull, after presenting the various relationships possible within his explanatory system, demonstrates in a convincing manner that the theoretical system is adequate to handle many empirical phenomena. The various postulates, when combined, will predict such psychological occurrences as spontaneous recovery, reminiscence, the superiority of distributed practice, the empirical law of less work, and so forth.

It is essential to remember that, although Hull may refer to peripheral phenomena as a basis for his theorizing, his postulates and corollaries are never stated in such terms. Hull's constructs are intervening variables, only *indirectly* measurable within the requirements of his specific system. Thus, as *formally* stated, reactive inhibition is actually nowhere; it is merely I_R , a variable related to antecedent conditions and subsequent resultants. It is impossible to conceive of reactive inhibition as independently measurable, without changing the nature of the postulates. Yet, at the same time, Hull recognizes the full possibilities of peripheral formulations when he talks of effector processes as a basis for his intervening variable. Hull, unlike Pavlov (71), never attempts to give his construct of inhibition any status in the central nervous system. Reactive inhibition is a purely behavioral construct, a variable which relates stimulus-response correlations on a conceptual level.

However, when we carefully examine Hull's assumptions we note that, whatever may be the "true" nature of I_R , the existence of the construct is *response-determined*. It is produced only when responses occur. But it is a well-known datum of psychology and sensory physiology that certain kinesthetic, afferent impulses are always produced by the responses of intact skeletal muscles. There appears to be no other kind of afferent discharge which might be considered to be purely a function of the organism's own responses. This type of stimulation to the organism has been called "backlash" stimulation as well as response-produced stimulation. Such results of responding are essentially peripheral effects; we know where they arise, approximately which peripheral segments of the great afferent systems are involved, and to some extent we can record such effects. Thus the concept of response-produced stimulation, besides being a potential intervening variable, is susceptible of independent measurement and brings us close to the underlying physiology of the organism. Miller and Dollard (69), in their theoretical treatment of learning, consider the role of work or effort in

terms of peripheral effects such as these. In essence, they imply that effortful responses produce stimulation which has the characteristic of negative *drive* stimulation as well as *cue* stimulation. Thus effort serves indirectly as both a motivating and a conditionable stimulus.

With reference to our specific problem, there appear to be several advantages of the more peripheral constructs over the type of constructs used by Hull: (1) the explanatory construct employed is ultimately capable of independent measurement; (2) the results of responding are broadened to include the cue aspects of response-produced effects; and (3) the deductions generated are more comprehensive, with several physiological implications; thus they facilitate inter-disciplinary research, often fruitful in the past. Actually, these advantages are more pragmatic than logical; it is believed that the same deductions can be made from peripheral theories as are made from Hull's theories, with the advantage of several physiological deductions which Hull cannot make. Such physiological possibilities will be discussed later.

In view of the preciseness of Hull's assumptions and deductions, yet with an eye to the possible pragmatic advantages of peripheral constructs, it seems reasonable to take the liberty of restating Hull's propositions. In doing so, the deductions will not be altered with regard to purely behavioral phenomena. But more deductions, of a physiological sort, may be added. The fundamental aspects of the role of effort in theory will not be disturbed; rather, they will be broadened. With these considerations in mind, we find it aesthetically pleasing to recast Hull's postulates in peripheral language. In their new form, they read as follows:

1. Whenever a reaction is evoked in an organism, action of the effector system provides adequate stimulation of the kinesthetic receptors of the responding members. Such kinesthetic stimulation, like any other stimulation, may serve either as *cue* or *drive* or both; it is potentially a conditioned stimulus complex as well as a negative drive stimulus complex. This is nothing more than the state of affairs implied by Miller and Dollard (69). In the same way that hunger contractions may result in effects which serve as cues for differential responses, so the kinesthetic components may provide cues. Similarly, in the manner in which afferent impulses from hunger contractions are thought to serve as positive motivation for the organism, so kinesthetic stimulation may in many cases serve as negative motivation. This is the converse, in operation, of afferent impulses from the hunger contractions of the stomach. As a drive, or negatively motivating factor, kinesthetic effects may act to weaken response potentiality, or probability of occurrence of the response which produced those effects. As both cue and drive, the effects of responding are here postulated as peripheral constructs with peripherally measurable functions.

2. The net amount of effective response-produced stimulation derived from a given response (related to frequency of nerve impulses, and extent of fiber

involvement, presumably) is a function of the amount of work involved in making the response, multiplied by some constant which serves as an index of the capacity of the responding member for continued work output. The total cumulative effects of responding are represented by two factors (analogous to Hull's two factors, reactive and conditioned inhibition): undissipated drive and cue stimulus traces, and conditioned resting responses, previously reinforced by reduction of negative drive stimuli in the presence of a constant stimulus situation.

3. The effects of response-produced neural processes either taken as cue or drive, dissipate in time, probably in agreement with the accepted principles of the *stimulus trace*. This implies some decay function of time.

Before bringing our theoretical discussion to a close, it is interesting to note that, in some respects, we have presented nothing new here. The concepts of drive stimulus and conditioned stimulus are to be found in Hull's *Principles of Behavior*. But Hull does not use these concepts to the greatest possible extent with reference to the role of work. Reactive inhibition, when considered in the light of the rest of Hull's system, and in the light of the theorizing of Miller and Dollard, is, possibly, an unnecessary concept. In recasting Hull's theory in peripheral language, an economy has been effected only to the extent that old, peripheral concepts can supplant the new intervening variables. To use the idea of response-produced stimulation instead of reactive inhibition does not limit the possible predictions of behavior. And such predictions of behavior may be made with fewer intervening variables. It must be admitted, however, that at the present state of our knowledge, *either* type of formulation will serve adequately to explain the existing behavioral data which depend on work as a negatively motivating factor.

General plan of the review. The amount of evidence bearing on the concepts of response-produced stimulation and the role of effort is considerably greater than most theorists have heretofore indicated. The history of relevant experimentation can be conveniently classified under these topical headings:

1. Studies of the "Law of Least Effort";
2. Studies varying effort-per-unit-of-time in conditioning and learning;
3. Studies on avoidance of repetition of responses;
4. Studies of the role of kinesthesia in the control of behavior.

THE LAW OF LEAST EFFORT

It seems fairly clear that if the occurrence of a response is to be followed immediately by the production of negative drive stimuli in proportion to the effort involved in that response, a "*Law of Least Effort*" would become a corollary of the assumptions proposed by Hull (47), and restated above. In fact, Hull made a great point of this

(p. 294), mainly because previous workers had considered the "Law of Least Effort" to be an axiomatic behavior principle, not a corollary of several higher-order postulates. However, in fairness to the workers who preceded Hull, it was also true that their experiments aided greatly in making possible the generalizations stated by Hull.

In 1920, De Camp (17) demonstrated that "other things being equal, the shorter of two paths will be traversed more and more until it becomes the usual one, provided a certain fractional difference in length exists between short and long paths" (p. 253). Two years later, Kuo (58) noted that animals tended to eliminate ill-adaptive movements in learning a complex, four-choice maze. His animals were found to prefer paths in the maze in the following order: short path to food; long path to food; confinement for a short time, followed by food; and finally, shock followed by food. The first two preferences substantiated De Camp's observations. In 1928, Gengerelli (33) noted that rats in a maze tended to follow the path which led to food in a shorter interval of time. He called this the "path of least resistance." It may be seen that De Camp's study, of necessity, confounded the temporal delay-of-reward factor and the effort factor, since long routes were also delay routes. (This confounding has never been satisfactorily unravelled without introducing an added task. See below, the discussion of Thompson's (85) experiment.) In 1930, Gengerelli (34) formulated his principle of "maxima and minima in animal learning" on the basis of his own, Kuo's, and De Camp's experimentation. Gengerelli considered that all animals took a path to a goal which represented the least possible amount of energy-expenditure. This principle was considered by Gengerelli to be axiomatic and incapable of explanation or induction. Hence it was an important step that Hull (47) later made in explaining Gengerelli's principle in terms of the reactive inhibition postulates. Wheeler (93), in 1929, also stated a "law of least action," but he used physical terms and a mechanical analogy to do so. Tsai (88), in 1932, published an experiment which seemed to present proof of a law of minimum effort. Animals were presented with the possibility of reaching food through two doors weighted with 0 vs. 20 grams, 0 vs. 50 grams, or 20 vs. 50 grams. He found most of the responses to be in favor of the lighter door. Tsai also used a situation in which rats could traverse one of four paths of equal length; but all paths contained an obstacle to be climbed over, some obstacles being higher than others. He found that 75 percent of the responses were in favor of the lowest barrier. Tsai referred to his own results, together with those of De Camp, Kuo, and Gengerelli, as being excellent evidence for a law of minimum effort.

A study closely related to those cited above was that of Yoshioka (96) in 1929. He found, using the traditional long versus short path situation, that the frequency of choices of the shorter path was directly proportional to the ratio between the lengths of the paths. Thus relative distances rather than absolute differences in length seemed to operate in determining choices. (The exact nature of this function has never been agreed upon. Grice's (38) recent treatment of the problem contained a discussion of the nature of the relationship.) It is to be noted that temporal delay was confounded with length of path, or work, as in previous experiments of the same type.

Probably the most penetrating study of the principle of least effort was that of Waters (90), in 1937. Waters tested the assumption that the "Law of Least Effort" operates without qualification. He used maze patterns in which all possible paths were the same distance to the goal, but each path contained a different number of turns. Rats tended to take the paths with the fewest turns. Human subjects, using a stylus maze of the same pattern, did not distribute their choice of paths as did the rats. Rather, they preferred the pathways representing either the most turns or the least, with few choices of paths with an intermediate number of turns. Waters went on to discuss the principle of least effort as formulated by Wheeler, Gengerelli, and Tsai. He stated that Wheeler's variables were unrelated to any psychological variables which we are able to quantify, and so were of little use to psychology. Even if we could identify Wheeler's concept of "energy relationships," we could hardly measure it with the present techniques of physiology or psychology. Physical concepts are not psychological concepts; they are derived from different kinds of measurements. On the other hand, Waters considered Gengerelli's law to be derived from psychological data. In fact, both Gengerelli and Tsai recognized the function of repetition in the gradual appearance of a choice of least-effort behavior-routes during learning. Waters stated that Wheeler should expect an absolute choice of the path of least effort in but one trial; in other words, physical invariability. Tsai's formulation was for Waters the most adequate, since it recognized the limitations imposed by thresholds of discrimination as studied by Yoshioka. Finally, Waters warned against accepting the "Law of Least Effort" without qualifications.

This warning proved to have been a pertinent one. For example, Fischel (30), in 1932, found that monkeys' preferences for peanuts over crackers were not changed by making it more effortful to obtain the peanuts. Fischel felt that positive incentives often are more potent than negative ones like work or effort. Again McCulloch (67), in 1934,

used a weight discrimination problem with rats. He trained rats to pull in the lighter of two trays of food, but position habits often made it impossible to establish this discrimination. This was certainly a violation of an unqualified "Law of Least Effort." McCulloch substituted a concept of "preferentials" to account for his results. "Preference" for right or left position often overrode "preference" for least effort. The experiments of Fischel and McCulloch gave ample evidence that the "Law of Least Effort," as a separate principle, had to be integrated with other principles of motivation, as well as with principles of learning, and thresholds of discrimination, in order to explain the experimental data. Waters' contentions were well-substantiated. They rendered partially invalid such theoretical statements as those of Gengerelli (34), Adams (1) and others, who had accepted the parsimonious implications of an unqualified "Law of Least Effort."

The most recent statement of a "law of less work" was that of Hull (47). Using an experiment of Grice (38) as his point of departure, Hull developed a substantiation of a law of less work as a corollary to his three reactive inhibition postulates. His corollary was stated as follows:

If two or more behavior sequences, each involving a different amount of energy consumption or work (W), have been equally well reinforced an equal number of times, the organism will gradually learn to choose the less laborious behavior sequence leading to the attainment of the reinforcing state of affairs (47, p. 294).

Grice's work (38) was assumed by Hull to substantiate the corollary. But we should note that Grice did not attempt to equalize the temporal delay factor in his experiment; temporal delay-of-reward and work were thus confounded. He presented rats with two paths to food, one always longer than the other. The combinations of distances were: 6 vs. 12 ft., 12 vs. 18 ft., 18 vs. 24 ft., 24 vs. 30 ft., and 30 vs. 36 ft. He reported an observed relationship between errors (taking the long path) and the ratio of long path to short path. Grice brought up the very important problem of the role of time and distance in his particular learning situation. For the most part, Grice considered the problem as one of temporal delay of reinforcement. But he did not offer temporal delay as the only interpretation of the data. Grice demonstrated that there was no consistent relationship between the learning performance of his rats and the measured amounts of time spent by the individual animals in the two paths. But there was a definite relationship between learning performance and distance to the goals. Grice stated:

These data are not in any way conclusive on this point, but they do suggest that, in maze learning at least, the effect of a given reinforcement on a response

is a function of the amount of activity on the part of the animals intervening between the response and the reinforcement, rather than temporal delay per se. Distance, of course, is closely related to the amount of activity required to reach the goal (38, p. 488).

In terms of our own introductory statements, it may be seen that "activity" might have been rephrased to read "work." Thus Grice was in essential harmony with Hull's postulates on reactive inhibition.

Very recently, Thompson (85) has unravelled the confounded time and effort variables in the maze. He used a symmetrical T-maze, but placed lever-boxes at the end of each arm of the T. The levers were differentially weighted, so that the rats had to press harder at one end of the T than at the other end, in the process of obtaining the food reward. Thompson's rats learned to select the path to the lever which was the more lightly weighted of the two. Here, temporal delay was the same, but work requirement was different for the two possible paths to food. These results supported Hull's "less work" corollary.

Hull referred mainly to the work of Gengerelli (33), Wheeler (93), Tsai (88), Waters (90), and Crutchfield (13) in finding substantiation for his corollary on less work. Actually, the Crutchfield experiment was a very special case of the least effort principle in operation, and one which probably offers some obstacles to Hull's theorizing. Crutchfield (13) trained rats to pull in food by means of a string and pulley arrangement. During training, Crutchfield varied the force required to pull in the food, as well as the length of the string to be pulled in, and the number of training trials. A separate group of rats was used for each variation. Then tests were conducted under three conditions of motivation and three conditions of force required to pull in the string. The factorial design of the experiment allowed for adequate statistical analysis, based on an economical number of rats. The test trial was so labelled because no food was available. The strength of response was defined as the amount of string the rat kept pulling in until the first work pause. Briefly, Crutchfield found:

1. The larger the force necessary to pull in the string during training, the longer the length of string pulled in under test conditions.
2. The longer the length of string an animal had been trained to pull in for food, the longer the string pulled until the first work pause by the rat during the test session.
3. The more training trials a rat had received, the less the length of string pulled in during the test.
4. Most string was pulled in under conditions of 24 hours' hunger at the test sessions (this hunger level was the same as that used in training).

5. The greater the force required of the rats to pull in the string in the test trials, the less the amount of string pulled in.

6. The velocity of string-pulling in the test was related positively to the number of training trials, and was related negatively to the force required during the test.

7. In general, each animal tended to expend from three to seven times the amount of effort in the test trial that he spent in a training trial, with the larger number of training trials producing smallest effort expenditures in the test trials.

Possibly Crutchfield did not demonstrate more than the fact that the rats might have "expected" food in the test trial, did not find food after expenditures of the usual amount of effort and time, and so "looked" further, in harmony with the "expectancy" established in training. Since only one test trial was made for each animal, it was really the first trial of an extinction series. Perhaps the most illuminating data on the effects of work could have been obtained had the test trials been carried on as a protracted extinction series; spontaneous recovery could have been tested after a suitable time interval. Such an experiment is certainly needed. Crutchfield showed that rats built up a general "expectancy" about the level of work that was required in obtaining the food reward. Such an "expectancy" was established in training trials. But the negative drive effects of work could not be clearly shown in this experiment. In fact, the rats seemed to change their level of work output with the special requirements of the situation, and probably maintained that level until the situation was altered during the tests. Hull's postulates did not take this adaptive principle into account. In terms of response-produced kinesthetic stimulation, however, adaptation phenomena can be appealed to. It may be possible that the "effective" level of the response-produced negative drive stimuli may be altered by habitual levels of energy expenditure. Thus the negative value of such stimulation may be modifiable, and may not always be equal to some absolute stimulation level. Such an hypothesis is a tentative addition to the postulates concerning the drive and cue value of response-produced neural impulses.

In general, the review of the literature on the "Law of Least Effort" has indicated that effort expenditure acts in opposition to positive motivation, in an amount related to the work required in performance of reinforced responses. Experiments on the "Law of Least Effort" contributed heavily to Hull's thinking, especially those experiments on long versus short path selection in the maze. Such experiments presented, for Hull's examination, a clear example of behavior that was influenced by differential work requirements.

EFFECTS OF VARYING EFFORT-PER-UNIT-OF-TIME IN CONDITION-
ING AND LEARNING*Acquisition*

The experimental work on the relative efficacy of massed and distributed practice has included a large variety of situations, using both human and animal subjects. This rich body of literature contributed heavily to Hull's supporting data in the formulation of the three reactive inhibition postulates. Because this literature has been so extensive, it will be possible to review only a few of the more influential studies, especially those upon which Hull has relied heavily.

In the first place, it is possible for us to think of the temporal spacing of trials or responses as a distribution of effort. Presumably the effects of effort, or response-produced impulses, will approach a minimum, asymptotically, in their decay during the time interval between responses. We might expect that the greater the effort required in each response, the longer the time necessary for the traces of response-produced effects to approach a minimum. Thus, in the case of massed practice, the course of acquisition of any learned or conditioned response, as represented by the conventional learning curve, would be depressed by the undecayed accumulations of drive stimulus traces (I_R , in Hull's system) which provide negative motivation, and by conditioned resting tendencies. But in the case where responses are relatively far apart in time, we should expect the negative drive stimulus traces to fade between responses, so that the learning curve would be more representative of maximal performance.

Consistent with the argument that spacing of trials is productive of a certain level of effort-per-unit-of-time, we can make a further prediction. If time interval between responses is kept at a constant value, and the effort required by each response varied, it should be possible to duplicate the results of experiments which have varied time interval alone. We reason as follows: If the magnitude of the neural involvement (in terms of frequency and number of fibers activated) resulting from response-produced stimulation is great, the traces (as well as possible effector processes) may take longer to decay to a minimal value. Therefore, if we increase the effort required in making a response, and do not increase the time interval between responses, the traditional learning curve, or acquisition curve, will be depressed. The presence of negative drive stimulus traces and learned resting responses will counteract positive motivational factors in the learning situation. To our knowledge, there have been no experiments in which effort-per-unit-of-time was systematically varied, to study the changes in the

acquisition curve, by varying the effortfulness of task. (There has been but one thorough study of extinction curves under different levels of effortfulness of task. See Mowrer (70)). For the most part, the studies of the effects of effort-per-unit-of-time on acquisition or learning have been studies on the temporal distribution of effort, rather than on effortfulness of the task.

One of the earliest experiments which reported decremental effects of continued responses in a curve of acquisition was a study of Pavlov (71). He found that in the acquisition of a conditioned salivary response the number of drops secreted on each trial gradually decreased if the number of trials in a session was greatly increased, but on the following day the first few trials showed complete recovery. Pavlov interpreted his results as follows: "It is seen that one and the same stimulus which is constantly reinforced and repeated many times at long intervals of time loses only little of its effect; the same stimulus when applied at short intervals of time quickly diminishes in its effect . . ." (71, p. 248). Pavlov believed that inhibition of cortical sensory "analyzers" was the cause of this phenomenon. His finding was a clear statement of the effects of massed and distributed practice in the acquisition of a conditioned reflex.

One of the earliest studies in this country bearing on the problem of effort-per-unit-of-time was that of Starch 80, in 1912. He had human subjects learn to associate numbers with letters of the alphabet, and he varied the time interval between responses from group to group. Starch found more rapid learning (in terms of errors) with spaced trials. Starch believed that his results indicated that the optional conditions of spacing gave newly-formed associations ". . . a chance to become settled and fixed" (80, p. 213). He attributed part of the decrement in the massed practice group to fatigue, however.

In the period between 1915 and 1935 there were many experiments like those of Starch. They had implications for educational psychology, and so were carried on with much enthusiasm. McGeoch (68), in his book, has reviewed these studies thoroughly. He has concluded: "The experiments on distribution of practice have found that the introduction of time intervals between practice periods of varying lengths yields more rapid learning, under a wide range of conditions, than does continuous practice . . ." (68, p. 147).

Calvin (6) has demonstrated that the acquisition of the conditioned lid reflex is greatly influenced by the time interval between responses. The learning rate for three responses per minute was almost double that for nine responses per minute. Not only the rate, but the asymptote

approached by the acquisition curve, showed the decremental effects of increasing the effort-per-unit-of-time by massing trials. Humphreys (48) has corroborated Calvin's results for the acquisition of the conditioned eyelid reflex. Humphreys, Miller and Ellson (49) have demonstrated clearly that the beneficial effects of spaced practice in conditioning experiments are due to "decremental rather than expectancy factors" (49, p. 202). These workers studied the effect of intertrial interval on the acquisition, extinction, and spontaneous recovery of verbal expectations in college students. They concluded from their data that spaced practice was of no advantage in either acquisition or extinction of verbal expectations. They did find spontaneous recovery of verbal expectations, but no clear course of progressive recovery. They assumed that "the work-decrement factor would presumably be at a minimum for a verbal choice" (49, p. 196). Therefore the effects of massed practice in eyelid conditioning must have been due to work decrement or work inhibition influences. (See also: Schlosberg, 75, Wolfe, 95.) Hovland (46) compared the trials needed for human subjects to learn nonsense syllable lists of different lengths, under conditions of massed and distributed practice. He found the superiority of distributed practice to be greater as the lists grew longer.

In general, we can state that Hull's expectations about the course of learning or acquisition have usually been substantiated. There have been a few exceptions, but these have been closely associated with the problem of insight and the discovery of relationships by human subjects. The verbal factor is therefore very important. For example, Lyon (64) found that the learning of poetry benefited from continuous reading. And Cook (11) found that massed practice facilitated the learning of meaningful material. These exceptions are probably not completely relevant to our main problem, because of the obscuring effects of higher verbal processes. We are able to accept the judgment that in general the data seem to fit theoretical expectations. Even under the extremely restricted conditions imposed by the Graham-Gagné (37) runway situation, Gagné (32) has shown that the acquisition of the conditioned operant (locomotor) response was more rapid with longer time intervals between runs, and that the latency of response asymptote was lower with longer time intervals between responses.

We may conclude, therefore, that increasing the effort-per-unit-of-time, by altering the time interval between responses, results in slower learning and a depressed asymptote for the acquisition curve. There are no data to tell us whether this would be so if we altered effortfulness of task rather than temporal interval between responses. There clearly is a program of research suggested by this factual gap.

Extinction and Forgetting

Extinction and forgetting curves have been extremely revealing with respect to the effects of effort-per-unit-of-time as an experimental variable. The reason for this is clear. If effort produces negative drive effects which fade slowly in time, massed practice produces an acquisition curve which is depressed. If there is a time interval between acquisition and extinction, or between learning and tests of retention, the decremental effects have time to decay. This predicts the phenomenon of reminiscence as an essential characteristic of extinction and forgetting curves, wherever the temporal interval between acquisition trials has been relatively small, or the effortfulness of task has been relatively great. In other words, wherever the factor of effort-per-unit-of-time has served to depress a learning curve, the interval between the last acquisition trial and first extinction trial may serve to allow dissipation of the decremental, response-produced effects. If the same high level of effort-per-unit-of-time persists in extinction, then the extinction curve should be steeper, or extinction rate greater, than in the case where the effects of work expenditure have been allowed to decay during protracted intertrial time intervals. The foregoing considerations have been discussed thoroughly by Hull (47) and by Buxton (5).

Pavlov (71) reported more rapid extinction with massed practice: "Yet another important factor in determining the rate of experimental extinction is the length of pause between successive repetitions of the stimulus without reinforcement. The shorter the pause the more quickly will extinction of the reflex be obtained, and in most cases a smaller number of repetitions will be required" (71, p. 52). Switzer (83) showed that the first few extinction trials of the conditioned eyelid response were often greater in amplitude than the last acquisition trials. Hovland (45) showed that, in the learning of nonsense syllables, the same effect in the retention or forgetting curve was observable. Such an effect, called reminiscence, was greatly enhanced when acquisition was carried on under conditions of massed practice. In yet another study, Hovland (44) demonstrated that reminiscence effects appeared in massed presentation of nonsense syllables. Such results readily fit the deductions from either Hull's or our own postulates. The learning curves above were depressed by response-produced effects, and since such products of responding are believed to decay with time, the learning level, as objectively measured, was objectively higher after a time interval was introduced.

Probably Hovland's (43) experiments on "inhibition of reinforcement" were used in Hull's recent formulations more than any other single source. Hovland conditioned the galvanic skin response to a

thousand cycle tone, under four conditions of temporal spacing of responses. The unconditioned stimulus was shock. Four groups of 20 subjects each were used, one group for each of the four following conditions: (1) 8 reinforced responses, one minute apart, with extinction being started immediately following the last reinforced trial; (2) 24 reinforced responses, one minute apart, with extinction also being started immediately after the last reinforced trial; (3) 24 reinforced responses, one minute apart, but with 30 minutes elapsing between the last acquisition trial and the first extinction trial; and (4) 24 reinforced responses, divided into three groups of eight responses each, separated by 30-minute intervals; extinction was started immediately following the last acquisition trial of the third group of trials. The one-minute intervals were varied slightly to prevent temporal conditioning. Hovland's extinction curves showed the following salient features:

When few reinforcements were given a continuous fall in the extinction curve results. . . . With a much larger number of reinforcements, however, the curve manifests an initial rise. . . . When the reinforcements are distributed . . . or when time is allowed to elapse between reinforcements and extinction . . . no initial rise is obtained (43, p. 432).

Hovland interpreted these results in terms of a principle of "inhibition of reinforcement." That is, he held that the reinforcement process itself generates a type of inhibition which acts like the inhibitory process found in extinction. Hovland was, presumably, thinking of the process in terms of a negative contribution of reinforcement itself. Since reinforcement was considered by Hull to be the reduction of drive stimulation in the presence of certain stimuli, Hovland's view would, if analyzed, mean that the organism is partly inhibited by the reduction of drive stimulation. As later writers have pointed out (see below, in the discussion of Heathers' (40) work), Hovland's original conception may have been unfortunate. Hull later suggested (47, pp. 292-293) that the reactive inhibition postulate was superior. Hull believed that the rise in Hovland's curve for Group 2 (see above) was not indicative of a transitory inhibition of the *process of reinforcement*, but rather that the rise was indicative of inhibition of response magnitude due to accumulation of I_R during the acquisition responses, as well as conditioned inhibition. Since the process of reinforcement has been assumed to strengthen S-R associations, it is difficult to see how inhibition of reinforcement could prevent the full strengthening of S-R bonds and at the same time strengthen the S-R bonds by merely dissipating with time. Then, too, Hull pointed out that inhibition of reinforcement is related only to time interval, and does not consider work factors. In our own

words, the effort-per-unit-of-time variable seems to be handled inadequately by Hovland's construct. Possibly Hovland's concept is highly specific to his conditioned response, the galvanic skin response, a response complex which may be found to produce relatively little "backlash" stimulation. That is our best guess about this disagreement between theories.

The experiments outlined above have varied the effort-per-unit-of-time during acquisition and extinction by altering the time intervals between responses. There is another method of accomplishing this variation: namely, by varying the work requirement, or effortfulness of task, rather than the time interval between responses. The outstanding experiment of this sort in the literature was that of Mowrer and Jones (70), in 1943. These workers trained rats in a lever-pressing situation, with the lever counterweighted in order to present varied amounts of work for the rats to do. During the acquisition trials the rats were given equal opportunity to press the lever, loaded by these weights: 5 grams, 42.5 grams, and 80 grams. The rats were then divided into three groups for the extinction trials: one group pressed only 5 grams; the second, 42.5 grams; and the third, 80 grams. A negative linear relationship was found between the number of responses required for extinction and the effortfulness of task during the extinction trials. The relationship could be fitted to Hull's equations.

There have been other opinions and data which have lent credulity to the reactive inhibition postulates, or our restatement of them, without actually testing the deductions directly. Brogden, Lipman and Culler (2), writing on the role of incentive in conditioning and extinction, stated:

Whereas conditioning is activated by a potent excitor (shock), the only incentive for extinction is inertia. The dog might keep on acting forever were it not easier now and then to stand still (2, p. 112).

These conclusions were made with reference to the course of acquisition and extinction of a conditioned forelimb flexion to tone, using dogs. In these conclusions we see a recognition of the decremental effects of merely responding on further responses from the same body members. Mowrer and Jones (70), with reference to the work of Brogden, Lipman and Culler, said: "Growing evidence indicates that extinction is a motivated adjustment in which fatigue plays an important role" (70, p. 369). Thus it is not surprising to find that Hull included such effects, derived from responding, within the framework of his principles of behavior.

The work of Fitts (31) should also be mentioned here. He studied

the perseveration of non-rewarded behavior in a lever-pressing experiment. Varying both the hunger level and the work requirement, Fitts found that the work requirement, or negative motivation, could be counterbalanced by the increase of positive motivation in protracted food deprivation.

In general, the study of extinction under varied levels of effort-per-unit-of-time indicates that increased effort produces decremental effects on succeeding responses. It should be pointed out that no one has ever systematically varied time interval between responses as well as effortfulness of task in a factorially-designed experiment. Such an experiment would yield important information concerning acquisition and extinction rates as a *joint function* of these two reciprocating variables. Such an experiment would be needed in order to equate temporal spacing units with effort units in some quantifiable manner. That is, increased time intervals between responses might be expected to neutralize increased effort requirements, insofar as response-produced decremental effects are concerned.

Reminiscence and Spontaneous Recovery

The topic of reminiscence, as related to the problem of response-produced drive stimuli, has been described above as an integral phenomenon of forgetting and extinction curves. The most extensive review of the experimentation on reminiscence was that of Buxton (5). He concluded that "reminiscence as a dependent variable seems to be related, to the type of learning technique employed, the type of subject matter, degree of mastery before rest, type of practice (degree of distribution), length of rest interval, and probably to serial position, rate of exposure of items and the amount of material to be learned" (5, p. 337). Buxton did not feel that work-decremental theories were as adequate as differential forgetting theories in handling the data on reminiscence.

There have been no clear experimental demonstrations of the relationship existing between effort-per-unit-of-time, during either acquisition or extinction procedures, and rate of spontaneous recovery from extinction. One might predict from Hull's postulates, and from our own restatement of them, that a high level of effort-per-unit-of-time would depress an extinction curve so that the spontaneous recovery rate might be greater. This suggests a program of experimental work.

THE STUDIES OF AVOIDANCE OF REPETITION OF RESPONSES

Closely related to the problem of response-produced negative motivation, as discussed by Hull, is the problem of short-term response-

produced effects. Hull did not emphasize these, in either building or testing his theories, so we shall discuss them separately.*

The relationship between short-term effects of responding and the general problem outlined by Hull becomes clearer if removed for a moment from the frame of reference of conditioning dynamics. If we assume that an organism is placed in a situation where one of two possible choices must be made at regular and successive times; and, further, if we assume that the behavior potentiality of both possible responses is about the same, then the performance of the organism should give some insight into the operation of short-term, response-produced effects. For example, in a T-maze where right-turning has been rewarded as often as has left-turning, in a sequence of forced choices, we would expect a tendency to avoid making the response which has been made most recently in time. We expect this on the basis of the fact that response-produced effects have been previously demonstrated to act at negative motivation, or in an inhibitory capacity. And in the two-choice situation this would tend to produce alternation behavior, the result of avoidance of repetition of responses. In multiple-choice situations this tendency should be noted in the fact that an animal makes choices which have been previously made most remotely in time.

The Rat in the Maze

More than thirty years ago, Hunter (50) noted that his rats tended to alternate their choices of two possible responses while learning an auditory discrimination. A few years later, in 1920, while studying the rats' behavior in the temporal maze, Hunter (51) noted such consistent alternation of choices from trial to trial that he assumed that rats possess an innate tendency to alternate (p. 9).

Since that time many psychologists have noted that rats tended to avoid making a response which has just been made. Tolman (87), in 1925, stated that the "exploratory tendencies" of the rat, as well as its innate tendency toward varying its responses, would explain the alternation of the left and right choices in the elevated T-maze. Tolman argued that the situation where the rat alternated its choices must have been unsatisfactory in some way, as far as the rat was concerned: the rat was either looking for an easier way to get to the food, or was looking for a place where there would be more food available on each trial. However, such an explanation was not readily accepted, and this stimulated experimental work.

Yoshioka (97), in 1929, found that rats tended to alternate slightly above chance expectations, when allowed to run freely in a maze with

* The "short-term response-produced effects" referred to here do not include the phenomena described by Hull as "behavioral oscillation."

two parallel alleys, each running the same distance from starting box to goal. The author concluded that "alternations, then, as far as our evidence goes, are not due to any innate alternation tendency, but rather due to behavior possibilities expedient to certain situations" (97, p. 264). This, of course, was not intended to be an explanation. In 1930, MacGillivray and Stone (66) allowed rats to run in a discrimination apparatus where successive choices were made. This situation was, in effect, a discrimination series in tandem, with five successive choices made on each run. It was found at the outset that a tendency to alternate had to be overcome before any learning of the discriminable aspects of the situation could occur. The authors stated that there was no evidence for "pure" trial and error behavior early in learning. Several factors governing early discrimination-learning behavior were listed, with the tendency to alternate being most important. No attempt was made to explain the phenomenon.

It is interesting to note here that in 1930 Lashley's discrimination apparatus was introduced; no evidence of a tendency to alternate has ever been mentioned in conjunction with this discrimination situation. As a matter of fact, in 1941 Jackson (57) found repetitive behavior (position habit) to be a characteristic mode of adaptation to the Lashley jumping situation when food reward was available at either door. Such apparently negative data will have to be explained as well as the more consistent data obtained on the T-maze and Y-maze.

The most extensive program of research on the tendency to avoid repetition of responses was that carried on by Dennis and his co-workers. In 1932, Dennis and Henneman (22) reported that the exploratory behavior of rats on an elevated multiple-T maze (single alternation spatial maze) was not true trial-and-error behavior. According to Woodworth, Thorndike, and many others, a rat, when placed on a maze, was supposed to make just about every possible error and success, due to the fact that rats seemed to explore the whole maze. Dennis and Henneman found this to be far from the actual case. Their rats did not explore the whole maze; two of them traversed six units without an error, on their first exploration. Several other rats, after but a few errors, traversed the remaining part of the maze correctly. The probabilities for such events occurring by chance are extremely small. Evidently a left-right-left-right sequence was extremely easy or "natural" for the rat; the authors did not explain their results.

In 1934, Dennis and Sollenberger (23) presented an interesting finding. Their rats' total activity in a maze decreased, in terms of entries into blinds, long before the animals secured a reward. This initial "exploratory" behavior by the rat in a maze suggested to the authors that some concept of adaptation had to be invoked in order to explain its exploratory behavior. The authors felt that the animals were exhibiting a *sensory* adaptation to the maze; the running responses could not be as easily elicited as when the rats first entered the maze. This was as-

sumed to be a process of adaptation to the stimulation provided by the maze. Dennis and Sollenberger stated that it seemed likely that a rat would "... become loath to take any pathway, i.e., the rats' locomotor activities would become almost completely negatively adapted to the maze" (23, p. 197). In terms of their theory, Dennis and Sollenberger stated that any reward which was received by the rat in the maze did not eliminate errors per se, but actually served to prevent complete "negative adaptation" to the true pathway, while other, erroneous paths were adapted-to negatively. This was certainly a unique way of looking at initial maze responses. In their first experiment, the authors used a single unit, 120° Y-maze, with 44 naive, hungry rats. The rats were placed on any arm of the symmetrical Y-maze, and were allowed to explore freely. There was no food available in the maze. The number of entrances into alleys decreased from 100 the first minute of exploration to 80 the second minute, 69 the third, 64 the fourth, and 41 during the fifth minute. The distribution of choices was uniform, or equal for the three paths of the Y. The experimenters found that 67 percent of the entries into the paths represented choices of a path which had been explored most remotely in time. The rats tended to avoid choosing a path which they had entered recently. In a second experiment, these same rats, when allowed to explore a multiple Y-maze for five minutes, made three times as many alley entrances compared to the single unit Y-maze. This increase in exploratory activity in the multiple unit maze was believed to be due to the diminished negative adaptation, since the space to be explored in the second experiment was much greater than that in the first experiment. In the second experiment the most recently entered path was avoided in 78 percent of the rats' choices. The most important finding for our own purposes was that the rats tended to avoid a pathway very recently explored. The authors explained this fact by their principle of negative adaptation. They believed that a new interpretation of initial maze learning behavior seemed to be required because of their results: errors eliminated themselves without reward under certain restricted conditions such as an elevated, single-alternation, spatial maze. The avoidance of repetition of a recently-made response would, under such circumstances, eliminate errors spontaneously if the first response was correct.

In another study, Wingfield and Dennis (94) found that alternations in a two-choice maze situation reached 94 percent with two trials each session. But with six trials per session the tendency to avoid repetition of responses was only 68 percent. These results were interpreted by the authors to mean that two trials per session would be superior for a multiple-unit maze of the single-alternation type. There was not a superior mnemonic value to this procedure; rather, there was thought to be less negative adaptation to the correct path with fewer trials per session.

In 1935, Dennis (20) made a careful comparison of the rat's first and second explorations of an elevated Y-unit. Of 190 trials, 82 percent of

the second explorations were runs to the unexplored branch of the Y. Next, Dennis employed an elevated T-unit, placing the rat on one of the top arms of the T for two successive trials. New wood was used on each trial, and the maze was moved about the laboratory between trials. The other top arm of the T was chosen on 77 percent of the first runs, since this was a straight path; but on the second runs, 58 percent of the responses were to the "stem" of the T-unit. Of the rats which ran straight ahead on the first run, 72 percent turned off into the "stem" on the second run. Combining data, 79 percent of the animals changed choices from the first to the second trial. Again, a negative adaptation interpretation was given for these results.

Dennis' final contribution in this particular facet of psychological research was a survey of "spontaneous alternation" behavior in rats, as evidence of *persistence of stimulus effects* (21). Dennis discussed the two-choice T-maze and Y-maze situation, with specific reference to the tendency to avoid repetition of responses. Dennis stated that such behavior indicated the persistence of traces of previous activity. He considered that such retention would last as long as one minute. (In one sense this is similar to the delayed reaction experiment. The rat must retain stimulus traces between trials and use them in making successive choices.) Dennis apparently did not have in mind concepts such as those of Hull. He appeared to be assuming that the rat remembered which way it had gone on the last trial and made a "decision" on that basis. Thus the term "spontaneous." But Dennis provided no hint of an underlying mechanism for spontaneous alternation in the two-choice maze situation. This behavior was left in about the same status that Hunter found it in 1914.

In 1940, Crannel (12) offered some new light on "spontaneous alternation," with a theoretical interpretation based on Tolman's system of behavior. Crannel suggested that Dennis' results might be fitted by the concept of "demand against distance." This "demand" explained the long path versus short path experimental results. But, in addition: "The condition of two equally short (or long) paths to the goal does not exclude the possibility of the rats' searching for a still shorter one" (12, p. 384). In a situation where only one of two "behavior-routes" was followed by food reward, the problem was easy for the rat. But in situations where the reward was the same for both paths, the rats' "demand against distance" became an insoluble problem. This was reported by Crannel to produce a large amount of VTE behavior at the choice point. In fact, Crannel reported that his rats in the T-maze showed more VTE behavior when they alternated than when they repeated choices. It was assumed that the rats were "looking for" a shorter path to food. This was not the same as considering response-produced effects to be the underlying mechanism for the alternation behavior.

The experiment of Heathers (40), in 1940, demonstrated clearly that

the alternation tendency of rats decreased as time between trials was increased in a controlled fashion. Heathers used a simple, elevated T-maze; the stem, or starting path was 30 inches long, as was each "arm" of the T. Heathers fed the rats no matter which arm of the T they chose. He gave the rats 10 trials per day for six days; the 10 trials were split into two sets of five trials each. The time interval between trials was defined as the time elapsing between the moment the first choice was made and the time the starting gate was opened for the start of the next trial. The "true" time between trials was acknowledged to be the time between choices, which was uncontrollable, due to latency and running time variability from trial to trial and rat to rat. For 15, 30, 60, and 120 seconds between responses, the percentages of alternation were 85.6, 83.5, 75.0, and 65.6, respectively. These results showed clearly that the tendency to avoid repetition of responses in the T-maze decreased as the time interval between responses was increased. Heathers also recorded running time. He found it to decrease with the longer enforced time intervals between trials. However, he found no difference in running time between those trials on which rats alternated and those on which rats repeated responses. To Heathers, "This indicates that the avoidance of repetition tendency as exhibited by an increase in running time is primarily in avoidance of running either pathway, rather than of the pathway last run" (40, p. 371). The alternation percentage was explained by Heathers to be a kind of phenomenon produced by "performance decrement." It seems clear that a general theory should be able to explain both running time and alternation data. Heathers invoked a tendency "not-to-run," plus a "performance decrement" dependent on the character of the last response. It should be noted that the characteristics of the "performance decrement" were compared by Heathers with Hovland's (43) concept of "inhibition of reinforcement." Heathers did not like Hovland's concept because it referred to the performance decrement as a function of reinforcement and time between trials rather than a function of the *reinforced reaction* and the *time between reactions*. Heathers believed that the "performance decrement" was like the refractory phase of simpler mechanisms. We shall discuss this analogy later.

Heathers' paper offered some important information concerning the underlying mechanism of alternation behavior. The fact that alternation behavior was still evidenced when the time between trials was 120 seconds indicated that the response-produced effects were capable of influencing responses over relatively long delay periods. Any conception of the *stimulus trace* derived from sensory physiology would have to account for such long persistence of after-effects. Effective stimulus traces of more than a few seconds have never been detected. Corroborating such physiological findings, Grice (39) has shown that the gradient of delay of reward is steep, persisting not more than five seconds or so, if

the kinesthetic stimulation of the delay period is at a minimum (in order to prevent secondary reinforcement of differential responses). Without going into Grice's experiments in full, it can be stated that he further found that providing kinesthetic stimulation during the delay period made it possible for rats to solve a discrimination problem with *longer delays of reward*. Presumably the kinesthetic stimulation provided a source of secondary reinforcement. In the same way, it is possible to consider that, in Heathers' experiment, persisting kinesthetic stimulation enabled the rats to retain the response-produced effects of each trial during the intertrial interval. The limit of such persistence of effects was probably near 120 seconds.

This time period has broad significance. Perkins (72) trained rats in an alley-type T-maze in which temporal delays of reward were used. The rats were detained in delay boxes on the two arms of the T. These delay boxes were randomly alternated from side to side in order to eliminate the effects of differential secondary reinforcement from the delay boxes. Food was available only on one arm of the T-maze, beyond the delay box. Perkins found that his rats could learn the T-maze problem with delays up to 120 seconds. The learning obtained with that temporal delay of reward was barely significant. The alternation tendency found by Heathers for the 120-second interval between trials was barely above the level of statistical significance. From these two very different experiments we are able to draw one general conclusion. The effective persistence of the effects of responding, both as cue stimuli and drive stimuli, seems to have a limit at about 120 seconds for ordinary *locomotor activity* in the rat. Later this may prove to be of physiological significance. Now we find it difficult to explain the 120 seconds figure. It seems to represent something fundamental about response-produced neural effects and their effectiveness during the temporal interval following their inception.

Heathers' study did not terminate the curiosity of psychologists about non-repeated-responses in the T-maze. Weitz and Wakeman (92), in 1941, thought that removing the rat before it had eaten its fill served as punishment in the two-choice situation used by Tolman (87), Dennis (21), and Heathers (40). This interpretation was thought to account for "spontaneous alternation" in conditioned response terms: the rats were being punished slightly after each rewarded response. Therefore, Weitz and Wakeman ran two groups of rats; one group finished a small pellet on each trial, while the other group ate a part of a larger pellet. The eating time was the same for both groups. Two trials per session were run for 30 days. The first group showed 73.7% alternation for the first 10 trials, decreasing to 52.5% on the last 10 trials; and the second group showed 68.8% alternation on the first 10 trials, decreasing to 47.7% on the last 10 trials. The difference between these groups was not reliable. But the running times for the second group

were significantly slower, fitting in with the punishment theory. It was also suggested that the animals' being picked up after each response was a punishing factor. The decrease in percentage of alternations over 60 trials (or 30 sessions), was notable. This decrease has also been reported by Tolman (88), and seemed to represent the operation of small, uncontrolled cues in the situation which acted to produce position habits. If such a situation affected only a few rats in a group, the mean percentage of alternations decreased considerably.

The last study to appear on this topic was that of Jackson (57), in 1941. He showed that rats which had developed position habits in the Lashley jumping situation, with food reward at either side, would, when placed in a maze used by Dennis, alternate 74 percent of the time. When wooden strips were placed between the Lashley jumping stand and the landing platform, alternations occurred on 28 percent of responses. Then a Y-maze was used, varying the angle of the "Y." There was no difference in tendency to alternate between 15°, 90°, and 120° (T-maze) angles, with the level at about 90 percent for all these rats. This was about the same level as that reported by Wingfield and Dennis (94) for two-trial sessions. When Jackson forced his rats to jump a 15 cm. gap at the end of the arms of the Y in order to get to the food platform, the first few trials showed 78 percent alternation, with this value decreasing to a chance level as the number of trials was increased. These findings are in agreement with the general fact that alternation behavior is *not* a characteristic of behavior of rats in jumping situations. Such facts are not easily explained by any of the explanatory concepts so far advanced.

Despite the exceptions enumerated above, many of the experiments on the avoidance of repetition of responses seems to fall in very well with an explanation such as that used by Hull (47), or with our own re-statement of it. Therefore, reactive inhibition, or negative effects of responding, can be postulated to account for the greater part of the experimental findings enumerated above. These response-produced effects are assumed to fade with time in the manner of the stimulus traces of kinesthetic stimulation. According to such an hypothesis, not only should alternation behavior increase as time between trials is shortened, but also it should increase with increased effort. This follows from Hull's second postulate, given in the introduction of this paper. It is a deduction which has not been tested experimentally and which suggests a program of research. Actually, the demonstration of the relationship between effortfulness of task and avoidance of repetition of responses would be very significant. The effects of *time interval variations* were predicted fairly well by the conceptualizations of Dennis (21), Heathers (40), and Tolman (87). In this case, Hull's theories add little. But the role of the *effort variable* is not a salient feature of the other theories. If it could be demonstrated that work or effort alters

the tendency to avoid repeating responses in a two-choice situation, we would be faced with the necessity of depending on those theories which consider response-produced stimuli, or reactive inhibition, not those which speak of "demands," "negative adaptation," or retention of an impression about "where I went on the last trial." For we have a logical right to suppose that work-increases will augment the amount and intensity of response-produced effects which are inevitably supplied by a given reaction of the organism. Therefore, the effort or work variable becomes crucial in testing the alternative hypotheses.

Refractory Period and Work Decrement

Robinson (73) summarized the principles of fatigue and work decrement which were known in 1934. In general, he reported that continued work was productive of effects which tended to decrease the rate of work done by an organism. These decrements, progressive and of varied duration, were assumed to be derived from two main mechanisms:

1. The chemistry of muscle metabolism progressively decreased the capacity for work during continued work. These chemical changes, the basis for fatigue were often of long duration and effectiveness, existing many minutes, and even hours, after the cessation of work.
2. The action of the nervous system somehow reduced the tendency for a member of the body to respond immediately after a response has been made.

Both of these tendencies, chemical and neural, were believed by Robinson to contribute to the work decrement (or fatigue, objectively defined). From an examination of these two factors we may conclude that, in most cases, the "performance decrement," described by Heathers (40), and the "work decrement," described by Robinson (73) in the field of fatigue, are somehow different. Admittedly, the distinctions are difficult to draw. The essential difference between the concepts appears to lie in the temporal duration of effects of responding. For example, fatigue was assumed by Robinson to involve the gradual accumulation of waste products of metabolism in amounts which make impossible the maximum functioning of the muscle groups or members. These waste products are eliminated over a period of minutes or hours, restoring the full efficiency of the muscles. These phenomena of fatigue have been shown to underlie the "work decrement." On the other hand, the "performance decrement" has been designated by Heathers to be a short-term affair, almost completely over within 120 seconds following a response. It is very probable that we are dealing here with the neural mechanism which Robinson called the "refractory phase."

Robinson included both types of phenomena under the concept of fatigue, since response decrements characterized both. This may or may not prove to be convenient for theoretical purposes. There seems

to be no very clear way of separating the short-term and long-term effects of responding if we measure only overt responses and response decrements. But, at the present stage of our knowledge, we can guess that the long-term work decrements are more truly characteristic of fatigue than are the short-term decrements. We should make it clear that, no matter how precisely we may wish to categorize the two types of phenomena, response-produced effects must be present in both cases, but probably in differing degrees. The fatigued muscle group "aches," while the nonfatigued muscle group seems to put forth less stimulation of a proprioceptive nature. (See Cason, 9, on the organic nature of fatigue.)

Robinson usually characterized the short-term decrements as "refractory phase" phenomena, to distinguish them from the long-term fatigue effects. In doing so, he clearly showed that he was well aware of the neural basis of such decrements. Dodge's (24) work influenced Robinson a great deal in this problem. Dodge, in 1913, found that there was a gradual decrease in the incidence of occurrence of the protective wink reflex as the time interval between stimulations was decreased. This was certainly a short-term "performance decrement," using Heathers' terminology. Dodge (25) held the opinion that neural refractory phase principles applied to the grosser and more elaborate mental processes, and so he used such principles to explain short-term response decrements or work decrements.

Among the experiments most often cited to illustrate the "law of refractory phase" are those of Thorndike (86) in 1927. Thorndike had subjects write a number every time they heard a word, with words being presented every 2.5 seconds. The chances of repetition in a series of ten such numerical responses was one-tenth. The results actually obtained showed a much lower fraction. Using a five-second interval between word presentations, the fraction of repeated numerals increased but this value never reached one-tenth. Dodge and Thorndike disagreed on the interpretation of these results. Dodge (26) maintained that they exemplified the operation of a refractory phase law; but Thorndike (86) felt that his subjects were verbally appraising the problem they were in, and that they were convinced that they were "smart" to avoid being repetitious. The decrease in number of repetitions with the decreased time interval seemed to support Dodge's interpretation. But in 1931, Luh (63) supported Thorndike in a review of the problem of "avoidance of repetitive responses." Luh repeated Thorndike's experiment, using three types of stimuli. They were: taps on the table at regular intervals of 1, 2, 4, and 6 seconds; irregularly presented taps on the table; and letters of the alphabet, presented at regular intervals. Luh found that the tendency to avoid repetitions was clear in five of his six subjects. The sixth subject had a definite repetitive tendency. All subjects showed tendencies for ascending and descending numerical sequences

which tended to produce non-repetitive responses. Luh concluded that ". . . the refractory phase in its strict physiological sense cannot be a plausible explanation for the avoidance of repetition . . ." (63, p. 279). He decided that verbal sets of hypotheses determined the results, thus agreeing with Thorndike.

In 1931 Telford (84) made an extensive study of the "refractory phase law" in simple reaction time, accuracy of judgment, and the general tendency to repeat actions. In a study of auditory reaction time, using $\frac{1}{2}$ sec., 1 sec., 2 sec., and 4 sec. time intervals between successive stimulations, Telford found a relative refractory phase, a super-normal phase (at about one second), followed by a lengthening reaction time. This was considered to be analogous to the functioning of single nerve fibers. When making successive judgments of linear magnitudes, Telford's subjects showed better accuracy as the time interval was increased from one-half second to four seconds. When asked to write a number from one to ten when they heard a nonsense syllable, Telford's subjects showed more repetitive sequences for the longer time intervals between stimuli.

Robinson (73, p. 607) believed that such evidence pointed to a more fundamental mechanism than merely the set, or thought, on the part of the subjects that it was much smarter to avoid repeating numerical answers. Robinson was careful to note that the "refractory phase" principle was a work decrement principle as well as a principle which tended to minimize work decrement by preventing successive responses of an identical nature. Robinson cited several experiments which showed the work decrement to behave in a way analogous to the refractory phase phenomenon. These experiments, too numerous to be described here, led him to propose a set of "principles of the work decrement." They were:

1. The work decrement of a given S-R (stimulation-response) connection is relative to the recency of the previous functioning of that connection (70, p. 605).
2. The work decrement of a given S-R connection is relative to the frequency of the previous functioning of that connection (73, p. 609).
3. The work decrement of a given S-R connection is relative to the connections existing between that S and other R's. This is the principle of competition (73, p. 614).
4. The work decrement of a given S-R connection is relative to the strength of that specific connection (73, p. 616).
5. The work decrement of a given S-R connection is relative to the qualitative integrity of the S throughout the work period during which the decrement develops (73, p. 617).
6. The work decrement of a given S-R connection is relative to the quantitative constancy of the S throughout the work period during which the decrement develops (73, p. 618).
7. The work decrement of a given S-R connection is relative to the decre-

ments that have developed in other S-R connections (73, p. 618). (This is a principle of transfer.)

A glance at these seven "principles of work decrement" reveals what is, at first thought, a surprising omission. The *magnitude* of the decrement of an S-R connection *bears no relation to the work required to respond*. The omission of such a relation seems understandable to us, however, when we look back to Robinson's conception of the work decrement. He considered it to be a refractory phase phenomenon. He did not look on it as a function of response-produced stimulation, or transitory inhibition, and thus was led to ignore the effort variable in refractory phase phenomena.

All the phenomena of the work decrement, reviewed by Robinson (73), can be explained within the framework of Hull's reactive inhibition postulates, or within our own restatement in terms of response-produced drive stimuli. The problem of refractory phase duration, and its relation to effort, has not been systematically explored.

Psychophysical Judgments

We have mentioned that Telford (84) found that judgments of linear magnitudes were more accurate with longer time intervals between judgments. This was probably not first-rate evidence for Hull's theory because many other factors, such as eye-movements for making comparisons, involved some duration. This would make judgments at $\frac{1}{2}$ sec. intervals very difficult. We can, however, appeal to another sort of evidence to show the generality of both Hull's postulates and restatements of them.

In 1920, Fernberger (29) showed that there are important "contrast effects" taking place within a series of judgments about weights. Using the method of constant stimuli, Fernberger found that, if a comparison stimulus which had just been judged as the lighter of two weights was paired again with a weight in the region of uncertainty, there was a strong tendency to judge that comparison stimulus as heavier. Fernberger considered this to be a contrast effect. But such a contrast effect seems to be analogous to the effects noted in the rats' alternation in a T-maze. If we consider that the rat is "judging" the path last run as the longer of two paths, since the rats' problem is an insoluble distance discrimination which may be considered similar to the uncertainty interval in weight discrimination, then the rats' alternation is like the "contrast effect." Irwin and Preston (56) have reported avoidance of repetition of judgments in difficult discriminations with human subjects. These authors found that the influences producing such behavior were below the level of "conscious" report. Such results are to be expected on the basis of the reactive inhibition postulates, though Hull has not yet extended his survey of experimental evidence that far.

"E.S.P." Experiments

The data on psychophysical judgments indicated a tendency to avoid repetition of responses, a tendency demonstrated by the rat in the maze, as well as by human subjects in number-word association experiments. Presumably, we should find the same tendency in a series of guesses by subjects in a so-called "E.S.P." experiment, since a series of "guesses" is not far removed in kind from responses in the uncertainty interval in weight judgments, or from responses in the T-maze where the distance discrimination is insoluble. Workers in the field of "E.S.P." have not taken this tendency into account in analyzing their data. Similarly, many of the critics of "E.S.P." experiments have overlooked the avoidance of repetition of responses. For example, Goodfellow (36), in presenting the data from the Zenith Radio "E.S.P." experiment, concluded that the tendency to avoid "symmetry" of responses explained the kinds of response patterns made most often. The Zenith Radio Program had "sent" from the radio studio series of "messages," by "telepathy," to the listening radio audience. The "messages" were several series of five items, involving two possible choices of response, such as heads or tails, white or black, etc. The guesses of twenty thousand subjects in the listening radio audience were accurate far above chance expectations. Goodfellow exhaustively analyzed over one million responses from the audience and concluded that two extra-chance factors explained the obtained better-than-chance accuracy of guesses. First, the subjects used certain sequences of responses which tended to avoid symmetry. Secondly, subtle suggestions were found in the radio announcer's instructions. Goodfellow stated: "When these factors are discounted, it becomes unnecessary to postulate telepathy to explain the results" (36, p. 632).

Skinner (77) took exception to Goodfellow's analysis. Skinner wrote: "Goodfellow notes the fact that subsequent guesses depend upon the preceding but dismisses the relation apparently as too complex for analysis" (77, p. 495). Skinner proceeded to show that the patterns of response made by the Zenith Radio audience demonstrated a marked tendency to alternate guesses, *to avoid repeating guesses*. This tendency operated to make many of the audience's responses either far above or far below chance expectations. Skinner therefore has explained the unusual kind of distribution of successes and failures recorded by Goodfellow from the Zenith Radio program. The fact that these extremes of success and failure added up to a level of success above chance was probably due to the fact that the sequences "sent" from the radio studio fitted certain tendencies of the audience to respond with a specific alternative on the first trial. For example, on the first guesses, 58.5% of the responses were "heads" for the heads-tails series. If the "message" were started with "heads," then the results would be expected to be above chance for large numbers of subjects. The tendency to al-

ternate choices was not explained by Skinner. He was interested in showing that there are great advantages to *an analysis of responses on the basis of previous responses*, a procedure which higher-level, perceptual principles fail to require. Skinner pointed out that greater descriptive power resides in his lower-level analysis. Goodfellow was looking for configurations and so overlooked some of the relations existing between successive responses.

Skinner did not go beyond stating that there was a tendency to alternate choices in such an insoluble problem as an "E.S.P." experiment. Possibly Hull's theory could be extended to encompass this type of data. If so, then we would expect more avoidance of repetition of guesses as the time interval between guesses is decreased; furthermore, if the effort involved in making a guess-response is increased, we would expect the number of alternations to increase in the two-choice situation. At least, these are the deductions from Hull's postulates, when we extend them. They suggest many experiments.

A summary of this section of our history is straightforward. In two-choice situations, where the response tendencies toward each choice are the same, due either to previous conditioning or to limitations in powers of discrimination, organisms tend to avoid repeating a recently-made choice response. Whether we observe a rat in a T-maze, or human subjects in psychophysical judgments, number-word association series or E.S.P. experiments, the conclusion appears to be the same. Such a tendency seems to be explained by postulates like those proposed by Hull or by a restatement of Hull's principles in more peripheral terms. Several testable deductions, involving the time interval variable and effort variable, appear to imply experiments similar to those described in this section.

EXPERIMENTS ON THE ROLE OF KINESTHESIS IN THE CONTROL OF BEHAVIOR

Since, in our restatement of Hull's reactive inhibition postulates, we have talked of cue stimulation as one of the results of responding, a discussion of the role of kinesthesia in the control of behavior becomes appropriate. As in the case of negative drive stimulation, it is reasonable to assume that the cues derived from responding are in some manner related to the effortfulness of the responses. Thus, with reference to our conceptual scheme, work or effort becomes an important measurable variable in experiments which were originally designed to show the role of kinesthesia in the stimulus-neural control of behavior.

Behavior in the maze. The literature concerning the kinesthetic control of maze performance in animals has been voluminous:

1. After experiments involving *surgical interference with exteroceptors*, Watson (91) decided that kinesthetic and intra-organic stimulation were essential for ordinary maze learning. But the results of Casper (10) and Honzik (42) indicated that rats were not dependent upon proprioception, and could not learn simple mazes on the basis of kinesthesia alone.

2. In experiments involving *manipulation of intra-maze and extra-maze stimulation*, the results are also conflicting. Carr and Watson (8), Dashiell and Helms (16), Shepard (76), and more recently, Hunter (54) have all offered evidence that maze proficiency could be acquired on the basis of kinesthesia alone. On the other hand the experiments of Carr (7), Dennis (18, 19), Macfarlane (65), Curtis (15), Honzik (41) and others have shown rats to be incapable of learning mazes when all extra-maze and intra-maze differential cues were well controlled.

3. In experiments involving *surgical interference in spinal cord afferent tracts* in the dorsal columns, the results were also contradictory. Lashley and Ball (59), Ingebritsen (55), and others have claimed that rats could learn mazes after their dorsal columns had been cut, presumably eliminating response-produced afferent impulses. But such a conclusion has been criticized by many, including Hunter (53), Honzik (42), Sugar and Gerard (82), and others, mainly because it could be demonstrated behaviorally that other proprioceptively-controlled acts could be performed after such spinal operations. It appeared that many tracts outside the dorsal columns could serve to carry proprioceptive impulses to the brain. The physiology of proprioception is far from being clearcut, making interpretations very difficult.

4. In experiments involving *surgical interference with higher levels of the central nervous system*, Lashley and McCarthy (60) have shown that rats which were deprived of cerebellar centers could literally stagger through a previously learned maze after the operation without making errors. External cues must have been operating, since response-produced stimuli should have been greatly altered by the staggering gait of the post-operate rats. Dorcus and Gray (27) agreed with Lashley and McCarthy. They excised whole muscle groups in the hind limbs of rats, and found maze learning to be undisturbed. Actually, neither of these experiments was conclusive, due to the possibilities of extra-maze and intra-maze *exteroceptive* stimulation of a differential nature.

Kinesthetic discriminations. There exist several examples of discrimination learning on the basis of kinesthetic or response-produced cues alone. Ruch (74) found a small DL in rats for the discrimination of inclined-plane angles. Two blind rats had a DL of only 10 degrees. In a later experiment, Ruch used 23 blind rats and found DL's from four to two degrees. He concluded that his rats possessed a high degree of kinesthetic sensitivity. Dashiell and Helms (16) obtained equivocal results in a kinesthetic discrimination problem in which rats were required to take the path (one of four arranged in the form of a cross) which was inclined most. In one sense, the long versus short path experiments of De Camp (17), Kuo (58), Gengerelli (33) and Grice (38)

may be considered to have been successful kinesthetic discriminations if we ignore the temporal delay factor. Ghiselli (35) offered inconclusive evidence on the mechanism underlying kinesthetic discriminations. He used the inclined-plane discrimination problem, with rats whose dorsal columns had been cut at the cervical level. The operates learned the discrimination as readily as did normals. Apparently the appropriate cues were getting by the spinal lesions, or other cues were being used. In general, the evidence indicates that rats can make discriminations on the basis of kinesthesia; though it is disappointing that a very interesting test, the elimination of the afferent pathways of kinesthesia, has not yet given us one of the limiting conditions of kinesthetic control.

An experiment of Van Vorst, Stone and Kuznets (89) is of interest here, because it combined in a unique fashion the problem of kinesthetic discrimination and kinesthetic cues used in learning a maze. These experimenters used a maze whose pathways led in three dimensions. That is, some of the pathways were inclined. Van Vorst *et al.* compared the performance of rats on two mazes of identical pattern, one of which had inclined plane alleys in certain fixed places, the other of which was level throughout the whole maze. Therefore, the sequences of left and right turns were the same for both mazes, but one maze offered added kinesthetic stimulation by the inclusion of inclined pathways. Two groups of rats were run on each maze. The groups of rats which ran on the inclined plane maze learned more rapidly than the groups which ran on the traditional, level maze. The authors concluded: "No evidence has been adduced to show precisely in what way the factor of inclination of pathway caused more rapid learning" (89, p. 347). However, it seems fairly clear that the introduction of more intense kinesthetic stimulation, in the case of the maze with inclined paths, served to offer more discriminable cue stimuli for learning the maze.

Distance discriminations. In 1942, Brown (4) selected a maze problem which was a purely kinesthetic one. Rats were required to run a certain constant distance on a maze straightaway before taking a 90 degree turn into a side alley. There were several such side alleys, perpendicular to the straightaway and equidistant from each other. Only one side alley led to a food reward, and that alley was always the same distance from the starting point of the maze. Partial spinal lesions were made in 54 rats, and these animals learned the problem to the same degree of accuracy as did 43 normal rats. Brown believed that there were nerve tracts outside of the dorsal columns which served kinesthesia in his rats. Previously, Crutchfield (14) had used somewhat the same

type of maze and found that the solution to this distance discrimination problem was a statistical matter. A relatively normal curve of incidence of choices, with a mode at one alley anticipatory to the correct side alley, was found to be the only evidence of learning. None of the animals attained 100 percent accuracy. Crutchfield found that increasing the hunger drive by food deprivation would shift the mode toward the correct side alley, farther along the straightaway. He believed that the discrimination was made on the basis of the expenditure of "psychological potential" an intervening variable conceived in order to explain the results of his particular experiment. Energy-expenditure cues were considered to be counteracted by increased positive motivation resulting from food deprivation.

Crutchfield discussed the interaction between the two factors in terms of Lewin's (62) type of vectorial analysis. Crutchfield assumed that two kinds of drive stimuli are capable of neutralizing one another. He concluded that "psychological distance" is partly a function of "psychological need." In terms of our own constructs and terminology, increasing the energy requirement should act to bring about a shift of the mode of responses, in the distance discrimination maze, toward the starting point. This suggests an interesting opportunity for experimentation.

Another experiment concerning the discrimination of distances by rats was that of Stellar, Morgan, and Yarosh (81). These workers used essentially the same situation as did Crutchfield (14) and Brown (3). After their rats had shown a reasonable degree of learning (similar to the distribution of choices reported by Crutchfield and with a mode near the correct side alley), they were subjected to operations in which cortical lesions were made. These lesions were varied, and when taken together as a "composite" covered most of the areas of the cortex. The only type of lesion which seemed to affect seriously the distance discrimination was that involving the frontal areas of the cortex. The authors concluded that normal rats performed the distance discrimination on the basis of the "symbolic process" postulated by Hunter (52) in order to explain the delayed reaction and double alternation behavior. According to the definition of the symbolic process given by Hunter, this process is a substitute for missing stimulation, probably involving substitute responses, and is capable of being recalled at an appropriate time. Providing that the distance discrimination involves a temporal delay factor, the symbolic process may be legitimately invoked; and to the extent that the rats use surrogate responses such as "counting,"

the "symbolic process" may be invoked without violating the definition of the process. But inasmuch as Stellar, Morgan and Yarosh found that the distance discrimination was neither based on constant time intervals between start and choice, nor based on "counting" the side alleys, it would seem that the "symbolic process" was not appropriately applied as an explanatory concept. Probably the traditional association of the "symbolic process" with the frontal areas of the cortex was an important factor in the reasoning of these experimenters. But there was no conclusive evidence in their experiment which would indicate that the lesions in the frontal areas had interrupted the operation of the symbolic process. Rather, we would argue that response-produced stimulation served as the cue for the correct responses, and that somehow the frontal area lesions served to disrupt the normal operation of such effects. In this way we confine ourselves to variables which are peripheral and more susceptible of independent measurement.

SUMMARY

The four categories of experimentation which have composed this history of the literature on response-produced effects have been unified by one central theoretical idea: we have assumed that response-produced effects (kinesthesia or proprioception) can serve both as *drive*, or motivating stimulation, and as *cue* stimulation. Since the work factor is assumed to influence the intensity and extent of such effects, the experimental problem becomes clear. First, we must find out empirically whether variations in effort produce variations in responses which are indicative of *negative motivation* in operation; and secondly, we must find out whether variations in effort produce variations in behavior which are indicative of changes in *cue stimulation* or *conditioned stimulation*. Most of the experiments cited in this review have contributed in one way or another to our understanding of such empirical relationships. The general conclusions may be summarized as follows:

1. In the experimentation on the *law of least effort*, or law of less work, it has been shown that work acts to produce negative motivation, providing a basis for discrimination between two distances to a goal.
2. In studies which have varied effort-per-unit-of-time in conditioning and learning, we have seen that the problem of massed versus distributed practice is, in reality, two problems: the problem of temporal spacing and the problem of effortfulness of responses. It was pointed out that there are many factual gaps, suggesting new programs for research, with reference to this kind of problem.
3. In experiments on the avoidance of repetition of responses, it has been shown that temporal spacing is crucial. Analyzed as a problem of distribution

of effort, new experiments were suggested, experiments which were designed to test the role of effortfulness in the production of non-repeated-responses. It was pointed out that the phenomenon of avoidance of repetition of responses could be found in many types of experimental situation, with human beings and with animals.

4. In studies on the role of response-produced, kinesthetic stimulation in the control of behavior, it was shown that effort or work could operate to produce cue or conditioned stimulation in the performance of several types of discriminations.

Thus, in addition to the presentation of empirical data which are relevant to the central problem of this review, we have tried to suggest lines of research which might help to fill in some of the factual gaps in the theoretical frames of reference which we have discussed at some length. Such suggestions, it should be pointed out, were with reference to psychological research, on a behavioral level.

However, in the introductory section of this survey it was pointed out that the use of peripheral constructs would encourage interdisciplinary research, as well as strictly behavioral research. What might some of these lines of research be? The simplest, and most obvious starting place is the physiology of the kinesthetic system. If, as has been postulated, the drive and cue effects of work or effort are mediated by this system, then alterations in the physiology of this system, either by means of extirpations or drugs, should result in definite changes in the relationship of work to the various categories of phenomena discussed in this survey. For example, if increased effortfulness of task results in more rapid extinction rates under normal conditions, there should be an alteration of this relationship when the functioning of the kinesthetic or proprioceptive system has been changed. The possibilities for such inter-disciplinary experimentation are believed to be numerous, and are believed to be important for a more complete understanding of the influence of work on behavior. Perhaps at some later date the behavioral phenomena associated with work or effort will be closely tied to the physiology of the kinesthetic system. At that time we shall be able to measure directly the peripheral constructs used in our theoretical discussion; the constructs will then no longer be logically-constructed intervening variables: they will be directly quantifiable, experimental variables. Then we shall have to move into the central nervous system for the establishment of new intervening variables, encouraging both behavioral and physiological experimentation, at a higher level of complexity. At least, such is the direction implied by the type of theory we have used in our analysis of the influences of work on behavior.

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