THE POTENTIALITY FOR CHANGE OF AN EXPECTANCY
AS A FUNCTION OF THE AMOUNT OF EXPERIENCE

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ROBERT ALFRED GOOD, B.S., M.A.

OHIO STATE UNIVERSITY

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Approved by:

Julian B. Potter
Adviser
To:

Melvin, Margaret, and Jeanne

for many reasons
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I. INTRODUCTION

The fact that individuals have predispositions to situations which they encounter is readily noted by the teacher who meets with a group of children during their first day of formal schooling. She observes differences in individual reactions to this novel situation and credits them, usually, to the prior training and preparation for school which the children have received. Children having similar reactions at this time, however, demonstrate differing rates and degrees of adaptation in the hours and days following. The differences in rate of change of overt behavior may be thought of as correlated with differences in changes more centrally construed.

The construct, which is presented here as possessing utility for explaining such phenomena, is expectancy. The need for constructs capable of handling complex events is indicated by the increasing popularity of intervening variables. Expectancy, when defined as an intervening variable, need have nothing of the mystic and appears to fulfill the intermediate position between the very basic constructs of contiguity, frequency, and primary need.
reduction, and constructs of the breadth of, possibly, frustration.

That a construct of expectancy has a potentiality for research purposes is hardly debatable. The weight of research under such a heading is growing impressively. As with any new approach, however, considerations accrue which were not apparent previously and now demand additional exploration. Some basic research is needed before the full utility of the construct can be realized. The techniques and means by which such basic research can be accomplished must be devised and tested for suitability in their own right. A primary consideration is the matter of measurement.

One test of the merit of any construct is the degree to which it is expressible in quantifiable terms. The convenience of numbers for communication, description, and testing makes quantification a necessary attribute of any construct. The difficulties involved in psychological measurement, however, are so great as to persuade many psychologists to use constructs which imply amount without presenting it explicitly. Examples of such utilization are frequently encountered in psychological literature, especially that portion referring to expectancy and level of aspiration.

"Levels" of expectancy and aspiration seem to imply a cross-sectional measurement which is limited because it gives no consideration to changes through time. While one numerical expression of
expectancy may seem equivalent to another numerical expression of expectancy, some factor should be expressed which presents historical information. This would enable differences to appear between presumably identical "levels" and would make changes in these "levels" more predictable.

It is this lack of refinement of the "level" approach which makes most of the vast body of level of aspiration data inapplicable with regard to the problem herein presented. The initial intent of most of the work was to find a reliable single measure of an individual's expectancy in many broad areas. This has proven to be an overly optimistic aim and the differences in experimental design prevent unification of the separate findings. Suggestions for improving methodology are given and incorporated into the present design.

The level of aspiration research has been performed, largely, by clinical and social psychologists. Research and theory concerning expectancy as such, have been produced by general and theoretical psychologists. This group has also been the one most interested in determining basic units for study in psychology and in analyzing the ways in which these units combine or fail to combine. Although the specific development of an expectancy has not been studied, hints may be gathered as to the probable form which such a development might take as a result of the work involving the formation of conditioned responses and similar connections. The present investigation is based on a consistent system
of psychological constructs and excludes the physiological orientation implied by constructs such as maturation and growth. As a result, those areas of general psychology are given no consideration.

To return more specifically to the problem at hand, the description of expectancy as an intervening variable implies its relationship to antecedent and consequent conditions. The position is, thus, that the historical conditions of which an expectancy is the outcome will prove important in determining the characteristics of that expectancy. Although no psychologist will deny that, in the present state of knowledge, information as to the historical development of a particular process will aid in predicting future trends of that process, sufficient emphasis has been given to the ahistoric, cross-sectional approach to justify a specific statement of a historical position. It is intended that a historical analysis will illuminate factors operating in the formation of an expectancy and serve to "explain" the degree of stabilization such an expectancy achieves.

This exposition has, thus far, been concerned with the formation of expectancies in general. Such findings as are derived should serve to give information regarding differences between individuals in expectancy. This applicability could also be extended to cover to explain differences in expectancies within the same individual. The question would be phrased as, "Why is individual
A so flexible with regard to his aspirations in this field and yet so rigid with regard to that field?" This type of information would be of great value to the clinician who faces the perplexing problems inherent in therapy.

The purpose of this study, then, is to investigate one of the conditions which is presumed to operate to determine the height of an expectancy and the modifiability of an expectancy. The study will not be all-inclusive in stating the determining factors of expectancies because the complex of relevant factors is obviously too great. It should serve to afford the opportunity for the rejection or tentative acceptance of certain hypotheses and further development of new ones.

With this stated objective, then, it was decided to attempt to create a meaningful situation in which people would be able to formulate quantifiable expectancies. These expectancies would be based on amounts of experience which could be varied for each of the groups, although these amounts of experience would be so controlled as to produce relatively uniform levels of expectancy at the end of the pretraining. After the initial expectancies had been stated, the subjects would be given an opportunity to modify them as a result of a single experience different from those previously encountered and as "psychologically equal" for the individuals as possible. The comparable
units are the differences between the two stated expectancies for each individual.
II. BACKGROUND

A. Contributions from the Level of Aspiration Studies

The concept of "level of aspiration," introduced by Dembo (published in 1931), made explicit the possibility of observing goal levels occurring in the course of relatively specific activity, designating some of the factors associated with fluctuations of such goals and linking the experimentally observed manifestations of goal-striving to the individual's behavior in other situations. (40, p. 333)

From this beginning, a vast body of research developed. The emphasis in the earlier attempts was to find a "typical" level of aspiration for the individual, but consistency from task to task was not found.

In line with Lewinian theory, the accent throughout the level of aspiration data is upon the achievement of goals. If all behavior is regarded as goal-directed, then something akin to a level of aspiration should operate in all situations. Hoppe seems to have regarded the construct in this fashion (40, p. 347). As the construct of expectancy is used herein, it appears that a level of aspiration represents a type of expectancy measure, but that the constructs are overlapping and not congruent. As a consequence, this portion of the paper will show methodological influence resulting from level of aspiration work as considered in the present experiment, but will neglect the findings specific
to level of aspiration.

Rotter (72)(73) indicates the need for more strict control of the experimental design in order to permit comparisons of results. He points out the need for controlling previous experience prior to the statement of an expectancy, the potentiality for inserting controlled success or failure experiences, and the comparability of subsequent expectancy statements. The basic design of this experiment follows that outline. He also predicted the tendency of individuals to aspire toward the middle of a range when they have no actual knowledge of how well they can do with regard to the range of scores for the particular task. Slight refinements induced by knowledge of the preliminary results of experimentation by Dean (11) enabled this experimenter to achieve reasonable uniformity in initial expectancies through the consideration of this tendency.

In the selection of subjects, the findings that subjects with a long history of failure in a task differ in height and spread of scores was considered. Control was attempted through the use of an extensive sample and the restriction of participation to those who would not be expected to have an unusual amount of success or failure experience in their personal histories.

Controlling experience demands that the scores be pre-arranged. This has been done in studies by Gardner (19)(20) and
Hilgard (36), and their use of time as a "basis" for scoring seemed to be a simple technique for convincing falsification. The scores had to vary for realism, as suggested by Hertzman and Festinger (34), and this leads immediately to sequence effects. These effects were not desired in the present experiment, but, since they were unavoidable, the amount of variation from trial to trial was minimized.

The consideration as to whether to interpose a strong positive or negative reinforcement was determined by the practical matter of obtaining cooperation by the schools, maintenance of good public relations, and finishing the experiment in a way satisfying to the subject. Klugman (48) notes a tendency on the part of subjects to attempt to guarantee success for themselves on the last trial in order to "avoid placing oneself relatively permanently in the area of failure." Although this appears to be an overstatement of the importance of the experiment to the average subject, it is undoubtedly true that an activity which ends in failure leaves one with a fairly persistent negative reaction. The positive reinforcement was chosen.

When the problem of interpretation of results arises, the constructs from level of aspiration vary. Hoppe's early writings spoke of "scales of reality." He stated that subjects did not respond to achievement as meaningful, or failure as disturbing,
when the standards appeared to deviate too greatly from a central position. He defined a realistic person as one who kept his expectancies and aspirations in line with what he had a reasonable hope of achieving. This apparently redundant statement meant that one who was able to cope adequately with the reality of the level of aspiration situation was most likely to be able to do so in situations of everyday life. This is in agreement with the current thinking of tests as behavior samples.

Frank (16)(17) and Chapman and Volkmann (8) prefer a concept of "frame of reference." Although the concept enjoyed a wide vogue, its experimental value was limited by its lack of quantitative formulation. Helson, as will be indicated, takes particular exception to this concept.

Johnson (46) devised an ingenious experiment in an attempt to provide mathematical data from a frame of reference. The task for his subjects was one involving pitching pennies on the floor. He had his subjects draw lines on the floor to fit their aspirations. The distances of these lines from the wall constituted the data. This type of experiment appears to have possibilities for elaboration.

Helson (32) started with a "frame of reference" concept, but recognized the weaknesses inherent in it. In discussing the matter of subjective norms, he states in criticism of Sherif, "but a norm which is 'subjective' and 'peculiar' to the subject can
hardly be said to satisfy the requirements for a scientific definition..." (32, p. 297) He was able to derive a mathematical statement of expectancy which he termed the 'adaptation level'. This term expresses the orientation toward psychophysical and perceptual studies which the originator has and the emphasis seems to be upon what the subject will see or feel, literally. No effort is made to structure the term so as to predict behavior where an almost unlimited variety is available to the person, but it should be noted that the intent was different, originally, so that this need not constitute a valid criticism.

Some of the level of aspiration writers speak of "probability gradients" which are subjectives and developed through experience by the individual. This construct seems to have a connotation of statistical approachability which makes it more acceptable to general psychologists (6). The goal of eventual objectification which is implied seems worthwhile.

The results of level of aspiration research are not considered to be, in the main, comparable to the present research. The intent was generally different and the types of tasks, controls, and interpretations varied accordingly. Consequently, these relatively brief notes serve to indicate such linkages as appeared pertinent.

B. The Theoretical Development of the Construct of Expectancy

Whether any completely novel construct ever appears in psychology is debatable. There are few people who will disagree
with the statement that new ideas are partially a function of the "old" ideas prevalent when the new ones were engendered. A consideration of positions taken with regard to the construct of expectancy should be useful in establishing its place in modern psychology, as well as in evaluating the future position it may be expected to attain. It seems wise, therefore, to present the viewpoints of prominent theoreticians with regard to this construct and some of the experimental evidence upon which the viewpoints were based.

Edward Chace Tolman (75), in his book Purposive Behavior in Animals and Men, took a position in sharp distinction to the current Watsonian behaviorism and based it largely upon a construct of expectancy. He proposed a different level of abstraction of behavior and emphasized the fact that he was concerned with molar acts rather than specific behaviors. This proposal was determined by his observations that "The first characteristic of behavior is its goal getting-to, or getting-from qualities." (75, p. 10) His basic units were not the S-R bonds of Thorndike (73)(74) or, later, Hull (35)(37), but were "means-end-readinesses," or "sign-gestalt-expectations." In describing the nature of these units, he said that "means-end-readinesses are of the nature of innate generalized sets, resulting from previous learning, or provided vaguely by innate endowment.... He is provided innately with certain means-
end-readinesses (judgments) and these become refined and specified through experience." (75, p. 34)

At approximately the same time that Tolman published his book, others were expressing dissatisfaction with the types of constructs being used to explain behavior, usually as derived from conditioning experiments. Dodge, in 1933, stated that "Thurstone rightly held that anticipatory reaction is a real, but strangely neglected indicator of intelligence. A large part, if not the largest part of a rational person's life is spent in planning for events that may not occur." (12, p. 198) Dodge did not follow his formulation through to its full ramification that all of a person's behavior is based on a prediction. He attempted to explain the phenomenon of anticipatory reaction in terms of neurological constructs.

Zener (84), on the basis of his work in conditioning dogs, was impressed with the directionality and appropriateness of the behavior. His observations of the forms which conditioned responses could assume made explanations of conditioning, as instances of substitution of the conditioned stimulus for the unconditioned stimulus, unsatisfactory. Such explanations would require that the response remain constant in the two conditions. He states, "It is clear that the behavior that occurs to the conditioned stimulus is not the complex of movement that occurred to the
unconditioned stimulus." Deviations in behavior are appropriate not to the presence of food, but to the expectation that it will appear (84, p. 393). This finding, however, was integrated by Zener into his "sign-urge" conception of behavior in which particular tension systems are released, rather than into an expectancy system for which it seems fitted.

Mowrer (62) was led to a variation of expectancy theory through a conditioning experiment with human subjects. Using a light as a conditioned stimulus and a shock as an unconditioned stimulus, he was surprised to discover that he got heightened galvanic skin responses, his response measures, whenever the light was turned on while the electrodes were in place, and without learning trials. This led him to state that, "This finding of the present investigation would seem to point to the conclusion that the mere frequency with which a given response is elicited by an unconditioned stimulus, paired with an originally neutral (secondary) stimulus, is not related in any very direct or important way to the establishment or persistence of new stimulus-response relationships." (62, p. 63) Even after conditioning, the removal of the electrodes removed the conditioned response. This was interpreted by Mowrer as proving that the response was dependent on a state of expectancy, or set, and was not "true" learning.

Mowrer, like Tolman, did not accept expectancy as a strictly learned phenomenon. Unlike Tolman, who seemed to consider expect-
ancies as largely innate and did not define their character fur-
ther, Mowrer described them as "due to a basic tendency on the
part of every reaction system once activated, to show, following
a relatively brief period of refractoriness, a more prolonged per-
iod of increased excitability." (62, p. 73) He recognizes the
similarity of his construct of expectancy and Tolman's "sign-gestalt-
expectation," but appears to go further by giving preparatory set,
or anticipatory tension the capability of functioning as a drive
and, thus, serving to extend the law of effect to conditioned de-
defense reactions (62, p. 74). This has led to fruitful research
and observations. An example of the types of observations he makes
is illustrated in his discussion of the experiments where subjects
"feel" shocks which are not given. He concludes, "The foregoing
considerations indicate that expectancy is an important determiner
not only in what individuals do, but also in what they sense." (62,
p. 83)

Brunswik (6), who agreed with Tolman's position regarding
expectancy, designed an experiment to test mutual hypotheses. Using
groups of rats which were rewarded on the two sides of a choice sit-
uation different proportions of times, he found that discrimination
followed the degree of difference in the ratios of reward for the
two sides. The differences in discrimination were much increased
when the unrewarded choices were given punishment. This led Brunswik
to a conception of expectancy which is somewhat actuarial. It is
worthy of note that he found, in this experiment, the repeated re-
versal of training type 100/0 with one group resulting in increasing
promptness of adjustment. This appears to anticipate later illumin-
ating studies by Harlow (30) and Held (66) in the development of
what might be termed "expectancy for change."

Clark Hull occupies a unique position with regard to the
theoretical framework upon which the present research is based. He
is the chief proponent of a construct of reinforcement. He has had
probably the greatest number of studies which attempt to show the
interrelationships of fundamental learning variables, and the relative
importance of these variables, done under his aegis. At the same
time, he is one of the chief critics of an expectancy construct. The
present study is in the peculiar position of being a somewhat Hullian
type of design for the investigation of a construct which he specif-
ically rejects.

The modifications produced through learning, according to Hull,
consist of changes in sHr, habit strength, through increments pro-
duced by the reinforcement in reducing primary need. This is accom-
plished through repeated runnings of the stimulus-behavior-reinforce-
ment sequence. Classical experiments illustrative of the principles
upon which this system is based are those of Williams (86) and Perin
(64). These similar experiments reveal gradients of resistance to
extinction of a habit in agreement with the predictions. A negatively
accelerated growth curve for strength of a habit results as a function of the number of reinforcements. These results bear a similarity to the predictions in this paper.

The adequacy of the constructs used by Williams and Perin and the significance of their results were questioned by others as a result of the experiments by Humphreys (38)(39). These experiments revealed that the sheer amount of reinforcement was an insufficient factor to explain the persistence of a response under conditions of partial reinforcement. The explanation which Humphreys presented, as a result of his discovery that partial reinforcement created a greater resistance to the extinction of a habit than did continuous, was that this difference was "due to a greater difficulty in forming an expectancy of continuous non-reinforcement." (39, p. 301) His definition of the construct of expectancy, however, was such as to make the criticisms leveled by Hull (37) and Stanley and Jenkins (43) seem merited, although they were not referred to him.

Hull stated, "For example, the trouble with such a concept of expectancy, as employed by Hilgard and Lewin, is that when we attempt to verify the hypothesis in which it appears we cannot tell how much expectancy to expect; neither do we know the magnitude of the reaction which the expectancy is expected to mediate."(37, p. 287) This was quickly answered by Lewin, who said that, "But looking over the literature on the Level of Aspiration, Hull would quickly see that reliable quantitative data about 'expectancy' can be obtained, that the conditions for changing 'expectancy' and the 'magnitude of
reaction' can be studied, at least in human beings." (37, p. 289) The validity of the Lewinian position is illustrated in the work of Irwin (42), where the reliability of the expectancy measure is sufficiently great to enable cases not revealing such reliability to be suspected of demonstrating an abnormal state of adjustment in the particular individuals displaying such variability.

Mowrer and Jones (61), attempting a rapprochement in the partial reinforcement issue, were led to give two explanations for the paradox. From experiments with rats, in which groups were given differing ratios of reinforced and non-reinforced trials in a Skinner box, they were convinced of the plausibility of a "response-unit" hypothesis. That is to say, although the experimenter continued to consider each activation of the lever as one response, the rat may consider a response to consist of the three or more activations necessary to produce one food pellet. If a single response is considered as that number of activations necessary to produce one reinforcement, then more responses will appear as the result of continuous reinforcement. This brings to the front the matter of the definition of what constitutes a response and what the meaning of an experiment is to the subject. The arbitrary nature of units of behavior is again affirmed, and the degree of structure which the experimenter imposes is evident.

The second hypothesis which Mowrer and Jones use as a possible explanation is that of discrimination. This hypothesis states
that the organisms are less able to discriminate the extinction trials during partial reinforcement experimental conditions. The conclusion is that discontinuity, rather than irregularity, is the important factor in resistance to extinction. The amount of change between the learning situation and the resistance to extinction situation, wherein the strength of learning is measured, is thus important. The merit of these hypotheses is yet to be determined.

A study which extended the applicability of the findings of Perin and Williams with regard to habit strength, in Hull's system, was that of Hall (29). Hull is fully aware of the limited amount of behavior which can be adequately accounted for in terms of primary reinforcement situations, alone. His constructs of secondary reinforcement and anticipatory goal responses are examples of the means by which he attempts to derive formulations for behavior which go beyond these limits. Hall, in a complex experiment involving numerous variables, investigated the relationship of primary reinforcement and secondary reinforcement. He found that, with an increasing number of primary reinforcements, there was an increasing tendency on the part of his animals to run to the position of the previously reinforced goal box. This was true during learning and extinction trials. The study is relevant in illustrating that a special type of strengthening has taken place during the learning trials. This strengthening is not specific to the primary reinforcement situation, since the strength of a secondary reinforcing agent increases as a
function of the number of times it has been associated with primary reinforcement. A secondary reinforcer may, temporarily, function in the absence of primary reinforcement and extend its influence to situations where primary reinforcement is absent.

It should be noted, in this debate as to the function of continuous reinforcement and in view of the continuing work on partial reinforcement by Grant and others (27), that a simple, overall explanation of the controversy will probably not be forthcoming. Many aspects of the work lend some credence to a concept which requires a portion of continuous reinforcement somewhere in the learning process. For example, Jenkins and Stanley (43, p. 196) indicate that continuous reinforcement is used to get the desired behavior started in most cases. They also point out that continuous reinforcement creates the most rapid response rate in the early stages of extinction, although the response drops out faster. The writer believes that a construct of expectancy can be utilized to achieve a beginning toward a resolution of the opposed positions.

This proposed attempt toward a resolution of the partial reinforcement controversy leans heavily upon work performed by Harlow (30). On the basis of a carefully planned series of experiments with chimpanzees, he explained many experimental results as due to the constant use of naive animals. This degree of naivete is seldom seen in the usual life situations. To quote Harlow directly:

The variety of learning situations that play an important role in determining our basic personality characteristics and in changing some of us into thinking animals are repeated
many times in similar form. The behavior of the human being is not to be understood in terms of the results of single learning situations, but rather in terms of the changes which are affected through multiple, though comparable, learning problems. Our emotional, personal, and intellectual characteristics are not the mere summation of a near infinity of S-R bonds. The learning of primary importance to the primates, at least, is the formation of learning sets; it is the learning how to learn efficiently in the situations the animal frequently encounters. This learning how to learn transforms the organism from a creature that adapts to a changing environment by trial and error to one that adapts by seeming hypothesis and insight. (30, p. 62)

It should not be inferred that Harlow restricts his conclusions to the more traditional learning situations. He expresses the opinion that "We believe that one of the very important factors in the development of the good personalities of our monkeys is the formation of social-emotional learning sets...." (30, p. 63) He also considers his research illustrative of the impossibility of making predictions without knowledge of previous learning experiences which the organism has had.

A readily discernible similarity is evident between the strong statement quoted from Harlow on the basis of his experiments, which have been verified with rats by Reid (66), and an earlier statement from a joint publication by Tolman and Brunswik (78). The latter two write:

Thus the wholly successful organism would be one which brings, innately, normal averagely "good" means-end-hypotheses and normal averagely "reliable" perceptual hypotheses; but which can immediately modify these innate hypotheses to suit the special conditions of special environments; which can note and include in its cue-system and in its means-end
Tolman's position was further bolstered by the initially independent research of Krechevsky (50)(79). On the basis of observations of rats solving discrimination problems, he rejected the idea that any behavior could be thought of as "chance." He chose the word "hypothesis" to describe the pre-solution behavior in order to emphasize the fact that it was systematic, purposive, involved some degree of abstraction, and did not depend entirely upon the immediate environment for its initiation and performance. He felt that some cognizance must be given to the apparent fact that the animal was always behaving in an attempt at adjustment. The particular form of these attempts was thought to originate in part from the organism, itself, on a basis of past experience. The construct involves centrality and directionality in the manner of the means-end-readiness of Tolman and was arrived at independently. The awareness of the men of their similar conclusions led to a joint publication.

A summary of the position of those who regard expectancy as a central construct is given in the following suggestion by Tolman:

All the problems of psychology—not only those of visual perception and learning—but all the more general problems of instinct, insight, learning, intelligence, personality, and emotion all center around this one general feature of the given organisms abilities and tendencies for adjusting to these actual causal textures—these actual probabilities as to casual couplings. (78, p. 75)
It would be an error, however, to imply that events occurring since
the initial presentation of these statements have left them untouched.
Along with the similarities of the Tolman and Harlow statements,
there are certain important differences. Tolman stresses innate char­
acteristics and appears to start his investigations with organisms
at a level showing some degree of sophistication. In any case,
Harlow appears able to include the data from opposing schools as
cases of fundamental learnings. It may be this relegation, by Tolman,
of factors to the innate characteristics of the organism which later
forced him to an admission of the inadequacy of his previous formul­
ations. In contrast to Harlow, who seems to have succeeded in making
opposing views more compatible, Tolman appears to have ignored the
possibility of revising his earlier definitions and postulates mul­
tiple types of learning (76a). Adequate consideration of the types
of learnings which are now presented would require a considerable
exposition. The possibility that at least several of these types
could be included under a revised expectancy construct remains.

Meehl and MacCorquodale (59)(57) have recently evaluated the
construct of expectancy and formulated an approach of their own to
it. They express the opinion that the results of research indicate
the need for systematic work on the expectancy construct to insure
its more rigorous formulation. According to them, the fractional
goal responses used by S-H theorists to explain circumstances where
an expectancy construct might be used are inadequate. They criticize
these "fractional goal responses" because derivations from the construct
tend to be made for special setups, rather than as a general theorem to be filled in. Another criticism is that this construct "is so poorly quantified that it is as readily available as a deus ex machina for non-expectancy theorists as the concepts of 'attention,' 'emphasis,' or 'perceptual threshold' are for Tolman." (59, p. 230)

Their own formulation adds an expectancy factor which specifies what is expected when a response has followed a particular stimulus. This is a "fundamental cognitive unit, the 'expectancy' (S₁RS₂)." (57, p. 2)

While the new formulation by Meehl and MacCorquodale represents an admirable attempt to place such a construct within the framework of traditional constructs, this structuralization will require additional research before an evaluation of its merit is possible. The authors admit to a sympathetic inclination toward some form of S-R-reinforcement theory and what they have done is essentially an elaboration of S-R theory.

This brief summary indicates that many experimenters have felt the need for a construct such as expectancy. Some have already formulated such a construct and the inadequacies of those formulations have been revealed. If expectancy could be considered comparable to what Harlow designates as a "learning set," developed into a complex factor through what are essentially Hullian principles but capable of eventually handling the situations presented by Tolman, some of the more prominent problems in the field of learning would be solved. The ambitiousness of such an objective is realized and immediate, conclusive attainment is not anticipated.
C. Derivation of the Construct within the Framework of Social Learning Theory

In order that the relevance of the present research be understood and its position within the framework of a series of related studies be appreciated, it is essential that something of a developmental picture of Rotter's Social Learning Theory of Personality be presented and accompanied by pertinent research findings. Because of the limitations of this paper, the presentation will be incomplete and biased in the direction of those portions which bear upon the topic.

Social Learning Theory may be described as a "need and availability" theory. That is, in the context of varying need states of the individual, those behaviors are utilized which have become "available" through having occurred and having been reinforced in the individual's past. It is essentially a learning theory and specifically restricts its constructs to the psychological level. Its predictions, or field of application, lies with those behaviors classified as "learned." The field of interest is the interaction of the individual and his meaningful environment. Because of the dominant importance of others as reinforcing agents, it is described as a "social" theory.

Another explanatory construct has been associated with "availability" in efforts to handle the reasons for the predominance of certain types of behaviors over other behaviors possible in any given situation. This is the construct of 'freedom of movement.'
originally defined in terms of the ratio of potential punishment to potential satisfaction of a group of related behaviors directed at a common goal. The child who has only one "pathway" available for recognition from his parents--throwing a temper tantrum--has little freedom of movement because this pathway brings punishment through loss of love and affection. Unless other pathways are made available, the child will persist in some form of this behavior, although the constant punishment may modify the behavior to a degree.

Crandall (10) used the construct of freedom of movement in this form. In his research, freedom of movement was measured by ratings of fantasy productions which revealed anticipated success or failure in particular areas. Series of TAT-like cards were used which were judged to relate to the area of physical skills recognition, academic skills recognition, or love and affection recognition.

Crandall was primarily interested in demonstrating the functionally related aspects of reinforcements, although the use of freedom of movement as an experimental variable was noteworthy. In line with his primary intentions, he had his subjects tell a series of stories in response to one-half of his pictures. This was followed by a failure experience in physical skills which was introduced "accidentally" by another experimenter in a different room. The subject was then returned to Crandall, who took a second series of stories based on pictures matched with the earlier ones. Judged changes in freedom of movement for the three areas revealed a gradient of effect.
The maximum decrease in freedom of movement, as induced by the failure experience, was shown in the area of physical skills. Diminished influences were shown with relation to academic skills, and love and affection was affected least. Reinforcement affects generalizing along a dimension of similarity of need was thus shown. In summary, frustration results in a general loss of freedom of movement and reward-punishment expectancy is a function of past reinforcements.

Analysis of the freedom of movement construct seemed to show two facets to it. The first was the relative anticipation for success, as opposed to failure, and the second was the desirability of the goal which could make the prize worth the game. These two facets were included in a revised formulation as expectancy and reinforcement value, respectively. Freedom of movement was redefined at a relatively molar level and used to mean the "mean expectancy of obtaining positive satisfactions as a result of a set of related behaviors directed towards the accomplishment of a group of functionally related reinforcements." (69, No. 6, p. 5) This leaves a need for a basic formulation of behavior at a more specific, or molecular, level.

The basic formula for behavior states that Behavior Potential is a function of Expectancy and Reinforcement Value. This is usually written as: BP equals f(E & RV). The Behavior Potential is defined as

...the potentiality of any behavior or act of the individual occurring in a given or explicit situation or situations, in relation to a given or explicit external reinforcement. That is, behavior potential is an abstraction which is an attempt to calculate the likelihood
of a particular behavior occurring in a given situation or situations having directionality or moving toward a specified goal or goals. (69, No. 4, p. 1)

Expectancy, in the same context, is defined as "the probability (internal) held by the individual that a particular external reinforcement will occur as a function of, or in relation to, a specific behavior in a specific situation or situations. Expectancy is independent of the value or importance of the external reinforcement." (69, No. 4, p. 1)

Reinforcement Value is the construct which represents the value of the goal or reinforcement to the individual, as compared with other possible goals or reinforcement. It is defined as: "The value of a reinforcement or its importance to the individual in terms of its preference position with expectancy held constant." (69, No. 4, p. 1)

This basic formulation of behavior has other formulae related to it. One of these, with which the present study is only indirectly concerned, is that the strength of a reinforcement can be calculated on the basis of the expectancy and the reinforcement value in the following fashion:

\[ SR = f(1-E \& RV) \]

Expectancy must fall between unity and zero. This formula is included to illustrate the centrality of expectancy in the system. Expectancy has its own formula and is conceived as possessing components. Discussion of this formula will be presented at another place.
Jessor performed an experiment which was somewhat similar to Crandall's, since it involved the generalization of the effects of reinforcement. He chose four tasks on a basis of goal-relatedness. All subjects stated expectancies and minimal goal levels for all of the tasks prior to receiving any information other than the names of the tasks and the range of scores. The statements of expectancy were similar to those in the present experiment, while the minimal goal level statements were statements of the lowest score which the individual felt he could make and still be satisfied with his performance. Following these initial statements, Jessor gave a positive or negative reinforcement on one task. This reinforcement might be a "weak" or "strong" one. The subject was then given an opportunity to restate his expectancies for the other tasks.

Jessor used 132 male college students, mostly freshmen. The tasks presented were eight verbal arithmetic problems, a motor skills test on the epicyclic rotary pursuitmeter, a vocabulary knowledge test—anagrams, and a "social skills test."

His results showed that expectancy and minimal goal level did change in the predicted direction after a positive or negative reinforcement, with expectancy changing significantly more than minimal goal level. These changes in expectancy and minimal goal level generalized and these generalizations followed the course predicted on the basis of functional relatedness. A portion of his design was directed toward an investigation of reinforcement value. The assumption was made that success on a mathematical test would be more
important to Commerce students because of their intended occupations. The results on this portion were not conclusive as to the influence of reinforcement value on the strength of a reinforcement. It was indicated, however, that when reinforcement value was constant, the amount of disparity, between expectancy and reinforcement received, influenced the strength of a reinforcement as measured by changes in expectancy.

Dean (11) performed a related experiment dealing with expectancy and minimal goal level at an earlier date. In recognition of the social aspects of Social Learning Theory, he introduced cultural standards to determine the influences which they would have on changes in expectancy and minimal goal level following controlled experiences of success or failure.

Four groups of college students, twenty in each group, were given trials on the Rotter Level of Aspiration Board, individually. The board was controlled to permit experimenter manipulation of the difficulty of the task. Experimental Group One was given a cultural standard and was consistently scored below it, while Experimental Group Two was given a cultural standard and was consistently scored above it. Each of these groups was paired with a control group which had no cultural standard, but which received the same experiences on the test.

The test hypothesis was that minimal goal level and expectancy level would be differentially affected by the presence or absence
of a cultural standard of performance. The conclusions reached were that the minimal goal level was reduced significantly less than the level of expectancy under conditions of negative reinforcement. Positive reinforcement produced no significant differences. The cultural standards failed to produce significant differences, also. It should be noted that some difficulties arose with regard to the controlling of the experimental experiences.

Lasko (52) developed a behavioral measure of expectancy which utilized the persistence of the response as a measure of the height of expectancy. He wished to determine the effects of both order and number of reinforcements on expectancy. To do this, he presented school children with a stick and a long board containing many holes. These holes were punched with the stick in sequence and candy was placed in some of them to provide a prearranged pattern of reinforcement. The children were instructed to continue using the stick until they were convinced that no more candy would be forthcoming.

With this "Yes-No" type of measure, he was able to verify the fact that partial reinforcement conditions created a significantly stronger expectancy, by his criteria, than did continuous reinforcement conditions. His results did not show any significant differences resulting from early, as opposed to late, massing of reinforcements during the learning trials. The differences, between the partially reinforced groups and the continuously reinforced groups, in resistance to extinction are attributed to the degree of contrast between
learning conditions and extinction conditions for these groups. This contrast is greatest for the continuous group, which was reinforced during each learning trial, and its members were most able to recognize the inappropriateness of their expectancy when no candy was forthcoming. This study left the effects of order undetermined, but revealed that reinforcement could not be reduced to "frequency-counting."

The results of this experiment by Lasko seem to be in agreement with the strength of reinforcement formula. In this formula, as has been noted, the difference between the expectancy and what occurs in the event is important. Rotter has theorized that "under conditions of continuous reinforcement, the curve for expectancy is not linear, but will show expectancy increasing in diminishing units, since the strength of each succeeding reinforcement would be less than the preceding one, as difference between the expectancy and occurrence is diminished." (7, p. 14)

This statement led Castenada (7) to devise a technique whereby the expectancy would be measurable at every trial and the change in expectancy produced by each reinforcement could also be measured. Number and order of reinforcements could be analyzed for influence to a degree not permitted in Lasko's experiment.

The equipment consisted of a wooden construction containing a series of individual "pockets." Each of these pockets contained a standard number of marbles. The subject was permitted to bet any
portion of the marbles of one pocket for one trial. The trials consisted of attempts by the subject to squeeze a hand dynamometer, the dial face of which was hidden from him, so as to produce a specific reading. In reality, the experimenter had the series of successes and failures prearranged.

Four groups of school children were used in the experiment. Three of the groups had equal numbers of positive and negative reinforcements, but differed in the absolute number of marbles available for wagering. This was done in order to find the relationship between the amount of accumulation and the individual's willingness to risk a loss, i.e. does a ten dollar wager represent the same degree of conviction when made by a millionaire or a graduate student? The fourth group had a different order of reinforcement given to them.

From this design, Castenada was able to find general support for a relationship for number as well as order, as they effect changes in the expectancy of potential success of a given behavior. Support is also derived for the belief that a relationship exists between the nature or direction of the reinforcement and changes in expectancy. A simple frequency count or reinforcement is again found inadequate to account for the data.

Expectancy is found to be uninfluenced by the rate of accumulation and to be higher with more reinforcements. Different orders of reinforcement produce significant differences, which appear to be a function of the fact that a given reinforcement is stronger when the expectancy for it is lower.
This completes the presentation of the work on expectancy which has been done to the present research. The accumulation of a cohesive body of research findings is taking place and further modification of the theoretical position with regard to expectancy would not be surprising.

Although the associated construct of reinforcement value has not, as yet, received the degree of attention that expectancy has, a reasonable beginning has been made. Austrin (3) investigated the attractiveness of activities, as influenced by negative and positive reinforcements. This was done by presenting a group of institutionalized children with a variety of play materials which they ranked in a preference order. This ranking took place after familiarity was achieved and careful counterbalancing was followed throughout this experiment to prevent position effects in any portion.

The child was given repeated opportunities to perform the play tasks. At a point in these trials, depending on the experimental group in which the child was placed, a strong positive or negative reinforcement was given by the experimenter. He would tell them, for example, "That's awful. You're the worst I've ever seen. You'll never learn to handle that right," etc. At the conclusion of all trials, the children were asked to rank the play activities again.

The results indicate that the external reinforcement did change the value of the activity for the child, with positive reinforcement
increasing the availability of the response. The increased rank, given an activity as a result of positive reinforcement, decreases with cessation of the positive reinforcement. The effects of a negative reinforcement decrease with time, also. Although one negative reinforcement reduces the total increase in availability produced by positive reinforcement, there is no relationship between this factor and the placement of the single negative reinforcement in a series of trials. There is some indication, not significant, that the "end negative" trial might be more effective than beginning or middle trials.

Rosenberg (66) has recently completed a research directed at discovering whether the value of a reinforcement will change as a function of its occurrence. In a design which parallels that of Austrin in some respects, she had school children rank their preferences for small candies. Two candies, of the five initially presented, proved to have values which were nearly equivalent and these were chosen as the basis for the comparison in the before-and-after measures and for presentation as the experimental reinforcements.

The possibility existed that, were only one of these candies used as the reinforcement, some factor peculiar to the specific candy chosen might influence the results. This necessitated the inclusion of both types of candy with all groups. As it was desired to present one of the candies a sufficient number of times to make significant changes possible in the measures, it was necessary to counterbalance the effect of number by having two groups. Classroom I received candy
A six times and candy B two times; Classroom II received candy A two times and candy B six times; Classroom III received each candy four times. The control classroom received no candy between the two occasions for ranking.

The conclusion derived from the study was that the occurrence of a reinforcement did not, within the limits of the study, change its value. At the present state of research, the results of the studies seem to indicate that reinforcement value is changed through its relationship with other reinforcement values, or possibly through changing expectancies for other reinforcement values.

The impression that all Social Learning Theory research is concerned with expectancy or reinforcement value is not intended. Other investigations related to psychological satiation and the definition of psychological needs have been carried out. Areas of research interest should grow with the number of projects accomplished. It is the expectancy facet of the theory, however, which is of immediate concern to this paper. A summary of the theoretical formulation of this construct has been delayed to this point, in order that it might appear in proximity to the statement of the problem.

Expectancy, as used throughout the theory, is not a simple construct. "It is not a probability determined in actuarial terms, but may be considered to be both: (1) a function of probability which can be calculated from past histories of reinforcements, necessitating the consideration of special problems such as the
immediacy of last reinforcements, patterning, reducing increments, interrelationships, etc.; and (2) a generalization of expectancies from other related behavior-reinforcement sequences." (69, No. 4, p. 3) Expectancy, therefore, has its own formula which is expressed as: "Expectancy is a function of the probability of occurrence as based on past experience (E') and the generalization of the expectancies of the same or similar reinforcements occurring in other situations for the same or functionally related behaviors (GE)." (69, No. 4, p. 3) Stated quasi-mathematically, E equals f(E' & GE).

In a relatively novel situation, it is presumed that generalization effects, chiefly, would determine the expectancy held by the particular individual. This could be presumed to be responsible for much of the variation found between individuals in the usual expectancy situation, at least in the initial stages. It is logical, on the other hand, to think that as experience on the particular task is increased, expectancies develop which are specific to the situation.

From this formulation of expectancy, based on the research findings and the thinking which accompanied the projects, a plan was developed which seemed capable of demonstrating the aspects of the construct, as currently conceived. Certain deductions, relative to the formulation, will be tested.
III. STATEMENT OF THE PROBLEM

According to the form in which the expectancy construct is presented in Social Learning Theory, certain hypotheses can be made regarding the formation and stability of expectancies. The purpose of this chapter is to make explicit those hypotheses and to place them into a form which will permit as accurate a test of them as possible. It is important, also, that some indication of the confidence which may be placed on the results be included.

1. The basic hypothesis made is that expectancies are learned and, therefore, a function of past experience, broadly defined.

2. In a novel situation, expectancies will be based on a generalization of expectancies from other related behavior—reinforcement sequences, and will show maximal individual differences when the individuals have had no experience on the specific task. This variability should decrease with experience on the task.

3. Since a specific expectancy is learned as a function of the number of reinforcements, other things being equal, the degree of stability of the expectancy—resistance to change—will increase as a function of the number of trials at a stated level.

4. Since an expectancy is a learned phenomenon, one formed
under conditions of spaced learning will be more stable, i.e. better learned, than one formed under conditions of massed learning. This hypothesis is based on established findings in the field and is not systematically derivable from Social Learning Theory.

A separate prediction was made and included as part of the experimental design, that initial expectancies would approximate twenty-eight in a novel situation where the possible range of scores is from zero to fifty. This is an extra-systematic hypothesis and is based on the analysis of data from Chapman and Volkmann (6) and preliminary research by Dean (11).

In order that these hypotheses might be tested, they were placed in the null hypothesis form as follows:

I. The initial expectancy statements of groups which differ only with regard to the amount of experience in a situation will show no differences in variance between groups greater than those which can be explained on the basis of chance, alone.

II. Groups, differing only with regard to the amount of experience in a situation and having approximately equal mean expectancies will not differ in the amount of change of their expectancies, following the presentation of a standard reinforcement, to a degree greater than that attributable to chance, alone.

III. Two groups, having identical histories and differing only in that one group received the same reinforcements under
conditions of relative massing and the other group received them under conditions of relative spacing, will not show differences in change following a similar reinforcement, to a degree greater than that which can be attributed to chance factors, alone.

The prediction made concerning the absolute size of the predicted expectancy by the naive group is untestable and considered of interest for the practical value it has in this design, only.
IV. METHODOLOGY

A description of the basic design of the experiment and the specific methodology used will be presented in this chapter.

A. Basic Design

The plan of the experiment was to have groups of people state an expectancy after they had different amounts of experience on a novel task. Controls were used to insure that the levels of these initial expectancies were approximately equal. A reinforcement was then introduced, this reinforcement differing from the previously stated expectancies to a uniform degree. A second statement of expectancy was then evoked. The difference scores between the first and second expectancies constituted the primary data of the experiment.

B. Subjects

The subjects used were all male members of four-year high schools. All were white, native-born, and "normal" students, presumably. Seventy-eight were selected from a high school in Pennsylvania and represented approximately half of the male population of that school. The only criteria for participation in the experiment were willingness to serve as a subject and attendance in a study hall. Very few refusals were received.
Forty-seven additional students were obtained from a high school in Ohio. This represented approximately ninety per cent of the male students in attendance. These students were selected by the principal on a basis of momentary availability, and referred by him to the experimenter.

All subjects were assigned to an experimental group prior to their first contact with the experimenter. The school populations were roughly similar in containing a large proportion of rural students. No further delineation of the subjects seemed desirable in terms of the generalizations which were to be drawn, and control of any unique variables was obtained, so far as possible, through the size of the groups.

C. Equipment

The equipment used was the "Assembly-Disassembly Test of Mechanical Ability" constructed by the writer. The materials were purchased in a five-and-ten cent store. Four red plastic bowls having five-inch long sides and standing one and three-quarter inches high were mounted at equal distances on a strip of composition board. The board was eighteen inches long and seven inches wide. These bowls were designated as "trays."

An unmounted red plastic bowl, seven inches in diameter, was used, also. This was called the "bin." Two sets of ten stove bolts, with nuts to fit, completed the test proper. These bolts were one-
quarter inch by one and one-half inches, and three-sixteenths by two inches, respectively. A set of pseudo "norms," a formal set of test directions, a clipboard, and a noisy stopwatch composed the rest of the experimental equipment.

The test was presented to the subject in the arrangement as given in Figure I:

Figure I

THE ARRANGEMENT OF THE MATERIALS

SUBJECT

BIN

TRAY #1  TRAY #2  TRAY #3  TRAY #4

EXPERIMENTER

Tray #1 contained one-fourth by one and one-half inch bolts, Tray #2 contained three-sixteenth by two inch nuts, Tray #3 contained one-fourth by one and one-half inch nuts, and Tray #4 contained three-sixteenth by two inch bolts.

D. Procedure

In all cases, the students were "tested" individually. Upon entering the experimental room, they were seated at a table before the test equipment and given the following instructions:
"A group of us are trying to make up a large test of mechanical ability. You know, something that we can give a guy in a couple of hours and tell pretty well how he will do in trade school or on a mechanical job later. This test is only part of that big one.

"One thing you need to find out is whether someone can handle small parts quickly and well. Since we don't know whether this test works or not, we're giving it to a lot of men in different ways. Some get a lot of practice before being tested, some get a little, and some get none. Since we don't know how good it is, and since it isn't fair to compare when some get more practice than others, it's important that you do not tell others your score on this test. Even your principal, Mr. _____, does not get the scores."

Upon receiving acquiescence to this condition, the experimenter then directed the subject's attention to the materials on the table.

"Now as for the test itself, here's the way it goes. You see the four red trays on the board? They contain two types of bolts and the proper kind of nut to fit each. The idea is to fasten each of the ten nuts of each kind on to the proper kind of bolt just so they will hold. Drop them into the large bin (indicating) as fast as you can. As soon as you have them all in the bin, you are to take them back out, one at a time. Take them apart and put them back the way they are now. You may reach with both hands, but only work with one nut and one bolt at a time.

"I will time you and give you a score. The scores run from zero to fifty (demonstrates norm tables, briefly). You see, the fewer seconds you take, the more points you get. I will also ask you, at times, to tell me what score you expect to get."

From this point, the procedure varied according to the group into which the individual had been placed. These groups were designed to follow as nearly as possible the arrangement given in Figure II.
It was not considered feasible in terms of the demands of realism to give identical scores on all trials by all groups. Since the predicted first expectancy for Group I was twenty-eight, this score was given to all other groups as their final practice score. Where more practice trials were indicated, their scores were varied as little as possible and arranged to give slight improvement during the session as compensation for the lack of overall improvement. All scores given prior to the statement of first expectancy were arranged in advance, given uniformly to each member of that group, and supposedly derived on the basis of individual performance time.

The remainder of the directions were thus varied according to group, as follows:

Group I. "Before you begin, I'd like you to tell me how many points you expect to make on your first test trial. You won't hit it right on the nose, but guess as close as you can." (Statement of El) "All right. Ready? Begin!"

<table>
<thead>
<tr>
<th>Group</th>
<th>No. of Reinforcements</th>
<th>Expectancy #1</th>
<th>Reinf. Trial</th>
<th>Expectancy #2</th>
</tr>
</thead>
<tbody>
<tr>
<td>I.</td>
<td>0</td>
<td>El</td>
<td>El plus 10</td>
<td>E2</td>
</tr>
<tr>
<td>II.</td>
<td>1</td>
<td>El</td>
<td>El plus 10</td>
<td>E2</td>
</tr>
<tr>
<td>III.</td>
<td>5</td>
<td>El</td>
<td>El plus 10</td>
<td>E2</td>
</tr>
<tr>
<td>IV.</td>
<td>5 (spaced)</td>
<td>El</td>
<td>El plus 10</td>
<td>E2</td>
</tr>
<tr>
<td>V.</td>
<td>15</td>
<td>El</td>
<td>El plus 10</td>
<td>E2</td>
</tr>
</tbody>
</table>
Group II. "You will get one practice trial before the test. Ready? Begin!" (At the conclusion of the trial) "You made twenty-eight on that one. How many points do you expect to make on your first test trial? You won't hit it right on the nose, but guess as close as you can." (Statement of El.) "All right. Ready? Begin!"

Group III. The directions are the same as with Group II, except that five practice trials are given before the first statement of expectancy. The scores reported to the subject for the practice trials are, in order, 25, 27, 29, 26, 28. All trials are run on the same day.

Group IV. The directions are the same as with Group III, except that the practice trials are spaced at one a day, with the statement of expectancy following the last trial on the fifth consecutive day.

Group V. This group was identical with Group III on the first day's practice trials, had scores of 26, 25, 27, 28, 29, on the second day, and was identical, throughout, with Group III on the third day.

Procedure, from this point, was identical for all groups. Each was informed that he had made his individually stated expectancies plus ten points, on his first test trial. The experimenter then said:

"You have made (El plus ten) on that test trial. What do you expect to make on the next test trial? (Statement of E2) All right. Ready? Begin!"

After the second test trial, the individuals were given scores which varied, but were above the expectancy as stated. This served to provide variation in the final scores given and helped to prevent the discovery of the deception of the prearranged scores.

In summary, the methodology for the groups was as follows:
Group I, with 25 subjects, stated an expectancy, had it reinforced plus ten, and stated a second expectancy.

Group II, with 25 subjects, had a "practice trial" of 28, stated an expectancy, had it reinforced plus ten, and stated a second expectancy.

Group III, with 25 subjects, had "practice trials" of 25, 27, 29, 26, and 28 on one day, stated an expectancy, had it reinforced plus ten, and stated a second expectancy.

Group IV, with 25 subjects, had "practice trials" of 25, 27, 29, 26, and 28 on separate days. On the final day, the practice trial was followed by a statement of expectancy. This was reinforced plus ten, and the subjects stated a second expectancy.

Group V, with 25 subjects, had "practice trials" of 25, 27, 29, 26, and 28 on the first day, "practice trials" of 26, 25, 27, 28, and 29 on the second day. On the third day, "practice trials" of 25, 27, 29, 26, and 28 were followed by a statement of expectancy, a reinforcement of it plus ten, and a statement of a second expectancy.

Thus, the chief difference between the groups is the number of trials which they have taken and the number of approximately equivalent scores which they have received prior to stating an expectancy. The necessity for achieving realism, by forcing a slight improvement throughout each day's trials, allows a possible source of criticism of the study, since the mean "practice trial"
score for the three longer groups was twenty-seven rather than twenty-eight. It was thought that, with the slight improvement in trend allowed, this would actually produce more equivalent subjective expectancies of twenty-eight at the time of test. The data presented in Table I seem to illustrate that this was successful.

The seventy-eight cases gathered in Pennsylvania were divided so as to compose twenty of the members of Groups I, II, and III, and eighteen cases of Group IV. Five additional members were incorporated into Groups I, II, and III, and seven were added to Group IV from the Ohio school. Group V was composed entirely of students from the Ohio schools.

The possibility of differences in the groups which would be due to this sampling was considered. A comparison of the Ohio members of each group with an equivalent number of Pennsylvania members of the same group, randomly selected, failed to reveal any differences approaching significance. In no case did the P-value become smaller than .40 and the direction of difference was not constant for one school as opposed to the other. Overall, there was a tendency for the Ohio students to be slightly lower in expectancy. This is felt to be partially a function of the arrangement whereby they were referred to the testing by the principal and, thus, greater authority was involved in the task.

**E. Observations Regarding the Method**

Information as to the meaningfulness of the experiment to
the individual subjects was obtained from comments and behaviors noted in the situation. The choice of a pseudo-mechanical task had been dictated by the fact that it would have meaning to a male population. Although many of the subjects appeared to take a "cavalier" attitude at the onset of the test, they quickly began discussing techniques and methods for increasing their speeds. Many subjects asked questions concerning their final scores and were assured that they had "done well," while being reminded of the fallacies of the test as mentioned in the directions. Despite the great possibilities for subjects to "get together" on the similarity of practice scores received, no instance appeared where the experimenter was challenged and the final subject was very much concerned about his progress and score.

In several instances, individuals possessed really outstanding dexterity on the task. These members appeared in each group and, since scores were controlled, this factor did not seem to have any relevance. Where specific trials were run which appeared to show a considerable improvement in speed, the experimenter anticipated doubts concerning the validity of scores and expressed surprise at the score first--mentioning how hard it is to really estimate time. This was accepted--probably because none of the subjects could conceive of so much time's being spent with each individual in order to get two statements of expectancy!

Preliminary research with a few subjects revealed arrangements necessary to insure a task requiring at least the minimum of
time sufficient to make it impossible to gauge durations accurately. This was about ninety seconds. The median time on the "norm" sheet was placed at 160 seconds, with the maximum time at 258 seconds.

The scores were so placed on the norm sheet that the middle section of scores showed a requirement of three seconds' improvement to gain one point, the slower end required four seconds to gain one point, and the faster end required two seconds to gain a point. This feature was included to help explain to the subjects how their scores could possibly improve so much after they had finished all relevant portions of the experiment.

As illustrations of the degree of realism achieved, it might be stated that the tests in Pennsylvania were administered in the guidance counselor's office. The program was explained to this man before any subjects were run. Despite his observations of the procedure at frequent intervals, a week and a half after the beginning of the experiment he suddenly stated, "Oh, I see what you're doing. You're not really testing them at all!"

At the Ohio school, the principal seized the opportunity to refer some problem students for special testing and these referrals were handled with the Grace Arthur Performance Scale, rather than the "Assembly-Disassembly."
V. RESULTS

This portion of the paper presents the objective results of the experiment.

The raw data of the experiment consist of two sets of expectancy statements from each of the experimental subjects. This material is presented in terms of group averages in Tables 1 and 2. Because of the design of the experimental, whereby the means were artificially forced toward equality at the time of the statement of the first expectancy, the only figures which fulfill the requirements for complete statistical analysis are the difference scores between the two expectancies. These data are presented in Table 3.

The variances of these difference scores, when tested for homogeneity by means of Bartlett’s test (14, p. 195), were found to be well within the acceptable range. The chi square, with four degrees of freedom was .84, which yields a P-value greater than .90. No possibility for interaction existed, so "t's" were computed between the means of all groups. The P-values obtained from these comparisons are included in Table 4.

If the sampling procedures may be assumed to have insured fairly equivalent groups prior to any participation in the experiment by the subjects, certain observations based upon the initial statements of expectancy may be made (Table 1). Although the means of the groups, as has been previously noted, were forced toward
Table 1
SUMMARY OF DATA FROM THE FIRST STATEMENT OF EXPECTANCY

<table>
<thead>
<tr>
<th></th>
<th>Group I</th>
<th>Group II</th>
<th>Group III</th>
<th>Group IV</th>
<th>Group V</th>
</tr>
</thead>
<tbody>
<tr>
<td>Mean $E(1)$</td>
<td>29.16</td>
<td>31.00</td>
<td>27.52</td>
<td>28.12</td>
<td>27.40</td>
</tr>
<tr>
<td>S.D. $E(1)$</td>
<td>6.972</td>
<td>4.326</td>
<td>1.603</td>
<td>1.140</td>
<td>1.470</td>
</tr>
<tr>
<td>S.E. $E(1)$</td>
<td>1.423</td>
<td>0.883</td>
<td>0.327</td>
<td>0.233</td>
<td>0.300</td>
</tr>
</tbody>
</table>

Table 2
SUMMARY OF DATA FROM THE SECOND STATEMENT OF EXPECTANCY

<table>
<thead>
<tr>
<th></th>
<th>Group I</th>
<th>Group II</th>
<th>Group III</th>
<th>Group IV</th>
<th>Group V</th>
</tr>
</thead>
<tbody>
<tr>
<td>Mean $E(2)$</td>
<td>39.60</td>
<td>41.76</td>
<td>32.14</td>
<td>34.52</td>
<td>32.08</td>
</tr>
<tr>
<td>S.D. $E(2)$</td>
<td>5.845</td>
<td>6.582</td>
<td>5.440</td>
<td>4.640</td>
<td>4.370</td>
</tr>
<tr>
<td>S.E. $E(2)$</td>
<td>1.193</td>
<td>1.314</td>
<td>1.110</td>
<td>0.947</td>
<td>0.892</td>
</tr>
</tbody>
</table>
equivalence, marked differences in the variances of the groups appear. A Bartlett's test for homogeneity of variance of the groups, at this point, reveals a chi square value of \(114.68\). A chi square value of only 13.277 is necessary for the .01 level of significance with four degrees of freedom. This constriction of variance in Groups III, IV, and V, is further illustrated through consideration of the raw data in the Appendix, which reveal that sixty-two of the seventy-five cases in these groups stated initial expectancies falling within the range of scores previously experienced by them. The remaining thirteen cases stated an expectancy one point above the upper limit of this range. All expectancies for these three groups were, thus, initially between twenty-five and thirty, inclusive.

Inspection of Groups I and II shows almost identical average difference scores. These difference scores paralleled the amount of the raise given in the reinforcement, ten points. The difference scores of the Groups III, IV, and V were sufficiently smaller than those of Groups I and II to make all differences across those lines significant. Neither Group IV nor Group V differed significantly from Group III.

**Interpretation of the Results**

The appearance of significant differences between the groups in a fashion related to the types of experience histories, shows a modifiability which seems most easily explainable in terms of
### Table 3
**SUMMARY OF DATA FROM THE DIFFERENCES BETWEEN EXPECTANCIES**

<table>
<thead>
<tr>
<th></th>
<th>Group #1</th>
<th>Group #2</th>
<th>Group #3</th>
<th>Group #4</th>
<th>Group #5</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Mean D</strong></td>
<td>10.44</td>
<td>10.76</td>
<td>4.92</td>
<td>4.40</td>
<td>4.68</td>
</tr>
<tr>
<td><strong>S.D. D</strong></td>
<td>3.95</td>
<td>3.93</td>
<td>4.32</td>
<td>3.98</td>
<td>3.66</td>
</tr>
<tr>
<td><strong>S.E. D</strong></td>
<td>0.808</td>
<td>0.804</td>
<td>0.880</td>
<td>0.810</td>
<td>0.750</td>
</tr>
</tbody>
</table>

### Table 4
**GROUP COMPARISONS OF DIFFERENCE SCORES USING TWENTY-FOUR DEGREES OF FREEDOM**

<table>
<thead>
<tr>
<th>Groups</th>
<th>S.E. of Mean Diff.</th>
<th>&quot;t&quot;</th>
<th>P-value</th>
</tr>
</thead>
<tbody>
<tr>
<td>1 &amp; 2</td>
<td>1.40</td>
<td>0.229</td>
<td>greater than .90</td>
</tr>
<tr>
<td>1 &amp; 3</td>
<td>1.19</td>
<td>4.638</td>
<td>less than .01</td>
</tr>
<tr>
<td>1 &amp; 4</td>
<td>1.14</td>
<td>3.528</td>
<td>less than .01</td>
</tr>
<tr>
<td>1 &amp; 5</td>
<td>1.10</td>
<td>5.236</td>
<td>less than .01</td>
</tr>
<tr>
<td>2 &amp; 3</td>
<td>1.19</td>
<td>4.900</td>
<td>less than .01</td>
</tr>
<tr>
<td>2 &amp; 4</td>
<td>1.14</td>
<td>3.825</td>
<td>less than .01</td>
</tr>
<tr>
<td>2 &amp; 5</td>
<td>1.10</td>
<td>5.527</td>
<td>less than .01</td>
</tr>
<tr>
<td>3 &amp; 4</td>
<td>1.20</td>
<td>1.230</td>
<td>less than .30</td>
</tr>
<tr>
<td>3 &amp; 5</td>
<td>1.14</td>
<td>0.211</td>
<td>greater than .90</td>
</tr>
<tr>
<td>4 &amp; 5</td>
<td>1.10</td>
<td>1.564</td>
<td>less than .20</td>
</tr>
</tbody>
</table>
learning constructs. With regard to the specific null hypothesis statements presented, the following conclusions seem justified:

I. The first null hypothesis statement is rejected at a high level of confidence, since the Bartlett test reveals significant differences in variance between the groups. These differences tend to be in the predicted direction, but do not completely conform to the prediction.

II. This hypothesis is rejected at a high level of confidence for Groups III, IV, and V. Group II does not permit the rejection of the hypothesis.

III. This statement of the null hypothesis cannot be rejected on the basis of the data. The use of a single-tail test of significance is not indicated, since such difference as does occur is not in the direction of the prediction.

It is of methodological interest to note that the mean first expectancy, for the group receiving no experience on the task prior to their initial statement of expectancy, was 29.16. This was adjudged sufficiently close to the predicted twenty-eight for the purposes of this experiment.
VI. DISCUSSION

From the results of the experiment, it is evident that a consideration of the history of reinforcement is necessary to predict and understand the behavior which will occur as the result of one unique reinforcement. The emphasis given in ahistoric approaches to the study of personality has led some psychologists (49) to negate the importance of understanding the background from which a particular behavior is derived, although Lewin (53), for one, specifically warns against this error. It is apparent that a construction of expectancy as a "level" must include a historical factor. The expressions of magnitude which are based upon a strictly cross-sectional approach run the risk of presenting, as equivalent, many events whose lack of equivalence can be demonstrated by the advent of any factor requiring a readjustment. Since the problem of the psychologist is to modify expectancies, or at least to predict under what conditions this will occur, the inclusion of historical influences is of foremost importance.

The assumption of a negatively accelerated curve form for the increased stability of expectancies as a function of amount of experience is not verified completely by the results of this experiment. It appears highly probable that this was due, in large part, to the particular design used. The revelation of the inade-
quacy of the design constitutes, probably, the most important finding of the research. This statement is based on the considerations which follow.

If, as was predicted, the curve for increasing stabilization of expectancies is negatively accelerated, then the "longer experienced" groups could not be expected to differ greatly from each other because of the approach of an asymptotic condition. In the experimental design, an expectancy of twenty-eight constitutes a relatively rigid baseline for the secondary expectancy statements. This limits the amount of constancy which can be obtained from any group, since all individual deviations will be in an upward direction. If Groups III, IV, and V are representative of the asymptotic condition, it is an indication that modification in the direction of stability occurs in the first few trials. This could be illustrated in a design calling for groups which received pretraining consisting of one, two, three, four, and five trials, each. The lack of significant differences between the groups which received five or more trials need not, therefore, be considered as invalidating the experimental hypothesis.

The lack of significant difference between Groups I and II seems to be more difficult to explain. If it is considered, however, that the group which received no prior experience was predicted to possess an average expectancy of approximately twenty-eight on the basis of generalization, then the single trial for which Group
II received twenty-eight would constitute nothing more than a confirmation of that same expectancy. Acceptance of the null hypothesis is not intended, but the opportunity for the groups to be "psychologically similar," in that they both possess a single reference point upon which to base their future expectancies, is great. To elaborate:

If the formation of expectancies for future events is conceived as a function of extrapolation made from the reinforcement history, then the modifiability of Groups I and II is easily understandable. Possessing only a single reference point, more or less well defined, they possess no knowledge of the slope of the curve of improvement--this improvement expectancy being a cultural phenomenon. As a consequence, the second trial, the experimental reinforcement trial for these groups, serves the most important function of defining the situation. In the experiment, the situation became defined as one of rapid improvement through learning and was accepted as such. For Groups III, IV, and V, however, the second practice trial defined the situation as one possessing the characteristics of a capacity measure. Improvement might occur, but it would appear in limited amounts. Confirmation for this expectancy was derived from the subsequent trials and it became stabilized quickly.

The foregoing explanation seems to be a plausible one with regard to the variance changes which occur. Although the decrease
in variance tends to follow the number of trials as predicted, it is noteworthy that Groups III, IV, and V displayed similar variances on the occasion of the first expectancy statements. If decreasing variance is a negatively accelerated function, then the asymptotic conditions may be assumed to have been reached in this regard, as well. Although the degree of significance of the difference between the variances of Groups I and II cannot be ascertained, it is of appreciable magnitude and in the predicted direction. This could be explained as based upon the confirmatory aspects of the single trial, which established the baseline of twenty-eight more definitely and, in conjunction with the expectancy for improvement, effected a truncating influence upon the lower portion of the variance spread. Here, again, an experimental design which included trials lying between one and five would be invaluable.

The incorporation of a "spaced" group for comparison with a massed group was allowed by the design of the experiment. Although there is, as yet, nothing in Social Learning Theory which could derive the problem, the wide spread findings regarding the efficacy of spacing made it appear probable that the arrangement would produce interesting results. These results might eventuate in the inclusion of appropriate constructs.

The inconclusive results of the comparison of the massed and spaced groups do not permit a rigorous interpretation. It
should be noted that Jessor (145) also failed to get results favoring spacing of trials. Her assumption is that her methodology prevented differences from appearing. A similar conclusion is reached by this experimenter, in that the individual trials were so widely spaced as far to exceed the optimal arrangement of spacing. The approach to an asymptotic condition would also minimize such differences as might appear. In any event, the problem of massed, as opposed to spaced, learning seems to be a molar problem which will require a sophisticated consideration, in that it will demand a thorough investigation.

The tendency for people, when confronted by a novel task with a possible score range of zero to fifty, to estimate their performance at slightly above the middle of the range is an interesting and, seemingly, rather stable phenomenon. As has been noted, there is nothing in Social Learning Theory to lead to this prediction, although the answer seems to lie somewhere in the area of learned social behavior. The finding is useful for the practical work of formulating problems such as the present one, in that it permits a baseline from which other measures may be taken.

It is notable that at least one member of each group showed an unusual readiness to readjust his expectancies upward. Whether such a readiness would be found if the modifications were based on a negative reinforcement is uncertain. Some explanations for these "special" cases is found in certain comments and observations which
these individuals volunteered.

A small number of subjects mentioned having some type of experience which they thought gave them peculiar qualifications for the task. In some cases the optimism was apparently justified, although not in all cases. These individuals appeared to accept their reported "trial" scores as valid, but to maintain a generalized expectancy in stable form, in abeyance. The one reinforcement trial was sufficient to enable them to reject their previous scores and accept that measure as their "true" measure upon which to base their future expectancies. These instances were comparatively rare and such individuals appeared in all groups.

Another explanation for the rejection of the controlled experience in the situation was expressed by the subject who stated, on the basis of the reinforcement trial, "I've got it now! I just tried a new method and it sure works!" Thus, in the face of fifteen experiences to the contrary and only one confirmation, he stated the second expectancy directly in accordance with the extraordinarily favorable trial. This type of incident could be controlled by the large number of subjects, only, and many of the subjects appeared to modify their techniques constantly in an effort to better their scores.

It can not be doubted that other personality characteristics, less directly related to the task, were also operating. One boy who had an obvious motor handicap was referred to the experiment. Rather than reject him as inadequate, and since he was certain to have
a minor success experience if he participated in the experiment, the experimenter allowed him to begin the "practice" trials. The question of including his results in the experiment was a real one, however, since groups with greater than average probability for failure experiences were excluded. The subject, however, became so distraught in the early practice trials that he could not continue. He was given support and reassurance and permitted to withdraw from the experiment.

Another subject discussed the possibility of generating poison gas through the action of the heat in a radio set which would make separate vials permeable. When this was followed by a discussion of the relative efficiency of jumping down the stair well, rather than walking down three flights, a conference between the principal and experimenter was held.

The subjects were generally cooperative and hard working. In order to avoid interfering with the face validity of the task, only sufficient emphasis was placed on the expectancy predictions to keep them meaningful. The statement that they would "not hit it right on the nose" was necessary to encourage them to make the predictions and yet avoid a negative reaction from misjudgment to the positive experience of the big reinforcement.

**Implications of the Research**

Although this research was designed, primarily, to furnish material in accordance with a specific theoretical position, some
ramifications to the field of clinical psychology, in its broader aspects, are indicated. It is evident that a construct of expectancy can be defined and made accessible to mathematical manipulati. The utility of the construct is reaffirmed.

The necessity for considering such a construct as the product of a historical succession of events is asserted and demonstrated. The inclusion of historical referents for events is an important consideration in any clinical framework.

The function which experience has performed in stabilizing an expectancy must be considered if efforts to change that expectancy are to be undertaken. It would appear that, in situations where movement is necessary for the morale of the patient or therapist, efforts should be directed towards those behaviors and situations which have had the smallest amount of consistent reinforcement behind them. If a patient displays reluctance to make an effort in an area involving no history of negative reinforcement, the therapist must seek the explanation in terms of generalized expectancy effects.

The demonstration of the rapid stabilization and formation of specific expectancies, in novel situations, would seem to attach a great importance to the first few therapy sessions. This has not been investigated. It appears that the type of expectancies, related to therapy and developed in these sessions, may show lasting effects in the course of the therapy. The importance of initial structure is accentuated.
The fact that the experienced groups tended to limit their expectancies to the variability range which they had experienced should be investigated. Groups could be arranged to differ in the amount of variance they experience. If the range of experience and expectancy correlates, then therapists could predict the range of expectancy, on a historical basis, which a patient would possess. This could be useful in keeping the reinforcements of the therapist at an optimal level, so as to avoid giving reinforcements which seem unreal to the patient because they far exceed his expectancies. Such reinforcements are rejected, or discriminated and considered as isolated events, and the patient continues to function on the basis of the earlier expectancies.

This consideration of the height, or amount, of the reinforcement which will produce the desired amount of change in an expectancy suggests a research problem. A design in which the interposed reinforcement size was the experimental variable should be rewarding. Jessor (14) included this factor in his experiment, but a more extensive experiment is needed.

These considerations seem to indicate that there is no shortage of problems for investigation. The fruitfulness of Social Learning Theory in the stimulation of research projects justifies the constructs chosen for inclusion.
VII. SUMMARY AND CONCLUSIONS

Rotter's Social Learning Theory of Personality places much stress upon a construct of expectancy in its formulations. This appears to be in line with current psychological thought. Comparative psychologists, social psychologists, psychologists interested in perception and psychophysical methods, and psychologists interested in aspiration all seem to be faced with the need for this, or a similar, construct. The quantity of research being compiled under this, or a related heading, appears to justify the postulation of the construct.

According to Rotter, the expectancy construct may be considered as composed of two sub-variables. One of these is designated E' and is defined as expectancy based upon the history of reinforcement within the specific situation. The other sub-variable is designated as GE and is the generalization effect as summarized from other related behavior-reinforcement sequences. These interact in a mathematically undefined manner to produce a specific expectancy for any given situation. This expectancy, in conjunction with the reinforcement value of the potential goal, operates to determine the specific behavior which will occur in the situation. Stated quasi-mathematically, E equals f(E' and GE).

It is hypothesized that an individual, when confronted with a novel situation, will be forced to rely upon generalization
effects in the determination of his expectancy, and, hence, his be-
havior. A group of such individuals would, therefore, show the
largest amount of variability during the early stages of dea-
with such situations.

If the group of individuals previously described were re-
ppeatedly faced with this situation, however, there would be an
opportunity for an expectancy specific to this situation to develop.
Since this specific expectancy would be uniform for all members of
the group, its influence would be shown in a decrease in variability,
although absolute uniformity among the members would be rarely en-
countered.

Correlated with the increasing tendency toward uniformity of
expectancy, as a result of increasing experience, would be an increased
resistance to modification. The prediction can be made, on this
basis, that a single strong reinforcement would show a differential
influence on groups which differed in the amount of experience they
had been given on a novel task.

The design of the experiment to test these hypotheses was
such as to afford an opportunity to test an extra-systematic hypoth-
esis with ease. It was decided, therefore, to investigate the prob-
ability that an expectancy which was developed through spaced
behavior-reinforcement sequences would be better learned, i.e. more
resistant to change, than would an expectancy developed through con-
ditions of massing an equal number of behavior-reinforcement se-
quences.
As a methodological consideration, it was necessary to predict with a fair degree of accuracy the score which people would choose from a possible range of zero to fifty as the one they were most likely to make. This would be prior to any experience on the task. Systematic formulations would not enable a sufficiently accurate prediction to be made, but examination of previous research led to a prediction of a value of twenty-eight, as the score most people would predict for themselves.

As a means of testing these hypotheses, 125 male high school students were divided randomly into five experimental groups of twenty-five persons, each. The experimental procedure which they followed was determined by their group membership. All of the subjects were "tested" individually on a pseudo "mechanical aptitude test." Prior to this "test," the subjects were given numbers of "practice trials" for which they had prearranged scores presented to them. The number of such "practice trials" was dependent on the group to which they had been assigned. The "practice trial" scores were arranged so that, regardless of the number of previous trials, all groups would state an expectancy approximating twenty-eight at the time of the "test."

The test consisted of asking the individual to state an expectancy for the next trial. Following the trial, he was informed that he had made a score which exceeded his stated expectancy by ten points. He was requested to make a second statement of ex-
pectancy for his next trial. These two statements of expectancy for each individual were then compared and the differences noted. These difference scores were combined by group and compared for significant differences between the groups in the amount of change induced as a result of the standard reinforcement of the ten-point increase.

The experience histories of the various groups were as follows:

Group I had no experience on the task prior to the first statement of expectancy.

Group II had one practice trial, for which they received a score of twenty-eight.

Group III had five practice trials, for which they received in order scores of 25, 27, 29, 26, and 28. All trials and the test were completed in a single session.

Group IV was given the same sequence of practice trials and scores as Group III, with one practice trial being given per day and with the test trials following immediately upon the last practice trial.

Group V was given practice trials and scores of 25, 27, 29, 26, and 28 on the first day; 26, 25, 27, 28, 29 on the second day; and 25, 27, 29, 26, and 28 on the third day. The test trials followed immediately after the
practice trials on the third day.

Statistical treatment of the scores for first expectancy statements indicates that variability is significantly greater in the two groups receiving the least prior experience. The remaining three groups had nearly identical variances at a much smaller level.

These results are taken as, in general, confirmatory to the hypothesis that variance decreases with experience. The confirmation of the expectancy of twenty-eight which was predicted for the "no practice" group, appeared to have the effect of truncating the lower portion of the variability in this group. No indication of the degree of significance of the difference in variance is possible, however.

The variances of the longer practice groups seems to have diminished to an asymptotic condition which had twenty-eight as its limiting baseline. The minimal degree of variance possible seems to be quickly reached during the first five trials.

Comparison of the amounts of change of expectancy induced by the single positive reinforcement reveals that Groups I and II modify in almost identical fashion. Either of these groups show change which exceeds that of Groups III, IV, or V by a significant amount. There are no significant differences among Groups III, IV, and V.
These results are interpreted as confirming the hypothesis of decreasing modifiability of expectancy with experience, but with limitations. If the predicted curve is negatively accelerated, the lack of significant differences among the longer practice groups is not surprising and the great similarity may be attributed to the approach of asymptotic conditions. This might be considered a flaw in the design, but indicates the importance of more thorough investigation of the processes occurring during the first five trials.

The similarity of the "no experience" and the "one trial" groups appears to be evidence against the hypothesis. The design was such, however, as to make the one trial a confirmation of the expectancy predicted under "no experience" conditions. This would make the groups similar psychologically and each of them would be forced to make future predictions on the basis of one point on the curve. Since they had no indication of slope and possessed the cultural expectancy for improvement, the influence of the positive reinforcement on them was considerable. The other three groups were identical in receiving sufficient trials to lead to an expectancy of little improvement on the basis of their extrapolations.

Group III did not differ significantly from Group IV and such difference as was evident was not in the predicted direction. This does not conform to the prediction that an expectancy based upon spaced reinforcements would be more stable than one based upon
conditions of relative massing. The suggestion is made that optimal arrangement of spacing may have been greatly exceeded in the experimental design. The approach to asymptotic conditions is relevant here, also.

The mean expectancy statement for Group I, which received no experience prior to the test, was 29.16. This is regarded as indicating a relatively stable phenomenon which may be incorporated for use in the design of experiments similar to the present one.

It was noted, further, that variance was markedly increased by the introduction of the larger reinforcement. This was most obvious with the groups which had become relatively uniform through the action of the larger amounts of experience. No interpretation is offered for this increase in variance, although it appears a suitable subject for further research.

These results are interpreted as illustrating the relative influences of an expectancy factor developed as the result of the history of reinforcement within the specific situation, and an expectancy factor derived from experience in related situations which is generalized to the present situation. The results of the experiment are, in general, confirmatory to the theoretical hypotheses regarding these factors as components of the overall expectancy.

Further research is indicated in order to delineate the slope of the curve for expectancy stabilization as a function of experience. There are indications that the most marked change in
expectancy occurs during the first few trials in a novel situation. It is possible that a still longer sequence of "practice trials" would reveal further influences on stabilization, but it is difficult to institute such a series in a realistic fashion.

Some of the factors which influence the relative degree of dominance of the generalized expectancy factor, as opposed to the expectancy factor generated in the situation, in the determination of the precise expectancy stated following a strong reinforcement appear related to individual personality variables. These variables are discussed in relation to spontaneous comments and observations by individual subjects.

The implications of the research are of several levels. It appears evident that a construct of expectancy can be defined and converted into mathematical terms. This assures the value of the theoretical postulation. The theoretical emphasis on an historical approach to such a construct is reaffirmed. The position taken in Rotter's Social Learning Theory appears capable of handling the data.

On the level of general clinical applicability of the results, some of the findings seem to possess suggestive qualities. It seems evident that an ahistoric approach to the study of personality in the clinic would, of necessity, lead to lowered predictability. Historical factors are necessary to differentiate identical expectancies in the prediction of change.

If expectancy is stabilized so rapidly in the first few
experiences, it seems obvious that much more careful investigation of the techniques and procedures used in the early therapy sessions is needed. These early sessions would appear to have a great influence upon the expectancies of the patient in therapy and, hence, on the entire course of the therapy.

Finally, it should be noted that when the aim of the psychologist is the change or modification of the expectancies of others, this change is most readily accomplished in those areas where the smallest amount of previous experience has occurred. This could be essential when the therapist desires to demonstrate to the patient the potential for change which the patient possesses.
BIBLIOGRAPHY


57. MacCorquodale, Kenneth, and Meehl, Paul E., "Preliminary Suggestions as to a Formalization of Expectancy Theory," accepted for publication in the Psychological Review.


APPENDIX
### STATED EXPECTANCES BY INDIVIDUALS WITHIN EACH GROUP

#### GROUP ONE

<table>
<thead>
<tr>
<th>Subject #</th>
<th>Expectancy #1</th>
<th>Expectancy #2</th>
<th>Difference</th>
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</thead>
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<td>12</td>
</tr>
<tr>
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<td>15</td>
</tr>
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</tr>
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I, Robert A. Good, was born in Milton, Pennsylvania, April 3, 1921. I received my secondary school education in the public schools of Lewisburg, Pennsylvania. Undergraduate training was undertaken at Bucknell University, Lewisburg, Pennsylvania, and the degree Bachelor of Science was received in 1942. After serving as a pilot in the Naval Air Corps, I was returned to inactive duty and returned to Bucknell for additional work during the summer session of 1946. I enrolled in the Graduate School, Department of Psychology, at The Ohio State University for Fall Quarter, 1946. The degree Master of Arts was received from The Ohio State University in 1948. While completing the requirements for the degree Doctor of Philosophy, in the Department of Psychology at The Ohio State University, I held the position of Graduate Assistant for the Spring Quarter, 1947; Assistant for the years 1947-48 and 1948-49; Psychological Intern at Harding Sanitarium, Worthington, Ohio, 1950; Assistant Instructor in the Department of Psychology, 1950-51; Assistant for the year 1951-52; and Clinical Psychologist (Trainee) with the Veterans Administration in 1952.