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A STUDY OF PROBLEM SOLVING CONTROL

by

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I INTRODUCTION

It has been estimated that $15 billion were spent during 1962 by governmental and industrial organizations to support research and development activities.[1] As a result of such activities our knowledge is increased, our economy is stimulated, and new products become available for our comfort and convenience.

The importance of such innovative activities has not been ignored. Schumpeter found variations in the rate of innovation sufficient to explain observed fluctuations in aggregate economic activity.[2] More recently questions of the viability of certain of our social institutions in a world where human innovative activity is having more and more widespread effects on our environment are being forced upon us.[3]

Despite its widespread effects innovation is essentially an individual human phenomenon. It occurs when someone is led to behave in such a way that this behavior has a significant effect on the behavior of others. Under this definition clearly Einstein and Edison were innovators but then so are we all. No one is so isolated that his behavior can be said to have no influence on others. For most of us, however, the extent of this influence is modest indeed.

1/ Federal Organizations for Scientific Activities 1962, National Sciences Foundation: Washington 25, D. C., Superintendant of Documents, U. S. Government Printing Office, 1963.

2/ J. Schumpeter, Theory of Economic Development, Cambridge, Mass.: Harvard University Press, 1934.

3/ John T. Dunlop (Ed.), Automation and Technological Change, Englewood Cliffs, N. J.: Prentice-Hall (1962).

In the work described in this paper we were not concerned with the effects of innovation. Neither did we consider in any detail those cognative processes which might explain the particular results of attempts to innovate. Our interest instead was in attempting to understand how these detailed processes which yield new forms of behavior are controlled or focused on particular parts of a complex environment. We were concerned more with the problem of understanding how problems are selected than in understanding how they might be solved. More specifically we were concerned with the process by which an industrial manager might focus his attention on particular variables in his environment. In the course of our study, however, we found no reason to believe that these processes should be in any way different from those which might take place in quite different situations.

To remain consistent with prior work which we accepted as relevent to ours we have called the detailed processes by which specific behaviors are selected problem solving.[1/] The process by which these processes are focused on problems we have called problem solving control.

We found that the process of problem solving control has been described in several theoretical frameworks.[2/] Most of these descriptions were concerned with interpersonal rather than individual behavior. In the case of a theory suggested by R. M. Cyert and J. G. March, however, very modest additions were required to yield empirically testable propositions regarding

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[1/] A. Newell, J. C. Shaw, and H. A. Simon, "Elements of a Theory of Human Problem Solving", Psychological Review, Vol. 65, (May 1958), pp. 151 - 166.

[2/] For a review of some of these see Warren G. Bennis, Kenneth D. Benne and Robert Chin, The Planning of Change, New York, Rinehart and Winston, 1961.

individual behavior.[1] [illegible] designed and executed.

As a result of this empirical work a mechanism based on success and failure as defined by independent dynamic goals or aspiration levels was shown to provide an inadequate explanation of the problem solving control process. Examination of subject protocols suggested the form of a new theory, however. This theory suggests that problem solving is focused on variables in the decision maker's environment by a discriminating process defined as a sub-set of attributes of that environment. The theory also suggests that attributes are added to this process only as they are required to identify a unique focus for problem solving. Some behavioral evidence was gathered which supported this theory subject only to some reservations about the effect of the process of observation on the behavior in question.

In considering the compatability of our theory with casual observations of industrial phenomena and the findings of others who have investigated the problem solving control process, we found encouraging consistency and the need for extensive empirical work.

Perhaps the most significant result of our study was that the processes which we found to explain problem solving control are quite similar to those which have been found on studies of detailed problem solving. We concluded, therefore, that once this similarity is firmly established by further research, we can look forward to a unified theory of human decision making behavior which will explain not only innovation but a wide variety of other human phenomena.

1/ R. M. Cyert and J. G. March, A Behavioral Theory of the Firm, Englewood Cliffs, N. J., Prentice-Hall, 1963, pp. 34 - 36.

Before turning to a more detailed description of our work, we have devoted the next chapter to a clarification of some basic philosophical issues which can be particularly confusing in the context of theories of human behavior.

II. TWO KINDS OF THEORY

A principal difference between positive and teleological or normative theories of human behavior is in their use of the concept of a goal. This chapter will be devoted to a discussion of the issues involved in this difference and to a clarification of the use of this concept in these two kinds of theories.

When we attempt to understand or explain why a particular event takes place, we find that the question can be interpreted in two ways. The word _why_ is ambiguous. It can be answered either in terms of the purposes served by the event or in terms of the prior conditions and processes which predetermined or caused the event. An explanation based on the first interpretation is called teleological while an explanation based on the second is sometimes described as mechanistic.[1/]

Philosophers have found no logical basis for choice between the teleological and mechanistic modes of explanation. In the limit both modes lead to what appear to be unanswerable questions. If a baker bakes bread for the purpose of selling it, and if he sells it for the purpose of making money, and he wants money for the purpose of buying food, clearly this string of objectives can be extended easily to a question of ultimate purpose which is, at least currently, impossible to answer. It seems, therefore, that teleological explanations are bounded by our knowledge of ultimate purpose.

[1/] For a more complete discussion of these terms see: Bertrand Russell, _A History of Western Philosophy_, Simon and Schuster, 1945, p. 38; and R. B. Braithwaite, _Scientific Explanation_, Cambridge University Press, 1953, Chapter 10.

On the other hand, if a baker bakes bread because he is a baker, and he is a baker because he chose to become one, and he chose to become one because his father was a baker, we find this string of explanations leading inexorably to a question of first cause which is also difficult to answer. Mechanistic explanations are bounded, therefore, by our knowledge, not of ultimate purpose, but of original cause.

Despite the logical disadvantages of both modes of explanation, we find that most theory can be roughly categorized as either teleological or mechanistic. Newtonian mechanics where the motions of bodies are explained in terms of prior properties like mass, velocity, and the forces acting on them is clearly mechanistic, while economic theory which assumes human behavior will accomplish the purpose of utility maximization is obviously teleological.

In some cases, theories in each mode have been devised to explain the same phenomenon. The path of a light ray through a lens system, for example, can be explained both mechanistically in terms of the refractive properties of the system and the characteristics of light, and teleologically in terms of the path which will minimize the time required for light to get from a given source to a given destination. Kepler devised a teleological theory of planetary motion which preceded Newton's mechanistic theory. Thus it appears possible, in principle at least, to discover dual theories, one mechanistic and one teleological, by which events can be explained. If this analysis is correct, an attempt to decide in general which mode of theory is appropriate to a given question is a fruitless one since, given sufficient effort, theories in either or both modes could presumably be devised. A much more appropriate question perhaps is, given that no satisfactory theory exists

what kind of theory seems most appropriate to attempt to build? This question grants the possibility of useful theories in either mode and suggests the appropriate criterion should be one of efficiency.[1]

In the attempt to devise theories of human decision making, teleological theories are particularly appealing. By introspection most of us are aware that the decisions we make are strongly affected by the goals we seek. It seems appropriate, therefore, to attempt to understand decision making in terms of such goals. The search for a general set of goals which will be useful in understanding the decision process has had limited success, however.

To illustrate the process of a search for predictive goals, consider the problem of devising a teleological theory which will predict the form of a body of water. Most observations indicate that a body of water seeks to minimize the distance from its center of gravity to the center of the earth. A theory based on water having this objective will frequently make correct predictions. Suppose, however, one were to half fill a bucket with water and whirl it suitably around his head. He would observe that water would stay in the bucket at the top of the arc even though the distance from its center of gravity to the center of the earth was not minimized. In this case, the theory would fail. To explain this event one might invent an *ad hoc* objective for water in whirling buckets or, if he thought of it, generalize the purpose attributed to water to one of minimizing its potential energy subject to constraints. The latter theory would explain both the static and dynamic phenomena in terms of the same goal and would, as a result, be a more powerful one than the original.

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[1] For a more complete discussion of some of the issues of these two modes of theory see: C. G. Hempel and P. Oppenheim, "The Logic of Explanation," *Philosophy of Science*, Vol. 15, 1948.

For such a theory to be valid, however, both the concepts of potential energy and the constraints would have to be operationally measurable.

Teleological theories of decision making have encountered analogous problems. While in situations of certainty people decide to choose the higher of two alternative amounts of money, in uncertain ones they frequently don't choose the maximum expected value. To explain this, the maximizing goal attributed to people in this situation has been modified in two ways. In one, another *ad hoc* goal of uncertainty avoidance is offered which is in the same spirit as an *ad hoc* theory of water in whirling buckets. In the other, the individual's goals are generalized to a measure called utility which is similar in kind to the generalization of potential energy in the water case. The chief disadvantage of utility as a predictive device is the difficulty, both theoretical and empirical, of operationally measuring it.[1] The attempt to do so continues, however, with optimism of variable over individuals.

During the past several years there has emerged a growing interest in mechanistic theories of individual and organizational behavior.[2] This interest appears to have arisen out of a feeling of dissatisfaction with progress and promise in teleological theories of decision making and the availability for the first time of a methodology by which complex mechanistic theories can be tested.

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[1] H. A. Simon, "Theories of Decision Making in Economics and Behavioral Sciences," *American Economic Review*, Vol. XLIX, June 1959, pp. 253 - 283; and G. P. E. Clarkson, *The Theory of Consumer Demand: A Critical Appraisal*, Prentice-Hall, 1963.

[2] A. Newell and H. A. Simon, "The Simulation of Human Thought," *Current Trends in Psychological Theory*, University of Pittsburgh Press, 1961, pp. 152 - 179.
R. M. Cyert and J. G. March, *Behavioral Theory of the Firm*, Prentice-Hall, 1963

In these (mechanistic) theories, behavior is viewed as a [illegible] consequence of complex cognitive processes operating on information [illegible] to the decision maker either from his memory or from his environment. [illegible] description, therefore, of the available information and the process used completely defines a theory which will predict specific sequences of behavior. Such a theory can be tested by comparing the predicted behavior to that exhibited by individuals or organizations under the specified conditions. These theories are based entirely on certain physical processes of sensing and symbol manipulation and teleological assumptions are not required.

Even in the context of such mechanistic theories the concept of a goal has proved to be useful for a variety of purposes. We shall describe three of these applications of the concept which differ subtly from each other. It should be emphasized, however, that each of these uses of this concept lies outside the structure of the theory which itself remains entirely mechanistic.

Probably the most common reason goal concepts invade discussions of purely mechanistic theory is that they provide convenient names for complex processes. It has been observed by those constructing mechanistic theories that the structure of a process which will explain rather complex behavior can frequently be separated into relatively independent and self contained sub-processes. These sub-processes in turn can be further separated into still more elementary and independent processes, etc.

In discussing or describing such a structure of elements it is, not surprisingly, far more convenient to name the various parts of the structure than to specify a process completely each time one must refer to

it. It is also not surprising perhaps that the name which gets associated with an elementary process is one which to the observer or designer describes what seems to him to be the goal or objective of the process. Thus goals are associated with mechanistic processes because they provide designers and observers convenient names for these processes. Perhaps some examples will help to clarify this point.

Certain torpedoes are constructed in such a way that the position of their steering mechanism is a function of signals received by a device sensitive to a relatively narrow range of frequencies of sound in water. The behavioral characteristics of these torpedoes in any environment can be completely predicted from a knowledge of their mechanical structure. We commonly speak of these torpedoes, however, as homing torpedoes because their controls are frequently designed in such a way that they will appear to seek a ship within range of their sensing equipment. This name, however, has no influence on the behavior of the torpedoes. Each torpedo follows a path which is completely determined by the interaction of its control system and its environment. Purpose may have existed in the mind of the designer or be attributed by the observer but it would be imprecise to conclude that the torpedo itself wants to find a ship. Thus goals, purposefulness, and objectives frequently enter discussions of mechanistic process as useful names for awkward ideas.

In discussing human behavioral mechanisms the need for differentiation among complex processes leads to more complex names. We describe a management policy as one which will yield both growth and profits to differentiate it presumably from one which yields just profit. We speak of a decision routine

in a chess game as one which attempts to gain an advantage in pieces and position to differentiate it from one which attends only to pieces. Thus the concept of multiple goals follows quite naturally from the need to have more specific names for complex mechanistic processes. At the risk of unnecessary repetition we will emphasize once again that even those more complex names do not influence the behavior of the mechanisms with which they are associated.

Besides acting as names, goal concepts are also used to specify criterion functions whereby two or more mechanistic decision processes can be compared. Goal names are seldom specific enough to specify a unique behavioral mechanism. For purposes of designing a mechanism, therefore, it is useful to specify a criterion by which the behavior of alternative mechanisms can be compared. Thus in the design of a service system for example, we might specify that customer delay and some measure of direct cost will be our criteria. We can then agree under certain specified conditions which of several mechanisms is preferred. We might associate the name efficient or economical with this policy and speak of it as serving this goal or having this purpose. Just as in the case of the torpedo, however, we can predict the behavior of this policy by specifying its mechanism and no knowledge of the purpose of its designer is required. Thus goals enter discussions of mechanistic theories as design criteria as well as names for the resulting process.

Closely related to the notion of design criteria, we sometimes speak of a particular value of a criterion function as the goal of the process under discussion. If we examine this use of the concept closely, we find again that while useful in talking about mechanistic processes it is not

necessary to their understanding or comprehension. Suppose [illegible] a merchant attends to a record of his monthly sales volume. If it falls within a certain range he maintains his price level; if it falls below this range, he reduces his prices. We can speak of that value of sales volume at which he will change his prices as his volume "goal" for it represents the value of this variable which this particular mechanism or policy will <u>appear</u> to seek to satisfy. Similarly a thermostat selects the settings on the controls of a furnace by measuring differences between indicated temperature and some reference setting. Here again we frequently describe the mechanism as one which attempts or seeks to maintain the temperature of the reference setting, when in fact this attribute of its behavior is entirely determined by the structure of its various mechanical components. Thus we use the term goal to describe equilibrium conditions of dynamic processes because to the designer or observer these processes appear to have as objectives the outcomes associated with these conditions.

To summarize, we have described three rather distinct ways in which the concept of goals came to be associated with purely mechanistic processes. First they are used as names for complex processes; second they are used as names for elements in functions by which alternative processes can be compared or evaluated; and third they are used to name particular equilibrium values of variables which the process appears to seek to maintain.

Clearly a basic difference between teleological theories and analytic theories lies in their use of the concept of a goal. In teleological theory, behavior is a variable whose value <u>depends</u> on its consequences. Behavior is assumed to be such that it will accomplish a given goal, e.g., profit maximization. In mechanistic theory on the other hand, behavior in a

particular environment determines its consequences. Behavior is [illegible] by specific processes operating on values obtained from memory or the environment. Certain of these values are sometimes described as goals because they determine equilibrium conditions in the particular behavioral mechanism in which they are used.

In the field of management when a teleological theory, developed in the context of a goal like profit maximization, fails to predict or describe behavior it is frequently converted into a prescriptive or normative theory. Thus methods derived to describe a phenomenon are used to modify it. It is not surprising that in such a process it frequently is difficult to identify the appropriate interpretation of the goals which are so much a part of the discussion. At one point the goal is that outcome which the manager is assumed to be pursuing e.g., profit maximization, and behavior is deduced under teleological assumptions. This leads to a specification of a behavioral mechanism which sometimes differs from that which the manager is observed to be executing. From a descriptive or predictive point of view one might be led to reject the assumptions of the teleological analysis and attempt to construct a better theory. This is seldom done. Rather the analyst maintains his theory and attempts to modify the manager's behavioral process toward that which he (the analyst) has deduced. For reasons which might lead one to further question the teleological basis for these derivations, managers are sometimes observed to be reluctant to adopt the changes in their behavior which are thus recommended. Names ranging from resistance to change to the problem of implementation have been used to describe this phenomenon. It can be a very serious problem to organizations which devote

considerable resources to this kind of analysis.[1] [illegible]
about this phenomenon which was one of the stimuli for the study which we
shall describe in the following pages.

Since that part of the implementation process having to do with the deduction of the proposed behavioral mechanism is well documented elsewhere,[2] we have concerned ourselves with questions relating to the process by which the manager controls his own behavior. More specifically we shall be concerned with the controls which appear to operate on these processes which modify behavioral routines. It is our hope that a better understanding of the control of these processes will lead to a better understanding of both persistent and changing behavior.

1/ For a discussion of this problem see: R. A. Hammond, "Making O. R. Effective for Management," Business Horizons, Spring 1962, pp. 73 - 82.

2/ See for example: C. W. Churchman, R. L. Ackoff and E. L. Arnoff, Introduction to Operations Research New York: John Wiley and Sons, Inc., 1957, or E. H. Bowman and R. B. Fetter, Analysis for Production Management Homewood, Illinois, Richard D. Irwin, Inc., 1961.

III. A THEORY OF PROBLEM SOLVING CONTROL

In this chapter we will outline a framework in which we can discuss the problem solving control process and some of the theory and evidence which has been used to explain this phenomenon.

If we accept the mechanistic mode for our discussion, we can characterize the process which underlies observable behavior as a well defined information processing routine. This routine senses stimuli from the environment, processes it in conjunction with other information available from memory, and selects certain motor processes to be executed. A theory of behavior, therefore, is completely specified by the specification of this information processing program.[1]

Variations have been noted, however, in the process by which behavior is selected under varying circumstances. If the situation is a familiar one, i. e., many attributes have appeared in the same context before, it appears that highly specific behavioral operators can be called directly to deal with the situation. A fireman answering an alarm and a pianist presented with a familiar piece of music appear to execute highly specific behaviors with relatively little hesitation for information processing.

On the other hand when people are faced with relatively unusual stimuli, like the opportunity to invest in a complex venture, we observe that considerable information processing takes place prior to the selection of a behavioral routine which will indicate acceptance or rejection of the opportunity.

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[1] For a more complete discussion of this point of view: A. Newell and H. A. Simon, "The Simulation of Human Thought," Current Trends in Phychological Theory, Pittsburgh, Pennsylvania, University of Pittsburgh Press, 1961, pp 152 - 179.

If we name the behavior in each type of situation decision making, we can differentiate between the two by calling one a programmed decision and the other unprogrammed.[1] This distinction suggests that in the routine situation a ready made program exists which will select satisfactory behavior, while in the latter case it was necessary to construct a behavioral program ad hoc by means of more general processes. We shall refer to these more general processes as problem solving processes, while the simple behavioral processes which are employed in routine situations will be called responses. Clearly, both processes are problem solving processes but in most cases this distinction is more helpful than confusing.

Like most systems of classification, extreme cases are easy to identify while those cases which lie close to the dividing line between the categories are more difficult to classify properly. From the point of view of understanding behavior, of course, the important question is not how the observer can or should classify observed behavior, but rather how the decision-maker chooses to cope with a particular situation. It is easy to recall cases which suggest that quite similar situations are sometimes dealt with by means of routine responses while at other times they call forth problem solving routines of the highest order.

Consider the case of a manager who received a routine report each week on the percentage of defective products being produced by a manufacturing process. For several years he had dealt with this information routinely, i. e., he did nothing and thereby, in effect, approved the standard

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[1] These terms are suggested and discussed in more detail in: J. G. March and H. A. Simon, Organizations, New York, John Wiley & Sons, Inc., 1959, pp. 177.

operating procedure. It appeared that this particular situation was being handled satisfactorily (as far as this manager was concerned) by a well programmed response.

One week, however, he decided to "look into" the quality situation on this process. As a result, information was gathered and analyzed, experiments were designed and executed, product specifications were changed, and a new operating procedure was written for the process. After this burst of problem solving activity was completed the situation reverted to one where the new operating procedure was repetitively applied and little if any problem solving activity was devoted to that part of the process. Thus it appears that situations can be moved back and forth across the boundary between programmed and non-programmed decision making by the decision maker himself. What is not so apparent, however, is the process by which this switching takes place.

A number of theorists have recognized the importance of this process which seems to be close to the heart of theories of both individual and organizational behavior. In individual behavior it is intimately related to all of the work on learning theory for we can conjecture at least that it is the action of these higher level problem solving routines on the response patterns which is what we commonly call the learning phenomenon. To understand when and how these processes are brought to bear on behavioral routines, therefore, is of very fundamental interest to learning theorists.

In organization theory certainly some of the most important and interesting questions have to do with innovation and change in the routines by which the organization's problem solving routines are applied to on going operations is vital to our understanding of this aspect of organizational

behavior. [illegible] resistance to change which organizations and individuals [illegible] in the face of attempts at innovation.

Perhaps because of the lack until recently of a theoretical framework which could permit a precise description of the processes involved in routine and problem solving behavior, most theoretical work on questions of problem solving allocation has not been focused on the structure of the process. A number of peripheral areas have been investigated with results which are of some interest, however.

Because of its fundamental importance to psychologists, psychiatrists, sociologists, and many other branches of social science the phenomenon of change itself has become a focus for considerable theoretical and empirical work. In the human relations literature[1] studies of the process of change have been reported in contexts varying from individual psychotherapy, to [illegible] sociological processes of coping with community problems, to socioeconomic problems of changing work methods used by factory employees.

In the theory which has evolved with respect to this process, behavior is treated as a physical entity subject to Lewinian forces and resistances.[2] The change agent's role is one of relaxing or unfreezing the resistive forces, exerting pressure on behavior in order to move (change) it to a preferred location, and then to re-establish the stabilizing environmental forces to ensure that behavior will be held (frozen) in its new position.[3] This

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[1] Lippett, J. Watson, B. Westley, The Dynamics of Planned Change, Harcourt Brace and Company, New York, 1958.

[2] K. Lewin, Field Theory in Social Science, D. Cartwright (ed.), New York, Harper, 1951

[3] K. Lewin, "Frontiers in Group Dynamics," Human Relations, 1947, pp. 5 - 41

[illegible] framework of problem solving routines which we have [illegible]. [illegible] consider unfreezing to be a name for those processes which [illegible] the application of problem solving routines, change to be the execution of these routines, and freezing to be the specification of a new response [illegible] these two theoretical views of the change phenomenon differ only in nomenclature. The empirical work done by those interested in the change phenomena suggests that change is a recognizable on - off process but no explicit mechanism has been hypothesized for its control.

Studies which come closer to suggesting a control mechanism for problem solving processes are reported in the psychological literature under the general topic, level of aspiration. While the original studies of this phenomenon were directed at quite different questions, several theorists have recently suggested this concept may be a useful one in discussing problem solving control processes.

In 1930 Hoppe suggested a new procedure for obtaining a behavioral measure of personality.[1] His procedure was carried out by Dembo in 1931 and she gave his measure the name, level of aspiration.[2] This name was strongly suggested by the experimental procedure: A subject in a laboratory situation was asked to perform a sequence of similar tasks where his performance could be measured along a single performance scale, e. g., time, distance, number of errors, etc. After completing each task the subject was given a measure of his performance (score) and asked to state the level [illegible]

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[1] F. Hoppe, "Erfolg und Misserfolg," Psychologische Forschung, [illegible] pp. 1 - 62.

[2] T. Dembo, 1931, "Der Arger als Dynamisches Problems," Psychologische Forschung 15, pp. 1 - 144.

performance he proposed in anticipation of the next [illegible] which he aspired on the next task was defined as his level of aspiration for that task. Dembo and all writers since have defined level of aspiration as "the level of performance in a familiar task which an individual explicitly undertakes to reach."

Over the course of the series of trials (1, 2, ...n.. N) which constituted an experiment the performance on each trial (p_n) was noted as well as the level of aspiration for the next trial (a_{n+1}). Following the completion of N trials the following statistic was evaluated from this data:

$$D = \sum_{n=1}^{N} (a_{n+1} - p_n) / N$$

This statistic was imaginatively named the average difference score for the subject for the experiment. Large values of the average difference scores were interpreted as evidence of such attributes of personality as self confidence, optimism, etc., while small or negative scores indicated fear of failure, lack of self confidence, etc. on the part of the subject. Many of the early experiments were concerned with questions of task independence and correlation with other measures of the same attributes. Evidence on both sides of each of these questions is reported.[1]

By 1941 enough interest and effort had been invested in studies of aspiration level phenomena that J. D. Frank published a review article in which he concluded:

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[1] J. W. Gardner, "The Relation of Certain Personality Variables to Level of Aspiration," Journal of Psychology, 1940, pp. 191 - 206.

> "The significance of the studies of the level of aspiration lies in their demonstration of a promising experimental approach to the problems of success and failure, of the formation of goals, and of the self and its relation to personality structure, achievement, and the social environment."[1]

It is interesting to observe that during the ten years from 1931 to 1941 some of the interest in the level of aspiration phenomenon as a basic descriptive statistic had shifted to the consideration at least that this measure might yield insight into such behavioral variables as goals. This trend continued.

In 1944 K. Lewin[2] and others again reviewed the literature on the level of aspiration and in addition suggested that the level of aspiration is an essential element in the understanding of behavior. They also suggested a theory of the process by which aspiration levels might be established. Their theory is teleological in that it predicts that the selected level of aspiration will maximize a criterion involving, (1) valence values which are subjectively associated with success and failure at various aspiration levels and (2) the subjective probabilities of success and failure at these aspiration levels. The theory suggests that that level of aspiration will be selected which will maximize expected valence. Except for terminology the analysis is quite similar to more recent discussions of statistical decision theory.

1/ J. D. Frank, "Recent Studies of Level of Aspiration," Psychological Bulletin, 1941, pp. 218 - 215.

2/ K. Lewin, T. Dembo, L. Festinger, and P. Sears, "Level of Aspiration," in J. M. Hunt (ed.) Personality and Behavior Disorders, Vol. I, Ronald Press, 1944, pp. 333 - 378.

In demonstrating their model the authors use probabilities and valence values suggested by some empirical work of Escalona,[1] Festinger,[2] and Gould and Lewin.[3] These investigators are reported to have found that the valences associated with success and failure change rapidly over that range of aspiration levels where the probability of success changes from near [illegible] to near zero. Outside this range both probabilities and valences are observed to change slowly

The authors claim neither empirical validity for their model nor that the process they describe is similar to the process subjects consciously use. They seem unconcerned about the latter point and optimistic about the possibility of the former. No report of an attempt to empirically validate the model has been discovered, however.

Since 1944 there have been a number of attempts to add the refreshing simplicity of aspiration levels to the somewhat tangled web of indifference curves implied by utility theory. H. A. Simon[4] suggested it to solve some of the observed psychological difficulties which utility models encounter as theories of behavior, and S. Spiegel[5] suggested it as an avenue for

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1/ S. K. Escalona, "The Effect of Success and Failure Upon the Level of Aspiration and Behavior in Manic Depressive Psychoses," University of Iowa Studies of Child Welfare, 16, 1940, No. 3, pp. 199 - 302

2/ L. Festinger, " A Theoretical Interpretation of Shifts in Levels of Aspiration," Psychological Review, 49, 1942, pp. 235 - 250.

3/ P. Gould and K. Lewin, Unpublished.

4/ H. A. Simon, Models of Man, New York: John Wiley & Sons, Inc., 1957, pp. 241 - 260.

5/ S. Siegel, Level of Aspiration and Decision Making," Psychological Review, Vol. 70, No. 1, January 1963, pp. 51 - 60.

experimental work on utility functions. A review of these and other attempts to marry level of aspiration phenomena with utility theory was published by W. H. Starbuck in 1963.[1]

Until 1958 virtually all attempts to include the concept of level of aspiration in theories of behavior took place in the context of [illegible] theories. The level of aspiration concept was used merely to suggest some attributes of the form of the criterion (utility) function. Behavior was still assumed (predicted) to be that which would maximize this criterion. Thus in these theories the level of aspiration affected behavior only through its effect on the form of the criterion function and no direct behavioral implications were hypothesized.

In 1958, however, J. G. March and H. A. Simon suggested quite a different use for the concept of aspiration level.[2] In outlining a mechanistic theory of human and organizational behavior, they found that they required a theoretical mechanism to control the rate of search (problem solving) activity that an organism would undertake. Working backwards from observed and introspective evidence they postulated a dynamic model which related the rate of search to the level of satisfaction through a variable they called the level of aspiration. While this name suggested the possibility of a relationship between this variable and the empirical work on this concept reported in the literature, the authors made no attempt to establish it.

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[1] W. H. Starbuck, "Levels of Aspiration," Psychological Review, Vol. 70, No. 1, January 1963, pp. 51 - 60.

[2] J. G. March and H. A. Simon, Organizations, New York: John Wiley & Sons, Inc., 1959, pp. 48 - 49.

Their model clearly suggests, however, that in the context of [illegible] theory the level of aspiration could have direct behavioral [illegible] might be observed

In 1963 R. M. Cyert and J. G. March use the concept of level of aspiration extensively in their mechanistic theory of the firm.[1/] They hypothesized:

1) that explicit levels of aspiration (goals) are maintained by members of an organization with respect to a large number of variables in which they have some interest.

2) that individual and organizational performance is compared to these aspiration levels to evaluate performance on a success-failure scale.

3) that failure to achieve the level of aspiration is the trigger for problem solving (search) activity. = DIFFERENCES

Thus Cyert and March suggest that their level of aspiration is a vital part of the mechanism required to explain problem solving control not only in some general sense, but also with respect to individual, and therefore, easily observable variables.

Basing their conjectures both on the published findings and their own empirical work Cyert and March go on to suggest the following dynamic properties for the level of aspiration.

1) In the steady state, aspiration level exceeds achievement by a small amount.

[1/] R. M. Cyert and J. G. March, *A Behavioral Theory of the Firm*: Englewood Cliffs, N. J., 1963, pp. 34 - 36.

2) Where achievement (performance) [illegible] rate the level of aspiration will exhibit [illegible] achievement

3) Where achievement decreases, aspiration level will be above achievement.

The authors point out that these propositions are derived from simpler assumptions about the process which generates aspiration levels. These consist principally of the assumption that current aspiration is the result of an optimistic extrapolation of past achievement and past aspiration.

The authors further suggest that a model which would completely explain the process by which levels of aspiration are generated might not completely describe the problem solving control process. In addition to this process they see the need for an attention focusing process which will permit the decision maker to attend at any one time to a sub-set of the set of variables in his environment. They suggest that it is the interaction of this focusing mechanism and an aspiration level modifying mechanism which will constitute the problem solving control process which they see as required. Throughout this discussion the authors emphasize the lack of empirical evidence with respect to the phenomena they are discussing and the tentative nature of their description.

Before summarizing our review of the literature relevant to problem solving control we should mention the extensive theoretical and empirical

work of H. A. Simon,[1] A. Newell,[2] F. M. Tonge,[3] and G. P. E. Clarkson.[4] By showing that mechanistic theories of human decision making processes are feasible alternatives to the more classical teleological theories [illegible] have provided both the impetus and philosophical basis for this study. [illegible] for very good reasons their work has been conducted in the context of [illegible] specific problem areas we feel it is not directly applicable to the question of how these problem areas are selected over time. Since this question [illegible] the one which concerns us here we will proceed in the context of the other theories which have been suggested to explain this phenomenon. We shall have occasion to return to this work on problem solving in later pages, however.

In the preceding pages we have attempted to present both a theoretical framework and a survey of the literature relevant to a discussion of problem solving control processes. We began by separating behavior into two broad categories--programmed behavior and unprogrammed behavior. The distinction between these categories is based on the extent to which behavior is based on ready made and available behavioral processes. We named these higher level routines problem solving processes. The question as to whether a particular situation calls forth programmed or unprogrammed behavior depends

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1/ H. A. Simon, The New Science of Management Decision, New York: Harper and Brothers, 1960, and "Theories of Decision Making in Economics and the Behavioral Sciences," American Economic Review, Vol. XLIX, June 1959, pp. 253-[illegible]

2/ A. Newell and H. A. Simon, "A General Problem Solving Program for a Computer," Computers and Automation, Vol. 8, July 1960, pp. 10-17, and A. Newell, J. C. Shaw, and H. A. Simon, "Empirical Exploration of the Logic Theory Machine," Proceeding of the Western Joint Computer Conference, February 1957, pp. [illegible], and The Simulation of Human Thought, op. cit.

3/ F. M. Tonge, A Heuristic Program for Assembly Line Balancing, Englewood Cliffs, N. J., Prentice-Hall, 196[illegible].

4/ G. P. E. Clarkson, Portfolio Selection: A Simulation of Trust Investment, Englewood Cliffs, N. J., Prentice-Hall, [illegible].

entirely on how the decision maker chooses to [illegible]. [illegible] his process of choice, however, seems to be important to theories of individual and organizational behavior.

Virtually all theorists who have considered this problem agree in the context of their own theoretical framework that problem solving is a reasonably well defined phenomenon. In the context of the human relations literature it has been called unfreezing or changing while those working on level of aspiration phenomena observe shifts in level of interest or effort in accomplishing various levels of performance. Thus problem solving appears to be a relatively observable on-off phenomenon.

In the context of teleological theory the theorists have been led to postulate discontinuous utility functions to explain the shift in behavior due to the evoking of problem solving routines, and some have argued that this concept may be useful in discovering some empirically valid basis for teleological theories of behavior.

Those working in the context of mechanistic theory on the other hand have postulated that problem solving control can be explained by a process closely similar to that which generates the level of aspiration. This process establishes criteria whereby the level of performance on various environmental variables can be evaluated on a success-failure scale and the conjecture has been suggested that failure is the trigger for problem solving processes.

IV. DESIGN OF AN EXPERIMENTAL SITUATION

In view of the lack of [illegible] to the hypothesis that success and failure are the critical variables in the control of problem solving activity, we designed an experimental situation in which this hypothesis could be tested. In the course of design it was necessary, however, to consider theories of behavior outside this specific area in order that the effects of other processes might be controlled. In describing the experimental situation, therefore, it seems appropriate to review briefly the structure of the over-all process which was hypothesized prior to the design.

If we assume that the human organism is capable of receiving only a small sample of the total information available from the environment, and further that it is capable of processing (attending to) only a small fraction of the information which it can receive and that this processed information is the basis for behavior, then to understand behavior we need a theory of how the limited information processing capacity is allocated to the received information and how the limited receiving capacity is allocated to the available information. The hypothesis used for design purposes was that two coding processes could provide at least the framework for such a theory.

The first coding process is that of categorization. This is a process whereby the decision maker codes the raw data he receives into what might be called variable classes. For example, an executive in a firm attends to a set of reports on such classes of variables as defect rates, share of market, labor cost per unit, work force size, forecast demand, etc. These variable classes contain far less information than is available in his environment and as such his system of categories constitutes a filter by which he controls the information he receives. Other examples can be

considered which suggest that the [illegible] process [illegible] filtering method not confined to executives or managerial behavior.

The process by which categories are constructed is described in the psychological literature under the general title of concept formation and has for the most part been studied in laboratory situations where other parts of the decision process could be largely suppressed.[1] This process has also received attention under the general name, pattern recognition, and reports of work on this process have emerged from an interesting variety of theoretical disciplines.[2] It seems clear that the theories of this process whether under the name concept formation or pattern recognition will fit into a general theory of decision making as the first of the two coding processes being described here.

If we assume that information from the environment is coded into a set of measures on a well defined (but not necessarily constant) set of variable classes, the next problem facing the decision maker is to allocate his limited processing abilities over this set of variables. It has been suggested that this allocation is accomplished by the second coding process. This process is one which codes variable classes into two categories by means of values called aspiration levels or goals.[3]

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[1] For a description of this work see: Bruner, Goodnow, and Austin, A Study of Thinking, Wiley, 1956

[2] H. D. Block, "The Perceptron: A Model for Brain Functioning I," Reviews of Modern Physics, Vol. 34, No. 1, January 1962, pp. 123 - 142.

M. Minsky, "Steps Toward Artificial Intelligence," Proceedings of the IRE, Computer Issue, Vol 49, No. 1, January 1961, pp. 8 - 30.

[3] See pp. 12 for a discussion of this use of the goal concept.

Measures on variable classes are constantly received from the environment, e. g., workforce size - 100 men, estimated share of market - 62%, etc. These measures are compared with goal values on the appropriate variable class and, at least as a first approximation, a simple categorization results. If a measure exceeds its goal value (where exceed is defined within the definition of the variable class) this variable class is a member of the set of variable classes which requires no further information processing. If, on the other hand, a goal exceeds the measure received, this variable class is a member of the set of variable classes which constitutes problems, and requires information processing of the problem solving type, i. e., find a behavior routine to reduce the difference between the measure and the goal.[1/] The execution of the routine discovered by this processing contitutes observable behavior.

The existence of goals on each class of variables does not guarantee, however, that the information processing called for will bear any particular resemblance to the information processing capacity of the decision maker. A particular setting of goal values could overload or leave idle the problem solving mechanism. These two possibilities can be considered separately.

Assume for the moment that goal settings are such that demands for information processing exceed the decision maker's capacity to do this work. Two mechanisms for solving this type of allocation problem exist. One first is to ignore one or more variable classes and thereby remain

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1/ A. Newell and H. A. Simon, "A General Problem Solving Program for a Computer," Computers and Automation, Vol. 8, July 1960, pp. 10 - 17.

ignorant of the fact that capacity is exceeded. The second is to reduce goals downward in such a way as to bring the number of problems within the capacity of the processing system. It is suggested that both methods of resolving this problem may be used.

Assuming conversely that the information processing capacity is not being fully utilized at a given goal setting, two alternatives are also open to the decision maker. He can either enlarge the number of variables to which he will attend or he can raise goals on the existing classes. Here, too, it is suggested that both mechanisms may be used.

While the four mechanisms just described would produce an allocation of processing capability, these descriptions offer no insight into the process by which these mechanisms accomplish this allocation. It is, therefore, to the problem of goal modification and the problem of controlling the set of variable classes to which we will now turn.

Assuming as a first approximation that the information receiving and processing system is sequential, a single sequential process must be capable of performing the work of the four mechanisms just described.[1]

While it is probably true that the categorization process described earlier is carried on in conjunction with the modification of goals and variable sets, we assumed for simplicity that the total set of variable classes is given and will remain fixed. The following theory was hypothesized for the mechanisms described above. (See figure 1).

The set of all variable classes and associated goal values is stored in a memory device. The decision maker, aided by his environment, has a

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[1] For a general discussion of these mechanisms and their implications for behavior see: March and Simon, Organizations, Wiley, 1959.

priority scheme which governs the sequence in which he will attend to the various variable classes. Schemes such as a series of reports at different intervals like weekly labor reports, daily accident reports, and the annual reports of a subsidiary, are suggestive of formal procedures used to accomplish this sequencing. When a measure is received it is evaluated by comparing it with the stored goal value for that variable. If the measure exceeds the goal, performance is satisfactory and no problem solving is undertaken. The goal value which defines success and failure may be modified as a result of this experience, however, and the new goal value is stored for the next time it is required. It is by this modification process that goals might appear to drift upward on success.

If on the other hand the received measure fails to meet the established goal value, problem solving is called for. When this process is complete an action program may be undertaken which will require some time to have its effect. The goal value for this variable is modified in accordance with the outcome predicted by the problem solving routine and stored for the next reading on performance.[1]

One can make some general propositions about the performance of such a routine. For example, one would expect the loop involving satisfactory performance to require less time to execute than the loop involving unsatisfactory performance and the concommitant problem solving routines. Thus when the environment yields measures which exceed their associated goal values, one would expect it would be possible if the environment were

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[1] It should be emphasized that in the context of this theory success and failure are the controls on problem solving and the goal modification process merely changes those measures which define successes and failures

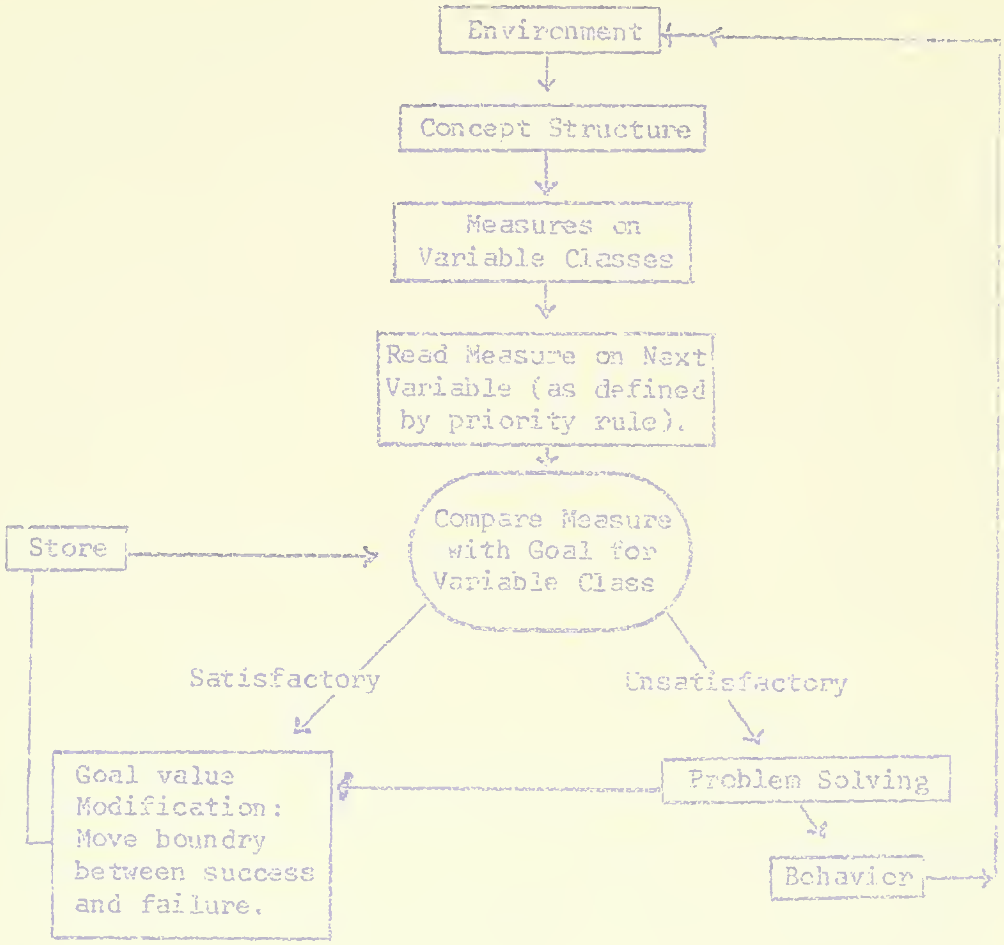
Environment
Concept Structure
Measures on
Variable Classes
Read Measure on Next
Variable (as defined
by priority rule).
Store
Compare Measure
with Goal for
Variable Class
Satisfactory
Unsatisfactory
Goal value
Modification:
Move boundry
between success
and failure.
Problem Solving
Behavior

FIGURE 1.

providing measures below goal values.

It is interesting to note that the decision maker himself determines whether the environmental measures are above or below the associated goal values through his choice of a goal modification rule. If, for example, a measure which is satisfactory under the existing goal results in the goal being sharply raised before it is stored, it is quite likely that over time this variable will become unsatisfactory and thus constitute a problem in the future. Similarly, if the goal associated with a variable class which is currently a problem is sharply lowered when corrective action is undertaken, this action will almost surely be successful. Thus the goal modification process interacts with the priority process and the environment in controlling the set of variables to which the decision maker will attend and the amount of problem solving activity he will undertake.

To summarize, three processes have been described which when acting together control both the information which the decision maker receives and the set of variables to which he will allocate his limited problem solving ability. The first of these processes is that of categorization. The second is a priority assignment process, and the third is a process of goal modification. The existence of these three processes is the basic hypothesis which guided the design of our experimental situation.

Since it appeared to be possible to control for the effects of these three processes we decided to attempt to test one of the assumptions of this theory. More specifically we decided to test the hypothesis that problem solving is controlled by failure to achieve a particular performance level on a perceived variable.

Our experimental procedure was as follows. After reading a prepared set of instructions,[1] a subject was asked to indicate a number in a blank space opposite trial number one on a data sheet[2] on a desk before him. The data sheet was laid out to provide for one hundred trials and the subject understood that additional sheets were freely available if he required them. He also understood from the instructions that the experimenter would compare the number he (the subject) wrote for trial number one (and all succeeding trials) to a corresponding number(s) on a previously prepared list of numbers which the subject could not see. If the experimenter found in this comparison that the subject's number was higher than that on his prepared list, he announced that the subject had lost on that trial. If on the other hand the subject's number was lower than the number on the prepared list the experimenter announced that the subject had won on that trial. The subject was instructed to circle the number he had written down on all trials where he was told he had won. In either event the subject proceeded at his own speed to write down a number for the next trial. This process continued until the subject had won fifty times. No limit was imposed on the total number of trials.

The instructions read by the subject before the first trial also contained information about a system of rewards which he could associate with his performance. He read that each trial would cost him a fixed fee specified by the experimenter prior to the first trial. Thus if, as in the case of most subjects, the fee was $.10 per trial, fifty wins in one hundred trials would cost the subject $10, fifty wins in fifty trials would

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[1] See Appendix A.

[2] See Appendix B.

cost $5, etc. On the revenue side, the subject understood that he would win the face value of the number he wrote down on each trial he won. Thus at the end of the experiment the subject could determine his total revenue by adding those fifty numbers he had circled (won) over the course of the experiment. The difference between this total and the total trial fee would be his net earnings for the experiment. The instructions included a sample calculation to clarify these revenues and costs. Subjects were instructed to attempt to make as large net earnings as they could but understood that they would be paid a fixed fee for their participation in the experiment.

With respect to the numbers on the experimenter's prepared list the instructions indicated that they had been determined by choosing at random and with replacement from a carefully selected set of numbers. No information regarding any parameters of this set of numbers was given the subject, however.

Figure 2 indicates the relationship believed to exist between this experimental procedure and the theory discussed on pages 26 and 32 and represented in Figure 1. Figure 1 is reproduced in Figure 2 over the title "Theory" for convenience. The other diagram in Figure 2 represents the same process as it would apply to the experiment.

We argue as follows: The environment of the subject was largely determined and controlled by the experimental situation. Variations in this environment were of course introduced into the experimental situation by variations in the subject's experience and perception of the instructions but the systematic effect of these variations were hopefully controlled by observing the behavior of several subjects.

Large parts of the subjects' concept structure were given in the instructions. The relationship between the numbers he wrote on each trial

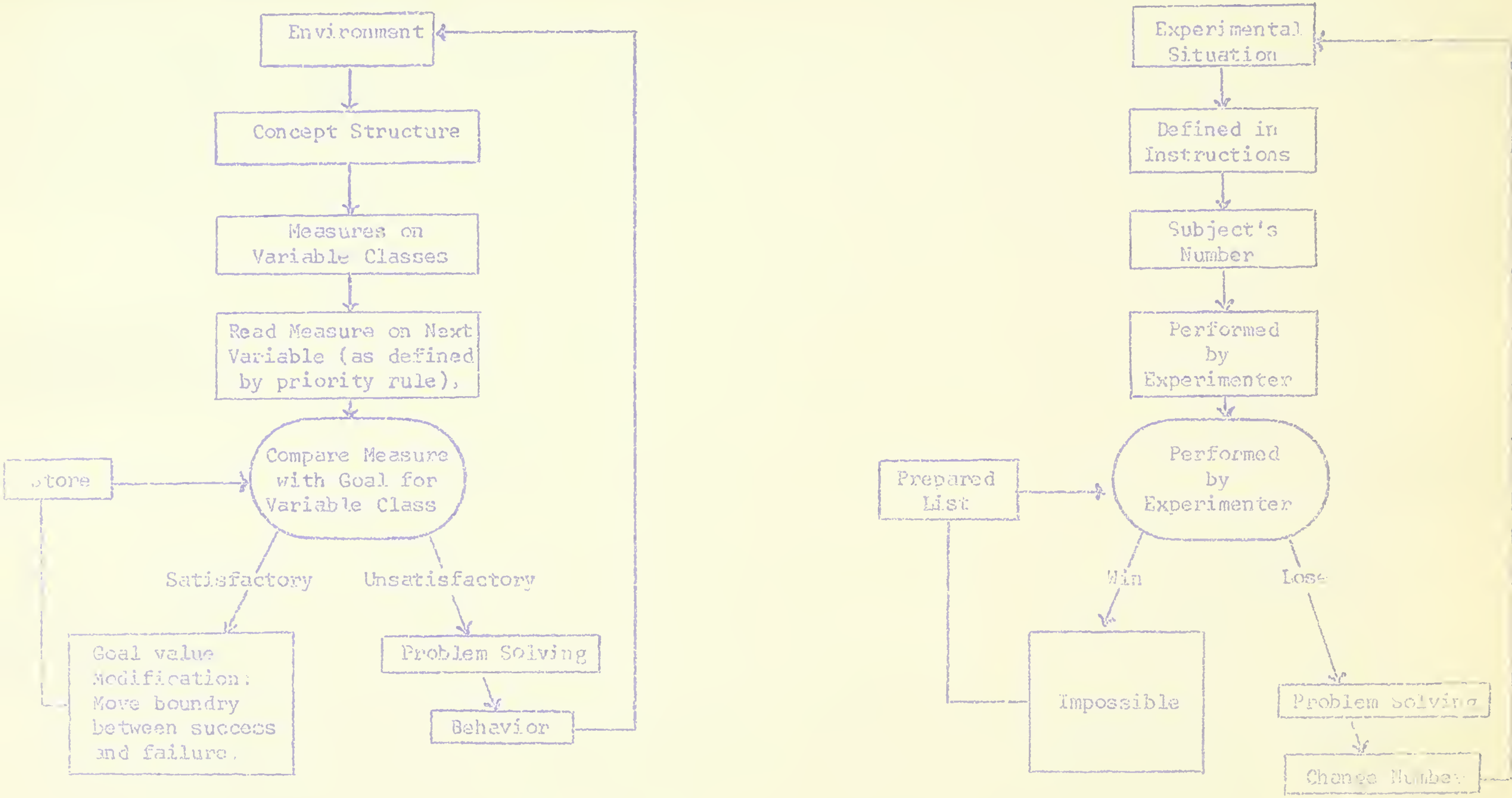
Environment
Concept Structure
Measures on Variable Classes
Read Measure on Next Variable (as defined by priority rule),
Compare Measure with Goal for Variable Class
Store
Satisfactory
Unsatisfactory
Goal value Modification: Move boundry between success and failure.
Problem Solving
Behavior
Experimental Situation
Defined in Instructions
Subject's Number
Performed by Experimenter
Performed by Experimenter
Prepared List
Win
Lose
Impossible
Problem Solving
Change Number

FIGURE 2

the numbers on the experimenter's list, and the [illegible] of [illegible], together with a description of the application of these terms for a particular criterion were carefully explained.

Measures on the only significant variable in the experimental situation were well defined in the instructions. The subject controlled completely the number he wrote on his data sheet while the goal value which would determine the success or failure of this activity appeared on the experimenter's list. Thus measures and criteria of success and failure (win - lose) were clearly present.

Since the subject was not told, in general, the number on the experimenter's list which partly determined the outcome of each trial, he could not modify his perception of success and failure with this knowledge. For example, if he wrote the number 1.74 and had known that he lost because the experimenter's number for that trial was 1.73 he might have considered this a less serious loss than if he had known the experimenter's number had been .45. By withholding information about the numbers on the experimenter's list, therefore, it was possible to focus clearly on the experimental variable of success (win) or failure (lose) and the subject had no part in defining this variable once he had selected a particular number.

Under our hypothesis this variable (success or failure) was predicted to control the subject's problem solving routines. It was predicted that problem solving routines would be evoked and applied only when behavior (the number written down on the data sheet) failed to yield satisfactory performance. The application of these routines, however, was difficult to observe even when the subjects attempted to describe the process of their decision with respect to the number for the next trial. It was

decided, therefore, to approximate this observation by assuming that a change in a subject's number was evidence of the effect of problem solving processes modifying the routine which had yielded the previous number, while no change in the subject's number would be evidence of no problem solving modification. This assumption presumed that a change in routine would always yield a change in behavior which was, of course, not in general true. This assumption was not important in the case of most subjects, however, for they seldom left their number unchanged from trial to trial.

To summarize, the design consisted of an extremely simple experimental situation where a subject's application of problem solving routines could be observed under conditions of success and failure. The hypothesis to be tested was one which suggests that failure is the trigger for problem solving activity.

V ANALYSIS AND DISCUSSION OF EXPERIMENTAL RESULTS

The experimental situation and procedure described in the preceding chapter was designed to test the hypothesis that problem solving processes are controlled by the success or failure of a behavioral routine. Under this procedure a subject was placed in a situation where his behavioral routine yielded a specific output, i. e., a number. The experimenter evaluated the success or failure of the subject's routine by comparing the number which it yielded with a previously prepared number on a list corresponding to the series of experimental trials. The subject received only success-failure feedback on the output of his behavioral routine. If the subject changed his number from the preceeding trial, this was accepted as evidence of problem solving modification of the behavioral routine which had yielded the previous number; if the subject did not change his number from trial to trial it was assumed that no problem solving modification had taken place. Under the hypothesis no change in number was predicted following successful numbers and a change in number was predicted following unsuccessful numbers.

Thirty-two subjects participated in variations of the basic experiment. These variations consisted of changes in some of the economic parameters of the experimental situation not relevant to the hypothesis now under discussion.

These thirty-two subjects were drawn from among the graduate students and the faculty of the School of Industrial Management at Massachusetts Institute of Technology and as such probably do not constitute a random sample of the population of the United States. The conclusions which can be drawn from the experiments therefore must be interpreted in light of the sample evidence on which they are based.

The series of thirty-two experiments yielded 4052 observations of the effect of success and failure on the application of problem solving routines. Of these 1728 trials were successful (wins) and the remaining 2324 trials were failures (losses).

Since our hypothesis was quite specific we could easily describe the critical attributes of data on number changes. The hypothesis predicted no (0) number changes would occur following those 1728 trials on which the number was successful and, under the assumption mentioned earlier, 2324 changes in number were predicted following those 2324 trials on which the number was unsuccessful. The data did not support these predictions. Instead of no (0) changes following the 1728 trials on which the subjects' numbers were successful there were 998 changes. Instead of 2324 changes following the 2324 trials on which the subjects' numbers were unsuccessful there were only 831 changes. While the latter result could conceivably have been due to problem solving modifications which resulted in no change in behavior, the former result is unexplainable under the hypothesis and we were forced, therefore, to reject it.

As is always the case when an attempt is made to establish the empirical validity of a proposition, not only the proposition itself is tested but also all the auxiliary propositions which were necessary to the testing procedure.[1] When a test is successful it is easy to focus on the major proposition as the meaningful result and to ignore those supporting propositions which were also

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[1] M. R. Cohen and E. Nagel, An Introduction to Logic and the Scientific Method, New York, Harcourt, Brace & Co., 1934, pp. 219 - 220.

tested. When a test is unsuccessful, however, it is in most cases necessary to question all aspects of the experimental procedure before rejecting the major proposition. But in doing so we must guard against the temptation to always attribute failures to the auxiliary assumptions. For by this process we can indefinitely protect our major propositions from confrontation by test and lose thereby the principal advantage of empirical research. We must at some point let the data tell us if a proposition is false even though it is our own.

In view of the fact that the test we have just described failed to be supported by empirical results it seemed appropriate that we review and re-evaluate some aspects of the design.

We had attempted to test the influence of success and failure on problem solving control. In the theory of problem solving control which suggested our hypothesis, what the subject will consider to be success and failure is not made clear. In our design we assumed that these attributes, i. e., the success or failure, of the subject's behavioral routine were determined completely by the feedback of win-lose information by the experimenter. We also assumed that the subject would consider his behavioral routine to have been successful when he heard the word win and unsuccessful when he heard lose. If this assumption were incorrect, it could very well nullify our result and indicate a more appropriate test should be devised.

Since the theory does not make explicit the process by which the subject will identify when his behavioral routine has been successful, it is untestable until propositions which define these evaluations are added. Clearly a large number of such propositions are possible and the test which is appropriate to validate the completed theory will depend to a considerable extent on the

propositions selected

It should not be inferred from the fact that a large number of propositions can be added to the theory to make it testable, that any proposition is a valid candidate. A proposition which relates the definition of success and failure to subsequent evidence of problem solving control processes would be an inappropriate addition. For example, if we assert that those behavioral routines are successful which are not followed by problem solving and that those behavioral routines are unsuccessful which are followed by problem solving we have turned a potentially testable hypothesis into a tautology. We must therefore seek a definition of success and failure which is independent of the process we wish to explain.

The proposition we selected for purposes of our test was that a subject in this experiment would associate success with a win on a single trial and failure with a loss. After rejecting the hypothesis including this proposition, we tried several other propositions to see if we could find one that would simultaneously be independent of the problem solving phenomenon and be supported by the data. We tried for example the proposition that success is a function of the outcomes of the previous two trials. We predicted that only if both numbers on these trials were wins would the subject consider the number successful. Under all other circumstances we predicted he would consider the number a failure. In the course of such exercises in ingeniousness we were encouraged from time to time but in each case we found as we attempted to explain more and more data we had to add more and more ad hoc propositions. In each case we eventually violated our own standard of simplicity for a useful proposition and were forced to reject

the major hypothesis. As Douglas Gasking[1] has pointed out, however, "what we find neat, simple, easy, and intellectually satisfying surely depends rather on our psychological makeup, than on the behavior of measuring rods, solids and fluids, electrical charges--the 'external world'." This comment made about theorizing the physical sciences seems appropriate to theorizing in general. Within our standards at least we found no empirically valid proposition.

Just as the theory of problem solving control does not suggest an operational definition of success and failure, it also does not specify the variables on which these attributes will be defined. In our test we assumed that they were defined on the output of the subject's behavioral routine, i. e., the number he wrote down for each trial. In view of our experience, we decided to admit defeat with respect to our search for independent definitions of success and failure which would explain problem solving control in terms of this variable and turned instead to the consideration of other variables. One set of these was suggested by a review of subject protocols recorded during the experiments.

Suppose following each trial the subject evaluated his experience on that trial with respect to two measures. Suppose these measures were each two valued:

Measure I → Win / Lose

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[1] Douglas Gasking, "Mathematics and the World," in J. R. Newman (ed.), The World of Mathematics, Vol. 3, New York, 1956, pp. 1708 - 1722.

Measure II — Win More / Win Same

Suppose further that these measures were evaluated sequentially and that on each level, success and failure were associated with each value of the measure as follows:

Measure I — Win (Success) / Lose (Failure)

Measure II — Win More (Success) / Win Same (Failure)

The process of evaluation of success and failure would proceed as follows:

Suppose the subject had just lost on Trial 20 with the number 1.47. He would have failed with respect to Measure I and under the theory we would predict problem solving prior to Trial 21.

Suppose on the other hand the subject had just won on Trial 20 with the number 1.47. He would have succeeded with respect to Measure I but he would have failed in Measure II because he did not win on a number higher than 1.47 which was conceivably possible. Thus he would undertake problem solving to pick a number for Trial number 2.

Clearly these propositions suggested that in our experiment the subject would undertake problem solving on every trial because under no circumstance could he succeed simultaneously with respect to both measures. Since these

propositions were independent of the process of [illegible] they could be added to the theory which predicts [illegible] problem solving to make that theory testable.

Under this form of the theory our previous predictions for the [illegible] of our thirty-two experiments were considerably changed. Under the new propositions we predicted that problem solving would occur between every trial and, admitting the possibility that problem solving changes might have no immediate behavioral effect, we predicted 4052 or fewer "number changes" over the course of these experiments. The data we gathered is not inconsistent with this hypothesis. We observed 1829 changes.

Since any outcome of our experiment would have supported the revised hypothesis our enthusiasm over this result was considerably restrained. We had modified the hypothesis to the point where it was no longer testable in the context of the situation we had designed. This did not imply that the new hypothesis was untested. Despite this outcome we did not conclude that the results of the first series of experiments had been useless. It had led us to reject several propositions which we might otherwise have thought were true and in addition suggested the need to test a different set of propositions.

Before proceeding to the design of a new test, however, it seemed appropriate to review the progress of our investigation. We had tested and rejected the hypothesis that problem solving is controlled by the success or failure of a single output of a behavioral process. We were led to hypothesize instead that problem solving will occur with respect to one of two variables defined on the output of the subject's behavior routine on every trial. This new hypothesis, while untested, suggested a disturbing possibility about the procedure used in the first experiment.

Assume for the moment that the new hypothesis is true: problem solving does take place following each trial. In the first experiment we gave the subject only one variable because that was the simplest case and completely tested the hypothesis we held at that time. In doing so we also forced the subject to focus his problem solving on that variable simply because no other was available. It might well have been for this reason that problem solving was demonstrated on successful trials as well as on failure. It simply had no where else to be applied.

This argument suggested a simple modification in the experimental procedure which would remove that constraint and provide the subject, under some circumstances at least, a choice between applying his problem solving routines to a successful variable or an unsuccessful one. The modification was the following:

In an experimental situation much like the one described above a subject was asked to specify two numbers on each trial. The experimenter compared these numbers to corresponding numbers on two previously prepared lists. Using the same rules as before he informed the subject he had won or lost on each number. The subject circled those numbers (if any) on which he won and proceeded to the next trial. The reward system in this experiment was such that the subject won the sum of the face values of those numbers on which he had won at the completion of a specified number of trials. There was one more fundamental change. In the first experiment the subject was free to change his number from trial to trial as he liked. In fact, for purposes of our test, whether or not he changed his number was the variable of interest. In the second experiment, however, we imposed a constraint on his behavior. In proceeding from the numbers he had written

on one trial to those he wrote on the next, the subject was free to change at most one of his two numbers. The other one had to remain unchanged. He was free to leave both unchanged if he liked. He was also free to choose the number to change and the amount it would be changed. But at least one number on each trial had to be the same as it had been on the previous trial.

We employed this constraint for two reasons. First we added it to force an explicit choice of that variable to which problem solving would be applied. For example if on Trial 20 the subject had written 1.47 and 2.15 and the experimenter had told him he had lost on 1.47 but had won on 2.15, under the imposed constraint the subject would have to choose between problem solving (changing) on a variable which was successful and one which was a failure. Without this constraint our earlier observations indicated he might be led to change both numbers. The constraint was added, therefore, to guarantee data which would test the hypothesis that failure is the trigger for problem solving. On those trials where both numbers win or lose the hypothesis makes no prediction and these trials therefore would offer no test of the theory.

The second reason we added the constraint was because we felt it more realistically represented the problem of problem solving control faced by decision makers outside our experimental situation. Most decision makers (humans) over time face a large number of variables in their environment. Due to the apparently serial nature of parts of their information processing mechanism, however, they attend to these variables virtually one at a time. By forcing a choice between alternative variables, therefore, we believe the constraint made the experimental situation more "realistic."

To summarize, a subject in the second two variable experimental situation was asked to produce two numbers on each of a series of trials. Following each trial the experimenter told the subject whether either or both of these numbers had been successful under criteria which had been previously established. The subject was then asked to produce two more numbers one of which was required to be the same as the corresponding number on the previous trial. The subject was expected to attempt to maximize the sum of those numbers on which he won over the course of the experiment.

The hypothesis which led to this design consisted of the following propositions:

1. A change in number from trial to trial is evidence of problem solving.
2. Subjects use the outcome of the last trial to determine the success or failure of the behavioral routine which produced it.
3. On those trials where there is a choice subjects will undertake problem solving where their behavioral routine has been unsuccessful.

In the context of our second experimental situation the test of this hypothesis was planned as follows:

1. Run a subject in the experiment.
2. Attend to those trials where one of the subject's numbers had won and the other had lost.
3. Predict a number change in these situations on that number which had lost.

We conducted this test with subject A, a graduate student in Industrial Management at M.I.T. Of the 100 trials which constituted the experiment, both numbers the subject specified were successful (won) on [illegible] trials, neither was successful on 11 trials, and one of the two numbers was successful on 43 trials.

The subject changed one of his two numbers, i. e., exhibited evidence of problem solving, on 96 of the 99 occasions on which he could do so. This observation supported to some extent at least our conjecture that problem solving takes place between every trial regardless of its success or failure.

The hypothesis we were testing, however, was concerned only with the focus of problem solving on those 43 trials on which one number was successful and the other was a failure. We predicted that we would observe 43 number changes on those numbers which were unsuccessful and no (0) changes on those numbers which were successful.

The data did not support this hypothesis. We observed 30 number changes on those numbers which were unsuccessful (versus a predicted 43), 10 number changes on those numbers which were successful (versus a predicted 0), and three trials where neither number was changed (versus a predicted 0). We could explain the latter result in the same way we explained no change in number in our earlier experiment, butthose ten instances where the subject focussed his problem solving on the successful variable when faced with a choice between successful and unsuccessful variables clearly violated our hypothesis.

We repeated this experiment with 19 other subjects drawn both from M.I.T.'s School of Industrial Management and from the Harvard Business School with the results shown in Table I.

Subject	Number of Trials	Number of Trials [illegible] Problem Solved [illegible] demonstrated	Number of trials where [illegible] numbers won	Number of trials where [illegible] (1) number won	Number of Trials where [illegible] (0) number won	Number of number changes on win	Number of number changes on lose	[illegible]
A	100	96	46	43	11	10	30	[illegible]
B	75	53	45	23	7	3	18	[illegible]
C	75	36	38	32	5	4	17	[illegible]
D	50	34	31	16	[illegible]	5	[illegible]	[illegible]
E	75	74	40	26	9	7	15	[illegible]
F	50	49	32	16	2	0	16	0
G	50	38	[illegible]	22	[illegible]	2	19	[illegible]
H	75	[illegible]	28	30	17	9	18	[illegible]
I	50	47	20	25	5	15	9	[illegible]
J	50	47	19	22	9	11	9	[illegible]
K	50	32	29	18	[illegible]	2	16	[illegible]
L	50	47	15	28	7	13	14	[illegible]
M	50	36	27	19	4	2	15	[illegible]
N	50	49	14	29	7	10	16	[illegible]
O	50	49	34	15	1	0	14	[illegible]
P	50	49	14	26	10	18	8	0
Q	75	25	23	30	22	1	5	[illegible]
R	50	48	19	23	8	[illegible]	7	[illegible]
S	50	49	21	25	4	7	17	[illegible]
T	50	49	34	15	1	0	14	[illegible]

Table [illegible] Results in second [illegible] experiment

Of the 20 subjects only three (F, O, and I) behaved as predicted under our hypothesis. We were forced to conclude, therefore, that the hypothesis did not offer a general explanation of the behavior of those subjects who participated in our experiment.

Until this point in our investigation our hypothesis had been, as suggested by the theory from which they were derived, completely deterministic. We predicted failure was the trigger for problem solving and clearly one observation to the contrary was sufficient to cause the rejection of such a hypothesis. As we have seen our experimental procedure yielded data which far exceeded this minimum criterion. A simple mechanism based on success and failure (at least as we were able to define and observe them) seemed to have been clearly demonstrated to be an insufficient explanation of the problem solving control processes demonstrated by those who participated in our experiments. We reconsidered both our general theory and our specific hypothesis in light of these empirical results.

In the theory which we accepted as the basis for our experimental design, problem solving is applied to a variable if a perceived measure of it (the variable) fails to meet a performance standard (aspiration level or goal value) which the decision maker has established for that variable. We saw in our first experiment, however, that problem solving seemed to be applied continuously to a single variable independent of its performance. It was not until our second experiment where we added a constraint on the subject's problem solving ability which did not permit him to attend to all the variables in his environment, that we were able to induce behavior which appeared to be problem solving control. In the second experiment the subject appeared to withhold his problem solving capability from one variable and to

devote it to the other. In the context of received theory we interpreted this behavior as the result of satisfaction with one variable and dissatisfaction with the other and it was also in the context of that theory we searched for a mechanism which independently modified the definition of success and failure in such a way that we could predict the focus of problem solving attention.

So long as we are concerned with only <u>one</u> variable in the decision maker's environment, the possibility of discovering such a binary mechanism has some appeal. The mechanism merely has to predict one of two mutually exclusive attributes of the variable; i. e., when it calls forth problem solving and when it doesn't. Despite the fact that we have not been able to discover such a mechanism, its existence does not appear to be a logical impossibility.

As our interest extends to other variables in the decision maker's environment, however, the appeal of a <u>set</u> of such independent mechanisms fades considerably. In this case we must hypothesize a set of mechanisms which independently defines success and failure in such a way that only <u>one</u> variable will be judged a failure at a time. The point here seems to be an important one.

So long as we are interested in a system which can be in either of two states, i. e., problem solving or not problem solving on a single variable, a binary attribute can easily discriminate between the events of interest. When the system can be in either of two states on each of several variables, a set of independent binary attributes can discriminate between the events of interest on each variable. When we add the constraint, however, that the system can be in a specific state, i. e., problem solving, on at <u>most</u> one

of the variables at a time, it becomes unlikely indeed that a set of independent binary mechanisms would in general accomplish the required discrimination. It seemed so unlikely, in fact, that we were led to reject this theory of problem solving control.

In summary, we have designed and executed tests of hypotheses which suggest that failure is a trigger for problem solving activity. We found in the case of a single variable that success also triggers problem solving and were led to conjecture that problem solving may be continuous. By adding a second variable to the experimental environment and by placing a contraint on the subject's problem solving ability, we were able to test the failure hypothesis in a situation where the subject had to choose between a successful and an unsuccessful variable. The hypothesis was again rejected. Finally, we argued theoretically that the discrimination required to allocate problem solving in a multivariable environment exceeds that which could be reasonably expected from independent processes of success-failure determination. In the next chapter we shall consider the possibility of non-independent processes of problem solving control.

VI COGITATION AND EXPLORATION

We have argued that the process by which decision makers select the variables to which they will devote their limited problem solving ability is important to our understanding of individual and organizational behavior. We have designed two experimental situations where this behavior could be observed in subjects under reasonably controlled conditions.

We found that when faced with a single variable environment, subjects demonstrated no need for problem solving control. They appeared to apply these processes continuously to the variable at hand.

When we increased the number of variables to two, however, and imposed a constraint on problem solving, we found that the control behavior in which we were interested was demonstrated. It was easy to show that independently defined attributes of success and failure were in general insufficient to provide the discrimination required by the decision maker's limited problem solving ability. On the other hand, once this discrimination was accomplished and behavior executed, it would always be possible to describe problem solving control in terms of success and failure. As we pointed out earlier, such a description would be tautological. We rejected, therefore, our theory which suggested that success and failure are _the_ controlling attributes in the process of problem solving control.

In attempting to formulate a new hypothesis we found two main ideas suggested by our empirical work most helpful. The first of these was that problem solving could be considered a continuous process. This suggested that the control mechanism could be conceived of simply as a process which allocates or focusses problem solving on a specific variable. As a result we could shift the question of the intensity of problem solving to those

routines which would be selected to deal with this variable. The [illegible] idea was that problem solving control includes a consideration of [illegible] of attributes of the decision maker's environment. Since almost any environment contains a large number of attributes we can consider those which the subject selects for discrimination to be a sample of those available. Similarly we can then consider any hypothesis which we might construct to contain a sample of attributes from the same environment. In testing a specific hypothesis, therefore, it seemed possible to be able to compare the number of correct predictions yielded by our hypothesis to that which would have been generated by a random sample of attributes. In other words the idea that the subject's control process must be based on a sample from a large population seems to suggest the possibility of testing our hypothesis against an "equally likely" alternative hypothesis.

We must hasten to point out, however, that this change in experimental procedure does not reflect a change in our confidence that behavior is deterministic. We would not argue that our subject's behavior was random. We accepted only the shortcomings of our own understanding.

As an example of this testing procedure, consider the following process of problem solving control which can be used to explain behavior in the second (two variable) experiment described in the last chapter.

1. If on the last trial one number won and one number lost, problem solving will be allocated to the number which lost.

2. If on the last trial both numbers won but on the preceding trial only one number won, problem solving will be allocated to the number which won on the preceding trial.

3. If on the last trial both numbers won and on the preceding trial neither or both numbers won, problem solving will be allocated to the number which was not changed from the previous trial.

4. If on the last trial both numbers lost but on the preceding trial only one number lost, problem solving will be allocated to the number which lost on the preceding trial.

5. If on the last trial both numbers lost and on the preceding trial neither or both numbers lost, problem solving will be allocated to the number which was changed from the previous trial.

This particular process of control is based on some protocol data which suggests that some subjects attend primarily to losses. Only when both numbers are winning do they attend to winning numbers. In case both numbers are winning, they attend to each number in turn. In case both numbers are losing, however, they attend to one number until it wins.

If we apply this process to the data which the subject had before him on each trial in our second (two variable) experiment, we find in the case of subject G we correctly predict the focus of his problem solving on 36 of the 36 trials on which problem solving was demonstrated and the process we have specified would make an explicit prediction. Under the hypothesis that problem solving was allocated by chance, this number of correct predictions in a sample of 36 observations would occur less frequently than 1 time in 1000.

We make no claim of generality for this process although it perfromed reasonably well (10% level of significance or better) on several other subjects. We include it only to indicate the complexity of the control process which is required to allocate problem solving in even so simple a situation.

as this one designed to limit the subject's environment to only two variables.

In order to get a somewhat more valid insight into the nature of the problem solving control process which subjects use in this simple situation, we undertook to have subjects describe their own process by the following procedure. We told individual subjects that they would participate in an experiment exactly like that described as the second (two variable) experiment in the last chapter. The difference was that they would participate only through a rule which they would have to specify in advance. In order to give them some basis on which to prepare their rule, they were given the opportunity to perform a number of trials "by hand". In some cases no limit was placed on the number of these practice trials. In other cases this number was limited to conserve time.

Twenty-one subjects prepared rules by which they were willing to earn the rewards offered by the experiment. (These rewards in the range of one to four dollars were actually paid on the basis of the performance of the subject's rule.) Three of the rules which resulted are shown below and several others are shown in Appendix D.

Subject Z:

Rule for two-number situation:

"The starting point is $1.00.
The primary attention is paid to the losing number.
Leave winning number to adjust itself. If you win
on both numbers at the start, pick left number and
increase it $.10.
If you win on both, increase the winning number that
you picked by $.10. When you lose on one number,
decrease the number by $.20 at first to avoid losses
and on the next trial increase the market that seems
to be on a winning streak by $.10. Keep this pattern."

Subject Y:

Rule for two-number situation:

"Loss rule: - $.11
Win rule: + $.05
If a loss: (one or more)
1. Always invoke loss rule first.
2. Always operate loss rule on higher number
3. If #2 ambiguous, operate loss rule on historically higher average value.
4. If #3 ambiguous, use loss rule on left number.

If two wins:
1. Use win rule on higher number.
2. Use win rule on higher average value.
3. Use win rule on left number first."

Subject X:

Rule for two-number situation:

"Trial 1. Write $1.00 for each number.
Trial 2. If one win, decrease loser by $.20.
If two wins, increase right by $.10.
If two losses, decrease right by $.20.
Trial 3. If one win, decrease loser by $.20.
If two wins, increase lowest number by $.10.
If two losses, decrease highest number by $.20.
If two numbers are the same in any trial and either 2 losses or 2 wins, change right number.
Do not go above $1.50 or below $.70.
Follow rule for trial 3 on remainder of trials."

While these rules differ from each other in both structure and behavior, the general pattern of problem solving control which we had conjectured was clearly present in each of them. Each subject identified certain attributes of his experience with each of his numbers and defined a decision rule based on these attributes which would select the number to which problem solving would be allocated.

For example, subject 1 used the concepts of win or lose, win on both, one, or neither number, higher or lower number, higher or lower average for a sequence of numbers, and right or left number in his rule to control his problem solving operators. These operators, however, were quite simple. If problem solving was allocated to a number which had won he applied the operator: increase number $.11. If problem solving was allocated to a number which had lost he applied the operator: decrease number $.05.

Since the set of all possible attributes which would have been defined on streams of data like those before our subjects is very large, it is not surprising perhaps that there was some variation in the attributes which each selected. It is more surprising perhaps that the variation was so slight. We have listed below all those attributes relevant to problem solving control which were mentioned in the rules prepared by our 21 subjects. Beside each attribute the number of subject who used it is indicated.

1.	Win, Lose	21
2	Win, both-one-neither (last trial)	20
3.	Left, Right	17
4.	Higher, Lower	9
5.	More sequential wins on same number	5
6	More plays at same number	2
7	More losses at same number	2
8.	More previous wins	2
9.	More trials since change in number	2
10.	Higher previous winning number	1
11.	Next higher previous winning number	1

12.	More [illegible] losses	1
13.	Larger maximum number which has [illegible]	1
14.	Difference between numbers	1
15.	Higher average	1
16.	More losses in a row on same number	1
17.	Larger [last loss + .5 (Number of wins since last loss) - Last number]	1

The number of attributes used in the rules of individual subjects ranged from two to nine. The average number of attributes per rule was [illegible].

Before discussing some of the interesting implications of this evidence we should consider carefully the relevance of these data to our investigation. If the rules written by our subjects accurately describe their problem solving control process, then the data is clearly relevant. How can we be sure, however, that these rules are descriptive of these behavioral processes and not just arbitrary responses to the request to write down a rule? An honest answer is that we can probably never be sure. We do have some evidence which may bear on the question.

On the positive side we have some belief that the rules are at least partially accurate because our subjects appeared to work very diligently in their preparation and they were willing to have the rule behave in their stead in a situation where various tangible and intangible rewards were at stake. Somewhat more convincing, however, is the fact that elements of the weitten rules are quite similar to elements of protocols of subjects who were working under no pressure to formalize their decision process. Some of the same attributes which we have just listed from the written rules were mentioned frequently in our protocol data.

[illegible] written down did not describe each of the [illegible] [illegible] exhibited prior to their specification. In other words the subjects did not seem to follow their own rules very well. We might explain this by saying that the subjects modified their rules as they gathered experience and that the rule which they specified would only be expected to describe future behavior which was not observed. We think this would be only partially true, however. By observation of measures, if nothing else, we are convinced that the subjects' rules evolved and were modified by their experience over the series of "hand" trials. However, to say that once specified their rules would be immune to further experience seems naive. We would suggest instead that the evidence contained in the written rules be viewed as a cross section of an on going process by which these rules are developed rather than some immutable characteristic of the various individuals who devised them. By accepting this interpretation of our subjects' rules we must also accept the responsibility to include a modification mechanism in any theory which we might derive from this evidence.

Another probable defect in evidence contained in the rules written by our subjects is that in some ways they may be *more* complete than the behavioral mechanism which they were intending to describe. This possibility at first seems paradoxical but from observations of the subjects during their rule writing we believe it is true. Consider the rule prepared by Subject [illegible] above. In selecting the number to modify in the case of two wins he decided to choose the higher number. In real play this would have taken care of [illegible] of the cases but in writing a rule he recognized that this criterion might not discriminate in all cases so he added the attribute [illegible]

average value. But this, too, could [illegible] be equal to [illegible].
To take care of this remote possibility, we added the arbitrary rule [illegible] the left number" In writing rules, these remote possibilities had to be taken care of if the subject wanted to be precise. As a result, it seems quite likely that the rules which evolved may have covered situations in which the subject had had no experience. Thus the written rules may very well have been more complete than the behavioral routine which they were intended to describe.

In addition to this perhaps subtle possibility there is, of course, the very real danger that the subject's written rule did not contain some of the more subtle attributes to which he might have attended to because they were so hard to describe. The experimental situation did not encourage long or involved rules although they were certainly not prohibited. Some subjects waxed quite eloquently. We feel we must admit, however, the very real possibility that the need to write specific rules may have led to some censorship of complex concepts.

In spite of all these defects, which may very well be in the rules which our subjects have written, we feel that there is enough merit in this evidence to provide the framework for a theory of problem solving control which can be tested by perhaps more appropriate experimental methods. We believe the data to be well worth considering.

Perhaps the most obvious characteristic of the attributes defined by our subjects is that they consist of relative rather than absolute measures of the variables in their environment. Adjectives like higher, lower, more, both, neither, etc., appeared in virtually every attribute by which the control process allocated problem solving. By avoiding absolute and independent

[illegible] like greater than $1.50, the subjects [illegible] increased the discriminating power of their control process. It is unlikely, indeed [illegible] even a large number of variables like those our subjects dealt with would simultaneously be equal on a sequence of relative attributes. Thus by using [illegible] attributes of the variables in their environment, the subjects [illegible] reduced the number required for discrimination.

[illegible] of the fact that a few discriminant attributes would suffice [illegible] circumstances, many subjects' rules were in some sense incomplete. That is, one could construct situations in which the rule which had been [illegible] would not be able to [illegible] problem solving because its attributes would not [illegible]. When these situations were noticed and pointed out to the subjects either before or during simulation of play with their rules, they [illegible] to take care of the situation. In some cases the [illegible] a [illegible] discriminant like left or right which would [illegible] [illegible] of [illegible]. The frequency with which the attributes left and right were used [illegible] by this property. These [illegible] the discrimination tree by guaranteeing an unambiguous [illegible] [illegible] circumstances.

[illegible] of a rule "incomplete", the [illegible] of attributes and, therefore, the [illegible] was [illegible] of the [illegible] which the [illegible] attributes [illegible] "[illegible]" [illegible] used [illegible] [illegible] right [illegible] required or [illegible] needed.

Thus because of the discriminating [illegible] of the [illegible]

chosen, our subjects were able to unambiguously allocate their attention to individual variables by means of very simple control processes. Some of these attributes were used frequently while others were required very infrequently. It seems perfectly possible that a subject could behave for long periods of time with a relatively incomplete control process simply because those events requiring more complete discriminating power did not occur. We will discuss this point further when we attempt to construct a theory of problem solving control in the next chapter.

VII TOWARD A THEORY OF PROBLEM SOLVING [illegible] RELATIONSHIP TO OTHER PHENOMENA

We began this investigation of problem solving control processes by constructing a theory of behavior in which these processes were one element. We hypothesized a structure for this element and designed and executed an empirical test of this hypothesis. We concluded from this test that problem solving control processes are not based on independently defined measures of success and failure.

In order to replace this element in our over-all theory we conducted some exploratory laboratory work which suggested a somewhat different hypothesis of the problem solving control process. In this chapter we will attempt to fit this element into our over-all theory of behavior. We will then consider the revised structure from several theoretical points of view.

In Chapter IV we suggested that stimuli from the environment are filtered through a concept structure which yields a stream of measures on the variables defined by this structure. We did not discuss the process of concept formation at that time but did refer to some theoretical work which is currently being focussed on it. On the basis of the observations we have described in the preceding pages, however, we believe that the problem solving control process is intimately related to the process of concept formation.

We have seen that our subjects, using very few attributes were able to discriminate easily between the variables in our experimental situation. We observed that attributes were added to their discrimination processes only as they were required. In other words, it appeared that so long as an incomplete (in some theoretical sense) discrimination process would discriminate it was used. Only when an event occurred such that the process failed to

discriminate among [illegible] the [illegible] concept structure. Thus we suggest that one trigger for concept formation, at least processing at that level, is the failure of the problem solving control process to allocate problem solving to a single variable.

It is interesting to note that we have been led to postulate failure to discriminate as a trigger for concept formation. The similarity between this hypothesis and our earlier one which held that failure to achieve a particular goal was the trigger for problem solving is striking. It is important to note, however, that they are different. In our earlier theory success and failure were attributes defined on certain intervals of measure scales. In our current hypothesis they are defined on a discrimination process. Under our present hypothesis we would predict that failure to discriminate is independent of the welfare of the decision maker.

It is also interesting to note that when an attribute is added to the discrimination process, which we have hypothesized, the decision maker will attend to that variable which is selected as a result of the addition and will ignore those other variables which were competing for his attention before modification. Under our earlier theory we would have called this change goal modification because some of those variables which were unsuccessful (i. e., demanded problem solving) did not get it and would have to be called, therefore, successful. Thus the goal modification process which we had hypothesized earlier emerges quite naturally, although in substantially different form, in our new hypothesis. Goal modification, we now suggest, is accomplished by specific addition or modification of the concept structure of the problem solving control process. If our hypothesis is

empirically true, it will explain why it may be difficult to define independent dynamic definitions of success and failure. It also suggests that further efforts to discover goal modification mechanisms which will predict problem solving may be fruitless.

Once the decision maker has accomplished discrimination of his environment, the application of problem solving to the variables selected is, under the new hypothesis, straightforward. He applies operators which he has developed to deal with the particular variable which he has selected. Under our earlier hypothesis we did not describe in any detail either the form or the process of development of these operators. We referred only to the extensive work being devoted to these processes. In the context of the revised hypothesis, however, we find that the problem solving control process may also be important to problem solving itself.

To take an extreme example, consider a situation in which the external environment is changing extremely slowly with respect to the decision maker. That is, the environment is yielding constant values of all his attributes. A laboratory situation or perhaps a factory work station might be examples. Under our hypothesis, if a set of attributes do not call for problem solving on a unique variable, attributes will be added until it <u>is</u> called for. Thus new attributes are added which define new problems and the environment is made to change by <u>internal</u> changes in the decision maker. On the other hand, if the environment changes in such a way that the problem solving control process cannot discriminate among the various demands, simplifying or generalizing

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[1] Note that this condition is completely defined by those attributes by which the decision maker chooses to view his environment

attributes will be added to reduce the number of competing variables. In other words, under our hypothesis the decision maker's problem solving control process modifies his <u>view</u> of his environment to match his problem solving capacity. We would predict, therefore, that problem solving involving subtle variations in detailed concept structure will take place in externally tranquil environments, while problem solving involving generalization or routinization, sometimes called policy making, will be undertaken by those whose attribute structure defines a rapidly changing world.

This prediction is perhaps casually supported in many industrial situations. Industrial engineers who establish ingenious controls on the environment of factory workers find themselves unable to define it in sufficient detail to prevent the workers' problem solving process from discovering ways to increase their earnings over the rates expected of them. Thus we find innovation in routine situations where every attempt is made to prevent it.

On the other hand, we find most middle managers are subject to rapidly changing events and expectations. We find also that most industrial organizations engage in a variety of programs to stimulate innovation among their managers. If we conclude from this that innovation is felt to be lacking among middle managers, we find a lack of innovation where it is most encouraged.[1]

Our hypothesis not only suggests that these observations would occur but also suggests that the level of innovation in both cases may be about the

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[1] This observation has led March & Simon to propose a Gresham's Law for Planning which suggests that programmed activity drives out unprogrammed activity. For this and other interesting observations see: J. C. March & H. A. Simon, <u>Organizations</u>, New York: John Wiley & Sons, 1958, pp. 185.

same. It may require just as much innovative energy to merely survive in a changing environment as to discover new ways to deal with a stable one. We seem to have developed a cultured preference for one over the other, however.

To summarize our discussion of the theoretical changes we have been led to make over the course of our investigation, we have reproduced the schematic diagram of the structure on which we based the design of our first experiment next to a diagram of the structure we have proposed to replace it in Figure 8.

The chief modification which we have made is in the structure of the control device. We now suggest, rather than a process which independently adjusts aspiration levels on various variables in such a way that failure will constitute the trigger for the application of problem solving, that problem solving control is a process which allocates essentially continuous problem solving capability to individual variables in the decision maker's environment. It does so by adding attributes as necessary to a discrimination structure.

It might appear that we have suggested an impractically complex allocation scheme to replace an impractically complex goal modification process. We believe, however, that this is not true. In the first place, as we have seen in the rules written by our laboratory subjects, relatively few attributes are required to discriminate among variables which are by many attributes identical. We believe, therefore, that the number of attributes which are required to yield behavior as complex as human behavior will be well within the bounds of human information processing capacity. In the second place, the control process we have suggested would be considerably more complex if we implied that when an attribute is added to the discrimination structure

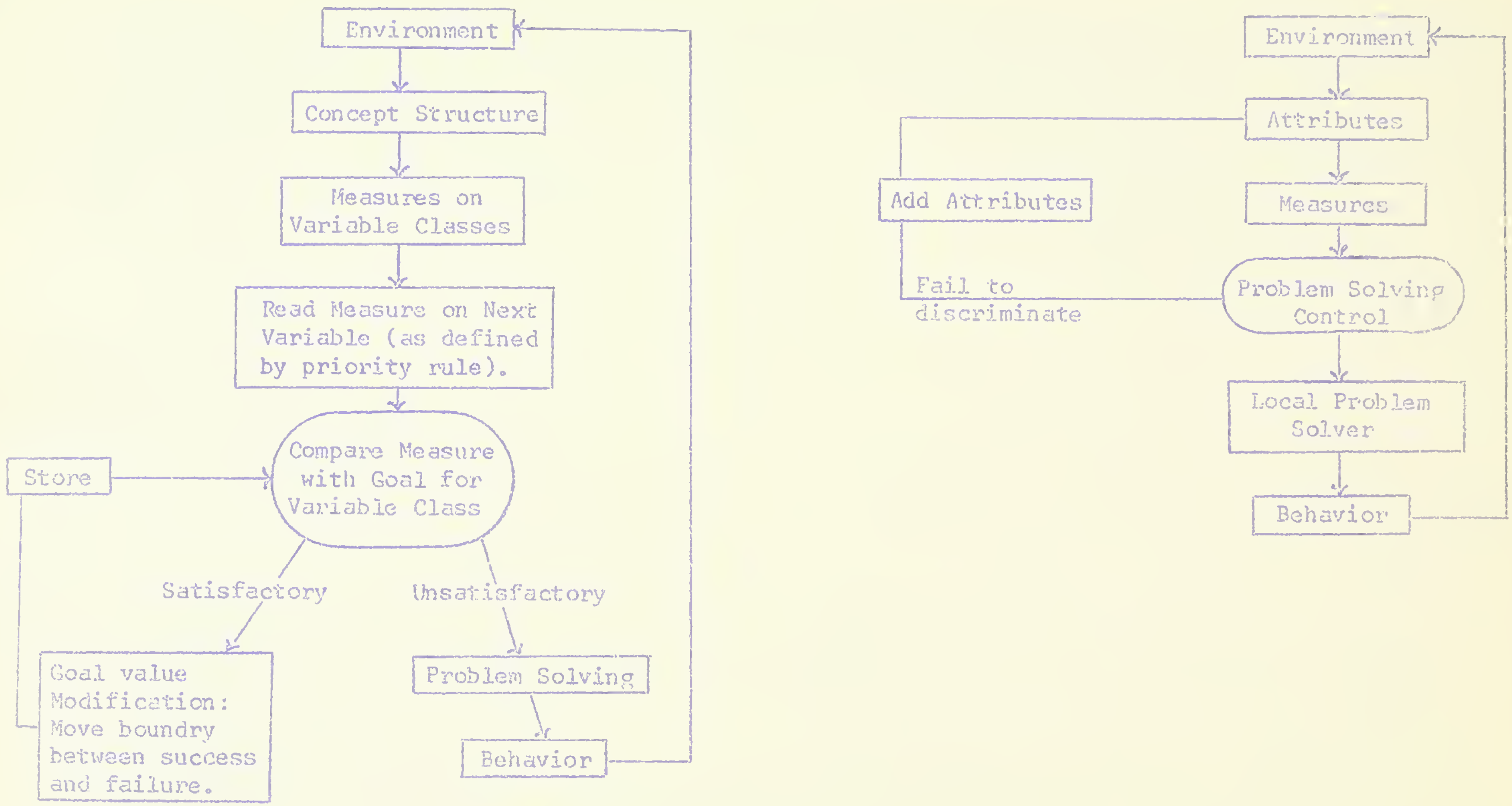
Environment
Concept Structure
Measures on Variable Classes
Read Measure on Next Variable (as defined by priority rule).
Compare Measure with Goal for Variable Class
Store
Satisfactory
Unsatisfactory
Goal value Modification: Move boundry between success and failure.
Problem Solving
Behavior
Environment
Attributes
Add Attributes
Measures
Fail to discriminate
Problem Solving Control
Local Problem Solver
Behavior

FIGURE 3

that it is added simultaneously to all parts of the structure where it [illegible] be applicable. We have made no such requirement and believe that such a requirement would complicate rather than simplify the decision maker's over-all control process. We believe that attributes are added locally to the discrimination structure as failure to discriminate occurs. (See the rules written by Subject Y, pp. 59.) These local additions to the discrimination structure imply a modest amount of processing only in those cases where the existing structure fails to discriminate. Third and finally, we believe the structure we have conjectured is practical because it behaves in such a way that it guarantees a feasible structure. As more processing is devoted to control and less to problem solving, a decision maker would devote less energy to the control of his environment and (presuming even modest effectiveness on his part) the environment therefore would begin to yield attributes which would compete for his problem solving attention. Our theory suggests that in such a situation attributes will be added to reduce these simultaneous demands and at the same time reduce the processing required for control. Thus over time we would predict a tendency toward equilibrium such that the control attributes trigger problem solving behavior at a rate which just matches the capacity of the decision maker to perform problem solving. In this equilibrium situation, no new attributes would be required and no innovation in problem solving would be called for. These systems would balance. We suspect that this would be a rare observation because it would require fortuitous exogenous effects to maintain the balance. We feel we can argue for the practicability of the system which we have conjectured on the basis of its adaptability to variations in demand within some limiting capacity for processing.

We regret the necessity to argue so casually for the structure we have conjectured. We believe, however, that until such a structure is specifically hypothesized, it will be difficult to design useful empirical work which might add validity to such conjectures.

In the next chapter we will outline a number of experimental tests which we feel would provide the beginnings of the validation which our argument so obviously requires.

Before proceeding to that discussion, however, in order to clarify our perhaps inadequate description of the process we have conjectured, we will attempt to show its relationship to several other theoretical structures.

Theories of Change Phenomena

In Chapter III we referred to some theoretical and empirical work on the phenomenon of change. It was pointed out that this phenomenon is virtually identical to the process of problem solving as we have defined it and suggested that a theory of problem solving control should be consistent with the frequently observed and reported behavior which has been named "resistance to change". We will consider first the behavior and then our interpretation of it in the context of the theory we have proposed.

In the theory on change clients are people who behave. More than this, they behave in such a routine manner that a sample of their behavior can be classified as changed or not changed.

Change agents are people who interact with clients in order to get them to change their behavior. Change agents are not always successful, and when a client behaves the same way after the efforts of a change agent as before, he is said to have resisted change.

The methods employed by change agents vary greatly. Sometimes they use face to face conversations with clients to effect change. Sometimes they use such impersonal methods as the mass media.

A considerable amount of careful empirical work suggests that change agents are most successful when the client actively participates or at least thinks he participates in the change program. This participation can vary from an explicit committment to a given proposal to active interaction in the design of the change. At a more general level the change process appears to be most successful when the client is active during the change process than when he is passive.

We would explain these observations in the context of our theory as follows: First, we would expect routine behavior to occur when the problem solving control process generates demands on the problem solver which closely match its capacity. We have argued that the processes of problem solving control and problem solving itself will interact in such a way that this should not be an unlikely observation. Thus, our theory predicts the routine behavior from which change can be measured.

Our theory further suggests that this routine behavior is controlled by a discrimination structure defined on attributes of the decision maker's (clients) environment. We would predict, therefore, that so long as both the relevant environment and this structure is unchanged, the decision maker's behavior will be unchanged. We have further suggested that changes in this structure are made locally when the structure fails to discriminate a unique focus for problem solving. This failure may only be discoverable in the context of all the other attributes and the discrimination structure. In other

words, only when the decision-maker is forced to *behave* in the face of the conflict between his old structure and the new demand will new attributes be added to generate different behavior. By not undertaking *behavior* he may not recognize the conflict between his structure and that implied by the exhortations of the change agent and thereby leave his old structure unchanged. The resulting lack of change in his behavior would be called resistance to change.

Thus the theory we have proposed is not inconsistent with the observations of those who have studied the change phenomenon. It predicts both routine behavior and the need for behavioral activity to induce a change in the behavioral control process.

Utility Theory

In Chapter II we discussed at some length the relationship between teleological and mechanistic theories of behavior. We indicated that our study would be conducted in the mechanistic mode. Because of the widespread interest in teleological theories, however, we shall discuss what we see as the implications of this study for theories of behavior based on utility maximization.

While our experiments generated a considerable amount of data which might be analysed from a teleological point of view, we shall discuss only two examples to illustrate some of the problems we see in that approach to the understanding of individual human behavior.

In each of the experiments the experimenter compared the number(s) written by the subject to the number(s) on a previously prepared list(s). If the subject's number(s) was the larger of the two, the experimenter said

"lose"; if it was the smaller he said "win". Given knowledge of the form and parameters of the probability distribution from which the experimenter's numbers were drawn, it was easy in each case to compute that number which the subject should write in order to maximize his net earnings over the course of the experiment. As a result, it was possible in each experiment to compare actual behavior with that which was predicted by a theory which asserts that behavior will maximize expected net earnings.

There were two difficulties in making this comparison. First, since the subjects did not know the form or the parameters of the distribution from which the experimenter's numbers were drawn, it seemed unreasonable to expect the subject to immediately undertake optimum behavior. Thus, instead of analysing the trial by trial behavior, we took either what appeared to be the central tendency of the last few trials in the experiment or in some cases we asked the subject at the end of the experiment to choose one number which he felt would maximize his net earnings. In those cases where we could compare these two estimates they were quite consistent.

Since both the theoretical prediction and our observations were quite specific, we can report that none (0) of our seventy plus subjects chose that number which maximized his expected net earnings. A few, perhaps five chose numbers which were close (plus or minus one-tenth of a standard deviation) to the optimum. But most were considerably farther away.

In one series of experiments in the single variable situation, we noticed that those subjects who modified their number on each trial selected final numbers in the neighborhood of the median of the distribution from which the experimenter's numbers had been drawn. Since it was easy to show that

in this experimental situation any process which symmetrically increases numbers which win and decreases numbers which lose will tend to the median of the experimenter's numbers, we compared the subject's behavior with this model. We found general agreement. Step sizes were not always symmetrical and there were exceptions to the directions of the changes but these were few and typically occurred early in the experiment.

The economics of this series of experiments were such that the number which would maximize expected net earnings was one standard deviation above the median. Thus, we found a discrepancy of one standard deviation between the numbers selected by some of our subjects and that number which would maximize their expected net earnings. This brought us to the second problem. We had no very good reason to suspect that our subjects actually tried to maximize their expected net earnings. They frequently said in their protocols that they wanted to make as much money as possible from the experiment but what they meant by this was not made more precise.

In an attempt to explain our observations in the teleological mode we assumed that the subjects wanted to maximize some function which included both frequency of wins and earnings. In other words we assumed that the subjects had in fact maximized their utility by playing in such a way that they won more frequently than they would have at the optimum but that this difference in frequency was worth the difference in expected net earnings.

To test this utility model, we revised the economics of the situation such that the number which maximized expected earnings was at the median minus one standard deviation instead of at the median plus one standard deviation. Thus the frequency of wins at the optimum was changed from about one out of five to about four out of five. If the subjects wanted to win more

frequently in return for lower expected earnings, they would [illegible] number smaller than the median minus one standard deviation. On the other hand, if they behaved in such a way that they symmetrically increased their number when it won and decreased it when it lost, this rule would be unaffected by the change. This theory would predict that they would continue to choose numbers near the median. The test was clear. If subjects chose numbers less than the mean minus one standard deviation, this would support the utility model. If, on the other hand, they chose numbers greater than the mean minus one standard deviation, this would support the mechanistic model.

Every subject who participated in the revised experiment, some of whom were the same as those who participated in the earlier experiment, chose numbers <u>higher</u> than the mean minus one standard deviation. It should be pointed out, however, that because such demonstrations were not our primary interest in these experiments, we did not pursue this line of investigation to more conclusive results.

Besides these observations in the context of the single variable experiment, we generated some data in our two variable experiments which were also disturbing from the point of view of theories of behavior based on utility maximization. We mention these observations not only because of their relevance to utility theory but because they demonstrate clearly the relative importance of problem solving and problem solving control processes.

In the single variable experiment no constraint was imposed on the subject's problem solving process. In our two variable experiment subjects had to select one of two variables to which they could apply their problem

solving routines on each trial. Since we ran some of our subjects through both one and two variable experiments, we could directly compare their behavior in a constrained and unconstrained situation. In a single variable experiment one of our subjects chose to make symmetrical changes in his number on wins and loses and as we have just discussed, he appeared to pursue, and in fact chose as his final number, the median of the distribution from which the experimenter's data had been drawn.

When faced with two variables and a constraint on his problem solving he appeared to follow the rule we described on page 54. He attended to his losses, if any, before attending to numbers which were winning. When he attended to a loss he always decreased the number; when he attended to a win, he always increased it.

If we take as the equilibrium condition of this process that point on the distribution from which the experimenter's data was drawn where the subject is equally likely to increase a number as to decrease it, we can compute this point by solving the following equation:

$$p^2 = 2[p(1 - p)] + (1 - p)^2$$

Where p = probability of a win at equilibrium.

p^2 = probability of a win on both numbers and therefore a step up.

$2[p(1 - p)]$ = probability of only a win on only one of the numbers and therefore a step down.

$(1 - p)^2$ = probability of a loss on both numbers and therfore a step down.

The solution to this equation is $p = .71$ which implies that the rule we have described for the two market game will reach equilibrium [illegible] defined it at a point on the distribution where each number will win 71% of the time as opposed to the single variable game where this rule will reach equilibrium at 50% wins.

We ran a subject through both the one and two variable situations with data drawn from identical distributions. In the single variable game the subject won 21 trials out of 50 or 42% and in the two variable game he won 101 trials out of 150 or 67%. Neither of these frequencies deviate significantly from our predictions at the .05 level.

These results are very difficult to explain in the context of utility maximization. In the single variable game the subject behaved as though he preferred to win 50% of the time at a number near the median of the experimenter's data. In the two variable experiment, however, he behaved ten minutes later as though he preferred to win 70% of the time on numbers seven-tenths of a standard deviation below the median. Such a shift in preference under the change in experimental procedure we have described is inconsistent with every formulation of utility theory with which we are familiar.

We were gratified that these results were so well explained in the context of the theory we have suggested, but they are interesting from another point of view as well. The steady state behavior of the rule we conjectured for our subject is <u>independent</u> of the step size. That is, if steps are symmetrical in the directions we have specified, then the subject's behavior in the steady state is independent of their size. This suggests that in some situations at least problem solving control processes have a more powerful effect on some attributes of behavior than the problem solving which they control.

To summarize this discussion of utility theory, we have reported two sets of observations which are predicted in the context of our [illegible] theory but which are inconsistent with a theory of utility maximization. We are led as a result to be less than optimistic about the predictive power of teleological theories of individual behavior.

Cognitive Dissonance

As the result of some observations of rumor generation and transmission, Leon Festinger proposed a theory of cognitive dissonance on which he and others have done extensive empirical research in a wide variety of situations.[1/] We believe that our theory of problem solving control may provide a mechanistic explanation of the dissonance phenomenon.

Festinger defines dissonance as a state which exists when two cognitive elements in an individual "do not fit together". In the context of our theory we would describe this situation as one where the process of problem solving control containing these elements could not make an unambiguous choice of a variable to which to allocate problem solving behavior. We would predict attributes would be added to the process until discrimination could take place. Festinger describes this process as a pressure "to reduce or eliminate dissonance".

He points out that dissonance can be reduced either by modification of the cognitive elements themselves or by changing the environment. We have described the first of these processes in some detail. By adding new attributes of the environment to a discrimination structure, we have suggested that decision-makers not only bring about internal consistency but

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[1/] L. Festinger, A Theory of Cognitive Dissonance, Stanford, California: Stanford University Press, 1957.

also revise their view of "reality".

Some of Festinger's most interesting work has been concerned with explaining the process of behavioral change. He has explained, for example, in the context of his dissonance theory why active participation of the object of change should facilitate the change process. Since we have already covered this point in our discussion of resistance to change, we will take one other of Festinger's results and attempt to show its consonnance with our theory.

In studying situations where individuals are forced to undertake an externally specified mode of behavior, Festinger finds an interesting relationship between the "strength" of the force[1] applied and the amount of permanent change in behavior which remains after the force is released. If the force is just large enough to effect compliance, the permanent change in behavior is maximized. Forces too small to effect compliance have little or no affect on behavior while forces much greater than that required for compliance only accomplish compliance while they are supplied but have little permanent affect.

We would explain these results in the context of our theory as follows: When the decision-maker finds that he faces the problem of discriminating between his "normal" mode of behavior and one which has been proposed to him, he must add attributes of the situation to his discrimination process until he is able to make a choice. As we have suggested earlier, in any complex

[1] This term refers to force in the context of Lewin's force field theories of behavior.

situation the number of attributes from which we can draw is very large. Suppose, however, in the case where force is "small", no conflict is recognized and no change in behavior occurs either during or after the application of the force. In the case where force is "large", the attributes selected are all defined in the context of the force situation. Consequently when force is removed, those attributes relevant to the specified behavior are all removed and the problem solving control process selects "normal" behavior again. When the force is "just right", however, new attributes are added not only from the force situations but also from the local environment. These latter elements, therefore, constitute a change in the problem solving control process which will continue to affect behavior after the force is removed.

While this interpretation of these results hardly constitutes empirical validation of our theory, we find it encouraging that our predictions are compatible with those made under the theory of cognitive dissonance. It seems possible that by suggesting a process of dissonance reduction, our theory may offer a new methodology for further empirical work on important and interesting phenomenon of cognitive dissonance.

Behavioral Theory of the Firm

Since most of our empirical work was directly suggested by propositions made by Cyert and March[1/], we have already discussed most of the implications

[1/] R. M. Cyert and J. G. March, *Op. Cit.*

of our work for the behavioral theory of the firm. We [illegible] "aspiration level like" goal values on each variable in the decision-maker's environment provide insufficient discrimination to determine behavior. We found instead that a focussing mechanism which Cyert and March saw as necessary in addition to a goal modifying process was sufficient in itself to explain behavior. Thus, in this area, a rare event has occurred. As the result of our empirical work, we have concluded our study with a simpler theory to explain the same behavior than that with which we started.

It might seem that by rejecting the success-failure trigger for problem solving we have raised some question with regard to those parts of the Behavioral Theory of the Firm which uses these attributes as a mechanism to explain internal change. We will argue on the contrary that what we suggest not only is consistent with current formulations of this theory but also may suggest interesting new areas for research on it.

In their model Cyert and March hypothesize measures or attributes of the environment of decision makers within the firm which are compared to goal values on these same attributes to control the rate of search or problem solving which will be undertaken. Since in most firms such attributes, measures, and goal values are institutionalized through an accounting system, it seems entirely possible, in fact quite likely, that they would be important attributes in the process by which decision makers in firms focus on problems. The only assumption required to make the theory we have suggested compatible with the current formulation of the behavioral theory is that the amount of problem solving which is triggered by this process always matches the capacity of the organization to perform it. Since we have argued that the process we have described will lead the processor toward

this matching of demand and capacity we see nothing wrong with this assumption as at least a first approximation.

It is interesting to consider the possibility of removing this assumption and replacing it with one which asserts that problem solving capacity of the organization is constant and that the demands of the institutionalized control system are variable. We might conjecture as do Cyert and March that when the demands of the institutionalized attribute structure are all satisfied that most (although probably not all) problem solving will be allocated to better satisfying the members of the organization by internal bargaining over work rules, recreational activities, etc. These uses of the problem solving capacity undemanded by the control system are what Cyert and March call organization slack and we find it completely consistent with our theory of problem solving control.

On the other hand, we can easily imagine situations when the demands of the institutionalized control system might exceed the problem solving capacity of the organization. The behavioral theory of the firm as it is now formulated does not recognize this possibility. It assumes instead that innovative activity can and will always be applied to all variables which demand it. It would be interesting to investigate the effect of removing this assumption and replacing it with any one of several possible models which would allocate search (problem solving) under a constraint. In something so complex as an organization it would be difficult to predict without empirical observation just how such a constraint might operate, but it appears to be an interesting area in which research could be conducted either in the field or in the context of a computer model or preferably both.

While on the subject of institutionalized problem solving control processes (budgets) we will point out one of the predictions of the theory. A budget will have a maximum behavioral (focussing) effect when it generates problems at the same rate the organization can deal with them. If it generates problems too slowly, attention will be allocated by other attributes. If it generates problems which exceed the problem solving capacity of the organization, attributes will be added to reduce this demand. It is not at all clear that these attributes will focus problem solving on variables related to the budget. The possibility of individuals solving the problem by leaving the firm might be considered for example and this variable might well become the focus for their innovative activity.

In summary we have found our theory of the problem solving control process to be consistent under not unreasonable assumptions with current formulations of the behavioral theory of the firm. It appears, however, that the behavior of a firm under heavy demands from a formalized control system is an area in which further research might be usefully conducted.

Mechanistic Theories of Human Problem Solving

We suggested earlier that we would have occasion to discuss again the pioneering work which has been done in devising mechanistic theories of human problem solving behavior (see page 24). As we indicated there we did not conduct our research in the context of these theories because they have not been directly applied to the kinds of behavior in which we were interested. If we compare the results of our study of problem solving control to those processes which have been found to explain human problem solving behavior

we find large areas of agreement in both process and structure. Clarkson[1] found that a trust officer selects those stocks which he purchases by means of discriminating processes acting on well defined attributes of his environment. In the context of our theory if we consider the act of purchasing to be a behavioral routine which can be applied to a stock, then the trust officer's discrimination process is completely analagous to the problem solving control process we have investigated. Since those particular attributes used by the observed trust officer always yielded a unique output, Clarkson did not observe the process of adding attributes which we observed when no choice could be made.

On the other hand, Feigenbaum's[2] work on verbal learning behavior indicates clearly that attributes of non-sense syllables are only added as required to discriminate between two syllables. Thus our observation of an attribute structure which is only as large as it needs to be for discrimination is supported by Feigenbaum's work. Since the subjects in our experimental situation were not given a set of attributes which would "guarantee" discrimination we got some insight into the process by which people control the size of their discrimination processes. By using arbitrary attributes independent of their experience (e. g., left, right) subjects were able to cut off the growth of their discrimination structures. This observation suggests a possible mechanism whereby people can deal with noisy feedback, a problem which Feigenbaum's EPAM process has not faced.

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[1] G. P. E. Clarkson, Op. Cit.

[2] E. A. Feigenbaum, "Simulation of Verbal Learning Behavior," Computers and Thought, E. A. Feigenbaum and J. Feldman (Eds.), McGraw Hill, New York, 1963.

To summarize we have found that our speculations [illegible] the process whereby people choose the [illegible] in which they [illegible] problem solving, support and are supported by work which has been done on activities where the focus of problem solving was largely controlled. As a result we are led to conclude that the so called higher levels of the [illegible] making process are very similar to the processes operating at the lower [illegible].

Summary

In this chapter we have suggested a theory of problem solving control to replace the one which we rejected as the result of our empirical tests. We have argued that the problem solving control process as we have defined it appears to be an important part of both the processes of concept formation and problem solving. The processes we found to explain problem solving control appeared in many ways similar to those which have been found to explain problem solving that we were led to conclude that the structure of the processes at both levels may indeed be the same.

We discussed the compatibility of our theory with observations made in the context of a variety of other theoretical structures. We found not only that our theory was compatible with these others but also that it seemed to offer more complete explorations of some of the phenomena.

We recognize that the theory we have proposed is at this stage insufficiently supported by direct test. In the next chapter, therefore, we will outline a program of empirical research which if successful will place our theory on a sound empirical base

VIII. SUGGESTIONS FOR FUTURE RESEARCH, SUMMARY AND CONCLUSIONS

While we are encouraged by the degree to which the theory of problem solving control which we have suggested is consistent with other observations and theories of this process, it seems clear that more direct testing is called for. We shall outline what seems to be the most promising program for accomplishing this task.

At a general level the program is a simple one. We should proceed to construct specific models of problem solving control behavior within the framework of the theory which has been suggested and compare the predictions of these models with the behavior of those individuals whom the models were designed to describe. To the extent that these models predict the behavior of the individuals for which they were devised and to the extent that appropriate variations of these models describe other individuals, our confidence in the theory will increase. To the extent, however, that specific models fail to describe individual behavior we will question first the formulation of the models and then the framework of the general theory.

This process of validation is clearly no different than that we would prescribe for any theory which alleges to explain any observable phenomenon. Such a general prescription, however, hardly constitutes a program of research. We shall turn, therefore, within this general procedure to consider some more specific suggestions.

The theory we have proposed makes specific predictions of two aspects of problem solving control behavior. First, it asserts that problem solving control is exercised by means of a discriminating process which operates on a well defined sub-set of those attributes which can be defined on the stimuli received by a decision maker. Second, it asserts that the attributes

which are included in the discrimination function, [illegible] the discrimination process fails to select a unique focus for problem solving. It would seem that testing can proceed on both of these aspects of the theory. We will consider each in turn.

To test the proposition that problem solving is controlled by a discriminating process operating on a well defined set of attributes of the decision maker's environment we might look for the following: (1) A decision maker who deals with a variety of problems such that the effect of his control process is observable. (2) A decision maker whose control process is well adapted to his environment such that new attributes are required infrequently, and (3) a decision maker whose control process is based largely on formalized attributes of his environment like published reports or other objective measures. (4) A decision maker willing to discuss and/or describe his process of problem solving control with the person performing the test of the theory.

The test procedure would be quite straightforward. By means of interview and observation the observer could get insight into the attributes included in the discrimination process by which the decision maker focusses his attention on the various problems before him. To the extent that this set of attributes would be processed by a discriminating function which would predict the focus of his problem solving this aspect of our theory would be supported.

A variety of industrial positions would appear to be potentially fruitful areas for such an investigation. The quality control manager in a manufacturing organization might be expected explicitly to shift his focus of attention over the set of manufacturing operations which contribute to the quality of the finished product. Some parts of his discrimination process

would probably be formalized, e. g., X and R charts, [illegible] may be more suitable, e. g., knowledge of the competence of various [illegible] and their employees or the importance of a particular order. The process of problem solving control of a quality control manager might be of interest not only with respect to theories of problem solving control but also with respect to theories of quality control. It might be interesting to see, for example, the degree to which explicit and frequently expensive quality control charts affect the focus of those analytical processes which they are maintained to control.

The process by which a salesman chooses to call on customers and the process by which manufacturing orders receive the attention of those scheduling manufacturing facilities are suggested as other interesting situations where the standards we have set down for this aspect of a testing program might be met.

For purposes of testing that aspect of the theory concerning the process by which the problem solving control process is constructed we would suggest work in slightly different situations. Here it would seem desirable to find: (1) A situation where a decision maker must deal with a variety of variables such that his allocation of problem solving is observable. (2) A situation with which he has had little or no experience so that he will frequently encounter situations where a discrimination process based on past experience would not apply directly. (3) A situation simple enough so that the set of attributes from which he has to draw will be relatively small. And, (4) a situation in which the subject can and is willing to cooperate with the person conducting the study by giving interviews and protocols of his decision process.

Since the objective of the study would be to test the hypothesis that attributes are added to the discrimination process only as they are required it would be necessary to have some means of gaining knowledge of the discriminating structure over time. In any kind of situation where the subject is free to behave even within certain constraints on his problem solving capability, it is extremely difficult to be sure which of a variety of possible discriminating structures might be yielding the behavior which is being observed. For purposes of testing this hypothesis, however, it would be necessary to know this structure.

Despite some of the obvious disadvantages of the method, we believe the best procedure to get at this kind of question is the one we have used and described. It seemed that by having the subject specify a decision rule by which he would respond to his experience we got considerable insight not only into the form but also the process of problem solving control. If, instead of having the subject prepare a rule once and for all, we permitted him to revise it as he gained experience, we could observe those circumstances where he modified it and the nature of the modification. Thus by observing trial by trial modifications in the control process itself we should be able to get insight into its evaluation.

Once the form and general process of evaluation of the control process is established we can proceed to work in several directions. In the direction of more basic research we might attempt to understand the process by which those attributes which are added to the control process are identified and selected. Research in this direction would proceed toward more fundamental understanding of such phenomena as cognitive dissonance and resistance to change. While we argued in the last chapter that our theory may provide a

mechanistic explanation for these phenomena [illegible] until the theory can be more specific about those attributes which will be added under various circumstances.

Another direction in which we might proceed from an established theory of problem solving control is toward a mechanistic theory of interpersonal, small group, and organizational behavior. Since the problem solving control process acts as a filter between the environment and the decision maker's behavioral mechanisms, it seems to provide a theoretical structure for the study of such concepts as authority, influence, and personality which have been long recognized as important to theories of interpersonal relations. The process by which one might go about constructing a model of that part of the problem solving control process which deals with stimuli from other people is not completely clear, however. One procedure which might be useful in approaching this problem is the following: Using experimental situations much like the ones we have described, let two subjects independently arrive at decision rules with which they are satisfied. Then bring them together and without revealing their rules to each other have them come to a rule which they will jointly accept. Protocols of this process might suggest not only the nature of the processes which deal with interpersonal stimuli but also shed some light on the individual process as well.

Without having performed such experiments we can only predict that they may be useful. The exact nature of the attributes which make up the process which deals with interpersonal stimuli is at the moment unpredictable.

Despite our rather parochial discussion of the problem solving control process we can see no reason, once its form and general process of evolution

[illegible]
component of the more general theory of human decision making, of which it is a part. In fact, in the context of that theory its identity may be impossible to preserve. As we have indicated the processes we found to control the application of problem solving operators appear to be virtually identical to those which others have found within the operators themselves. Thus it appears that simply by continuing to build more and more models of more and more individuals and arguing this within the framework of the general theory we can not avoid doing research on those phenomena we have identified as problem solving control processes. And since, in this work, those processes of the most interest will receive the most attention perhaps we can take comfort in the fact that the processes we have called problem solving control will receive the attention they deserve.

To summarize the work described in this paper it might be said we have proceeded from an untested hypothesis to a somewhat different theory which will require considerable more work before it is fully validated. Our empirical work was somewhat more conclusive on the hypothesis we rejected than it was in offering support to the theory we proposed to replace it. Nevertheless we find our evidence modestly persuasive and are encouraged by the fact that our theory seems to explain more while remaining compatible with a variety of other work which has been done on the same phenomenon.

We began our study with an interest in the process by which innovations are undertaken in organizations. Choosing to work in the context of mechanistic theories of behavior we defined two kinds of behavior. Programmed behavior is that which is undertaken in situations with which the decision maker is quite familiar and his behavior varies only slightly in response to

to the same stimuli. Non-programmed behavior on the other hand is under-taken when a situation evokes those activities which have been called search or problem solving. Since innovation appeared almost by definition to be the result of problem solving activity we undertook to investigate the process by which such activity is controlled.

After reviewing several theories which have been suggested to explain the process of problem solving control we chose a hypothesis which suggests that <u>failure</u> to achieve an aspiration level on a particular variable is the trigger for problem solving activity. We showed how this hypothesis might fit into an over-all theory of behavior and designed in this context an experiment to test the hypothesis.

We found in the case of a single variable environment that success and failure did not control the application of problem solving routines. In considering this result, however, we were led to suspect that a single variable environment might be a special case and we proceeded to design a test of the hypothesis in a two variable situation. Here again, however, we found that success and failure did not explain the allocation of problem solving to the variables. In considering this result we were able to show that we might expect this result in general.

In order to generate a new theory of the control process we observed the behavior of subjects in an experimental situation which forced them to be explicit about their process of problem solving control and we were led to theorize as follows:

1. Problem solving activity is a continuous rather than an intermittant process.

2. It is allocated to particular variables in the decision maker's environment by means of a discriminating process

defined as attributes of the environment.

3. Attributes are added to this discrimination process as necessary to generate a constant demand on the problem solving processes of the decision maker.

We showed that this theory not only predicts certain casual observations of innovative activities in industrial organizations but also provides a more complete explanation of observations made under a variety of other theories.

We pointed out that the theory we devised to explain problem solving control behavior is virtually identical to those which have been devised to explain complex problem solving and learning behavior.

Perhaps our most basic conclusion, therefore, is that the so called higher level cognitive processes appear to be the same as those which have been found to explain more routine kinds of behavior. This conclusion suggests that while these distinctions which have been made between programmed and unprogrammed behavior, between persistence and change, and between creative and routine behavior may be useful for some purposes, these terms should not be permitted to suggest that essentially different theories are required to explain the behavior to which they refer. It appears that models carefully built within a single theoretical framework may explain all of these phenomena.

A theory which promises such scope in explanation but which at the same time remains vulnerable to empirical tests is a comparatively recent event in the social sciences. Its significance, therefore, we believe would be difficult to exaggerate. It now appears that only painstaking work lies between us and the ability to understand as well as name such phenomena as

management control, resistance to change, motivation, influence, [illegible] personality, judgment, etc. And with this understanding will surely come more appropriate bases for management behavior and more effective means of management education. If the theory we have suggested is correct, attributes of that new environment will be identified which will suggest new foci for problem solving and the task ahead will appear to be just as large and full of promise as ours does now.

APPENDIX A

Instructions for Experimental Situation I

APPENDIX A

Instructions for Experimenters Situation

Instructions

The experiment in which you are about to participate is a part of a research project designed to investigate certain fundamental processes of human decision making. During the course of this experiment you will have the opportunity to make a number of decisions. These decisions will, by a process described below, determine a measure of your performance in the experiment. This measure will be expressed in dollars and cents and you can think of these as your earnings in the experiment although no payoff will be made. You should attempt to make your decisions in such a way as to make your earnings as large as possible.

On the forms provided you can see that the experiment consists of a series of trials and a decision by you on each trial. Your decision will consist of choosing a number to write opposite each trial number. The numbers you choose can be thought of as dollars and cents, can be positive or negative, and can be of any size you choose. There is absolutely no limit on the number you choose on each trial except that it be in the dollar and cent form.

After you have chosen a number for a trial I will compare that number with the number corresponding to that trial on a long list (5,000 numbers) which I have prepared. Note that the number on my list changes at each trial whether yours does or not. If, when I compare your number with mine on any trial, I find my number is higher than yours, I will say "you win". In this event you will circle your number and note beside it the cumulative number of wins up to that trial. You can think of yourself as winning the money implied by these circled numbers.

If, on the other hand, the number on my list corresponding to the trial is smaller than the number you have written on your list, I will say "you lose" and you can proceed to the next trial.

You will continue to play until you have won 50 times. The number of trials this will take will vary depending on how frequently you win. The minimum number of trials, of course, is 50 but there is no rule as to the maximum number. You may take as many trials as you like or find necessary to win 50 times. Do not feel constrained one way or the other by the fact that the prepared form has 100 trials on it. You may use more or less trials if you like.

In order to limit the number of trials you may find it desirable to play, however, a fee is imposed which you must pay per trial whether you win or lose. Since the fee will vary from experiment to experiment, you will be told the size of the fee per trial before you begin to play.

For example, suppose you played as follows and the trial fee were $.10 per trial.

Trial Number	Decision	Experiment or Re[illegible]
1	.75	Lose
2	.60	Lose
3	.50	Lose
4	.50	Win

Your earnings through trial 4 would be:

.50 = Sum of circled values
-.40 = Four trials at $.10 per trial
.10 = Earnings through trial 4

Are there any questions about procedure?

Now just a word about the numbers on my list. These numbers were selected carefully but then arranged in a random sequence such that any number can follow any number as one goes down the list. You will be given no information about the set of numbers from which this list was selected.

Since this experiment is designed to reveal certain aspects of your decision process, it would be helpful if you can describe the process by which you select the numbers you write on your list as you go along. If you forget to do this, you may be reminded by the experimenter.

Since most subjects will, like yourself, come from within the school, you are requested not to discuss your experience here outside this room because prior knowledge would, no doubt, influence future subjects and nullify their value to this study.

APPENDIX B

Sample Data Sheet

DATA SHEET

NAME ______________________ DATE ______________________

Trial No.	Decision	Trial No.	Decision	Trial No.	Decision	Trial No.	Decision
1	______	26	______	51	______	76	______
2	______	27	______	52	______	77	______
3	______	28	______	53	______	78	______
4	______	29	______	54	______	79	______
5	______	30	______	55	______	80	______
6	______	31	______	56	______	81	______
7	______	32	______	57	______	82	______
8	______	33	______	58	______	83	______
9	______	34	______	59	______	84	______
10	______	35	______	60	______	85	______
11	______	36	______	61	______	86	______
12	______	37	______	62	______	87	______
13	______	38	______	63	______	88	______
14	______	39	______	64	______	89	______
15	______	40	______	65	______	90	______
16	______	41	______	66	______	91	______
17	______	42	______	67	______	92	______
18	______	43	______	68	______	93	______
19	______	44	______	69	______	94	______
20	______	45	______	70	______	95	______
21	______	46	______	71	______	96	______
22	______	47	______	72	______	97	______
23	______	48	______	73	______	98	______
24	______	49	______	74	______	99	______
25	______	50	______	75	______	100	______

APPENDIX C

Sample Data Sheets--First (single variable) Experiment

DATA SHEET

NAME Subject # 1 DATE 10/4

Trial No.	Position	Trial No.	Position	Trial No.	Position	Trial No.	Position
1	2.00	26	(1.75) 14	51	3.00	76	2.50
2	(1.50) 1	27	(2.50) 15	52	(2.50) 25	77	2.00
3	2.00	28	(2.50) 16	53	3.00	78	2.00
4	1.50	29	3.00	54	2.50	79	2.00
5	1.50	30	(2.00) 17	55	(2.00) 26	80	(1.50) 35
6	1.00	31	2.25	56	3.00	81	2.00
7	(1.00) 2	32	2.00	57	2.00	82	2.00
8	1.50	33	(2.00) 18	58	2.00	83	(2.00) 36
9	1.00	34	2.50	59	(2.00) 27	84	2.50
10	(1.00) 3	35	2.00	60	(2.50) 28	85	(2.00) 37
11	(1.00) 4	36	(2.00) 19	61	(2.50) 29	86	(2.00) 38
12	(1.25) 5	37	2.25	62	(2.50) 30	87	(2.50) 39
13	1.50	38	2.00	63	3.00	88	2.50
14	(1.00) 6	39	2.00	64	2.50	89	(2.00) 40
15	(1.50) 7	40	1.75	65	2.50	90	(3.00) 41
16	(1.50) 8	41	(1.50) 20	66	2.00	91	2.50
17	(2.00) 9	42	2.00	67	(1.50) 31	92	(2.00) 42
18	2.50	43	(2.00) 21	68	2.50	93	3.00
19	(1.50) 10	44	2.50	69	2.00	94	(2.00) 43
20	2.00	45	2.00	70	2.00	95	2.25
21	(1.50) 11	46	(1.75) 22	71	2.00	96	(2.00) 44
22	1.75	47	(2.50) 23	72	(1.50) 32	97	2.00
23	(1.75) 12	48	2.50	73	(2.00) 33	98	2.00
24	(2.00) 13	49	2.00	74	2.25	99	(2.00) 45
25	2.00	50	(2.00) 24	75	(2.25) 34	100	(2.50) 46

DATA SHEET

Subject # 1 DATE 10/4

Trial No.	Decision		Trial No.	Decision	Trial No.	Decision	Trial No.	Decision
1	2.50		26		51		76	
2	(2.00)	47	27		52		77	
3	3.00		28		53		78	
4	2.50		29		54		79	
5	2.00		30		55		80	
6	2.00		31		56		81	
7	2.00		32		57		82	
8	2.00		33		58		83	
9	2.00		34		59		84	
10	(2.00)	48	35		60		85	
1	(2.50)	49	36		61		86	
	3.00		37		62		87	
	2.50		38		63		88	
	(2.00)	50	39		64		89	
			40		65		90	
			41		66		91	
			42		67		92	
			43		68		93	
			44		69		94	
			45		70		95	
			46		71		96	
			47		72		97	
			48		73		98	
			49		74		99	
			50		75		100	

DATA SHEET

NAME Subject # 2 DATE 8/21/62

Trial No.	Decision	Trial No.	Decision	Trial No.	Decision	Trial No.	Decision
	1.00 1.	26	1.70 18	51	2.10	76	
	.90 2.	27	1.80 19	52	2.10 31	77	
	.85	28	1.90 20.	53	2.15 32	78	
	.90 3.	29	2.00	54	2.20	79	
	.90 4.	30	2.00 21.	55	2.15 33	80	
	1.00	31	2.20	56	2.15	81	
	.90 5.	32	2.20	57	2.15	82	
	.95 6.	33	2.00 22	58	2.10	83	
	1.00	34	2.10 23	59	2.10 34	84	
	1.00 7.	35	2.15	60	2.10 35	85	
	1.20 8.	36	2.10 24	61	2.10 36	86	
	1.30 9.	37	2.20	62	2.10 37	87	
	1.50	38	2.00	63	2.20	88	
	1.40 10.	39	1.90	64	2.10	89	
	1.40 11.	40	1.90	65	2.10 38	90	
	1.45 12.	41	1.80 25	66	2.10 [illegible]	91	
	1.50 13.	42	1.90 26	67	2.00 39	92	
	1.50	43	2.00 27.	68	2.10	93	
	1.30 14.	44	2.10	69	2.10	94	
	1.35	45	2.00	70	2.00	95	
	1.35 15.	46	2.00 28	71	2.10	96	
	1.50	47	2.10 29	72	2.00 40		
	1.50 16	48	2.20	73			
	1.60 17	49	2.10	74			
	1.70	50	2.10 30	75		100	

DATA SHEET

Subject # 3 DATE 3/17/62

	Decision	Trial No.		Decision	Trial No.		Decision	Trial No.		Decision
1	10.00 L	26	17	(1.05)	51		1.62	76	47	(1.90)
	5.00 L	27	19	(1.10)	52	33	(1.62)	77		2.00
	2.00 L	28	20	(1.15)	53	34	(1.62)	78		1.95
	(1.00) W	29		1.25	54	35	(1.62)	79		1.90
	1.50 L	30	21	(1.30)	55	36	(1.65)	80	48	(1.75)
	1.00 L	31		1.20	56	37	(1.70)	81		1.80
2	(0.50 W)	32		1.30	57	38	(1.72)	82		1.75
3	(0.50 W)	33	22	(1.25)	58		2.00	83	49	(1.65)
	0.75 L	34	23	(1.27)	59	39	(1.92)	84		1.65
4	(0.50 W)	35	24	(1.30)	60	40	(2.00)	85	50	(1.50)
5	(0.60) W	36	25	(1.35)	61	41	2.10	86		
6	(0.70)	37		(1.40)	62		(2.20)	87		
7	(0.70)	38		1.60	63		2.50	88		
	(0.72)	39	26	(1.50)	64		2.30	89		
	(0.72)	40		1.55	65	42	(2.20)	90		
0	(0.72)	41	27	(1.50)	66		2.25	91	/	
1	(0.74)	42	28	(1.55)	67	43	(2.20)	92		
2	(.75)	43	29	(1.60)	68		2.22	93		
3	(.80)	44		1.15	69		2.20	94		
4	(.85)	45		1.65	70		2.10	95		
5	(.90)	46	30	(1.60)	71		2.00	96		
	1.00	47	31	(1.65)	72	44	(1.75)			
6	(.95)	48		1.70	73	45	(1.85)			
7	(1.00)	49		1.65	74		2.00			
	1.10	50	32	(1.60)	75	46	(1.90)	100		

DATA SHEET

NAME Subject # 4 DATE 17 Aug 62

No.	Decision		Trial No.	Decision		Trial No.	Decision		Trial No.	Decision	
	(.20)	1	26	(2.00)	24	51	– 3.00		76	3.10	
	(.20)	2	27	(2.00)	25	52	– 3.10		77	3.10	
	(.20)	3	28	(2.00)	26	53	– 3.00		78	3.10	
	(.25)	4	29	– 3.00		54	– 3.00		79	3.10	
	(.25)	5	30	– 3.00		55	– (3.00)	33	80	3.10	
	(.25)	6	31	– 3.00		56	3.50		81	3.10	
	(.30)	7	32	– 3.00		57	3.50		82	3.10	
	(.30)	8	33	2.50	–	58	3.50		83	3.10	
	(.30)	9	34	2.50	–	59	3.50		84	– 3.00	
	(.35)	10	35	2.50	–	60	3.50		85	– 3.00	
	(.35)	11	36	(2.50)	– 27	61	3.50		86	– 3.00	
	(.40)	12	37	2.50	–	62	3.50		87	– 3.00	
	(.40)	13	38	2.50	–	63	3.50		88	– 3.00	
	(.50)	(14)	39	2.50	–	64	3.50		89	– 3.00	
	(.60)	15	40	2.50	–	65	3.50		90	– (3.00)	36
	(.70)	16	41	(2.50)	28	66	– 3.00		91	– 3.00	
	(.80)	17	42	2.50	–	67	– 3.00		92		
	(.90)	18	43	(2.50)	29	68	– 3.00		93		
	(1.00)	19	44	2.50	–	69	– 3.00		94		
	(1.20)	20	45	2.50	–	70	– 3.00		95		
	(1.50)	21	46	(2.50)	30	71	– 3.00		96	(3.00)	3
	1.50	21	47	(3.00)	31	72	– (3.00)	34	5		
	(1.50)	22	48	– 3.00		73	– 3.00		6 25 1		
	(1.50)	23	49	– 3.00		74	– 3.00		2 15		
	1.50	23	50	– (3.00)	32	75	– (3.00)	35	10 1 12 16		

APPENDIX D

Sample Data Sheets--Second (two variable) Experiment

PART I

DATA SHEET

NAME Subject # M DATE

Trial No.	Decision	Trial No.	Decision	Trial No.	Decision	Trial No.	Decision
1	3	26	2	51		76	
2	(2.5)	27	(1.75)	52		77	
3	2.6	28	(1.8)	53		78	
4	(2.55)	29	(1.9)	54		79	
5	(2.59)	30	2.2	55		80	
6	2.6	31	(2.1)	56		81	
7	(2.57)	32	2.15	57		82	
8	(2.58)	33	(2.12)	58		83	
9	2.6	34	2.12	59		84	
10	2.59	35	2.1	60		85	
11	2.57	36	(2.05)	61		86	
12	2.5	37	2.07	62		87	
13	2.45	38	2.05	63		88	
14	(2.2)	39	(2)	64		89	
15	2.3	40	2.02	65		90	
16	(2.25)	41	2.01	66		91	
17	2.26	42	1.8	67		92	
18	2.24	43	(1.5)	68		93	
19	(2.15)	44	(1.6)	69		94	
20	2.2	45	1.75	70		95	
21	(2.18)	46	1.65	71		96	
22	2.2	47	(1.5)	72		97	
23	2.19	48	1.7	73		98	
24	2.17	49	(1.6)	74		99	
25	2.13	50	(1.7)	75		100	

(L) 16 L=13 W = 21 L = 29 %W = 42%

PART II

DATA SHEET

NAME ________ Subject # M ________ DATE ________

Trial No.	Decision (L)	Trial No.	Decision (R)	Trial No.	Decision (L)	Trial No.	Decision (R)
1	1.0	26	1.0	51	3.3	76	1.6
2	1.5	27	1.0	52	3.4	77	1.6
3	1.5	28	1.3	53	3.4	78	1.65
4	1.5	29	1.2	54	3.6	79	1.65
5	1.6	30	1.2	55	3.6	80	1.55
6	1.6	31	1.1	56	3.7	81	1.55
7	1.7	32	1.1	57	3.7	82	1.45
8	1.75	33	1.1	58	3.8	83	1.5
9	1.75	34	1.2	59	3.75	84	1.5
10	1.85	35	1.2	60	3.75	85	1.45
11	1.85	36	1.25	61	3.75	86	1.5
12	1.9	37	1.25	62	3.8	87	1.5
13	1.9	38	1.2	63	3.8	88	1.55
14	2.0	39	1.2	64	3.8 3.85	89	1.6
15	2.2	40	1.2	65	3.9	90	1.6
16	3.0	41	1.2	66	3.9	91	1.65
17	3.0	42	1.5	67	3.9	92	1.55
18	4.0	43	1.5	68	4.0	93	1.55
19	5.0	44	1.5	69	4.0	94	1.6
20	4.5	45	1.5	70	4.3	95	1.6
21	4.0	46	1.5	71	4.2	96	1.6
22	3.5	47	1.5	72	4.2	97	1.7
23	3.5	48	1.6	73	4.2	98	1.65
24	3.5	49	1.7	74	4.	99	1.65
25	3.3	50	1.7	75	4.	100	1.6
	L=4		L=9		L=4		L=8

PART II
DATA SHEET

NAME Subject # M DATE

Trial No.	Decision (L)	Trial No.	Decision (R)	Trial No.	Decision	Trial No.	Decision
1	4.0	26	1.5	51		76	
2	3.8	27	1.5	52		77	
3	3.8	28	1.55	53		78	
4	3.9	29	1.55	54		79	
5	3.9	30	1.5	55		80	
6	3.9	31	1.48	56		81	
7	4.0	32	1.48	57		82	
8	4.2	33	1.48	58		83	
9	4.2	34	1.45	59		84	
10	4.2	35	1.48	60		85	
11	4.3	36	1.48	61		86	
12	4.25	37	1.48	62		87	
13	4.25	38	1.46	63		88	
14	4.25	39	1.48	64		89	
15	4.2	40	1.48	65		90	
16	4.0	41	1.48	66		91	
17	4.0	42	1.5	67		92	
18	4.0	43	1.47	68		93	
19	4.1	44	1.47	69		94	
20	4.0	45	1.47	70		95	
21	4.0	46	1.48	71		96	
22	4.0	47	1.5	72		97	
23	3.8	48	1.5	73		98	
24	3.8	49	1.55	74		99	
25	4.0	50	1.55	75		100	

L = 9 L = 10

$L = \frac{17}{75} = 23\%$ $W = 77\%$ $L \frac{44}{150} = 29$

$L \frac{27}{75} = 36$ $W = 0$

PART III
DATA SHEET

NAME ______ Subject # M ______ DATE ______

No.	Decision (L)	Trial No.	Decision (M)	Trial No.	Decision (R)	Trial No.	Decision
	1.5	26	1.5	51	1.5	7	
	1.7	27	1.5	52	1.5	7	
	1.7	28	1.5	53	1.2	7	
	~~2.0~~ 1.7	29	2.0	54	1.2	7	
	~~2.0~~ 1.7	30	3.0	55	1.2	8	
	1.7	31	3.	56	1.5	8	
	1.8	32	3.	57	1.5	8	
	1.8	33	3.5	58	1.5	8	
	1.8	34	3.5	59	2.0	8	
	1.8	35	3.7	60	2.0	8	
	2.	36	3.7	61	2.0	8	
	1.8	37	3.7	62	2.0	87	
	1.8	38	4.	63	2.0	88	
	1.8	39	4.5	64	2.	89	
	2.2	40	4.5	65	2.	90	
	2.	41	4.5	66	2.	91	
	2.	42	6	67	2.	92	
	2.	43	6	68	2.5	93	101/150 60%
	1.8	44	6	69	2.5	94	
	1.6	45	6	70	2.5	95	
	1.6	46	6	71	2.6	96	
	1.6	47	6	72	2.45 ~~[illegible]~~	97	
	1.6	48	5.5	73	2.45	98	LEFT L = 20, W = 30, P(ω) = 60
	1.6	49	5.5 ~~[illegible]~~	74	2.35	99	
	1.6	50	5	75	2.35	100	M = P(ω) = 70

L = 10   L = 5   L = 9   R = P(ω) = ?

PART III

DATA SHEET

NAME ______ Subject # M ______ DATE ______

Trial No.	L Decision	Trial No.	M Decision	Trial No.	R	Trial No.	Decision
1	1,7	26	5	51	2,35	76	
2	1,65	27	5	52	2,35	77	
3	1,6	28	5	53	2,35	78	
4	1,6	29	5	54	2,5	79	
5	1,6	30	5,5	55	2.5	80	
6	1,6	31	5,5	56	2,6	81	
7	1,7	32	5,5	57	2,6	82	
8	1,7	33	5	58	2,6	83	
9	1,7	34	5	59	2,7	84	
10	1,8	35	5	60	2,7	85	
11	1,65	36	5	61	2.7	86	
12	1,65	37	5	62	2,8	87	
13	1,65	38	5,5	63	2.8	88	
14	1,65	39	5,5	64	3	89	
15	1,75	40	5,5	65	3	90	
16	1,75	41	5,7	66	3	91	
17	1,75	42	5,7	67	3,3	92	
18	1,75	43	5,7	68	3,2	93	
19	1,8	44	5,7	69	3.2	94	
20	1,8	45	5,8	70	3,2	95	
21	2	46	5,8	71	3,2	96	
22	2	47	5,8	72	3,5	97	
23	1,8	48	5,8	73	3,5	98	
24	1,8	49	5,8	74	3,6	99	
25	1,8	50	5,5	75	3,6 [illegible]	100	
	L=10		L=10		L=5		

APPENDIX E

Samples of Rules written by Subjects in Second (Two variable) Experiment

Subject A:

"When both numbers lose reduce the left market by $.05.
When one number loses reduce the losing bid $.05.
When both numbers win raise the number which has been constant for the greatest number of trials $.05.

Exception:

1. If the same number wins twice in a row, and the other number loses twice in a row, raise the winning number $.05 and leave the losing bid alone."

[illegible] 4:

[illegible] at $1.00 each. If both win, add $.05 to [illegible]

After that, if both win, add .05 to the [illegible] if they are equal, to the one that has had the [illegible] previous wins. (If both have had the same number, add to the one with the highest previous winning number. If this is the same, choose the left arbitrarily).

If one wins, and one loses add $.05 to the winning amount only <u>the case if the next highest bid has been tried twice or more and lost each time</u>. (This applies to one wins case).

If both lose, subtract $.05 from the lower, or if equal, the one with [illegible] or if that is equal, [illegible] the lower [illegible] number, etc.

[illegible] 8:

"[illegible] both numbers at $1.50.

If both win, keep the numbers fixed at $1.50.

If both lose, drop the left to $1.25. Proceed in this manner until both win again, then raise the left number back to $1.50. If only one number loses, lower that number to $1.25 until they win again, then raise that number back up to $1.50."

Subject F.

"Starting at 0.60 I will [illegible]. If I lose in one number I will hold that number constant and raise the other. If both win go up in [illegible] numbers. If both lose, hold both constant."

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