AN EXAMINATION OF THE USE OF SOCIAL REINFORCEMENT TO CONDITION SUGGESTION BEHAVIOR

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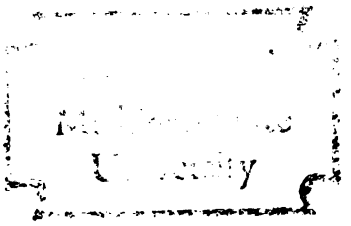

ABSTRACT

AN EXAMINATION OF THE USE OF SOCIAL REINFORCEMENT TO CONDITION SUGGESTION BEHAVIOR

By

Bruce Bernard Saari

The present research tested the general hypothesis that operant conditioning, using only social reinforcers, could affect the acquisition of influence attempt behavior in problem solving groups.

Ninety-six undergraduate psychology students (48 males, 48 females) participated in one hour experimental sessions as members of twenty-four single sex, four person teams. Subjects selected the team in which they would function based on their availability for experimental sessions. During each session a team attempted to solve two multi-solution problems, the first of which provided base line data on influence attempt behavior and the second of which constituted the treatment period. Each team received one of six treatments (contingent praise, punishment, or implementation; non-contingent praise or punishment; and a control condition) based on a random assignment of teams to conditions. The research manipulated the treatments on an individual basis by providing each subject with an individual light display through which the experimenter presented the treatments. Pre-experimental instructions defined the meaning of the lights in the display.

Results of the study did not support the operant conditioning hypotheses. Of seven specific hypotheses based on operant conditioning theory, only those of the null form resulted in predicted relationships. Tests of hypotheses which predicted positive effects of reinforcement indicated significant changes from operant levels, but such changes ran in the direction opposite that predicted by operant learning theory.

A discussion of these unpredicted results includes an evaluation of alternative explanations for their occurrence. Comment is also made concerning the results of the testing of other hypotheses and a combined interpretation follows. The discussion concludes by noting the shortcomings of this research and by presenting possible avenues of future exploration in light of the present findings.

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TABLE OF CONTENTS

LIST OF TABLES

CONCEPT DEVELOPMENT

Introduction

Some authors (e.g., Schein, 1970; Katz and Kahn, 1966) consider organizational development necessary for organizational survival in a changing environment. This belief is generally based on the theory that organizations function as open systems which are required to constantly interact with the outside world. This interaction takes many forms and affects almost every phase of an organization's operations.

One of the more continual interactions that takes place is the movement of the people who make up the organization from the outside environment into the organization and back out again. Since this movement is one that occurs daily, there is a potential for this interaction to be a major conveyor of environmental change to the organization. If a change in some area of life occurs in the outside environment, this change may be brought to bear on an organization by its members. For example, the increase in personal control over one's life which seems to exist due to recent decisions of the Supreme Court may result in the members of an organization desiring a revision of their organization's structure or formal by-laws to permit similar increases in personal control over appropriate areas of their work life.

It is such a change that managerial systems such as participative decision making plans (PDM) are designed to accommodate. Such plans are geared to permit the individual involved in the functional use of a decision to also be involved in the making of that decision. If some segment of an organization decides to change the organization's structure and implement a participative decision making plan in order to help the organization better fit into its environment, it appears necessary that those individuals who will be allowed more personal control or influence over their work must also undergo some type of personal development or change. Thus, it becomes obvious that proper administration of the installation of a PDM plan requires an understanding of the changes necessary in individual members to deal with modifications in the organization's structure.

Such a need for understanding yields many researchable questions which, when answered, should permit more effective action on the part of a change agent in implementing a PDM plan. However, to be manageable, such questions must be approached individually. The present research examines one portion of the question of how people in organizations learn to exert influence or control over various areas of their work life.

Recent Research

Burtnett (1973) addressed a similar question by attempting to demonstrate the existence of causal links between organizational variables assumed to affect the success of PDM and the changes in personal influence patterns in an organization installing a PDM program. In doing so he noted some of the theoretical relationships

which may exist between influence, participation, and organizational effectiveness. This discussion is quite germane to the present research.

From a review of the pertinent literature on influence, participative decision making, and the Scanlon Plan as a form of PDM, Burtnett developed the following ideas. Extending the influence concepts of Likert (1961) and Tannenbaum (1968), Burtnett asserted that the basic premise underlying his research was that "personal influence is inherent in participation," (p. 43). Since other researchers (e.g., Morse and Reimer, 1956; French, et al., 1958; Seashore and Bowers, 1963 and 1969; Coch and French, 1948; Lawrence and Smith, 1955; and Lewin, et al., 1938) had already demonstrated that participation can improve organizational effectiveness as measured by such things as productivity, turnover rates, absenteeism, and communication patterns, Burtnett observed that increased influence should improve organization effectiveness through enhanced participation. After defining the Scanlon Plan as a type of PDM system, he noted that the Scanlon Plan becomes a vehicle for the exercise of personal influence. Thus, it becomes clear that the solution of his proposed research question is important in contributing to the body of knowledge surrounding organizational effectiveness and the development of PDM programs. The same argument can be made for the present research since it deals with the very similar question of how people in an organization learn to influence or control parts of their work life when the organization makes a change toward participative decision making.

While providing a rationale for undertaking both studies, the above observations also demonstrate the theoretical connection between influence and participation on which Burtnett's research was based. Since influence and participation are assumed to be so interdependent, it seems correct to hypothesize, as Burtnett did, that organizational variables which control PDM should also control changes in influence patterns. However, Burtnett obtained little empirical support for his hypothesis. It is this problem which has prompted the present research and which will be the focus of the next portion of this discussion.

To solve the problem of why Burtnett's research resulted in the ambiguity that it did may not be an easy question to answer. However, some attempt at solving it must be made if there is to be an understanding of what controls influence patterns or relationships in an organization. The present research is designed as such a study, focusing first on both the logical and methodological quality of Burtnett's research.

To reconstruct the theoretical framework developed by Burtnett, previous research and discussion on which he based that framework must be re-examined.

The foundation of his framework comes from the definitions of influence and participative decision making. Quoting Cartwright's (1965) definition of influence, Burtnett begins as follows:

> (a) the agent exerting influence, who for convenience is denoted O, (b) the method of exerting influence, and (c) the agent subjected to influence, denoted P. When an agent, O, performs an act resulting in some change in another agent, P, we say that O has power over P (p. 4).

This definition can be extended to cases where O and/or P are groups of people rather than individuals since there is no reason to assume that groups cannot act as a unit in changing the behavior of some other individual or group or vice-versa. Note also that all three parts of this definition of an influence relationship are necessary, but not sufficient, conditions of influence since no single part is capable of resulting in influence by itself.

Coupled with this definition of influence, Burtnett presents participative decision making in Lowin's (1968) terms: PDM is a "mode of organizational operation in which decisions as to activities are arrived at by the very persons who are to execute those decisions (p. 69)." Integrating this definition with the definition of influence given above, Burtnett starts his actual framework by asserting that influence can be considered inherent in participation (p. 43). To determine the stability of his framework, the logical accuracy of this premise must be established.

Assuming that "inherent" means "essential to," consider the participative process which follows from Lowin's definition. Some member, A, of an organization performs a given function, X. At some point in time, X requires some type of decision if it is to continue as a function of the organization. PDM encourages A to make that decision and then requires the organization to react to X by either retaining it under its new form after the decision or by relinquishing X as a function. The necessity of influence in participation is thus established as almost axiomatic in the definition of participation.

From a further review of the literature, Burtnett presented a discussion of the impact of participation on an organization's

effectiveness. The literature involved has been noted above in the earlier discussion of Burtnett's research. If participation is as beneficial to the effectiveness of an organization as the literature reviewed indicates, and if influence is inherent in participation, as it has been shown to be, the control of influence parameters through the manipulation of variables which enhance participation would be logically plausible and should lead to enhanced organizational effectiveness. Therefore, the theoretical framework for Burtnett's study of influence in a PDM system appears to be consistent and logically correct. The point of focus of the present study must now shift to an examination of Burtnett's methodology for testing his hypotheses if a solution to the problem of nonsupportive results is to be found.

Clearly, the major operational definition of interest in understanding his research is that of influence. Since the research method took a survey approach, influence was defined by a series of responses to questions. These questions were in a five-point Likert-type format requiring a respondent to report how much he perceives himself determining the course of something or, how much say he has about some area of the work situation or, how much control or influence he has over a given situation. This operational definition of influence departs from Cartwright's theoretical definition in that the operational definition includes Tannenbaum's (1968) division of influence into an actual component (i.e., this is the way it is) and an ideal component (i.e., this is the way it should be) and also adds an importance measure (i.e., this part of my job is important). From this series of three-part questions Burtnett can

arrive at an operational definition of a given type of actual and ideal influence and the importance of the influence to the individual by determining scores on various sets of questions.

Another concept which Burtnett was required to operationalize was that of the organizational variables which enhance PDM. Lowin (1968) lists fourteen such conceptualized variables from which Burtnett has chosen five. These five are: 1) organizational commitment to PDM; 2) the amount of public feedback in the suggestion system; 3) the degree of difficulty of the problems handled by PDM; 4) the clarity of goals in the organization; and 5) the linkage which exists between suggestions and bonus payments. As in the case of influence, these variables were defined and measured by a series of questions in the questionnaire. The questions were five-point Likert-type items but lacked the three part character (actual, ideal, and importance) of the influence questions. Scores on subsets of these questions could be combined to provide a measure of each variable.

The final concept which Burtnett needed to operationalize was that of causality, since he used correlated survey data as the method of research. Drawing on the methodology literature in sociology (e.g., Simon, 1954), Burtnett describes a technique called cross-lagged panel correlations as a means of obtaining causality indicators from survey data. The first step in operationalizing causality is the measurement of pertinent variables at two points in time. After this has been done, the theory of cross-lagged correlations can be applied. This theory holds that a causal relationship is asymmetrical with respect to time and thus correlations between cause and effect variables across time should be unequal. Such a method never actually

permits proof of complete causality, nor does it completely handle the problem of third variable causality, but the theory does assert that predominant causality can be determined. Therefore, what can be found is an operational form of predominant causality.

Use of cross-lag correlations on questionnaire data indicates that the question of interest in determining the adequacy of this method rests in the determination of the psychometric quality of the measures used. A second technique-type question which may also arise deals with the proper use of the cross-lagged correlation technique. In his discussion of the results, Burtnett pointed out the deficiencies of his method in both of these areas by demonstrating that neither was completely adequate. Considering the psychometric analysis of the questionnaire, it seems logical to agree with his desire for more adequate research in the future. However, before proceeding to describe a methodology which may alleviate some of the problems encountered by Burtnett, one additional area of theoretical discussion, not previously mentioned, should be considered.

Since influence is often considered a personal attribute, the study of group patterns, as was performed by Burtnett, may need to be expanded to consider some treatment of personal patterns as well. To do so focuses on the personal changes necessary for the implementation of PDM that were noted in the introduction to this discussion. Such a focus points up a possible link between research like that performed by Burtnett in the interest of organizational development and the research around personnel development techniques which are available to organizations for use in helping their members adjust more completely to their organizational environment.

Personnel Development and Operant Learning Theory

Although personnel development is often equated with training practices and skills, it may be the case that such development goes far beyond the confines of formal training sessions, and functions on a more individual basis than is assumed in some organizational personnel development plans. J. W. Gardner (1964) intimates such a possibility while addressing the problems faced in developing an organizational or societal immunity to decay. He says, "A society decays when its individuals and institutions lose their vitality (p. 2)." He takes a rather Gestalt-like position and sees the component parts of the organization (society) as the key to its vitality. Amplifying this, he also notes that the processes which cause an organization to become rigid and lose its vitality are those same processes which result in maturity. Thus, to mature without becoming rigid seems to be the logical and ultimate goal.

According to Gardner, such a goal is reached only through the efforts of self-renewing men who are defined as those individuals (emphasis added) who "systematically pursue and explore their full range of potentialities, and for whom the processes of self-discovery never end." He postulates that organizational vitality is a function of individual development, and that this development is a continual process. Such development may be the life-line of the future for the organization while remaining a function of the development of individuals in that organization.

Such a theory of individual development being an integral part of an organization's vitality is also consistent with the philosophy of participative decision making as discussed above (which is based

on McGregor's (1960) Theory Y). Other writers, such as Blum and Naylor (1968) also note this consistency. "In Theory X the emphasis . . . is on the goals of the organization. In Theory Y the emphasis is directed toward the . . . individual (p. 431)." Therefore, it can be concluded that the emphasis of personnel development in a participative decision making system should be on a continual, individual basis which will benefit both the individual and the organization.

This necessarily presents the problem of understanding the processes of individual development. Such an understanding may then be used in administering personnel development plans. It should be noted, however, that such concern for individual development processes has been the focus of the psychology of learning for many years. Thus, knowledge from the field of human learning should be helpful in understanding and facilitating such processes. This conclusion is the major emphasis of several articles dealing with the theoretical application of learning theory to management theory (Aldis, 1961; Griffing, 1965; Nord, 1969; and Jablonsky and DeVries, 1972).

One of the major concepts that has come out of learning research is that behavior is controlled by its consequences. This has come to be the foundation of operant learning theory. Operant conditioning is basically defined by the Law of Effect which states that any behavior followed by pleasant or satisfying effects will be learned while behavior followed by unpleasant effects will not be learned (Thorndike, 1932). Those effects which are satisfying or pleasant are called reinforcers while those which are unpleasant are called negative or aversive stimuli. (Of course, there are also neutral

effects which are neither reinforcers nor negative stimuli, and which do not affect the learning of behavior unless they are associated with reinforcing or aversive stimuli).

B. F. Skinner (1938) gives one of the more thorough reports of early research on this topic while discussing some of the theory involved in the operation of this law. He rephrases the law somewhat by defining behavior as only those operations performed by some organism on its environment which are observable and definable by other organisms. He also defines learning as the process of increasing the strength or probability of a given behavior through the administration of the proper reinforcers. To operationalize the concept of strength or probability more completely he says, "The strength of an operant (any behavior not under stimulus-response control) is proportional to its frequency of occurrence, (Skinner, 1938, p. 21)." Thus, he views operant learning theory as functional in describing the acquisition of observable, definable behaviors whose frequencies (or rates of emission) can be recorded. It is from such an operational definition that research has proceeded. It will be to such research that the remainder of this literature review will be devoted, focusing primarily on cases where the research is aimed at applications in real-life learning situations. Such a focus should demonstrate how results obtained from operant learning research might be useful in personnel development programs.

Concerning operant research in general, Kimble (1961) proposes that such researches

> have as their aim the delineation of a set of functions describing the manner in which certain parameters of

> reinforcement control performance in the learning situation (p. 137).

This is consistent with the Law of Effect in that if behavior is controlled by its consequences, those consequences (i.e., reinforcers) should be considered the primary independent variable, while frequency of behavior emission becomes the obvious dependent variable, in any pertinent research. Research using such variables has provided much support for the theory of operant conditioning in a highly controlled situation (Skinner, 1961). It has pointed up the importance of reinforcement in any learning situation that involves operant behavior. Skinner has gone so far as to say that such research has shown that "almost any instance of human behavior involves contingencies of reinforcement . . . (Skinner, 1968, p. 333)." Elsewhere he says,

> In short, in the field of human behavior as a whole, the contingencies of reinforcement which define operant behavior are widespread if not ubiquitous. Those who are sensitive to this fact are sometimes embarrassed by the frequency with which they see reinforcement everywhere . . . (Skinner, 1966, p. 31).

Thus, it appears some researchers assume that reinforcement is an active part of all human learning. Research designed to determine the effectiveness of the principle in field situations will now be discussed in an effort to demonstrate findings which would aid in the conceptual development of personnel programs around operant conditioning theory.

A large portion of the research involving human operant conditioning outside the laboratory has focused on changing behaviors which are considered abnormal. Such behavior modification research has been reported in several places (e.g., Ullmann and Krasner, 1965;

Ulrich, Stachnik, and Mabry, 1966; Bandura, 1969; Wolpe, 1969; Mikulas, 1972). However, the great majority of this research is not particularly germane to the present research since it deals with modifying behavior in therapy-like situations. It should be noted, however, that in individual situations, behavior therapy techniques have been highly successful, resulting in more constructive and useful behavior patterns for the patient. The usefulness of reinforcement principles outside the laboratory begins to be demonstrated by this body of research.

A second area of concentration focuses on the use of programmed instruction (PI). This method of education is based on two key principles of operant learning. The first is that reinforcement is most effective when presented immediately after the desired behavior is emitted, while the second is that complex behavioral responses can be built up by the processes of shaping and chaining (Reese, 1966).

The research surrounding PI and its automated sister, the teaching machine, is quite thorough and very diverse. Many areas of human learning, including formal education and on-the-job training, have been researched to determine the effectiveness of this method in applied settings (Lieb, et al., 1967; Mayo and Longo, 1966; Welsh, et al., 1965; Lysaught, 1961; and Margulies and Eigen, 1962). Although this research demonstrates the effectiveness of operant conditioning outside the laboratory, it does not approach the question of the ubiquity of reinforcement principles outside a structured learning context.

There is some research which demonstrates the use of operant principles outside the confines of a laboratory setting and apart from the structure of some form of PI training program. Those research efforts are of special interest here because they demonstrate cause for proposing that operant learning is capable of contributing to personnel development within organizations.

Ayllon and Azrin (1968) report extended longitudinal research aimed at developing an entire motivating environment through the use of operant conditioning principles. Working in one unit of the Illinois State Hospital System, the researchers conducted several experiments in which they used the hospital patients as subjects. The studies varied in their basic design and immediate objectives, but all were intended to test aspects of general "principles" used to develop the motivating environment. The authors state,

> . . . previous research . . . provides little information on how to use reinforcement theory in a complex and fairly naturalistic environment. Most studies of reinforcement theory have taken place in laboratory situations or in a room where the individual has been isolated from others . . . The only conclusive way of determining whether the Laws of Reinforcement and Extinction can be used as the basis of designing a complex motivating environment is, of course, to try it (p. 6-7).

By the end of the first year of work, the research methods for this study had been broadly outlined on the basis of the laws of operant learning. The remainder of the research time (which ran until the writing of the 1968 publication, or about four years) was spent modifying, revising, and developing particular aspects of the procedure. To accomplish this, when procedures were found effective in practice, as measured by an increased frequency of functional behavior emission on the part of the patients, a "general rule" was formulated on the

basis of earlier psychological research evidence. Such a formulation then led to other testable procedures which the researchers claim may have never occurred to them otherwise. "Every rule led to some application to determine its usefulness, and every procedure found useful was the basis of another rule (p. 82)."

The iterative process described above resulted in at least twenty-three general rules which were experimentally demonstrated to be useful in increasing motivation of the patients. Some examples of these rules are:

1) Target Behavior Rule: Describe the desired performance in behavioral terms.

2) Variation of Reinforcement Rule: Use many variations of a known reinforcer to discover new ones.

3) Multiple Reinforcer Rule: Use many different types of reinforcing stimuli with a given individual.

These rules and their derivations are too cumbersome to enumerate here, but all involve some direct application of operant theory to developing a motivating environment. If so many general rules were empirically shown to be useful, the experimentally developed environment was probably, as the authors state, effective in motivating the behavior of hospital patients.

The research discussed above is in some ways reminiscent of the behavior modification work discussed earlier. However, since this research demonstrates the possibility of constructing an entire living environment on the basis of operant conditioning principles, it seems too important to be overlooked.

Moving from a hospital setting to an elementary classroom, Krumbolz and Goodwin (1966) studied learning in a formal setting

without the structure of PI or teaching machines. To test hypotheses concerning the efficiency of operant learning techniques in such a situation, the experimenters attempted to develop more appropriate task-oriented classroom behavior in second grade boys (who normally exhibited disruptive behaviors) by manipulating the amount of social reinforcement the boys received.

It was hypothesized that operant conditioning could be effective in increasing the time spent on task-oriented behaviors by inattentive pupils. To test this hypothesis, a sample of boys with problem behaviors was selected from three schools within one school district in California. The teachers of these boys were then assigned to experimental and control groups, and the experimental group teachers were trained in methods of social reinforcement which they could administer to the boys in their classes. The control group teachers received placebo training designed to maintain their present use of social reinforcers. All teachers and pupils were then observed in their normal classrooms by trained observers who followed the conditioning process and recorded pertinent information on the administration of reinforcement and the time spent on task-oriented behaviors.

The results of this study were, in general, not supportive of the hypothesis. In almost all cases of testing secondary and derivative hypotheses, results actually ran opposite to the predicted direction, based on operant learning theory. That is, in all cases but one, the control group actually showed (statistically) non-significant but greater increases in time spent on task-oriented behaviors than did the experimental groups. The one case where this

was not true was in relation to time spent on independent task-oriented behaviors. In this case, the results were also statistically non-significant, but they were in the hypothesized direction. This lack of clarity in results may be due to ineffective manipulation of the independent variable, as the authors note, but more importantly it points up the necessity of extreme care in conducting such a real-life type of study in this area.

Sarbin and Allen (1968) studied another aspect of the operant conditioning of classroom behaviors that comes even closer to approximating the conditioning of operant behavior in normal adults in an organizational setting. Using four member students in a graduate seminar, the effects of social reinforcement on verbal participation in group discussions were studied.

Using Skinnerian principles, four subjects (two with a high operant level of participation and two with a low operant level) were unknowingly subjected to a contingency program of verbal approval, head nodding, and other supportive actions. The two high operant level Ss were deprived of social reinforcers contingent on participation to decrease participation while the two low level Ss were given positive contingent reinforcements to increase participation. All seminar sessions were recorded on tape so the percentage of total participation could be figured accurately, but secretly for each S.

The results of the study indicate that although low operant level Ss never exceeded high operant level Ss in percentage of the group's total participation, there was a definite change in the amount of time spent participating by each group. Low operant Ss increased

their percent of total participation by about fifteen percent while high operant Ss decreased their percent of total by the same amount. Thus, the possibility of operant control of participative behavior using fairly intelligent young adults as subjects seems reasonable.

Azrin (1960) approached the problem of operant conditioning of adult humans in a controlled work setting in a direct way. Using rest periods as reinforcement for performing a simple but strenuous work task, and administering them on both fixed interval and fixed ratio schedules, it was demonstrated that rest, when administered on some response contingent schedule, is capable of increasing the rate of work output. However, since this study involved a controlled work setting and little actual learning, along with no interaction between working Ss, its generalizability to personnel development in an actual work setting is somewhat limited.

Gupton and LeBow (1971) report a study using operant principles in a real-life work situation. Using two male subjects who were part-time telephone solicitors in a large company, the researchers studied the effects of a contingency program on the rate of performing a low probability behavior, when the opportunity to perform a high probability behavior was made contingent upon performing the low probability behavior. The low probability behavior in question was selling new appliance contracts while the high probability behavior was renewing old contracts. By making the opportunity to sell five renewal contracts contingent on the sale of one new contract, the amount of sales for both types was increased over previous operant levels. A reversal phase in the design strengthened support for the notion that operant control of productivity was attained. To

quote the researchers,

> The main implication of this study is that costly job analysis designed to increase productivity could perhaps be replaced by observing response rates, designating high and low probability behaviors and instituting the appropriate contingencies. The monetary savings and effectiveness of this behavior management approach make it worthy of pursuing (p. 82).

This research demonstrates the possibility of using operant conditioning in a work setting. However, it does not address the question of conditioning an interactive behavior among workers as posed by the problem of acquiring influence patterns.

Summary of Learning Research

The last two researches reported demonstrate, to some extent, the direct applicability of operant conditioning techniques to management theory as was suggested earlier. The studies preceeding them are indicative of other areas within a work setting that may be susceptible to operant conditioning in a fashion similar to that of the last two studies.

The final two studies demonstrate techniques from which management of an organization stands to gain the most or where operant conditioning is used for the benefit of management. The three preceeding studies indicate situations where there may be individual gains in obtaining a more personally useful behavior repertoire while also demonstrating some potential gain for those in charge of the individual's behavior. The gap between the two types of studies is obvious. Research in work settings has not sought mutual gains. It appears necessary that if personnel development is to be understood and conducted on a continual and individual basis, research must be performed which will fill this gap.

Another indicator of the need for more applied operant research is its dearth in the present training literature. Although training research has often focused on the effects of various feedback programs, none of this work has approached the problem from an operant point of view. Rather, such research has primarily tested the potency of degrees of feedback specificity in relation to both positive and negative feedback. J. P. Campbell, in a review of the training literature (1971) points out the fact that training research has not previously pursued the area of operant conditioning and behavior modification in an effective way. After noting the behavior modification research discussed above (e.g., Bandura, 1969) as the major training research in this area, he concludes his review by saying,

> If we are ever to make training and development a profitable enterprise in terms of important behavior changes we must at least . . . Take an intelligent plunge into the methods and concepts of behavior modification . . . (p. 586).

Therefore, with respect to the problem posed by Burtnett's research and the desire to understand the acquisition of influence patterns in an organization, one plausible research approach is to pursue the possibility that operant conditioning could enhance participation in a work setting which allows some form of participative decision making. Such research would demonstrate the consistencies between individual development as the basis of organizational immunity to decay, participative management which focuses on the individual, and operant learning theory which may contribute to both of the above areas. Although the Sarbin and Allen article reported above appears to address this question, it is inadequate in at least two respects. These two areas will be discussed below.

Hypotheses

The connection between the potential applicability of some learning phenomenon to personnel development and the ambiguity presented by the results of Burtnett's research was initially indicated in Burtnett's discussion of his results. He said,

> It should be pointed out that the exercise of personal influence called for by the Scanlon Plan is typically not found in most organizations and therefore the people at all levels in a firm implementing the Plan must learn appropriate influence styles (p. 87).

If this is the case, the learning process involved in obtaining a personal influence pattern must be understood before research can look for variables which might enhance influence. To demonstrate the existence of an operantly conditioned learning process then, is the objective of the present research. The development of such a deomonstration requires two things--a set of operational definitions of the variables under study and a set of hypotheses on how those variables should be related if such a learning situation exists.

Studies of learning generally follow Skinner's definition of behavior as a model for defining their dependent variable as some observable activity which the organism under study emits. To be consistent with previous learning research, and in an effort to avoid the measurement problems encountered by Burtnett, logic seems to dictate that the concept under study in the present research (i.e., influence) be defined in a behavioral manner. Since no part of Cartwright's three-part conceptual definition of influence is logically stronger than the other two (all three are necessary, but not sufficient conditions for the influence relationship to occur), the best operational definition would be the most behaviorally-based

part. This necessitates choosing the act of agent O in exerting influence over P as the dependent variable to be examined. Since such an act is not influence by itself, the observed behavior may only be termed an influence attempt. Because it is commonly held by organization developers that the greater the number of influence attempts which exist in a PDM system, the more likely it is that PDM will succeed, such a focus on an individual's influence attempts may not be as limited as it might appear.

The second variable of importance in any research is the independent variable being manipulated (most commonly a reinforcer in learning research, or one of its parameters, as noted earlier by Kimble). The measurement problem encountered by Burtnett, and mentioned previously, requires the study of only those independent variables which are observable. In the social context in which influence usually occurs, those independent variables which are assumed to control the operant behavior of influence attempts can be either reinforcers or aversive stimuli. Therefore, the following social variables have been selected for examination: 1) Individually received praise contingent on individual performance; 2) Individually received implementation contingent on individual performance; 3) Individually received aversive stimuli contingent on inadequate individual performance; 4) Individually received noncontingent praise; and 5) Individually received noncontingent aversive stimuli.

Before actually describing the methods of experimentation used in the present research, it seems appropriate to present a set of hypotheses concerning the relationships between these variables and the

dependent variable. These hypotheses are based on the knowledge gained from the learning literature reviewed above.

Hypothesis 1

Considering the basic premise of operant conditioning (i.e., behavior which is reinforced is learned or acquired), it is hypothesized that individually received contingent praise will cause the frequency of acceptable influence attempt emission to increase significantly above the operant level of emitting acceptable influence attempts.

Hypothesis 2

As in the case of contingent praise, individually received contingent implementation of acceptable influence attempts will result in a significant increase in the frequency of acceptable influence attempt emission above the operant level of emitting acceptable influence attempts.

Hypothesis 3

Because operant conditioning relies on the efficacy of positive reinforcement, it is hypothesized that individually received aversive stimuli (contingent on unacceptable influence attempts) will result in no significant change in the frequency of acceptable influence attempt emission from the operant level of acceptable influence attempt emission.

Hypothesis 4

Similarly, because operant conditioning also requires that reinforcement be contingent on performance for learning to occur,

it is hypothesized that noncontingent praise will result in no significant change in the frequency of acceptable influence attempt emission from the operant level of acceptable influence attempt emission.

Hypothesis 5

Likewise, it is hypothesized that noncontingent aversive stimuli will also result in no significant change in the frequency of acceptable influence attempt emission from the operant level of acceptable influence attempt emission.

Hypothesis 6

Unless there is some inherent problem in the experimental method, it is hypothesized that a control treatment condition (where neither reinforcing nor aversive stimuli are presented) will result in no significant change in the frequency of acceptable influence attempt emission from the operant level of acceptable influence attempt emission.

Hypothesis 7

In comparing the treatment conditions to the control group, it is hypothesized that individually received contingent praise and implementation will both cause significant increases in the frequency of acceptable influence attempt emission above that occurring in the control condition, while contingent punishment, noncontingent praise and noncontingent punishment will cause no change from the frequency of acceptable influence attempt emission when compared to the control group.

Corollary 7-A

It is hypothesized that there will be no significant difference in the frequency of acceptable influence attempt emission for the treatment conditions of contingent praise and contingent implementation.

Corollary 7-B

It is hypothesized that there will be no significant difference in the frequency of acceptable influence attempt emission for the treatment conditions of contingent punishment, noncontingent praise, noncontingent punishment, and the control condition.

Stated generally, the above hypotheses predict that the treatment conditions will cluster into two groups in terms of their effectiveness. Individually received contingent praise and contingent implementation will be equally effective in conditioning acceptable influence attempt behavior, while contingent punishment, noncontingent praise, noncontingent punishment and the control condition will be equally ineffective in conditioning such behaviors.

METHODOLOGY

The Sarbin and Allen research reviewed above appears to address a problem similar to the one posed by the present research. However, in testing the hypothesis that operant conditioning controls participation in a group setting, their research encountered certain difficulties which must be appropriately dealt with before drawing the conclusion that influence attempt behavior can be operantly conditioned using social reinforcers.

The first of these difficulties is the use of apparently ipsative measures of participation. To imply, as Sarbin and Allen do, that an increase in the percentage of total participation experienced by reinforced Ss is independent of the decrease in the percentage of total participation time experienced by non-reinforced Ss in the same group appears dubious. Such an implication makes it difficult to conclude that operant conditioning is the only possible cause of changes in participation percentages.

Secondly, the research was primarily concerned with increasing verbalization among the reinforced Ss. There was no attempt to manipulate one particular type of participation (e.g., statements of new information), making it difficult to know if all types of participatory behavior were equally controlled by the conditioning process. It appears feasible that what could have occurred was an increase in something such as repeating what someone else had already

said or simple question asking behavior. Therefore, it is not possible to conclude that influence attempt behavior has been demonstrated to be controlled through operant conditioning procedures.

A final difficulty with the research is the use of only two subjects in each of the two treatment conditions. Although this is a frequent occurrence in learning studies, it must be recognized as a limiting factor in the generalizability of those studies' results. Some attempt at increasing sample size must occur if generalizations about human learning are to ever be meaningful.

A second research effort which should be considered as important to the methodological development of the present research is a study conducted by Oakes (1962). In this study he attempted to condition response classes, as defined by Bales' Interaction Process Analysis (1950), in a group discussion. Four subjects were seated around a table on which two cardboard screens stood. These screens were placed in a manner to prevent subjects from seeing one another. Each subject had a light display placed in front of him through which reinforcement was presented. Each such group was given a clinical case study and three possible explanations for the behavior described in the case. Subjects were then instructed to discuss the three explanations in terms of how well and why they explained the described behavior. Subjects were also instructed not to come to a group decision. The group discussion was then viewed through a one-way mirror by an experimenter who administered reinforcements to each S when the S exhibited a behavior in the Bales' category which was being reinforced in his group. All twelve of Bales' categories

were considered and two groups were reinforced for each category. After the experimental session was over, two other experimenters categorized the individual behaviors into Bales' groupings based on written transcripts of tape recordings of the session and tabulated each individual's behavior total for each of the twelve categories. The analysis was then based on this categorization, comparing a category of behavior for the individuals in the twenty-two groups not reinforced for that specific category with the same category for the individuals in the two groups which were reinforced for that category. Based on such an analysis the author concludes that he found "support" for the conditionability of Bales' category "giving opinion." None of the other eleven categories' conditionability gained empirical support.

Ignoring the fact that the "supportive results" are no more than what would be predicted by chance alone, the author's interpretation of the results still seems somewhat tenuous in light of the methodology employed. The difficulties with the methodology and their impact on interpretation are as follows. First, no operant levels of behavior were established for any of the categories. Thus, there do not seem to be sufficient grounds to argue that any category of behavior was actually modified. It could be that reinforcement had no effect and that the obtained results are due solely to differential operant levels within a given category. Within a group of subjects, all the author can conclude from his data is a relative difference in emission rate for one behavior category as compared to the others. A second problem arises from the fact that the analysis of variance design was required to compare a mean computed on eight

people with a mean computed on eighty-eight. Such unequal cell sizes must naturally raise questions about the interpretation of the results. Thirdly, in using the categorization described above, the author reports no reliability estimate between the categorization of a behavior as defined by the first experimenter (who also defined the reinforcement contingencies) and the categorization of that same behavior by the other two experimenters (who provided the data for the analysis). Thus, there is no assurance that a behavior which was categorized as being reinforced in the analysis was in fact subject to a reinforcement contingency in the experimental session. A fourth difficulty is in not allowing the Ss to see each other. Such a situation greatly reduces the face validity of the method for research in group problem solving. The final difficulty to be mentioned which weighs heavily on the present research is Oakes' finding in relation to the behavior category called "giving suggestions." The methodology employed (i.e., giving Ss a finite set of solutions and asking the Ss not to reach a group decision) seems to limit the appropriateness of this type of response. This fact seems to be borne out by the results in that few behaviors actually were categorized as "giving suggestions." Thus, Oakes' design does not seem to permit an adequate test of whether "giving suggestions" or any other behavior category can be modified by operant conditioning.

In light of the difficulties encountered in interpreting Sarbin and Allen's and Oakes' results, it seems efficient to develop a methodology which is capable of avoiding such pitfalls and which yields direct answers concerning the conditionability of influence attempt behavior. The methodology presented in the remaining portion of this

section was designed with these two primary considerations in mind. A third factor which influenced the design was the set of implications drawn from Burtnett's research.

Subjects

The subjects used in this research were 96 undergraduate college students (48 males, 48 females) obtained by the researcher through the Human Subjects Pool at Michigan State University during the summer and fall terms of 1974. This pool is made up of all students enrolled in the introductory undergraduate psychology classes for a given term, and all subjects who participate in such experiments earn extra credit toward their class grade. Subjects were asked to participate in an experiment in group problem solving and were each given three extra credit points for their participation.

Experimental Method

The experimental phase of this research was conducted in two parts. The first phase was a brief meeting of subjects with the experimenter for the purpose of assigning subjects to four-person groups. The second part of the research involved an hour meeting of each four-person group. Each four-person group was made up of four males or four females. No mixed sex groups were used in the research.

In the first session, subjects reported to the experimenter's office and were asked to fill out a biographical questionnaire which asked for information on name, age, race, years of college completed, any previous leadership experience, and grade point average. The subjects were instructed that all of these data would be used only in

a descriptive manner and that no individual data would be released to anyone but the subject himself. After completing the questionnaire, subjects were asked to sign up for an hour experimental period in which they would participate in group problem solving as members of a four-person group. Since subjects could only sign up for experimental periods which were compatible with their schedule, random assignment of subjects to groups was impossible. To correct for this, assignment of four-person teams to treatment and control conditions was on a strictly random basis.

The second experimental session involved the actual experimental portion of the study. Combining some of Oakes' method with a method developed by Hoffman, Burke, and Maier (1965) in the study of influence in participative problem solving groups, this hour session was conducted as follows.

Subjects reported to the experimenter's office and waited there with the experimenter until all four group members arrived (the same experimenter met all groups and conducted all the experimental sessions). Subjects were then conducted to a laboratory and seated around a square table. The team was then instructed to practice group problem solving by attempting to collaborate on a work-related problem for fifteen minutes in an effort to develop a series of rationales for and concomitant solutions to the problem. The complete set of instructions for this part of the experiment are presented in Appendix A along with a copy of the problem used in this practice period. The behavior exhibited in this phase served as a measure of the operant level of each individual's influence attempt behavior.

At the end of this fifteen-minute session, the subjects were instructed that they were now going to be asked to work for thirty minutes on another problem which was in need of a solution. Subjects were then presented with the work-related problem and permitted to work on it for thirty minutes. Subjects were also instructed concerning the meaning of the light display on the table in front of them, thus defining social praise and punishment. Complete instructions for this period and a copy of the problem used for this thirty-minute session are presented in Appendix B.

During this period, the treatment assigned to the group was administered to each individual in the group (See Table 1 for a description of assignment of groups to treatment conditions). Upon completion of the thirty-minute period, the subjects were partially debriefed, given credit for the experiment, and dismissed. Before leaving they were also told that if they would like to receive a full debriefing on the experiment, they could receive such feedback by mail by leaving their address with the experimenter.

In a more specific sense, this hour experimental session included the following. The two problems the group had to solve are adaptations of two role playing exercises Maier presents in his book Supervisory and Executive Development (1957). The problems are 1) "The New Truck Dilemma"--the fifteen-minute practice problem and 2) "The Parasol Assembly Bottleneck"--the thirty-minute problem.

Each group was seated around the table in alphabetical order according to last names to reduce the possibility of extraneous proximity effects as much as possible. Before beginning the group activity, participants were told that the nature of the research

TABLE 1. Treatment Conditions and Distribution of Subjects

CONTINGENT PRAISE	CONTINGENT IMPLEMENTATION	CONTINGENT PUNISHMENT	NONCONTINGENT PRAISE	NONCONTINGENT PUNISHMENT	CONTROL
4 Groups (2 Male & 2 Female)	4 Groups (2 Male & 2 Female)	4 Groups (2 Male & 2 Female)	4 Groups (2 Male & 2 Female)	4 Groups (2 Male & 2 Female)	4 Groups (2 Male & 2 Female)
16 Subjects	16 Subjects	16 Subjects	16 Subjects	16 Subjects	16 Subjects

required that the group session be tape recorded. This required that each participant announce his number (Ss' numbers assigned within each group by seating arrangement) before participating in the group discussion (Oakes used a similar identification system with little apparent difficulty). The group was then instructed concerning the fifteen-minute problem and permitted to work on it. A copy of the problem was left with the group. During this time, the experimenter recorded the operant level of each individual's acceptable influence attempt behavior. The experimenter was seated at another table in the same room as the group, but outside the group's circle of activity.

This recording process raised two operational problems, the first of which is the definition of acceptable influence attempt behavior. Although much research, including Oakes', has relied on Bales' categorization and definition of behavior, the present research used a method of behavior categorization developed by Hoffman and Maier (1964). Because the influence attempts discussed above are intended to simulate the influence system developed in Scanlon Plans, it seems important to use a categorization technique which approximates that situation. Although Bales has a category called "giving suggestions," he makes no differentiation between solution-oriented suggestions and other kinds of suggestions. Since the Scanlon System is designed as a problem solving device, such a distinction seems too important to overlook. The Hoffman and Maier system permits such a distinction in that it categorizes only solution-oriented behaviors. Thus, the Hoffman and Maier system seems more appropriate for the present research.

This system operates as follows. An observer of a group meeting is required to categorize each person's behavior whenever such behavior is judged by the observer to be a solution-oriented one. This behavior is categorized and recorded by means of a shorthand symbolism which indicates what type of solution-oriented behavior has occurred, and who has emitted the behavior. The types of behavior categorized by this system and their shorthand symbols are: 1) S--statement of a solution--complete and partial descriptions of solutions; 2) J--justification--arguments supporting the solution; 3) A--agreement--expression of agreement that the solution is a good one, without justification; 4) V--vote--attempt to solicit support for a solution; 5) R--reconnaissance--questions seeking detail about the solution; 6) Q--questions--interrogative statement implying doubt or criticism about a solution; 7) C--criticism--statement criticizing the solution.

In the present research, the above categorization was employed, not as a means of actually recording behavior, but as an operational definition of influence attempt behavior. Since the Scanlon System is intended primarily as a positive way of confronting problems, only those behaviors judged by the observer (experimenter) to fall in the "S" or "J" categories were considered acceptable influence attempts. Then, in the experimental period, reinforcement was also administered according to this criterion. Similarly, unacceptable influence attempts and the criterion for aversive stimulation were those behaviors falling in the "Q" and "C" categories.

To actually record these behaviors in the practice session, a mechanical recording device was used. As stated earlier, each session

was tape recorded, giving a complete record of the group's verbal interaction. To provide a record of the pertinent behavior categorization, an event recorder was also employed. This recording device was made up of 1) a twelve-button control panel with three pushbuttons for each subject, and 2) a six-pen event recorder. One button on the control panel for each subject was called the behavior button. It was directly attached to a separate pen on the event recorder for each subject. Each time an influence attempt behavior (whether acceptable or unacceptable) occurred, the behavior button was pressed and the pen recorded a mark for that subject. The other two control panel pushbuttons for each subject were 1) a reinforcement recording button (attached to a common pen for all subjects) which was pressed to record the emission of acceptable attempts or the presentation of a reinforcer (during the treatment period); and 2) a punishment button (attached to a common pen for all subjects) which was pressed to record the emission of unacceptable influence attempts or the presentation of punishment.

The three records which result for these pens must then be interpreted in conjunction with each other to provide complete information on which subject emitted the behavior and whether or not it was an acceptable influence attempt (i.e., the behavior buttons' records identify the subject, and the reinforcement and punishment buttons' records identify the type of behavior). It should be noted that the use of such a recording system was facilitated by the use of continuous schedules in the treatment period. This is not necessarily a hindrance to the research since in the treatment period under study, it is probably necessary to use continuous schedules to maximize conditioning

effects during such a short conditioning period (thirty minutes). It should also be noted that records of noncontingent treatments could be made by a slight recoding of the pens' records.

When the fifteen-minute practice problem was over, the subjects were instructed concerning the thirty-minute problem and allowed to work on it. The appropriate contingencies were then applied.

As stated earlier, these contingencies employed socially available stimuli. Thus, both reinforcers and punishers were socially defined. Reinforcement took the form of praise or implementation. These were administered in the following manner. Placed on the table in front of the subjects was a small square box with two recessed lights on each verticle face of the cube, thus there was a set of two lights facing each subject which no one else could see. Prior to the start of the thirty-minute problem, the subjects were instructed concerning the meaning of the lights in the box and were encouraged to do their best in solving the problem. For praise, a green light in the box was defined as indicating that an emitted behavior was helpful to the group in solving the problem. For the implementation condition, the green light was redefined as indicating a solution of high enough quality to be used in future research on the problem (subjects in this condition were told that they were part of a pilot study examining the usefulness of the problem). For punishment, a red light was defined as notification that previously emitted behavior was considered unacceptable in helping the group solve the problem. In the noncontingent situations lights were defined as general indicators of the nature of the behavior and were not directly related to immediately preceeding behavior. Naturally, in the control

condition lights were not defined and subjects worked on the problem for thirty minutes with no lights being presented.

The lights in the box were activated by the experimenter from the control panel by pressing the reinforcement or punishment record button for a given subject. While these buttons operated a common pen, they were wired to only activate one subject's light at a time. It should also be noted that all lights are powered by a master switch which permits the recording pens to function while the lights are shut off for recording in the operant period and under the control treatment condition.

Thus, when each experimental session was completed, there was a tape recording of the entire session and a paper tape recording of the influence attempt behavior and reinforcement or punishment contingencies for each subject. Simple clerical translation of the paper tape records into numerical frequencies of behavior thus provided the basic data for the analysis.

Before performing the analysis on the data, one further step in data preparation was required to avoid a difficulty encountered in interpreting Oakes' results. To obtain an estimate of interrater reliability, a second experimenter was trained in the use of the recording devices and in the operational definitions of the criterion used for administering reinforcement or punishment. Then by listening to the tape recordings of a sample of the sessions, the second experimenter was able to develop a paper tape record similar to that developed originally by the first experimenter. Using these two independent evaluations of the subjects' behavior, an estimate of interrater reliability was calculated.

RESULTS

Interrater Reliability

In an experimental situation where criterion definition is based on the judgment of an observer or an experimenter, it is necessary to assess the reliability of that judgment across raters. As noted earlier, this is one piece of evidence lacking in the Oakes' research which makes the results of that study difficult to interpret.

In the present study, interrater reliability was obtained by having a second experimenter rescore a sample of the group discussions, as discussed in the methods section above. After this rescoring process was completed, the two experimenters' paper tape records were divided into three-minute intervals and the number of suggestions accredited to each subject by each rater was tabulated for each of these intervals. The use of four groups in the rescoring process resulted in 240 such intervals. A Pearson Product Moment Correlation computed between raters based on this tabulation resulted in a coefficient of .417. Clearly, there are grounds to question the interrater reliability of the method. Possible explanations for this and a discussion of its impact on the research appear in the discussion section below.

Hypothesis Testing

Although low interrater reliability was obtained, the data analysis proceeded. It should be noted that the effect of this unreliability would be in the direction of decreasing the power of the analysis. Therefore, any significant results which are obtained with the present analysis would also exist in a similar set of data not affected by unreliability. A further rationale for proceeding with the analysis despite the reliability problem is presented in the discussion section below.

The tests of Hypotheses 1-6 were based on a repeated measures analysis of variance (ANOVA) design. The entire experimental design is presented in Table 2. This table shows all the treatment conditions and the subjects included in each. The tests of these first six hypotheses are each based on an individual block in the design which included one of the six treatment conditions. In such tests, each subject serves as his own control and the factors considered important are an effect on the dependent variable due to the levels of the treatment (defined as time units in the present research) and an effect due to subjects. Since subjects were randomly assigned to treatment conditions and received all levels of a given treatment, the model is a mixed effects model and, according to Kirk (1968), does not require consideration of an interaction effect. Even if such an effect does exist, its presence is appropriately accounted for in the Mean Square for the residual and the Mean Square for the treatment in any test of treatment effects. The test for a subject effect is not viable unless the treatment levels are also a random sample of possible treatment levels, due to the inappropriateness of the mixed model

TABLE 2. Repeated Measures ANOVA Design

Treatments		Time Period 1 (15 Min. Practice)	T_2 (15 Min.)	T_3 (15 Min.)
C_1 Contingent Praise	S_1			
	S_2			
	.			
	.			
	S_{16}			
C_2 Contingent Implementation	S_{17}			
	.			
	.			
	S_{32}			
C_3 Contingent Punishment	S_{33}			
	.			
	.			
	S_{48}			
C_4 Noncontingent Praise	S_{49}			
	.			
	.			
	S_{64}			
C_5 Noncontingent Punishment	S_{65}			
	.			
	.			
	S_{80}			
C_6 Control	S_{81}			
	.			
	.			
	S_{96}			

Note. --Ss indicate subjects; T's indicate time periods; and C's indicate treatment conditions.

error term. Obviously, treatment levels in the present research could not be considered a random selection. (For a complete discussion of additivity of effects in this type of design, see Kirk, 1968, pp. 135-139). The error term used in this type of design is a residual term accounting for the remaining variance after the variance due to treatments and subjects is removed.

Also reported below in conjunction with the tests of each hypothesis, are the results of tests for sex differences within a given treatment condition.

> Hypothesis 1: Individually received contingent praise will cause the frequency of acceptable influence attempt emission to increase significantly above the operant level of emitting acceptable influence attempts.

Before actually testing the hypothesis, a test for a significant difference in response frequency due to sex for both the operant and treatment periods was conducted. This test was, for both periods, an independent measures t-test of the difference in mean frequencies of acceptable influence attempts. The test for the operant period resulted in a t of .323 with 14 degrees of freedom (see Table 3 top section). The difference in mean frequencies between the sexes for the operant period is not significant. Likewise, the test of a significant difference in mean response frequencies during the treatment period resulted in a t of .643 (Table 3 top section). Again there are 14 degrees of freedom (df) resulting in a nonsignificant difference in frequencies in the treatment period.

Because the tests of sex differences were insignificant, all sixteen subjects were combined into a single design and the test of

TABLE 3. t-Test* of Differences in Response Frequency Between Sexes

Treatments	t for Operant Period	t for Treatment Period
Contingent Praise	.323	.643
Contingent Implementation	.084	.372
Contingent Punishment	1.369	.147
Noncontingent Praise	.110	.401
Noncontingent Punishment	.451	.462
Control	.253	.661

*Note. --14 degrees of freedom for each test

the hypothesis was run. As indicated in the first section of Table 4, an F of 8.704 with 2 and 30 df based on a Mean Square (MS) of 57.869 was obtained. According to a standard application of the F distribution, this result indicates a significant difference in mean operant and treatment response frequencies at the .01 level. Such a standard interpretation must, however, be approached with caution since in any repeated measures ANOVA design there is a possibility of violating the assumption of symmetry in the variance-covariance matrix for the levels of the treatment.

The likelihood of violating this assumption is increased in a design such as the one used in the present research where each subject constitutes, in essence, an entire block within the design used to test a given treatment effect. If this assumption of symmetry has been violated, it is likely that a positive bias will occur in the results of a standard F test.

Therefore, to avoid misinterpretation of the F in the present situation, the use of the Geisser-Greenhouse Conservative F Test is appropriate (KirK, 1968). This procedure is an approximate method which can indicate the need for an exact multivariate analysis of the data if the symmetry assumption is suspected of having been violated. The test is based on the premise that the true univariate F for a case where there has been a violation of this assumption can be approximated by the use of the conventional F test with reduced degrees of freedom (Box, 1954). Geisser and Greenhouse (1958) have added to this premise by demonstrating that the lower limits to which the degrees of freedom can range in such a situation are 1 and n-1 for the treatment and error terms, respectively. Therefore, by calculating

TABLE 4. Repeated Measures ANOVA Table--Hypotheses 1-2

Treatment	Source	SS	DF	MS	F	P <	Conservative F P<
Contingent Praise	Time	115.792	2	57.896	8.704	.01	.01
	Subjects	265.119	15	17.675	*		
	Residual	199.568	30	6.652			
Contingent Implementation	Time	92.542	2	46.271	14.334	.01	.01
	Subjects	111.609	15	7.441	*		
	Residual	96.828	30	3.228			

*Note.--F ratio not appropriate test in the present mixed model situation.

the conventional F and reducing the degrees of freedom used in the F test to their lower limits, a conservative approximation of an exact multivariate solution to the problem is obtained. If this approximation results in a significant conservative F, the researcher can be sure that an exact multivariate approach, which would account for any heterogeneity in the variance-covariance matrix, would also yield a significant result.

Use of the Geisser-Greenhouse Conservative F is indicated in Table 4 under the column "Conservative F, $p <$." As indicated in Table 4, the results of the conservative F test (using 1 and 15 df) are also significant at the .01 level for the test of the first hypothesis. Thus, to the extent that the symmetry assumption may have been violated, the difference among mean response frequencies remains strong enough to attain statistical significance.

However, the question of interest in testing the hypothesis involves a directional difference and not just a significant difference. An examination of mean acceptable influence attempt frequency for the operant period and the two halves of the treatment period show surprising results (See top portion of Table 5). The mean response frequencies (4.125 and 3.5) for the two treatment periods are lower, rather than higher as hypothesized, than the mean frequency for the operant period (7.0625).

A post hoc comparison of the differences among these means based on a modification of the Tukey procedure appropriate for this repeated measure design (Kirk, 1968, p. 144) revealed a significant difference for both halves of the treatment period when

TABLE 5. Mean Response Frequencies for Operant and Treatment Periods

Treatments	Mean Frequency (Operant)	Mean Frequency (Treatment)	Mean Frequency (Treatment)
Contingent Praise	7.0625	4.125	3.5
Contingent Implementation	5.313	2.25	2.5
Contingent Punishment	4.25	5.688	4.75
Noncontingent Praise	5.375	4.688	5.625
Noncontingent Punishment	5.625	4.938	4.563
Control	5.938	5.125	4.75

each is compared to the operant period but no significant difference when compared to each other. The obtained statistics are (critical value for q at .01 = 4.45): 1) $q_{3;30} = 4.54$ ($p < .01$) for operant vs. first half of the treatment period; 2) $q_{3;30} = 5.52$ ($p < .01$) for operant vs. second half of the treatment period; and 3) $q_{3;30} = .997$ (n.s.) for the first half vs. the second half of the treatment period. Thus, there is a significantly smaller frequency of response in both treatment periods than appears in the operant period. This reversal from the predicted result will be examined more fully in the discussion section below.

> Hypothesis 2: Individually received contingent implementation of acceptable influence attempts will result in a significant increase in the frequency of acceptable influence attempt emission above the operant level of emitting acceptable influence attempts.

The test for a significant difference between sexes in terms of mean response frequencies for the operant and treatment periods result in t's of .0843 and .372, respectively (See Table 3). Considering fourteen degrees of freedom as the basis for each of these tests, there are no significant differences between the sexes in mean response frequencies for either period.

Lack of sex differences again permitted the combination of all sixteen subjects into one analysis for the hypothesis test. Referring to the second section of Table 4 for Contingent Implementation, note that in comparing mean response frequency for the operant period and the two halves of the treatment period, a significant F of 14.334 is obtained with 2 and 30 df, MS = 46.271 ($p < .01$). The conservative F with 1 and 15 degrees of freedom is also significant

at the .01 level. As in the case of contingent praise, contingent implementation resulted in a strongly significant difference in mean response frequencies between the operant and treatment periods.

However, the question of interest, as with the test of Hypothesis 1, is in the directionality of the significant difference. Consideration of the second portion of Table 5 reveals a pattern similar to that obtained under the contingent praise condition. That is, mean response frequencies are lower for the two treatment period halves (2.25 and 2.5) than that for the operant period (5.313). Post hoc comparisons of these means also resulted in a pattern similar to that of the contingent praise case. The two halves of the treatment period have significantly smaller mean frequencies than the operant period while not being significantly different from each other (q's in the same order as in the contingent praise case): 1) 6.815; 2) 6.258; 3) .557--all with a critical value of 4.45 for $p < .01$ and all based on 3 means and 30 df. Again the results run counter to the predicted direction. Further discussion of this result is presented below.

> Hypotheses 3-6: Because operant conditioning relies on the efficacy of contingent positive reinforcement, four treatments were predicted to have no significant effect on the frequency of acceptable influence attempt emission. These four treatments are: 1) Contingent punishment (Hypothesis 3); 2) Noncontingent praise (Hypothesis 4); 3) Noncontingent punishment (Hypothesis 5); and 4) Control condition (Hypothesis 6).

Consideration of the four lower sections of Table 3 reveals that the tests for significant differences in mean response frequencies between sexes for each of these treatment conditions and for both the operant and treatment periods resulted in the

acceptance of the null hypothesis in all cases (i.e., there is no significant difference between the sexes in terms of mean response frequencies). Therefore, tests of all four hypotheses can be based on four analyses, each of which includes all sixteen subjects in a given treatment condition.

The results of these four analyses are presented in Table 6. As hypothesized, none of these treatment conditions resulted in a significant F indicating no significant effect on response frequencies due to the treatments. Conservative F tests are not necessary for these treatments since such tests correct for a positive bias in the standard F tests. Nonsignificant standard results are necessarily nonsignificant when subjected to the Conservative F tests. Therefore, those treatments which were hypothesized, on the basis of operant learning theory, to be noneffectual were, probably noneffectual in altering the response frequencies of acceptable influence attempt emission. The word "probably" must be emphasized in that these tests are tests of null hypotheses. Such tests must be viewed with caution in light of their suspect character among many researchers.

> Hypothesis 7: In a comparison of individual treatments with the control treatment, individual contingent praise and implementation will both cause significant increases in the frequency of acceptable influence attempt emission over that of the control condition, while contingent punishment, noncontingent praise, and noncontingent punishment will result in no significant difference from the control group.
>
> Corollary 7-A: There will be no significant difference between contingent praise and contingent implementation in terms of their effectiveness in conditioning acceptable influence attempt behavior.

TABLE 6. Repeated Measures ANOVA Table--Hypotheses 3-6

Treatment	Source	SS	DF	MS	F
Contingent Punishment	Time	18.292	2	9.146	1.832
	Subjects	280.439	15	18.696	*
	Residual	149.748	30	4.992	
Noncontingent Praise	Time	7.542	2	3.771	.971
	Subjects	218.429	15	14.562	*
	Residual	116.508	30	3.884	
Noncontingent Punishment	Time	9.293	2	4.647	.897
	Subjects	351.207	15	23.414	*
	Residual	155.417	30	5.181	
Control	Time	10.524	2	5.262	1.336
	Subjects	101.379	15	6.759	*
	Residual	118.700	30	3.939	

*Note.--F ratio not appropriate test in the present mixed model situation.

Corollary 7-B: There will be no significant difference between contingent punishment, noncontingent praise or punishment, and a control condition in their effectiveness in conditioning acceptable influence attempts.

The test of this hypothesis and its corollaries required three steps in terms of data analysis. The first step in this process is a test for significant differences in mean response frequencies during the operant period across treatment conditions. This test was based on a one-way analysis of variance of individual response frequencies across treatments and resulted in a non-significant F of 1.547 for 5 and 90 df and a MS of 13.219. Therefore, it can be concluded that there are no significant differences in operant levels across treatments. This eliminates the possibility that differences in treatment rates are strictly a function of differential operant rates.

The second step necessary for testing this hypothesis involves determining whether or not there is an overall significant F when all treatment conditions are included in a one-way analysis of variance based on individual response frequencies for the treatment period. If such a test is nonsignificant, there are no grounds on which to justify the post hoc comparisons necessary to test this hypothesis and its corollaries. Such an overall test resulted in an F of 3.903, 5 and 90 df and MS = 85.817 ($p < .01$). Clearly the necessary post hoc comparisons are justified since there is some overall effect detectable at the .01 level of significance.

Using the Tukey test which permits all possible pairwise comparisons among a set of means, Hypothesis 7 was tested by comparing the mean response rates for the various treatments to the

control condition. (Again, this is a test of a null hypothesis which should be kept in mind.) As predicted, the contingent punishment and noncontingent praise and punishment conditions were not significantly different from the control condition in terms of mean frequencies of responses in the treatment periods. Also, the predicted significant difference between contingent implementation and the control condition occurred ($q_{6;60} = 5.125$, $p < .05$). However, this difference is plagued by the directionality problem which occurred in testing Hypotheses 1 and 2.

Contrary to the hypothesis, contingent praise did not result in a significant difference in mean response frequency when compared to that of the control group. The results were also in the direction opposite that predicted by the hypothesis causing interpretation problems similar to those implied above.

Tests of Corollaries 7-A and 7-B resulted in the predicted equality of effectiveness between the two treatments which employed contingencies in agreement with the basis of operant learning theory and between those treatments which were contrary to the basic premise of operant conditioning. That is, no significant differences were found between the contingent praise and implementation conditions or between the contingent punishment, noncontingent praise or punishment and the control condition. Thus, the treatments clustered in terms of their effectiveness in the manner predicted by the hypothesis. The only difficulty in the clustering involves the direction of significant results in the contingent praise and implementation conditions.

DISCUSSION

The present research was proposed as a test of the usefulness of operant conditioning principles in the development of individuals who must learn a new style of influence because the group or organization to which they belong is undergoing an organizational change. The theoretical appropriateness of such a test has been developed at length in the literature review presented above, but the major points should be reviewed before proceeding.

1) It was noted by Burtnett that people who are members of an organization which is moving toward participative decision making probably need to learn new influence patterns before they can participate in or facilitate such a change.
2) Such a focus on individual development may stretch the concept of personnel development into, as yet, unresearched areas of human behavior.
3) While stretching the concept of personnel development, the need for individual learning intimates a possible link between previous research in human operant conditioning and the potential research areas of personnel development in its expanded form.
4) The present research was developed to add some bit of information to that link by taking previously developed

laboratory techniques in operant conditioning, and applying them to the influence pattern acquisition process noted by Burtnett.

Thus, the question of interest in the present research lay in exploring how previously supported methods of social reinforcement could be applied to develop individual influence attempt behavior in a problem solving group.

Based on the fourth point listed above, seven hypotheses were generated for testing. These hypotheses were developed in accord with previous research results in the area of operant conditioning; thus, support of the hypotheses indicates support for the notion that the acquisition of influence attempt behavior is operantly controlled. Failure to find support for any or all of the hypotheses does not, however, prove that operant conditioning is not an appropriate approach to the problem. Rather, it could be the case that some important variable in the conditioning process (most likely, the reinforcer) has not been adequately captured by the research.

Before actually discussing the tests of the hypotheses, the problem of interrater reliability, noted earlier in the results section, must be considered. Clearly, the correlation of .417 is not at all adequate in terms of providing a strong basis for the interpretation of the results. Such a low correlation between two trained observers implies an inability to objectively define the criteria of adequate performance in behavioral terms. This is a rather serious problem in light of the fact that 1) much similar research has been conducted in the past and drawn conclusions

regarding the effectiveness of operant conditioning, without estimates of interrater reliability, which may lead to fallacious interpretations of the data in light of the unknown reliability of the measures employed, and 2) previous research in the area of test construction has supported the view that behaviorally based measures tend to yield high reliabilities (e.g., Smith and Kendall, 1963).

In order to interpret the rest of the results of this study in some meaningful way, some attempt must be made at understanding the reason for such a low obtained reliability. At least two possible explanations exist as to why the reliability coefficient might be as low as it is. The first of these is that the rater training may have been inadequate. Since the original experimenter was the present researcher, it seems unlikely that he did not fully understand the definition of the rating criteria for acceptable and unacceptable influence attempts. There may have been some inability on his part to retranslate the conceptual definition into an operational form, but in light of the behaviorally based nature of the definition of the criteria, it seems unlikely that such an inability would be major enough to cause such a drastic reduction in the reliability estimate.

The training for the second rater was conducted so as to familiarize the rater with both the conceptual definitions of the criteria and the operational use of them and the rating equipment. This training was accomplished by having the second rater read the criteria and their definitions and then discuss them with the original experimenter. After the second rater felt that he conceptually

understood the criteria, one sample group recording was selected from the experimental session tapes and the second rater practiced the rating method by operating the rating equipment while the original experimenter instructed, observed, and criticized the second rater. After completing the practice tape, the second experimenter listened to the sample of four tapes selected for the reliability esimation process and rated them without the first experimenter being present. The second experimenter rated these four tapes over a two week period whenever he had an hour free to listen to one tape. At the time of rater training, there appeared to be no reason to question the second rater's ability to understand and/or operationalize the appropriate criteria; thus implying that the low reliability is probably not strictly a function of faulty ratings on the second experimenter's part.

The second factor which may account for the high degree of unreliability in the data is a form of method variance. That is, the original experimenter's ratings were based on visual and audial contact with all of the groups, while the second experimenter's ratings were based only on an audial reproduction (i.e., a tape recording) of a given group. Although Oakes, as reported above, apparently used a subject numbering system to identify individuals on a tape recording with little difficulty, the second rater in this experiment expressed concern over an inability to identify people making influence attempt behaviors due to the subjects' inconsistent use of the numbering system. To conclude that Oakes used such a system effectively in the absence of adequate reliability data may have been premature and may require further support. Thus,

it is very possible that the second rater's ratings were heavily influenced by this methods factor. Such an influence appears more likely, at least intuitively, to explain a high degree of unreliability than do some assumed inadequacies in the rater training process.

Given such a low reliability coefficient and a plausible explanation of it, what can be done with the data? Two approaches can be taken to this problem. The first is to attempt to increase rater reliability while the second is to argue that because of the method variance noted above, it is the estimate and not the observational method which suffers from unreliability. At first sight, the former method may appear more elegant and easily accomplished by simply increasing the number of raters used in the experiment. This approach is inadequate for the present research, however, in that the scores obtained by an individual subject were the function of a single rater's behavior and not based on a sum or some other combination across raters. It is inappropriate, therefore, to correct such an interrater reliability coefficient as the present one for the number of raters based on the Spearman-Brown Prophecy Formula. Only when ratings are combined across raters to evaluate a subject can such a correction be used, or can additional raters be employed to correct the problem of unreliability.

Since the first alternative seems inappropriate, the present research adopted the second approach. Because the second rater expressed definite concern over his inability to identify subjects as they spoke on the tape, and because a partial review of the tapes indicated a rather consistent failure in subjects' use of the number

identification system, it seems reasonable to conclude that the unreliability expressed in the estimate is more a fault of the estimate than it is of the observational method. Clearly, further research would be necessary to fully support this hypothesis, but such an exploration is beyond the scope of the present study. Therefore, it is argued that the behavioral observation method is probably less at fault than is the process of getting the reliability estimate. Some data which support this judgment come from the Hoffman and Maier (1964) research on the development of the scoring system in which they report a median correlation between raters (each using an identical observation technique) of .95. On these grounds, then, the data analysis was deemed viable and the hypothesis testing began.

The tests of Hypotheses 1 and 2 resulted in relationships not predicted by previous research and theory. Although the results attained statistical significance, the direction of the obtained change in frequency of responses was opposite that of the predicted change. That is, contingent praise or implementation of a subject's suggestion behavior resulted in a decrement, rather than an increase, in the frequency of acceptable influence attempt emission. The result appears stronger for the implementation condition than for the praise condition (F's of 14.334 and 8.704, respectively). In the implementation case, one might argue that praise of a stronger and more specific nature was employed, thus resulting in a stronger change.

This reversal of direction of obtained results from the predicted direction implies that one of at least four possible relationships may exist. These four are 1) praise is not a reinforcer,

2) satiation occurs rapidly when praise is used as a reinforcer, 3) the "reinforcement" results in some aversive state which the subject then attempts to avoid, or 4) humans tend to pause after reinforcement while they consume the reinforcement benefits (i.e., there is a construct called overconfidence or resting on one's laurels). An examination of each of these possible interpretations of the data for the population from which the sample was drawn follows.

The first alternative explanation of these results appears contradictory to much of the previous research on human operant conditioning. Even when praise has been operationalized in as mechanical a method as that used in the present research, it has appeared to still have the properties of a reinforcer (praise's contingent presentation upon completion of a response has increased the probability of that response occurring again in the future as measured by response frequency, e.g., Zdep and Oakes, 1967). The bulk of the research data in the area seems to oppose such a generalized negative interpretation of the present study's results. Thus, it can probably be safely stated that the first explanation may be true insofar as the present research is concerned, but that this explanation does little to further the understanding of human behavior in terms of influence attempt behavior's acquisition. Rather than being a real explanation of the results, this alternative may only serve as a stimulus for circular arguments regarding the definition of reinforcers in this context.

The second alternative explanation also appears to suffer from a weight of research evidence contrary to it. Praise and other

similar social reinforcers have been used over extended periods of time in the maintenance of human behavior. This is especially apparent in the behavior modification literature noted in the literature review above. Another bit of evidence which opposes such an interpretation comes from the present research. If the need for social reinforcement was so minimal as to allow satiation of a significant nature in the first half of the thirty-minute treatment condition (as indicated by the significant results obtained for the Tukey tests run for each hypothesis), it would seem likely that noncontingent reinforcement would also result in a decrement in performance which, in fact, was not observed. Such a decrement would be expected strictly because complete satisfaction of a minimal need should reduce the drive state of the organism, no matter how this complete satisfaction is obtained. Therefore, it seems probable that the second alternative explanation is also inadequate in clarifying the obtained results.

Unlike the two previous explanations, the third and fourth explanations have some data bases which appear somewhat parallel if not actually supportive of the present results. Considering the third alternative explanation, researchers in human decision making have consistently supported the hypothesis that the human decision maker is incapable of adequately processing large amounts of information to arrive at a conclusion (e.g., Schmidt, Berner, and Hunter, 1973). It may be that such an inability results in an aversive state for the decision maker when an excessive amount of information necessary for a decision is foisted upon him. In the present context this translates into the development of an aversive

reaction when one individual generates more than a few suggestions or ideas which are reinforced as being adequate solutions to the problem. Thus, rather than being forced to make a decision which utilizes excessive information, the human decision maker may attempt to thwart the introduction of additional acceptable information by ceasing to investigate other alternative solutions to the problem facing him. Clearly, such an effect would be hypothesized to especially occur in ambiguous problem situations where more than one solution may be feasible, such as the present research setting.

Alternatively, the fourth possible explanation of the results is supported by a mass of anecdotal data in the sports world and by some anecdotal data available to the researcher, as well as by some hard research data. Examples of upsets abound where a "favorite" has been satisfied with previous performance and fails to prepare for alternative behavior which may be required in the future. Secondly, informal conversations between the researcher and workers in some companies employing the Scanlon Plan have indicated that people do tend to act in accord with the Plan when things which they can control are not going well rather than when things are going well. In other words, if people believe they are performing adequately enough for the system to have reinforced them in the past, they will make little effort to improve their performance and will focus on enjoying the reinforcer. Likewise, results from the laboratory have provided the basis for the well-known Law of Least Effort which simply states that an organism will emit the minimal behavior required to gain a reinforcement and then will proceed to consume that reinforcer.

To actually choose one or the other of the last two alternative explanations or interpretations of the data does not seem possible in light of available knowledge. A behaviorist could argue for the third explanation with as much fervor as a cognitive psychologist could argue for the fourth explanation. At present the data are not conclusive enough to make a final decision. It is possible that one or the other of the explanations could be supported or rejected by further research which would examine the physiological effects of administering praise to individuals operating in a problem solving group. Therefore, for now what can be concluded is that in the population under study contingent praise probably either elicits some type of aversive state which the organism tends to avoid, or else it results in consummatory behavior which interferes with the emission of an increased number of reinforceable responses.

Before proceeding to consider the remainder of the research results, one study which may indicate that the present results are not strictly a function of method variance should be considered. This study is one performed by Dunnette, Campbell, and Jaastad (1963). Using brainstorming techniques, the researchers investigated the effects of group vs. individual situations on problem solving behavior (i.e., generating as many solutions as possible to a given problem). The basic task of the subjects was very similar to that performed by the subjects in the present research. Similarly, if one considers brainstorming as a type of continuous reinforcement schedule (since all ideas are considered acceptable and criticism is not allowed), the research reported by Dunnette, et al. could be interpreted similarly to the present research. That is, in the

present research, the organism accepting all suggestions and criticizing none is the experimenter who thus plays a role similar to that of the members of the brainstorming group. In both researches, results show a significant decrement in performance when "reinforcement" is administered on a continuous schedule. Thus, there appear to be grounds for rejecting the notion that the present results are strictly a function of method variance, since a different but analogous method revealed similar results.

Continuing with the discussion of the present research results, it should be noted that tests of Hypotheses 3-7 and Corollaries 7-A and 7-B resulted in the predicted relationships. The directionality problems encountered in testing Hypothesis 7 are similar to those of Hypotheses 1 and 2 and may well be interpreted in a similar fashion, but the rest of the results of these tests are as hypothesized. Not only do these results of no significant changes due to contingent punishment or noncontingent or control treatments show some minimal support for the concepts of operant conditioning (to the extent that accepting a null hypothesis can be considered supportive), but they also eliminate explanations for the reversed results in testing Hypotheses 1, 2, and 7. For instance, the decrement in performance under contingent "reinforcement" is probably not a result of the problem not being adequately difficult or sufficiently long to occupy the groups' attention for the entire thirty-minute period since all four sets of groups involved in Hypotheses 3-6 maintained their operant level of performance throughout the treatment periods. A second alternative explanation which is eliminated by these results is that the decrement in performance

was a function of regression toward the mean due to excessively high operant rates in the contingent praise and implementation conditions. The test of significant differences among operant rates is nonsignificant implying that regression toward the mean could not result in such strongly significant results. A third alternative which these results tend to refute has been discussed above in terms of the satiation explanation of the reversed results.

Thus, it can be concluded that some of the concepts proposed by operant learning theory for the learning of influence attempt behavior in a group problem solving situation were minimally supported by the present research for the population under study, especially in terms of the noneffectual treatments (Hypotheses 3-6) and the clustering of treatments in terms of similar effectiveness (Corollaries 7-A and 7-B). Although this support is of the null hypothesis, it is worth noting in the present context. On the surface, the major premise of operant conditioning for learning in such a situation seems to be rejected since "reinforcement" was negatively effective, but such a conclusion appears hasty and ill-founded. It cannot be concluded from the present research that punishment or noncontingent responses or just "doing nothing" are the only ways of maintaining influence attempt behavior, nor can it be justifiably argued that praise is not a reinforcer or that operant conditioning is not applicable in such situations. Rather, one can only conclude that, at least for the population under study, the process of acquiring an influence pattern in a group problem solving situation requires more than just continuous social reinforcement of acceptable influence attempts. If these

results can be generalized to young adults of at least average intelligence, it may be feasible to argue that a change agent working with such a group will need to focus on more than just positive social reinforcement if his program of personnel development is to succeed. Such a situation is only intimated and not proven by the present research. The results of the Dunnette, et al. research lend some support to such an intimation, but again are not completely conclusive.

In order to completely explicate the obtained results and clarify their interpretation in terms of operant conditioning principles, further research is necessary. Such research could focus on 1) the problem of unreliability (perhaps by examining the effects of different types of group monitoring or the possibility of using several raters simultaneously), 2) a replication of the present research since the results run contrary to a large body of research data, 3) an examination of the generalizability of these results by examining the effects of social reinforcement on other populations, 4) the validity of these results in a field situation, or 5) some method of developing a data base for rejecting or accepting one or more of the alternative interpretations presented above. In regards to the last avenue of research, such a clarification between alternatives three and four may be made possible by monitoring some physiological response known to be associated with aversive stimulation or with consummatory behavior when administering social reinforcers to individuals in a group problem solving situation. The information overload concept might also be tested by using various variable ratio

schedules which should reduce the possibility of overload. Finally, the ultimate test of the hypotheses would be to employ a sample of subjects in a newly developed Scanlon Plan or other participative management system who were naturally subjected to alternate forms of social treatment due to different supervision or other types of authority figures in an organization and assess the effectiveness of social reinforcement on the modification of their influence attempt behavior.

APPENDICIES

APPENDIX A

INSTRUCTIONS AND PROBLEM FOR PRACTICE PERIOD

The following instructions and problem were read to each group at the outset of the practice period. A copy of the problem was left with the group as a reference source.

This experiment is concerned with examining how individuals function in groups to solve problems. Many previous research efforts have examined various aspects of this question in a variety of ways. The present experiment will employ a relatively simple method, similar to one used in some of the earlier research.

Your group will be asked to solve two job-oriented problems in the course of this one hour session. The first of the problems is a practice problem which allows you to adjust to one another and practice working in a group. You will work on this practice problem for 15 minutes.

After completing the practice problem, your group will be given a test problem. For this part of the experiment you will work as a group for thirty minutes. Please note that your performance on <u>each</u> problem will be recorded for future scoring and evaluation. This requires that the session be tape recorded and that I record your behavior on another kind of recording device. If no one has an objection to being tape recorded or a question concerning the information covered thus far, I will now explain the practice problem more fully and let you get started. The problem reads as follows.

The incident in this case hinges on the issuing of a new truck to some member of a work crew of workers, each of whom uses a truck in his work. The foreman finds himself in a situation in which he must make a wise and fair decision. Since the replacement of trucks has been infrequent in the case in question, the importance of making a right decision is apparent to the supervisor. I would like your group to consider itself an advisory board and make recommendations to the supervisor on how he might solve his problem.

Men work for the telephone company as repairmen. The job of a repairman is to fix phones that are out of order, and requires knowledge and diagnostic skills as well as muscular skills. Repairmen must climb telephone poles, work with small tools and meet

customers. The foreman of a crew is usually an ex-repairman and this happens to be true in this case. He has an office at the garage location but spends a good deal of time making the rounds, visiting the places where the men are working. Each repairman works alone and ordinarily does several jobs in a day. The foreman gives such help and instructions as are needed.

The repairmen drive to the various locations in the city to do repair work. Each of them drives a small truck and takes pride in keeping it looking good. The repairmen have a possessive feeling about their trucks and like to keep them in good running order. Naturally, the men like to have new trucks, too, because a new truck gives them a feeling of pride.

Here are some facts about the trucks and the men in the crew that report to Walt Marshall, the supervisor of repairs.

George, 17 years with the company, has a 2-year-old Ford.
Bill, 11 years with the company, has a 5-year-old Dodge.
John, 10 years with the company, has a 4-year-old Ford.
Charlie, 5 years with the company, has a 3-year-old Ford.
Hank, 3 years with the company, has a 5-year-old Chevrolet.

Most of the men do all of their driving in the city, but John and Charlie cover the jobs in the suburbs.

The new truck to be allocated is a Chevrolet.

Now, each of the men has his own personal feelings about the situation and they are as follows.

Role for George -- When a new Chevrolet truck becomes available, he thinks that he should get it because he has the most seniority and he doesn't like his present truck. His own car is a Chevrolet, and he prefers a Chevrolet truck such as he drove before he got the Ford.

Role for Bill -- He feels that he deserves a new truck and that it's certainly his turn. His present truck is old, and since the more senior man has a fairly new truck, he thinks he should get the next one. He has taken excellent care of his present Dodge, and has kept it looking like new. He thinks a man deserves to be rewarded if he treats a company truck like his own.

Role for John -- He has to do more driving than most of the other men because he works in the suburbs. He has a fairly old truck and he feels that he should have the new one because he does do so much driving.

Role for Charlie -- The heater in his present truck is inadequate. Since Hank backed into the door of his truck it has never been repaired to fit right. The door lets in too much cold air, and he attributes his frequent colds to this. He wants to have a warm truck since he has a good deal of driving to do. As long as it has good tires, brakes, and is comfortable he doesn't care about its make.

Role for Hank -- He has the poorest truck in the crew. It is five years old, and before he got it it had been in a bad wreck. It has never been good, and he's put up with it for three years. It's about time he got a good truck to drive, and it seems only fair to him that the next one should be his. He has a good accident record. The only accident he had was when he sprung the door of Charlie's truck when Charlie opened it as Hank backed out of the garage. He hopes the new truck is a Ford since he prefers to drive one.

Does anyone have any questions concerning the problem?

Your team may use any method you want to solve the problem, provided that you work as a group. The only requirement about scoring that you should keep in mind in choosing a method is that your group will be evaluated more heavily on the number and quality of possible solutions you come up with than on the one final solution you select. Therefore, it would probably be wise to put off selecting a final solution until the last few minutes of the fifteen or the later thirty-minute session. This last comment is also true of the evaluation of you as individuals (i.e., you are evaluated most heavily on the number and quality of solutions you come up with). Finally, because the session is tape recorded, please give the number that appears before you on the table before saying anything to the group. This will facilitate scoring of the tape later on and will also prevent your names from appearing on the tape.

Do you have any questions? If not you may begin working on the problem when I tell you to start.

APPENDIX B

INSTRUCTIONS AND PROBLEM FOR TREATMENT PERIOD

The following instructions and problem were read to each group at the outset of the treatment period. A copy of the problem was left with the group as a reference source.

Now that you have had some time to practice, I'm going to give you the test problem. In this case, all the instructions for the first problem still hold except that you have thirty minutes to work on the problem.

*To help you know how well each of you is doing as an individual, the box in front of you is going to light up. That is, if the things you have just said or done in the group are beneficial to the group's activity, a green light will come on in front of you. If what you have said or done is not beneficial, a red light will come on. Please keep an eye on these lights since they are the only individual evaluation you will receive during this meeting. Also, you will only be able to see your own lights. Any light leaks which might occur in the box are purely random and it is unlikely that they carry any meaning for you. Obviously, if you only get green lights, you are doing

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*Obviously, these instructions are for the contingent treatment conditions. Contingent implementation required that the Ss be instructed as follows. You are in a pilot study on the use of these problems. Any suggestion you may make which is considered satisfactory as a possible solution in future uses of the problem will be followed by a green light. Unsatisfactory solutions will be followed by a red light.

In the noncontingent treatments the instructions above were modified to remove the contingency implication. That is, subjects were told that a green light was indicative of generally beneficial behavior on the average. A red light indicated generally non-helpful behavior on the average.

In the control condition, the light display was not defined and this portion of the instructions concerning individual lights was simply dropped from the instructions.

quite well; if you only get red lights the opposite is true. Any mixture of lights is best interpreted as number of green vs. number of red (with the greater number indicating the direction of your performance). If there are no questions, I will now read the test problem and let you begin.

The problem concerns a crew of seven men who work on a circular assembly line. The crew members differ in ability for the job. Since the line is paced by the slowest member, and since productivity is low, the problem for the leader and your advisory group becomes one of improving the situation through various solutions.

Visualize a sub-assembly situation in which seven men, working in a circle, assemble a part of a car (a carburetor or instrument panel, for example). The article enters the circle at one point, and each person adds his pieces and pushes the unit to the next worker who adds his elements. When the unit leaves the circle, it is a completed part product.

The assembly work is simple and requires a minimum of training for each step. The aptitude requirement is primarily good finger dexterity. The materials for each assembly position are located in bins that are kept supplied by material handlers. Thus, each worker has his essential material at his elbow. The job has been analyzed by time-and-motion experts so that the positions are of equal difficulty. Pay is based on hourly rates.

The total factory production is dependent upon receiving the required number of assembled units from this station. The production is now so low that the factory production as a whole has had to slow down.

You are concerned with station C, producing at a rate of 60 units. The work piles up at the position of Joe Brown. The unit must pass through him (position 3), and he always has several piled up waiting for him. Foremen on the nonproduction jobs are not willing to accept Joe as a transfer. Joe is a man of 60 with 30 years service to the company. Emphasis on improving production has brought his deficiencies to light.

Again, each of the men has his own feelings about the situation and they are as follows.

Role for Hal Benton, Foreman -- The job has been analyzed by time-and-motion study men and the amount of work at each position is practically the same. The number 3 position (Joe's position) is, however, slightly easier than the others in that one less motion is required. Undoubtedly the previous foreman put Joe there to reduce the bottleneck. He realizes that he cannot pass Joe off to another foreman. Joe is a likeable person and it is Hal's impression that Joe gets along well with the other men in the unit.

Role for Bill, Position 1 -- He finds he can easily do more work but he has to slow down because Joe gets behind. In order not to make him feel bad, Bill holds back. He doesn't want to get Joe into trouble.

Role for Jim, Position 2 -- He and Bill work closely together and he is usually waiting for a part from Bill. This waiting for the part is more prevalent in the later part of the day than in the beginning. To keep busy he often helps out Joe who can't keep up. However, he's careful not to let the foreman catch him helping Joe because he might let Joe go.

Role for Joe, Position 3 -- He works hard but just isn't as fast as the others. He knows he's holding things up, but no matter how he tries he gets behind. The rest of the fellows are fine boys and simply have more energy than he does at his age.

Role for Sam, Position 4 -- Joe has trouble keeping up and so Sam sometimes grabs Joe's part and finishes it for him when the boss isn't looking. Joe is a bit old for the pace that's set and he feels the strain. For Sam the job is easy and he feels the whole job is slowed down too much because of Joe. "Why couldn't Joe be given less to do?" he asks himself.

Role for Hank, Position 5 -- He feels a bit uneasy in this job. There isn't enough to do so he has to act busy. If Joe could only speed up a bit. Why don't they move him out of the group? Is the company so blind that they can't see where the production trouble is?

Role for George, Position 6 -- He is able to keep up with the pace but on the last assembly job he was pressed. Fortunately Joe is slower than he is so he keeps the pressure off of George. He is determined that Joe not be moved off the line. Somebody has to protect people from speed-up tactics.

Role for Harry, Position 7 -- He gets bored doing the same operations over and over. On some jobs he gets variety by working faster for a while and then slowly. On this job he can't work fast because the parts aren't fed to him fast enough. It gets him down to keep doing the same thing over and over in slow motion. He is considering getting a job some place where they can keep a man busy.

Do you have any questions about the problem?

Remember, number and quality of possible solutions are the most important factors in scoring! You may start the problem when I tell you to begin.

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