INTERPERSONAL INFLUENCE AND DECISION-MAKING: MONETARY VERSUS NON-MONETARY REWARDS

Thesis for the Degree of Ph D.

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ROBERT K. SHELLY

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ABSTRACT

INTERPERSONAL INFLUENCE AND DECISION-MAKING:
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By

Robert K. Shelly

Interpersonal influence in small, task-oriented groups is explored by means of a mathematical model which was developed by Camilleri and Berger (1967). The model posits that influence is a decision-making problem for the actor, in which the actor attaches psychological rewards, or utilities, to components of the influence situation. The components to which utilities are attached by the model include approval for self-consistency and the approval of others in the situation. Mathematical properties of the model allow investigation of its equilibrium solution. Substantively, this limiting behavior may be approached in two ways. The first method involves the removal of rewards for self approval and the second involves the addition of other reward components to the situation. An experiment, employing many of the procedures used by Camilleri and Berger, was designed and conducted to permit the investigation of the second method, the inclusion of the reward component. Monetary rewards were assigned on each trial, thereby implementing the inclusion of this additional reward component. The monetary rewards were not distributed at the conclusion of each trial due to the nature of the experimental tasks. Rather, the subjects were told that they would receive their performance-based rewards at the end of the

experimental session. It was hypothesized that the per-trial reward would increase the probability that the subject would be influenced by the other participant, if the model were correct in its predictions. The predictions of the model were not obtained in the data from the experiment. In fact, no differences were observed between the condition on which per-trial rewards were made and the condition in which per-trial rewards were not made, other factors held constant.

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By

Robert KEITH Shelly

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Of particular significance in the work reported here has been the tutelage of Professor Hans E. Lee, who has directed the dissertation. I should also like to thank Professors Bo Anderson, Santo F. Camilleri, Thomas L. Conner, and James L. Phillips, who have tutored, cajoled, applauded, and kicked at the appropriate times in the course of my education. The assistance of the following people in the conduct of the experiments was invaluable: Mark Jurecki, Ann Newland, Wayne Olin, Kathy Phillips, Glen Sanders, and John Sorbet. I also wish to thank my wife Ann and the rest of my family for putting up with me while this was being done.

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CHAPTER I

INTERPERSONAL INFLUENCE

General Statement

Experimental evidence in the study of social influence has demonstrated that actors change their opinions, or behavior, as a function of the social rewards which they receive from others in the social situation. This phenomenon has been related to the degree of cohesiveness of the group, the status structure of the group, and the rewards which actors receive from others in the form of social approval. The experiment reported here was designed to investigate the relationship between influence, the status structure of the group, and the rewards available to actor in the social situation.

The acceptance or rejection of influence on the part of an actor may be viewed as a problem in decision-making for the actor. The actor must decide whether or not he will accept influence from some other actors. The decision-maker is assumed to weigh the advantages or disadvantages which a particular course of action will entail for him. The advantages, both positive and negative, of a particular course of action are usually distinguished by the social scientist as consisting of intrinsic, or self-satisfying, and extrinsic, displayable, components. While it is not usually assumed that the actor makes such an explicit distinction when considering rewards, the analytic distinction between

these reward components are preserved for the moment. It is assumed that the actor has expectations, or underlying beliefs, about the relative distribution of these rewards which are based on distinctions made by him.

The expectations, held by the actor, of his position in the group, relative to others, will determine his reaction to attempts to influence him. More specifically, the actor formulates ideas about the performance of others in the situation and employs these expectations in assessing the relative advantages of making a decision, or choosing a given course of action. Of particular concern is the manner in which actor feels rewards for the various alternatives are structured. Expectations regarding the willingness of others to approve or give financial reward for an alternative are likely to affect the desirability to an actor of choosing a particular course of action.

The implications for influence which is based solely on social rewards have been well investigated, as has the relationship between decision-making and monetary reward. Significant issues in the two areas of research remain unresolved. Theoretical explanations of influence acceptance have not taken into account the various alternative sources of reward an actor has available to him. Accounts of monetary reward and decision-making have not resulted in a sufficient account of how actors view such rewards. The research reported is an attempt at a solution to some of these problems.

A mathematical model of a social influence experiment is investigated, using monetary rewards. Briefly, the model views the acceptance of influence as a decision problem for the actor. The decision outcomes have associated with them social outcomes which are represented by

utilities. If monetary rewards are conceptualized as a utility, then the model may be exployed to generate predictions about how the actors will behave. The model of the experiment is one model of a more general theory of decision-making. The effort here is to generate an extension of that theory by extending the model of the experiment. The reader is cautioned to regard the utilities mentioned above as heuristic in character. The intent is not to assign numerical values to the utilities, but rather to regard them as theoretical constructs which serve to inform the process of abstraction and theory construction.

Review of Literature

Studies of the exercise of social influence in group situations have shown that actors accept influence differentially depending upon the size of the group confronting them (Asch, 1956), the attractiveness of the group to the actor (Schachter, 1951), the degree of veridicality of the stimulus (Sherif, 1935), and the position of the actor in the status hierarchy of the group (Bales, 1953; Sherif, et al., 1955; Harvey, 1953; Whyte, 1943). The common element of each of these studies is that actors develop, or have created for them, expectations about how they will perform at the group's task and adjust their behavior so as to maintain the expectation structure which they have developed from previous experience (Berger and Snell, 1961). More recently, Camilleri and Berger (1967) have posited an analysis of the acceptance of influence which is based upon the gains and costs of deciding to follow one course of action, i.e., accept influence, as opposed to another course of action, i.e., not accept influence. They focused on the social rewards that actor receives for his behavior.

The research reported here extends the scope of the decision-making theory of gains and costs, as applied to the analysis of influence in groups, to include a consideration of financial rewards.

The work of Asch, and that of subsequent investigators in the same tradition (principally Cohen, 1963, and Gerard, 1965), has shown that actors in a social situation will vary in the rate at which they conform to the opinions of others, or accept influence, depending upon the amount of support which they receive from others in the situation. Specifically, when confronted with discrepant information from his own perceptions of a veridical task and the perceptions of others as communicated to him, the rate at which an actor is likely to yield to the influence attempt is inversely related to the amount of social support he receives from others in the situation. Variations of the original study by Asch have borne out this relationship. They indicate that the effect is heightened if the actor is directed to perform the task as part of a group. The effect is decreased if the actor is in some way committed to his initial response or predisposition, thus providing his own social support, in the choice situation (Deutsch and Gerard, 1955). The interpretation given to these results is that the actor receives rewards from others when making a decision in a group. These rewards may be either positive--that is, the group supports the actor--or negative--that is, the group does not support the actor. The negative rewards are interpreted as costs for the actor. The actor is believed to behave in the group situation so as to minimize the costs of action (Homans, 1961).

Sherif (1935) has reported a similar phenomenon in situations where actors are confronted with ambiguous stimuli. Actors expressed

judgments about the movement of a light in a darkened room and when confronted with the responses of another actor became engaged in a mutual influence process which ultimately led to the construction of a group-based judgment. The basic results of this experiment, and that developed by Asch, have led to a concern on the part of some social psychologists about how far actors could be pushed before they would cease to conform, or accept influence (cf. Milgram, 1965, for an example of this concern).

Another tradition has focused on the conditions under which members of a group are likely to accept influence. The attempts of Schachter and those associated with him represent the first efforts to systematically explain the nature of the impact of features of the group on the rate at which an individual might be expected to accept influence. By varying the cohesiveness of groups, the extent to which the actor expected other members to be congenial, they were able to demonstrate that the acceptance of influence is positively related to the cohesiveness of the group (Schachter, et al., 1951; Back, 1951; Gerard, 1954). While demonstrating that the exercise of influence is positively related to the emotional climate within the group, these investigations do not address themselves to the issue of the effects of internal structure of the group on the rate at which influence is attempted.

Investigation of the variation of influence acceptance as a consequence of internal group structure begins to emerge with the work of Bales and his associates (Bales, et al., 1951; Bales, 1953; Bales and Slater, 1955; Heinecke and Bales, 1953). In the task-oriented groups observed in these studies, the actors developed stable status structures based on task competence. The status structure in turn determined the

extent to which actors were successful at influencing others. High status actors were more likely to exercise influence than low status actors and were also more likely to resist influence attempts than were low status actors. Similar findings are reported for groups which have fully developed status structures prior to the beginning of task activity (Harvey, 1953; Sherif, Harvey, and White, 1955; Whyte, 1943). Other investigations have shown that non-task-related status characteristics will have a similar impact on the distribution of influence in task-focused situations (Strodtbeck, James, and Hawkins, 1957; Strodtbeck and Mann, 1956; Torrance, 1954; Hurwitz, Zander, and Hymovitch, 1968). The hypothesis that actors with high status attributes will exercise more influence and be influenced less than actors with low status attributes has received substantial support.

The results of the above experiments have shown the rate at which an actor accepts influence to be positively related to the size of the group, the cohesiveness of the group, and the degree of veridicality of the stimulus. Conversely the degree of influence acceptance has been negatively associated with the degree of social support actor receives and the position of the actor in the status hierarchy of the group. An adequate theoretical explanation of these findings must necessarily consider a large number of variables; as yet no such explanation exists. We will consider an explanation which has been developed to handle one set of the results reported above, the basic ideas of which may be ultimately generalizable to the whole set of variables.

Berger and others (Berger and Snell, 1961; Berger, Cohen, and Zelditch, 1966; Berger, Cohen, Conner, and Zelditch, 1966; Moore, 1968, 1969; Berger and Conner, 1969; Berger, Conner, and McKeown, 1970) have

developed a theory to account for the repeated finding that status is directly related to the amount of influence exercised by an actor and that influence accepted by an actor is inversely related to status position. Briefly, they posit that actors form expectations of how they, and those involved with them, will perform on a task. Expectations are defined as "underlying beliefs . . . about their relative abilities to perform the task" (Berger, Cohen, and Zelditch, 1966, p. 48). Task performance is conceived of as having two components--success and failure. The behavior of actors in the task situation is posited to be a balancing process whereby those actors with high self-expectations of task competence are more likely to resolve conflicting perceptions in their own favor if the other actor(s) is expected to perform poorly at the task. Similarly, actors with low ability are more likely to resolve conflicting perceptions in favor of the other actor if they expect him to have high ability. Briefly, the theory accounts for behavior in situations where an actor first makes an initial decision, and then receives information about the decisions of other actors before making a final decision. The information may confirm the initial decision of the actor or contradict it. If the information contradicts the initial decision of the actor, he may either stay with his original response or change it in some way. In a binary decision situation, i.e., where there are two events, the actor is said to give a self-response if he stays with his original answer and an other-response if he changes his answer. The static, formalized verbal theory developed in Berger, Cohen, and Zelditch has been extended to a Markov model to represent the process when a sequence of conflicting perceptions is presented to a pair of actors (Berger, Cohen, Conner, and Zelditch, 1966; Berger and Conner, 1969).

Research efforts to test both formulations have been carried out in an experimental situation in which a pair of actors have been confronted with a series of disagreeing trials based on a stimulus which is ambiguous. Actors are first directed to make an initial choice, receive information on their partner's initial choice, and then make a final choice. The information received about the partner's initial choice is pre-programmed by the experimenter to disagree with actor's initial choice on a fixed schedule. It is the proportion of these disagreements which actor resolves in his own favor which are used as a test of the theory. Direct tests of the theory have supported the basic ideas when the status characteristic has not been directly manipulated (Berger and Conner, 1970). An extension of the experiment to include more than one characteristic has not produced results consistent with the theory's prediction (Berger and Fisek, 1970).

While the work on expectation theory has contributed to our understanding of how to represent the process of social influence, it has not directly answered the question of why actors are more or less likely to exercise influence or to be influenced. The Berger formulation considers the resolution of conflicting perceptions to be fixed in the sense that the actor is posited as selecting the response which balances his expectations and perceptions. By defining the resolution of conflicting perceptions as problematic for the actor, it is possible to view the balancing process as a decision-making problem for the actor. This shift in perspective parallels a shift from the explicational models of expectation theory, wherein the intent is to render more precise concepts, to the theoretical construct model wherein the intent is to derive predictions about the process. The distinction between the types of models is due to Berger, et al., (1962).

CHAPTER II

THE GAIN-LOSS THEORY

Camilleri and Berger (1967) have developed a theory, which is applicable in social influence situations, based on the utility to the actor of various components of the decision structure. Built on ideas developed to account for decisions in social situations by Homans (1961), Festinger (1957), and Lewin (1935), the theory posits that actors attach costs and rewards to particular features of the decision structure and choose one alternative over another based on the relative gains and losses involved. The construct employed in the theory is the idea that rewards and costs, in psychic terms, are defined as utilities.

A utility structure is attached to each alternative, A_i, in the choice situation such that:

$$A_i \rightarrow p_{i1}u_{i1}(x_{i1}) \wedge p_{i2}u_{i2}(x_{i2}) \wedge \cdots p_{im}u_{im}(x_{im}),$$

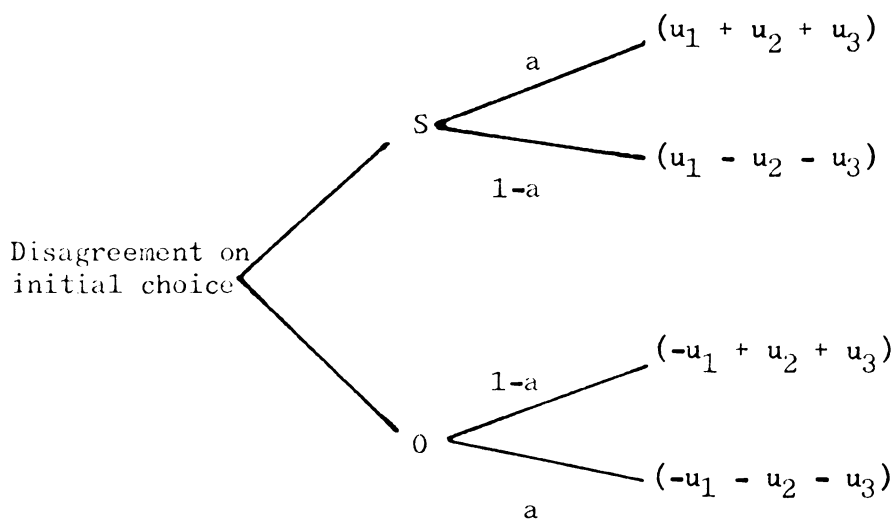
where the x_{ij} are components of the choice structure, u_{ij} are their associated utilities, and the p_{ij} are the probabilities with which the $u_{ij}(x_{ij})$ are realized. Choice of alternative A_i entails, in the strict logical sense, the associated utility structure for the actor. The utilities are conceived of as being the rewards and costs associated with an alternative. The expected gain of an alternative is then defined as the sum of the positive utilities of the components of that alternative minus the sum of the negative utilities of the rejected

alternatives (Camilleri and Berger, p. 368). If we denote expected gain as G_i for alternative A_i, the probability of choosing A_i, applying the Luce choice axiom (Luce, 1959) becomes

$$P(A_i) = \frac{G(A_i)}{\sum_{j=1}^{n} G(A_j)}, \qquad (1)$$

for n alternatives. The model is applicable in situations where outcomes are contingent because of the uncertainty of external conditions (Camilleri and Berger, p. 369).

Camilleri and Berger identified three utility components in the binary choice situation described in Chapter I. The three components were self approval for consistency, denoted by u_1, approval of other for being correct, denoted by u_2, and approval of the experimenter for being correct, denoted by u_3. The decision tree for the situation confronting the actor is represented as:



where a represents the subjective probability to actor that the chosen response is correct and S represents a self-response and 0 an other-response. The definitions cited above lead to the following expressions

for the expected gains of the alternatives:

$$G_S = [u_1 + a(u_2 + u_3)] - [-u_1 - a(u_2 + u_3)]$$

$$G_O = [(1 - a)(u_2 + u_3)] - [-(1 - a)(u_2 + u_3)],$$

and the probability of a self-response, by expression (1), is:

$$P(S) = \frac{u_1 + a(u_2 + u_3)}{u_1 + u_2 + u_3} \qquad (2).$$

The parameter "a" represents the subjective probability to actor that a given response is correct. No consistent a priori grounds exist for assigning a particular value to "a". The method used by Camilleri and Berger, assuming the parameter is a well-defined probability, is to assign "a" based on the number each actor has correct in the manipulation phase of the experiment as follows:

$$a = \frac{\text{number correct by actor}}{\text{number correct by both}} .$$

This convention is followed here.

The test of the model reported by Camilleri and Berger gives mixed results. The experimental situation involved creating a series of disagreements between actor and other about the correct answer to an ambiguous stimulus. The proportion of these disagreements resolved in favor of self was used as the data to test the model. Actors were assigned to one of three control conditions, where control represents the final decision rights for the group (full, equal, and no control), and one of four ability expectation states (high self, high other, denoted by [++]; high self, low other [+-]; low self, low other [--]; and low self, high other [-+]). The model gives a good fit, i.e., the

predicted values are close to the observed values, in all but the [-+] condition, where the model over-predicts the P(S). See Table 1.

TABLE 1

MEANS OF PREDICTED AND OBSERVED PROPORTIONS OF S RESPONSES BY CONTROL AND EXPECTATION CONDITIONS*

Expectation State	Full Control			Equal Control			No Control		
	Pred.	Obs.	N	Pred.	Obs.	N	Pred.	Obs.	N
[+-]	.75	.73	32	.77	.78	29	.80	.82	30
[++]	x	.60	31	x	.67	31	x	.71	37
[--]	.60	.52	30	.67	.65	32	.71	.73	33
[-+]	.47	.24	31	.55	.44	28	.60	.43	35

*Camilleri and Berger, 1967, p. 375.

The model also does well predicting the rank order of the data. Balkwell (1969) has proposed an explanation for the results, based on the interaction between the social structure and the self-esteem of the actor. His approach involves regarding the parameter "a" as variable and finding the best solution, i.e., that solution which minimizes the difference between the predicted and observed values. Another line of investigation, carried out here, is to investigate the limiting behavior of the model.

The algebraic structure of the expression for P(S) admits of two ways to investigate the limiting behavior of the model. Rewriting (2) as:

$$P(S) = \frac{\frac{u_1}{u_2 + u_3} + a}{\frac{u_1}{u_2 + u_3} + 1} \qquad (3)$$

allows a clear demonstration of this point. If we let x equal $\frac{u_1}{u_2 + u_3}$, then we are interested in exploring the following limit:

$$\lim_{x \to 0} P(S) = \lim_{x \to 0} \frac{x + a}{x + 1} = a.$$

X may tend to zero under two analytically distinct sets of circumstances. One approach to accomplish this is to construct an experiment whose treatments drive u_1 to zero. Another approach is to construct an experiment whose treatments make $(u_2 + u_3)$ very large in relation to u_1. The second approach was investigated here.

The enlargement of the denominator in the expression also may be accomplished in two distinct ways. One is to increase the values of u_2 and u_3 in the experiment. While logically the most reasonable course of action, this is also the most difficult to pursue experimentally. Verbal manipulations in the experiment, or increased inducements, to regard the experimenter and other as more important than in the original work are difficult to implement because one is not capable of assuring consistent impact of such manipulation across subjects. The second approach to incrementing the denominator is to assume that u_2 and u_3 remain constant and to add other utility components to the situation. The clearest means to accomplish this end is to add per-trial monetary rewards to the situation. In addition to payment for participating in the experiment, each subject was told that the team would receive a fixed monetary reward for each "correct" answer. The second alternative is pursued here.

The addition of the per-trial monetary rewards requires that additional assumptions about the utility structure be made explicit. Since both actors in the experiment can make final decisions, it is reasonable

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The algebraic structure of the expression for P(S) admits of two ways to investigate the limiting behavior of the model. Rewriting (2) as:

$$P(S) = \frac{\left(\frac{u_1}{u_2 + u_3}\right)^{+a}}{\left(\frac{u_1}{u_2 + u_3}\right)^{+1}} \qquad (3)$$

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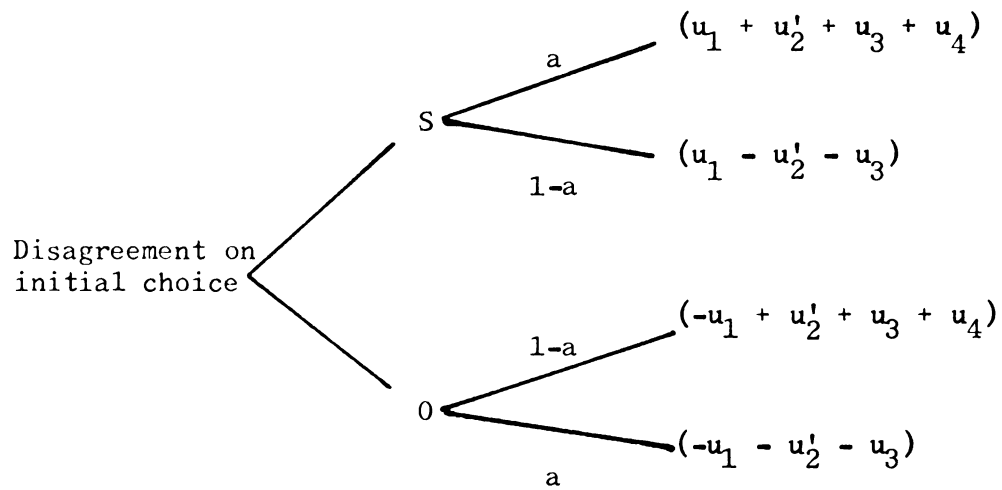
The addition of the per-trial monetary rewards requires that additional assumptions about the utility structure be made explicit. Since both actors in the experiment can make final decisions, it is reasonable

to assume that some change will occur in the value of u_2 as a consequence of adding the monetary rewards. Hence we will assume that:

$$u_2' = u_2 + \Delta u_2$$

where Δu_2 represents the change in u_2 as a consequence of the additional financial reward. The idea we wish to communicate with this change in u_2 is that wrong decisions on the part of the actor are potentially more costly to the group, and consequently to the other actor, than in the original experiment. As a consequence, there will result a change in the value to the actor of the social approval of the other. Since the actor does not lose money, i.e., there is no financial penalty for making an incorrect decision, when he makes an incorrect response the utility of money does not take on negative values. Specifically, we wish to assume that monetary rewards have value different from zero only when they are received. Hence, the choice structure becomes, letting u_2' denote $u_2 + \Delta u_2$ denote the monetary component:



and the expected gains of the alternative become:

$$G_S = 2u_1 + 2a(u_2 + u_3) + au_4$$

$$G_0 = 2(1-a)\,(u_2 + u_3) + (1-a)\,u_4.$$

Following the analysis of Camilleri and Berger, the probability of a self-consistent response becomes:

$$P(S) = \frac{u_1 + a(u_2' + u_3 + u_4/2)}{u_1 + u_2' + u_3 + u_4/2} \qquad (4)$$

For heuristic convenience (4) is rewritten to reflect the grouping of all utilities associated with the addition of the monetary reward under the term for $\{u_4\}$.[1] Hence (4) becomes

$$P(S) = \frac{u_1 + a(u_2 + u_3 + u_4/2)}{u_1 + u_2 + u_3 + u_4/2} \qquad (5),$$

where u_4 represents all utilities to the actor of receiving monetary rewards in the situation. The addition of u_4 to the trial structure, and the consequent change in the expression for the probability of a self-consistent response, leads to the substantive conclusion that actors who receive monetary rewards are more likely to accept influence from the other actor than are subjects who do not receive monetary rewards.[2] The parameter a, the subjective probability of being correct, is the limiting value for the expression P(S).

The addition of per-trial monetary rewards to the decision model of social influence raises the question of whether or not the experiment is comparable to others which have concerned themselves with the distribution of monetary rewards based on decision outcomes. The alternative

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[1] This represents a notational convenience which has a mathematical base if it is assumed that associativity holds.

[2] Subjects in the experiment were told that the group would receive ten cents for each correct response, the total amount to be divided equally between them at the end of the session.

explanations which are suggested are those which have sought to account for behavior in repeated plays of matrix games. These explanations have been extensively discussed by Luce and Suppes (1965). The principal concern of their analysis is an assessment of the viability of regarding decisions as consequences of utility and probability assessments made by an actor. They address themselves to situations which involve a series of ascertainable outcomes, i.e., the subject in the experiment receives information about the value of the outcomes, learns the probability with which they will occur, and knows immediately if he has been successful on any given trial. This set of constraints is not fully met in the present work. Subjects in the experiment discussed here can ascertain the value of various outcomes and may infer the relative frequency of their occurrence, but they do not receive feedback on their choices.

One of the principal assumptions of the analysis presented by Luce and Suppes is that subjects seek to maximize their outcomes when making a series of choices. It is possible to develop an alternative to the Camilleri-Berger model employing this maximization principle. One such model is presented below.

The outcome matrix for the decisions confronting subjects in the experiment is of the following form (the ideas used here are due to Luce and Raiffa, 1957):

		Player 1 Outcome	
		C	W
Choice	S	y	o
	O	y	o

Figure 1.--Decision outcome matrix.

where C represents making a correct choice, W represents making an incorrect choice, and y represents the contribution to the group product. The dominating strategy in this case is for each player to make a correct choice. However, since outcomes are not known to the players and appear to be contingent, it is not possible for them to employ the above matrix. It should be noted that the subjects are not given a payoff matrix specifying the outcomes for one course of action as opposed to another. The analysis is proceeding as if the subjects constructed such a matrix themselves.

It is necessary to specify a matrix of outcomes which takes into account the probabilistic features of the experiment. Such a matrix is represented by Figure 2.

		Player 2	
		S	0
Player 1	S	p_1b,p_2b	p_1b,p_1b
	0	p_2b,p_2b	p_2b,p_1b

Figure 2.--Probabilistic outcome matrix.

where the p_i represent the probability that actor i is correct, S represents a self-consistent choice, 0 represents an other-consistent choice, and b represents the outcome available to the group. If the p_i are not differentiated, then no dominating strategy exists. However, if the p_i are different, then it is possible to demonstrate a preference ordering for the available choices. Suppose $p_1>p_2$, then $(p_1b,p_1b)>(p_1b,p_2b)>(p_2b,p_2b)$. Hence, the dominant strategy is for Player 1 to choose his own alternative and Player 2 to choose Player 1's alternative consistently. For the monetary experiment outlined below, the following payoff

matrix results, where columns 1 and 2 have been interchanged for clarity of representation, and the value of ten cents has been multiplied by the probability of being correct based on the manipulation phase of the experiment:

		Player 2	
		β_1	β_2
Player 1	α_1	8.5,8.5	4,8.5
	α_2	8.5,4	4,4

Figure 3.--Payoff matrix.

The numbers represent expected amounts of money, in cents, accruing to the group as a consequence of the final decision on a given trial. Clearly, the maximal return results when the subjects play (α_1,β_1). The data are examined in Chapter IV to determine whether or not a significantly larger number of subjects pursued the maximal strategy in the monetary as opposed to the non-monetary condition of the experiment. Previous research has suggested that this comparison should result in a larger number of subjects pursuing the maximal strategy in the monetary condition of the experiment (Luce and Suppes, Castellan, 1965). Concomitantly, the probability of a self-consistent response should be higher in the pay condition for the [+-] expectation state and lower for the [-+] expectation state than for the control condition.

CHAPTER III

AN EXPERIMENT TO TEST THE EXTENDED MODEL

The suggested changes in the right-hand side of (3) lead to a design which may be mapped out as an analysis of variance presentation where the variables are ability, control, monetary rewards, and the nature of the expression $u_1/(u_2 + u_3)$. The total design has four ability by three control by two reward by two utility structure conditions, for a total of forty-eight cells. The results reported by Camilleri and Berger include twelve of these cells.

Procedural changes in the experiment, from the original Camilleri-Berger experiment, removed the possibility of using the Camilleri-Berger data for comparison purposes in testing the extension of the model. As a consequence it was necessary to collect data in both the monetary and non-monetary conditions of the experiment. The original experiment was least successful in generating predictions for the [-+] ability state, which were included in the test of the reformulation. Because of the structure of the experiment, the most obvious choice for the baseline condition was the [+-]ability state. By virtue of the formulation developed above, only one of the two utility ratios is of concern, i.e., the case where u_1/u_2+u_3 is not changed from the original formulation. The control condition under which the experiment is carried out was also of concern. By focusing on the equal control condition it was possible

to collect data on both ability conditions in the same control condition at the same time. Hence, the proposed test of the reformulation involves two ability, two reward, one utility, and one control condition, for a total of four cells. Specifically, the experimental conditions are (a) [+-] with per-trial rewards, (b) [+-] without per-trial rewards, (c) [-+] with per-trial rewards, and (d) [-+] without per-trial rewards.

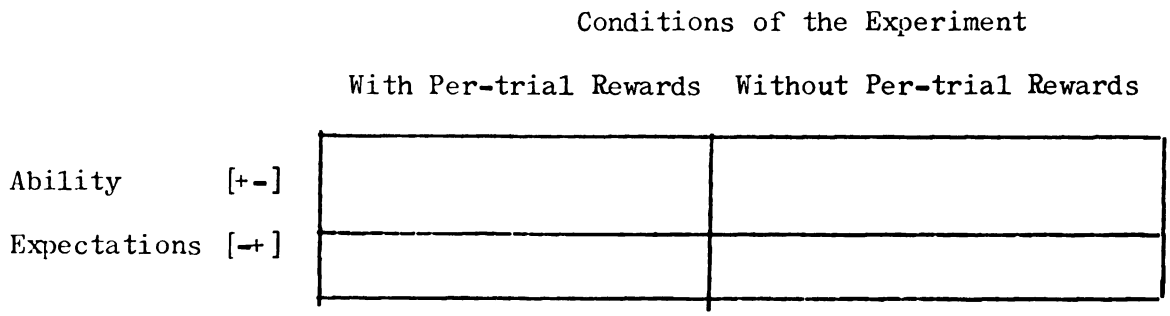

Figure 4.--Experimental conditions.

with the control condition and utility structure held constant; see Figure 4.

The subjects in the experiment went through a manipulation phase during which they were led to believe that they had either high ability, their probability of being correct was .85, or low ability, their probability of being correct was .40, and that their partner had either low ability or high ability. The subjects were then told that they were to engage in a cooperative decision task in which their goal was to do as well as possible at the task, i.e., get as many correct as possible. They then went through a series of ambiguous stimulus trials where they were asked to make initial and final decisions, receiving feedback on the initial decision of their partner. Each actor was told that he contributed equally to the team product, i.e., that the answers of each

actor would count equally. The stimuli were the same as those employed by Camilleri and Berger.

One half of the groups were also told that they would receive a fixed monetary amount for each correct response. The subjects were told that they would receive the total amount at the end of the session. Instructions for the experiment, the manipulation schedule, the post-experimental interview, and a description of the stimulus are included in the appendix.

The above features of the experiment are the same in the Camilleri-Berger experiment and in the present experiment. In addition, Camilleri and Berger employed two experimenters who served as the host and the boardman. The host had the responsibility of reading the principal portion of the instructions and general responsibility for conducting the experiment. The boardman had the responsibility of explaining the scoring procedure and recording the scores of the subjects during the manipulation phase of the experiment. The slides were presented to the subjects for a period of five seconds, which was counted off by the host experimenter.

In order to increase experimental control in the present study, two major changes were effected in the procedure. The instructions for the experiment were tape-recorded for play-back to the subjects. The justification for this change was to insure consistency of presentation across subjects. In addition, the interval of presentation of the slide was controlled by a Hunter timer to insure that each slide was presented for an equal interval. The interval of presentation was also decreased to two seconds in the hope that this would increase the degree of ambiguity of the stimulus. Since the slides were pretested with an

interval of five seconds, an analysis was performed to determine whether or not the choice structure for each slide remained in an interval about .50 as Camilleri and Berger suggest is necessary. The results are presented in the appendix.

Subjects

The subjects for the experiment were recruited as volunteers from introductory sociology and social science courses, and then scheduled for an appointment by telephone. An effort was made to determine which subjects would be highly likely to become suspicious prior to the telephone contact.[1] These subjects were dropped from the sample. A total of twenty-eight non-suspicious subjects were run in each cell of the experiment. Twenty-five subjects became suspicious of the various deceptions involved in the experiment and were not included in the analysis.

A significantly larger number of subjects, $p = .06$ by Fisher's exact test, became suspicious of the experimental manipulations in the monetary reward as opposed to the non-monetary reward conditions of the experiment. No systematic effort was made in the post-experiment interview to determine the effects of the monetary rewards on the perception of the experimental task. Informal questioning of some subjects indicated that they felt they were more sensitive to the experimental task

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[1]The volunteers were asked to list courses in Sociology or Psychology which they had taken. If the potential subject had had more than two courses in either discipline or had had a course in Social Psychology he was dropped from the sample. The principle reason for this was one of economy.

than they would have been without monetary rewards. If these reports represent a fair approximation of what occurred, then it is reasonable to conclude that the addition of monetary rewards makes subjects more sensitive to the task and consequently more likely to become suspicious of the deceptions of the experiment. See Table 2 for the distribution across conditions of the suspicious subjects.

TABLE 2

DISTRIBUTION OF SUSPICIOUS SUBJECTS*

Ability Condition	Without Monetary Reward	With Monetary Reward
[+-]	5 (.15)	7 (.20)
[-+]	1 (.03)	12 (.30)

p = .06 By Fisher's Exact Test.

*Figures in parentheses are the percentages of all subjects for that condition who were suspicious.

Insofar as was practicable, all pairs were composed of status equals with respect to age, class standing, and race. All subjects were males, and a total of 137 participated in the experiment reported here.

Recapitulation and Hypotheses

In a previous utility model of an experiment on social influence the expression for the probability of a self-consistent response was written as:

$$P(S) = \frac{\frac{u_1}{u_2 + u_3} + a}{\frac{u_1}{u_2 + u_3} + 1}$$

where u_1 represents the utility of a self-consistent response, u_2 represents the utility of the approval of the other actor, and u_3 represents the utility of the approval of the experimenter. There are two alternative means of driving the ratio, $\frac{u_1}{u_2 + u_3}$, to zero. The first alternative is to make u_1 very small, holding the denominator fixed. The second alternative is to enlarge the denominator. The latter alternative is the basis of the present experiment. This was accomplished by adding another utility component to the denominator, represented by u_4. Per-trial monetary rewards are employed as the utility component, u_4. The prediction of the model and major hypothesis is that actors are more likely to accept influence if they receive per-trial monetary rewards than if they do not receive such rewards. An alternative model was developed which leads to the hypothesis that the probability of a self-consistent response is higher in the [+-] state of the pay condition and lower in the [-+] state, than in the control condition.

The procedural changes effected in the experiment should have no effect on the rate at which actors accept influence. While they represent substantial departures from previously conducted experiments, the theoretical argument is not contingent upon the manner in which the experiment is conducted. Therefore, no substantial difference was expected between the results obtained by Camilleri and Berger and those obtained in the comparable conditions of this experiment.

In addition to the above analyses, the model was tested by employing one of the ability conditions to generate predicted values for the other condition in both the per-trial reward and the no per-trial reward conditions.

The predictions of the model are generated in the following way: Let:

$$P(S) = \frac{u_1 + a(u_2 + u_3 + u_{4/2})}{u_1 + u_2 + u_3 + u_{4/2}} = X_i = \text{obs } P(S)$$

and assume:

$$a = \frac{\text{number correct by actor}}{\text{number correct by both}},$$

from the manipulation phase, then:

$$\frac{\dfrac{u_1}{u_2 + u_3 + u_{4/2}} + a}{\dfrac{u_1}{u_2 + u_3 + u_{4/2}} + 1} = X_i$$

and it can be shown that:

$$\frac{u_1}{u_2 + u_3 + u_{4/2}} = \frac{X_i - a}{1 - X_i}$$

where $1 > X_i > a$. X_i is estimated by substituting the value for $\dfrac{u_1}{u_2 + u_3 + u_{4/2}}$ in P(S) and carrying out the indicated operations.

CHAPTER IV

RESULTS AND CONCLUSIONS

The model of the experiment set out in Chapter II makes predictions about the relative rate at which the subjects will make one response as opposed to another, the distribution of those responses over times, and the inter-trial dependence of the responses. From the reformulation of the model based on the addition of monetary rewards it was expected that the subjects would be more likely to accept influence in the monetary reward condition of the experiment. The predictions of stationary and independent response protocols based on the original model are not changed as a consequence of this reformulation.

The principal hypothesis, that the addition of per-trial monetary rewards would lead to a decrease in the proportion of self-consistent responses, was not supported. See Table 3.

TABLE 3

MEAN PROPORTION OF SELF-CONSISTENT RESPONSES

Ability Expectation	Camilleri-Berger Without Pertrial Reward	Shelly Without Pertrial Reward	Shelly With Pertrial Reward
[+-]	.78 N = 29	.77 N = 28	.76 N = 28
[-+]	.44 N = 28	.36 N = 28	.34 N = 28

Comparing the results obtained in the non-monetary with the monetary condition, no apparent difference in the rate of acceptance of influence is shown by the data in either ability condition.[1] Comparing the results from the non-monetary condition of this experiment with those of Camilleri and Berger shows no difference in the high ability condition, and an apparently significant difference in the low ability condition. A possible explanation for this result is advanced below. The results presented here also lead to a tentative rejection of the hypothesis that subjects maximize their outcomes in the experimental situation.

The hypothesis, derived from the model, which predicts independent response protocols was tested by constructing transition matrices for each of the conditions of the experiment. The matrices thus constructed represent the transition from trial N to trial N + 1 between the self and other responses. Each matrix is based upon 588 observations. Inspection of the matrices leads to the conclusion that the independence of trials hypothesis is not refuted by the data. See Table 4.

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[1]Inclusion of data from the suspicious subjects does not affect this conclusion.

TABLE 4

SELF-OTHER TRANSITIONS FROM TRIAL N TO TRIAL N + 1

Ability Condition		Without Pertrial Reward: Trial N + 1 S	0	With Pertrial Reward: Trial N + 1 S	0
[+ -]	S	.74	.26	.76	.24
Trial N	0	.85	.15	.77	.23

		Without Monetary Reward: Trial N + 1 S	0	With Monetary Reward: Trial N + 1 S	0
[- +]	S	.35	.65	.40	.60
Trial N	0	.37	.63	.30	.70

The conclusion based on this analysis of the data is that the experiment is an independent trials process. A stronger test of the independence hypothesis could be constructed by analysis of each inter-trial transition. This analysis was not pursued because of the relative lack of enough cases for analysis. Analysis of the transitions in the first and second half of the experiment analogous to that presented in Table 4, supports the conclusion that all conditions are independent trials processes. This analysis may be viewed as supportive of the stronger test of the data in that it is dependent upon fewer observations and is less subject to the masking of negative results as a consequence of shifts in behavior which are time dependent. Comparison of the transition matrices in Table 4 again substantiates the conclusion that there is no difference between the monetary and non-monetary conditions of the experiment.

The prediction of the model of stationary probabilities of response on each trial was investigated by constructing a graph for each condition of the experiment. The graphs represent the probability of resolving the disagreement with the other subject in favor of self across trials. These graphs are presented in Figures 5 and 6. Confidence intervals were constructed about the mean probability for each condition of the experiment. These intervals did not overlap between ability conditions of the experiment. From visual inspection of the curves in all conditions of the experiment there appears to be some support for the hypothesis of stationarity of the response probabilities. Comparing the curves between the monetary and non-monetary conditions of the experiment reveals that the monetary condition tends to produce more stable response patterns than does the non-monetary condition of the experiment. The frequency of violations of the confidence intervals is lower in the pertrial reward condition of the experiment than in the no pertrial reward condition. See Table 5.

TABLE 5

FREQUENCY OF VIOLATIONS OF THE CONFIDENCE INTERVALS

Ability Expectation	With Pertrial Reward	Without Pertrial Reward
[+ -]	2	3
[+ -]	1	4

P = .083 by Fisher's exact test

Analysis of the length of sequences of response was also carried out. That information lends support to the hypothesis that subjects in the monetary condition produced more stable response patterns than the

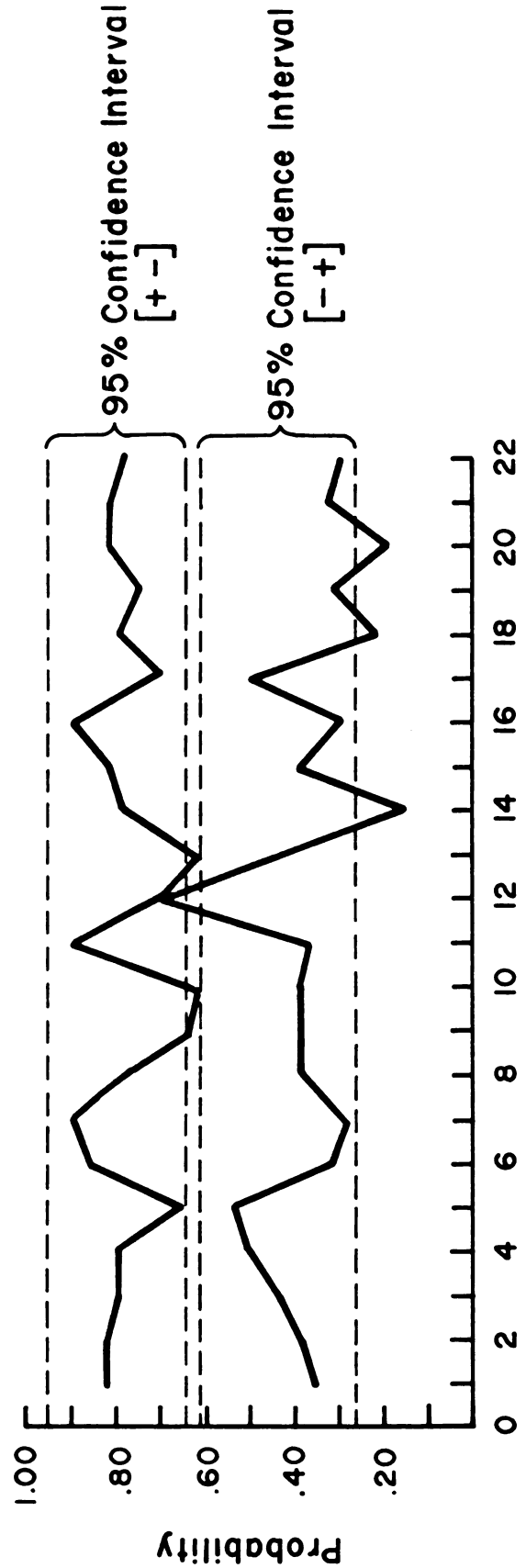
95% Confidence Interval
[+ -]
95% Confidence Interval
[- +]
1.00
.80
.60
.40
.20
Probability
0
2
4
6
8
10
12
14
16
18
20
22
Trial-by-Trial Pr [S-Response] Control Condition

Figure 5

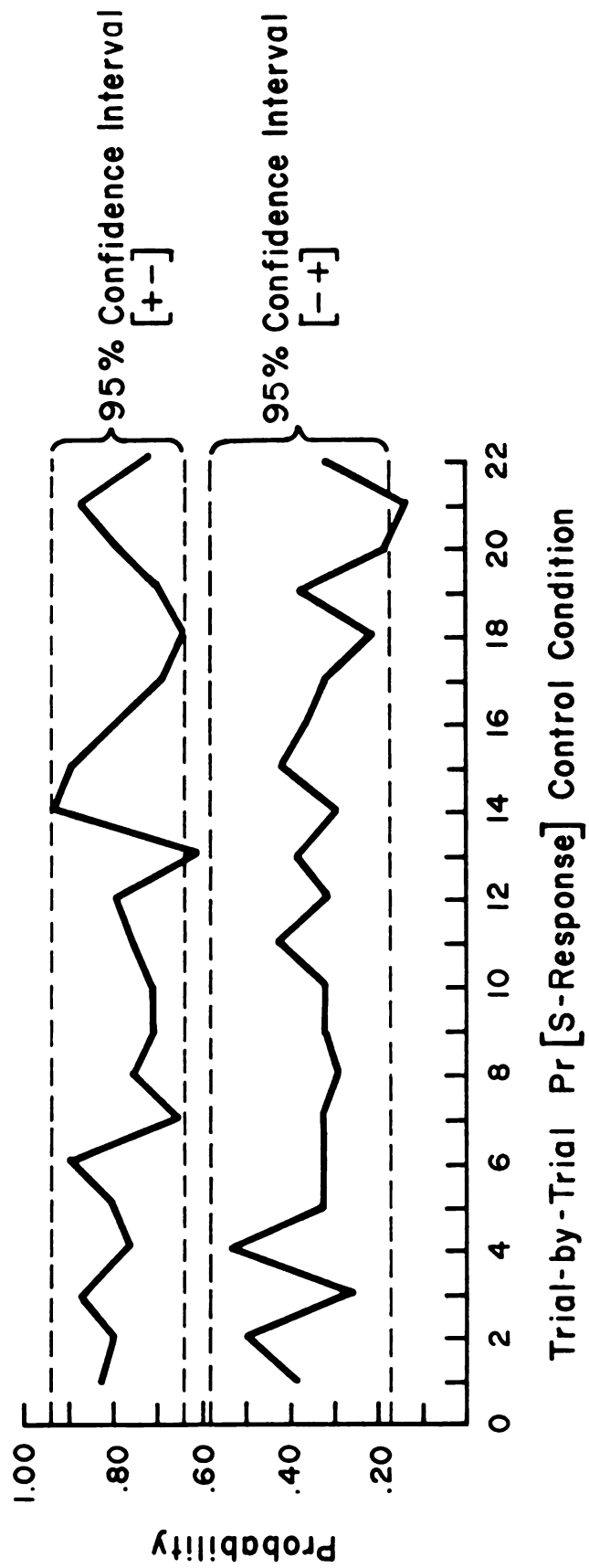
95% Confidence Interval
[+-]
95% Confidence Interval
[-+]
1.00
.80
.60
.40
.20
Probability
0
2
4
6
8
10
12
14
16
18
20
22
Trial-by-Trial Pr [S-Response] Control Condition

Figure 6

non-monetary condition. See Table 6.

TABLE 6

MEAN LENGTH OF RESPONSE SEQUENCE

Ability Expectation	Without Pertrial Reward	With Pertrial Reward
[+ -]	2.42	2.55
[- +]	2.00	2.33

The data presented above lend support to the following conclusions: (1) there is apparently no difference between the monetary and non-monetary conditions of the experiment with respect to either the mean proportion of self responses or the transitions from trial N to trial N + 1. (2) The monetary reward condition apparently tends to produce more stable response patterns than does the non-monetary condition of the experiment.

The fact that no difference is found in the proportion of self-consistent responses does not constitute a sufficient test of the maximization hypothesis. It could be the case that a subset of the subjects in the pay condition maximized their choices. The question then becomes one of the relative frequency of maximization behavior in the non-monetary as opposed to the monetary condition of the experiment. Subjects were classified in the following way for this analysis: (1) in the high ability condition a self response on all trials represented a maximization strategy and (2) in the low ability condition an other response on all trials represented a maximization strategy. Because of an insufficient number who met this criterion in either condition, the categories were relaxed so that the subjects could make up to two

"errors" in pursuing this strategy. The results showed no significant difference between the two conditions. See Table 7.

TABLE 7

NUMBER OF SUBJECTS WHO MAXIMIZED

Ability Expectation	Without Monetary Reward	With Monetary Reward
[+-]	3	1
[+-]	3	6

p = .19 by Fisher's exact test.

Exact tests of the model may be carried out in two distinct ways. One of these involves generating predictions of the proportion of self-consistent responses employing a subset of the data based on the method set out at the conclusion of Chapter III. This method yields results which clearly disconfirm the model. See Table 8 and 9.

TABLE 8

PREDICTIONS OF THE MODEL BASED ON THE [+-]ABILITY STATE

Ability Expectation	Without Monetary Reward		With Monetary Reward	
	Pred.	Obs.	Preds.	Obs.
[+-]	x	.77	x	.76
[-+]	.51	.36	.49	.34

N = 28 in all cells.

TABLE 9

PREDICTIONS OF THE MODEL BASED ON THE [-+] ABILITY STATE

Ability Expectation	Without Monetary Reward		With Monetary Reward	
	Pred.	Obs.	Pred.	Obs.
[+-]	.70	.77	.69	.76
[-+]	x	.36	x	.34

N = 28 in all cells.

The second method of testing the model involves generating a series of values for the expression $\frac{u_1}{u_2 + u_3 + u_{4/2}}$ and comparing them across conditions of the experiment. This set of calculations is carried out by solving for $\frac{u_1}{u_2 + u_3 + u_{4/2}}$ in the equation for P(S), where P(S) is assumed to be the empirical value for each condition of the experiment. This latter method of analyzing the model yields results which are consistent with the interpretation of the model that is presented in Chapter II, i.e., the utility ratio is smaller in the monetary condition than it is in the non-monetary condition. See Table 10.

TABLE 10

ESTIMATION OF THE UTILITY RATIO BY EXPERIMENTAL CONDITION*

Ability Expectation	Without Monetary Reward	With Monetary Reward
[+-]	.39	.33
[-+]	.06	.03

*a = .68 for [+-] and .32 for [-+] respectively, based on formula

$$a = \frac{\text{number correct by actor}}{\text{number correct by both}} .$$

However, this difference is small and, while consistent with the interpretation of the model, does not appear to be substantial.

To recapitulate, the principal results of the experiment preserve the generally reported result that high status actors are less likely to accept influence than are low status actors. For the particular experiment reported here, the addition of monetary rewards does not alter this result, although it does appear to have some effect on the trial-wise behavior of subjects. Apparently, the relative frequency of maximizing strategies does not change as a result of the inclusion of monetary reward. Examination of the predictions of the model leads to a negative conclusion about the model's ability to predict the rate of acceptance of influence, when the same strategy employed by Camilleri and Berger is used to estimate parameters of the model.

Four alternative explanations for the results of the experiment will be considered. These involve two distinct assumptions regarding the monetary reward manipulation in the experiment. The first two explanations are based on the assumption that the subjects in the experiment

pay attention to the monetary reward manipulations of the experiment; that is, we assume that the manipulation "worked". The last two explanations assume that the subjects do not pay attention to the manipulations of the experiment; that is, we assume that the manipulation did not work.

The first alternative to be explored is that the results of the experiment are valid and consequently lead to a rejection of the model. For the purposes of this analysis the focus will be on the lack of predictive power of the model as it relates to the rate at which actors accept influence. Both the Camilleri and Berger and this experiment yield results which fail to confirm the model in the differentiated status condition. A possible reformulation of the model, given these results, would involve the development of status specific decision trees. The data of Camilleri and Berger support the model in equal status conditions. An empirically based reformulation must then distinguish between an equal status and a differentiated status structure in the decision situation. The parameter a does provide a sufficiently strong distinction, in light of the results of the experiment.

The assumption is made that the parameter a may be decomposed into two values α_1 and α_2, where α_1, is applicable in equal status structures and α_2 is applicable in differentiated status structures. It is also assumed that the ratio of utilities is constant across status structures. Employing the appropriate composition rules, the model becomes:

$$P(S) = \frac{u_1 + \alpha_1(u_2 + u_3)}{u_1 + u_2 + u_3}$$

for the equal status conditions, and

$$P(S) = \frac{u_1 + \alpha_2(u_2 + u_3)}{u_1 + u_2 + u_3}$$

for the differentiated status structures. The experiment reported above does not permit a test of this reformulation, in that no information was collected in equal status structures. It is possible to check the plausibility of the reformulation by comparing its predictions with the results of Camilleri and Berger.

TABLE 11

PREDICTIONS BASED ON THE REFORMULATED MODEL

(EQUAL CONTROL CONDITION ONLY)

Ability Expectation	Predicted	Observed
[+-]	x	.78
[-+]	.55	.44

Table 11 presents the results of such an analysis. The values for the utility ratio reported by Camilleri and Berger were used in the computation. No improvement in the fit of the model and data is suggested by this result.

The relatively simple reformulation of the model has proved insufficient to the task of accounting for the results of the experiment. A more complex reformulation is suggested as a result of the above analysis. It will not be attempted here. Balkwell (1969) has developed a reformulation which explicitly takes into account status position and place in the decision structure of the experiment.

The second alternative to be explored is that the heuristic argument regarding monetary rewards is substantially correct, but that the experiment did not implement it well. For purposes of this alternative it is assumed that the model has heuristic value and is substantially correct with regard to its predictions. The question which must be answered if it is assumed that the value of 10¢ did not produce the desired is how large must u_4 be to drive the ratio $\frac{u_1}{u_2 + u_3 + u_4/2}$ to zero. Employing the values of the utility ratios for the non-monetary condition reported in Table 10, changes in the utility ratio necessary to produce significant effects in the proportion of self-consistent responses may be calculated. For the present analysis u_1 was assumed to have unit value and utility is assumed to be linearly related to monetary reward for purposes of treating u_4. To have reduced P(S) from the .76 observed in the non-monetary reward condition for high status actors to .70, a close approximation to a = .68, u_4 would have had to be 25 cents. To have reduced P(S) from the .36 observed in the non-monetary reward condition for low status actors from .36 to .33, a close approximation to a = .32, u_4 would have had to be 50 cents. The conclusion, on the basis of these calculations, is that the rewards offered for correct choice in the experiment were not sufficiently large to produce the theoretically predicted result.

The third alternative involves the assumption that the monetary manipulation was not successful because subjects did not attend to it. Many experiments involving monetary rewards for performance at a task have informed the subject of his progress by providing tallies or actually presenting rewards for each success (Luce and Suppes, 1965). This was not done in the present experiment because the task that was

involved was ambiguous, i.e., there were no objectively right or wrong answers. Annett (1969) makes the point that rewards, whether social or monetary, convey information to the subject in an experiment. This information is then used by the subject to effect changes in behavior. Since no such information is communicated in this experiment, no difference would be expected between the monetary and non-monetary conditions. Olin (1972) did convey such information to his subjects in the form of an injunction not to conform and observed a pattern of differences consistent with this interpretation.

The fourth alternative explanation assumes that the subjects took the monetary rewards into account, but acted according to norms which produced results counter to the theoretically predicted result. The principle argument here is that the subjects in the monetary condition of the experiment perceive that the distribution of rewards is potentially inequitable, given the distribution of abilities in the group. It is also crucial to assume that the subjects perceive a strategy which will restore equity in the distribution of rewards. The consequences of this strategy could have produced the result observed in the experiment; i.e., attempts on the part of those subjects in the monetary reward conditions of the experiment to arrive at an equitable reward distribution resulted in no difference between the monetary and non-monetary conditions of reward. No data is available to allow test of this interpretation.

Insufficient evidence is available to allow a choice between the above alternatives. Inferring from the data presented in Chapter III with respect to suspicion and the data presented in the trial-by-trial analysis, the conclusion that subjects attended to and acted upon the monetary manipulation leads to consideration of these alternatives

which accept the heuristic value as the most plausible. There are, however, no grounds to choose between these explanations. An experiment which takes into account differential information, levels of reward, and involves more status conditions should allow us to choose between the various alternatives which are presented above.

The discrepancy between the results obtained by Camilleri and Berger and the results of this experiment may be accounted for by noting that an experimenter was in the room during the experiment in the Camilleri-Berger experiments and was not present during this experiment. It is then postulated that the presence of the experimenter in the room with the subjects served as social support for the low status person in the group and hence led to a greater proportion of self-consistent responses than obtained in this experiment. In terms of the model, this interpretation would suggest that the subjects of the present experiment attached greater significance to the utility of experimenter approval than in the Camilleri-Berger experiment. The ambiguity which is a consequence of removing the experimenter apparently produces this result. Hence, one consequence of the changes in the experimental procedure is an apparent increase in the subject's desire to please the experimenter for persons in the low status position.

In conclusion, the experiment suggested by the heuristic use of the gain-loss monetary rewards did not produce results consistent with the predictions of the model. Four alternatives for explaining these results were explored. One, the outright rejection of the model was assigned a low probability of being correct, based on other data. An experiment was proposed to choose between the other three alternatives: (1) the implementation in this experiment was insufficient, (2) the subjects did

not receive sufficient information to base alterations of their behavior on, and (3) the subjects behaved according to norms not taken into account in the model.

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APPENDIX

APPENDIX

PROCEDURES MANUAL FOR THE EXPERIMENT

Operations in the Experiment

Sequence of Events

The subjects are first met in the waiting room and then escorted to the activity room, where they are seated in either chair and receive the initial verbal instructions. The tape is put on for the instructions (each tape is marked for the appropriate condition). If the subjects have questions, answers should be rephrasings of the instructions. During the first and second phases of the experiment, one of the experimenters should monitor the subjects to make sure that they do not talk to one another. Following the end of the second phase the subjects are escorted to the interview rooms and the interview is conducted. Do not use the terms "subject" or "experiment" in any conversations with the subject.

The Equipment and Its Operation

The equipment employed in the experiment consists of the ICOM, the slide projector and Hunter timer, the tape recorder, the event recorder, and the buzzer. Each piece of equipment should be checked out before an experimental session begins. This check-out includes being sure that all switches are turned on, that the Hunter timer is set properly, and that the slide projector's lamp is not burned out.

The ICOM has two switches at the top of the control panel which must be turned on. One of these is the "Relay" and the other is "lights." The switches marked "veridical" and "normal" are to be in the "normal" position for the instructions and during the first and second phase "tests." The switches marked "Agree" and "Disagree" are to be positioned according to the schedules provided for the second phase. The red "Relay Release" is pushed at the conclusion of each trial, after the data have been recorded, to reset the machine for the next trial. If this does not work, shut the machine off and on once to release the relays. Should this fail to reset the machine (turn the lights off), abort the session.

The slide projector has two switches on it: one, on the left side of the rear projection box, is the power switch; the other, on the right side of the slide projector, is the switch for the lamp. Both must be on for a slide to be shown; turn on the power first. Always turn off the lamp first when turning off the projector, and allow it to cool until the air coming from the vent at the rear of the projector no longer feels warm (about five minutes). Turn off the lamp between subjects.

To change a burned-out lamp remove the slide tray, unplug the power and shutter connections to the projector, and then move the projector out of the box. Turn the projector over. If you don't, the lenses which focus the lamp will drop out and likely break--they are nearly irreplacable. Remove the screw on the small trap door and replace the bulb. Reverse the procedure to reinstall the projector. Any session in which a lamp burns out during either of the trials phases should be noted on the data envelope. Include the trial number and phase.

The Hunter timer has a power switch, a start switch, and four rotary interval switches. Turn the power switch on before the experiment begins. The top rotary switch sets the interval in tens of seconds, the second in seconds, the third in tenths of a second, and the bottom in hundredths of a second. The first exposure is a demonstration slide and the duration is 12.09 seconds (top switch set to 1, second to 2, third to 0, and fourth to 9); after that all exposures are for 2.09 seconds (top switch set at 0, second at 2, third at 0, and fourth at 9). Turning the start switch on starts the exposure (start light comes on, at end of interval finish light comes on). After the finish of that exposure and before the start of the next, the start switch must be turned off.

The control for changing slides is a grey box with a three-position lever. Only the center position and the spring return down position are used during the experiment. The up position opens the shutter and keeps it open. This can be used as a check on the projector before the experiment. With the shutter in the center position, the timer controls the opening of the shutter. In the spring return down position, the slides are advanced. To advance the projector one slide, pull the lever down, then let it up--don't hold it down. If the lever is held down, the slides will keep advancing.

The tape recorder has an off-on switch to be turned on before the session begins. The volume should be checked before each session. The slide switch which resembles a trigger on the right side of the machine is pulled in to stop the tape during the tape for the "live" instructions. Use the tape for each condition which is marked for that condition.

The Esterline Angus event recorder records the responses of the subjects on a moving chart by means of ink pens. There is no main power switch; it must be plugged in and unplugged. Be sure to unplug it when it is not being used. There is a rotary switch on the left that turns on the chart drive motor for in/min and in/hr. We use in/min. When the motor is turned on, a light comes on; this is an indication light for the motor and not the main power supply. If it is plugged in, the main power supply is on even though the motor light isn't lit. A rotary switch on the right controls the speed: 12, 6, 3, 1.5, and 0.75 $\frac{\text{in}}{\text{min}}$ or in/hr. We use 12 in/min. At the beginning of the day, the inkwell must be filled and the pens primed. (See inking and priming instructions, pp. B-3 and -4 of instruction manual for portable model.)

Before each experiment and possibly during, the inkwell must be filled. At the end of the day, the inkwell and pens must be emptied and cleaned. (See instructions on cleaning, pp. B-11 and -10.)

Before each experiment, the chart must be labelled--XG number, ability condition by subject, experimental condition--and again at the end of the chart run at the end of the experiment. (See pp. C-6, -7, and -10 in Esterline Angus Instruction Book for mounting and removing charts.)

Eight pens are used to indicate subject responses, and have these functions:

	Subject	Initial/Final	Top/Bottom
1	1	I	T
2	1	I	B
3	1	F	T
4	1	F	B
5	2	I	T
6	2	I	B
7	2	F	T
8	2	F	B

Manipulated other's information is not recorded. Pen nine indicates when slide is finished until start switch is turned off.

During an experiment, the event recorder has to be watched for several problems: inkwell running dry--use a toothpick to check and fill when necessary; pens running dry or clogging--(See Instructions p. B-11) or reprime; pens sticking--jiggle.

Each piece of equipment should be checked before starting a session.

Control Sheets

The following page is a copy of the control sheet for each experiment. It is to be completely filled out for each subject who participates. Make every effort to get all the information. At the end of the session, place the control sheet and data records in one of the larger envelopes provided. Be sure to put the subject's name, the name of the host, the condition of the experiment, and the date and time on the outside of the envelope.

Interview Abstract

Name John Subject _____

Age 19 Manipulation [+ -] Control

Major Sociology Included? _____

School _____ Interview Tape No.: 00

Interviewer Shelly Group XG1

Date May 1, 1971 Time 1:50 Seating Position 1

No. of Changes 00

* *

1. Was there prior acquaintance? No B Yes (explain)
2. Were any of the experimental manipulations unsuccessful? No B Yes (explain)
3. Did the subject ever become suspicious? No B Yes (explain)
4. Would you include the subject in the sample? No B Yes (explain)
5. Did the subject change states during the experiment? No B Yes (explain)
6. Did the subject give an unusual response to any of the interview questions? No B Yes (explain)
7. Explanations and other things to be noted:

Instructions for the Data Records

The hand-recorded responses will be collected on the data sheets marked Phase I or Phase II for each subject. The information requested on the form is to be completed for each subject, and the subject's choices recorded by circling either the T or B for Top or Bottom for each trial. At the completion of the second phase this information is to be placed in the envelope for the experimental session. The following two pages are sample completed forms.

The machine-recorded responses are collected on the event recorder. A line should be drawn across the paper before you start each session, and the date, time, and condition noted on the paper directly below the pens. The person who records the data should check the pens by turning on the machine before the actual data collection begins. At the end of the second phase a line should again be drawn across the page to indicate the end of the session.

Experimenter's Roles

There are three roles, or sets of tasks to be carried out in the experiment. The Host greets the subjects, seats them, briefly introduces the study, and leaves for the first portion of the Phase I instructions. The Host then returns for the test part of Phase I and records the subjects' responses on the display panel. "Slides" has responsibility for the presentation of the instructions and the slides. "ICOM" has the responsibility of recording the data and operating the ICOM.

(Slides): Slides is responsible for the presentation of the slides and instructions. He is directly concerned with the operation of the tape recorder, the slide projector, the Hunter timer, and the

Choices for Phase I

Experimental Group XG1 Manipulation [+ -]Control

Name of Subject John Subject Seat Number 1

Date May 1, 1971 Time 1:50

Trial Number	Choice	
1	T	B
2	T	B
3	T	B
4	T	B
5	T	B
6	T	B
7	T	B
8	T	B
9	T	B
10	T	B
11	T	B
12	T	B
13	T	B
14	T	B
15	T	B
16	T	B
17	T	B
18	T	B
19	T	B
20	T	B

Phase II

Name of Subject John Subject

Date May 1, 1971

Time 1:50

Seat Number 1

Number of Changes 00

Group Number XG1

Condition [+ -]Control

Status Manipulation _____

Host Shelly

Included Yes No

Trial	I	F	
1	T	T	
	B	B	C
2	T	T	
	B	B	C
3	T	T	
	B	B	C
4	T	T	
	B	B	C
5	T	T	
	B	B	C
A 6	T	T	
	B	B	Agree
7	T	T	
	B	B	C
8	T	T	
	B	B	C
9	T	T	
	B	B	C
10	T	T	
	B	B	C
11	T	T	
	B	B	C
12	T	T	
	B	B	C

Trial	I	F	
A 13	T	T	
	B	B	Agree
14	T	T	
	B	B	C
15	T	T	
	B	B	C
16	T	T	
	B	B	C
17	T	T	
	B	B	C
18	T	T	
	B	B	C
19	T	T	
	B	B	C
A 20	T	T	
	B	B	Agree
21	T	T	
	B	B	C
22	T	T	
	B	B	C
23	T	T	
	B	B	C
24	T	T	
	B	B	C

intercom. The tape for the instructions should be put on the tape recorder and checked, so that no lag exists once the machine is turned on for the subjects. The tapes are labelled according to condition and the conditions should be run according to the schedule in the back of the manual. Slides is also responsible for giving the appropriate feedback on each trial of the first phase. (See the notebook marked Phase I manipulation for this schedule.)

(ICOM): ICOM is responsible for the operation of the ICOM, the event recorder, and the buzzer in the first phase. He is responsible for recording the data and resetting the ICOM after each trial.

A General Outline of the Experiment

Slides' Job	Recorder's Job
1. Tape recording of instructions started	
2. Demonstration slide presented	
	1. ICOM set in "Normal" mode
3. Test of Spatial Judgment Ability begun	2. Buzzer cues for slides 3. Subjects respond 4. Data recorded
4. Feedback to subjects after each trial	
5. Subjects told final tally of right-wrong by Host	
	5. Note on Esterline-Angus end of Phase I
6. Phase II begun	
7. Demonstration slide for Phase II	
	6. Buzzer cues for slides
8. Phase II trials administered	7. Data recorded for Phase II

ICOM Job
Phase I

Job Description	Comments
Beginning of tape instructions	
	During first phase instructions ICOM operator has to monitor instructions for occurrence of Demonstration slides
1. Set ICOM at "Normal"	
1[1]. Sound buzzer for Demo slide	
2. Show Demo slide #1 when indicated by tapes. Demo slide choices made by subjects	
3. Test part of Phase I a. Buzzer sounded b. Subjects respond c. Responses recorded d. Displays cleared by pushing "Relay Release" on ICOM panel	Turn on event recorder ICOM operator must monitor the event recorder and coordinate with the actions of the Slide operator Turn off event recorder at end of test part of Phase I
4. At conclusion of test phase set ICOM to "veridical" for Phase II demonstration	

ICOM Job
Phase II

Job Description	Comments
	Don't fall asleep
1. Demonstration slide a. Sound buzzer b. Subjects respond on initial choice c. Sound buzzer for second presentation d. Subjects respond	Listen to instructions for occurrence of Demonstration slide
1[1]. Set ICOM to "Normal"	
2. Test part of Phase II a. Set switches for agree/disagree b. Sound buzzer c. Subjects respond on initial choice d. Record data e. Sound buzzer for second presentation f. Subjects respond on final choice g. Record data h. Push "Relay Release"	Coordinate actions with "Slides" and monitor event recorder

Slides Job
Phase I

Job Description	Comments
1. Start tape after Host has entered observation room	
	Listen to tape for cues for live instructions
2. Present Demonstration slide after buzzer sounds	
3. Test part of Phase I	
a. Present slide after buzzer sounds	Coordinate actions with ICOM operator. Be sure you press lever on intercom when you talk to subjects
b. Give who correct/ incorrect after subjects respond	
c. Change slide after who correct/incorrect. Reset timer.	
d. Repeat 20 times.	
	Host gives results and signals start of Phase II

Slides Job
Phase II

Job Description	Comments
1. Start tape for Phase II after Host has changed display boards and returned to observation room	Listen to instructions for cues for live parts of instructions
2. Present Demonstration slide after buzzer sounds for both initial and final choice	
3. Present slides for test part of Phase II a. Present slide after buzzer for initial choice b. Reset timer c. Present slide after buzzer for final choice d. Change slide after timer shuts off. Reset timer e. Repeat 25 times	Don't fall asleep

Instructions

Phase I

(Observe waiting room. Try to enter just as second subject enters. Have subjects leave coats, books, etc., in waiting room. Begin by saying:)

(Host) Let me introduce myself. I'm Mr. _____. Take either chair. The instructions for this study will be given by recording over this speaker. By means of this intercom we can hear you and talk to you. So be sure to tell us if you have any questions or there is a mechanical failure such as the sound being too low, a slide not projecting properly, and so on. Otherwise, please do not talk during the study. As you can see, there is a number on the machine in front of you, Number 1 or Number 2. During the rest of the study each of you will be referred to by that number. We will start the tape now.

(tape) First, let me ask you not to push any of the buttons on the panel until I give you instructions for their use. Feel free to smoke if you wish.

(Host leaves study room)

(Slides: Start tape)

(Host) I'd like to thank you for being able to join us today. We think you'll find this to be an interesting as well as a rewarding experience.

We are members of a research team of social scientists who are interested in studying the way in which individuals and groups solve certain types of problems. Furthermore, we are

interested in studying the ways these problems are solved in different kinds of situations. Our work will be divided into two phases or parts, Phase I and Phase II. In each of these phases you will be asked to solve problems but under different conditions. I will explain the nature of these problems and conditions as we go along.

Let us now turn to Phase I of our work.

Within the last few years social scientists have found in their studies that individuals differ in their ability to accurately perceive the spatial relationships between figures. More simply, it has been found that when some individuals are presented with a set of figures, they are able to make accurate judgments about how those figures are placed in relation to one another. Other people do not seem to have this ability to the same extent. This ability to make accurate judgments about spatial relationships social scientists call Spatial Judgment Ability.

At this time we frankly do not know much about why some people have this ability more than others.

One thing we do know about this perceptual ability is that it is not necessarily related to other specialized skills that a person might possess. This means that people with high mathematical or artistic skills, for example, do not necessarily have high Spatial Judgment Ability.

Because of the importance of this Spatial Judgment Ability, social scientists are engaged in an extensive set of studies to examine this ability among college students such as yourselves here and elsewhere.

What we are going to do today is to give you an especially prepared test which is extremely accurate in measuring an individual's Spatial Judgment Ability. That is, this test distinguishes those who have a great deal of this ability from those who do not.

The test consists of a series of pairs of rectangular figures. Each rectangle is composed of smaller black and white figures.

We will proceed as follows: I will present to you on the screen above the speaker a slide containing one pair of the rectangular figures.

(Slides: Put Demonstration slide #1 on screen for 12 seconds)

In each pair, one figure has more small white rectangles than the other. That is, the color white will cover more of the area of one rectangle in the pair than it does of the other. Your task is to determine which of the two rectangles is more white in area. I will present a slide such as this for two seconds for you to study.

(Slides: Repeat Demonstration slide #1 for 2 seconds. Continue when slide is through. Change slide)

During the actual test, you will indicate which rectangle you think is correct by pressing the button labeled either

"top" or "bottom" below the statement on your board which reads "Final Choice." That is the bottom row of buttons on your board.

Your decision will then be registered on my board. Each decision you make constitutes one round or trial. In this phase, you will be asked to make twenty such decisions.

(Emphasize:)
These decisions which you make will enable us to measure your Spatial Judgment Ability.

After both of you have made your decisions as to which of the two rectangles is more predominantly white in area, I will announce and your host will record whether each of your answers is correct or not. That is, whether or not white does in fact cover more of the area of the rectangle you selected than it does of the other rectangle in the pair. In this way, you'll be able to tell how well you're doing as you go along. At the end of the test, your host will tally up your scores.

You may find that some of the slides will seem difficult to judge as the difference between the two rectangles in the area covered by white is sometimes quite small. It was found in previous studies that some individuals are able to make correct judgments on the basis of very slight, almost intuitive, cues and feelings. In general, we have found that people with high Spatial Judgment Ability <u>consistently</u>

make correct decisions and those with low Spatial Judgment Ability usually make incorrect decisions.

So that you have some idea of how well you might do, we have put on the Chart standards based upon previous studies of college students here and elsewhere.

This test has been administered to college students of your level in this part of the country and elsewhere. The standards are based on those studies. As you can see, a score of

13 - 16 correct out of 20 is a good score;

17 - 20 correct out of 20 is a rare occurrence and represents a superior individual performance;

9 - 12 correct is a poor score; and

0 - 8 right is also quite a rare occurrence and represents a very poor individual performance.

In general, the characteristics of this test are that people with high ability will usually score in the good or superior category. Likewise, people with low ability will score in the poor or very poor category. You can also see that although a person might get nine or ten out of twenty correct by merely guessing, this is a poor score. We find that most people who guess score about the same as people with low ability.

Before beginning the test, we'll go through a practice

slide so that you can get familiar with the procedure we are going to use in this first phase of our work.

At the beginning of each trial I will sound a buzzer and then present a slide containing two of the rectangular figures. You will see it for two seconds. As soon as the slide goes off, you are to choose which rectangle, either top or bottom, is more white in area. As soon as the slide goes off, you are to press the appropriate button under the words "Final Choice" on your panel.

Now this trial will not count on your Spatial Judgment score. It is just for practice.

I will first announce the presentation of the slide. The next slide is Demonstration slide Number 2.

(Recorder: Sound buzzer)

(Slides: Put on Demonstration slide #2)

(Tape: Allow 2 seconds after slide goes off)

(Slides: Change slide)

All right. You should have made your choice by now. When you've pressed your button, your decision is registered on your own board and on my board. During the test I will announce whether each of your decisions is right or wrong and your Host will record the result on the board. When this has been recorded, a button will be pressed.

(Recorder: Press "Relay Release")

The lights on the boards will go off and we will be ready for the next trial; we will repeat this procedure for the 20 slides in this phase.

Let me summarize these important points before we begin.

1. Each decision constitutes one round or trial.
2. In this phase of our work, you will be asked to make twenty such decisions.
3. You will have two seconds to judge the slide. Please make your response as soon as possible after the slide has been taken off the screen.

(Pause)

During this phase you should not in any way communicate with one another.

(Pause)

This is not an easy task. I suggest you study the rectangles carefully.

(Host re-enters)

(Host) Is everything clear? Number 1? Number 2? Okay. We will begin with slide Number 1.

(Recorder: Sound buzzer)

(Slides: Display slide)

(Slides via intercom:)

Number 1, you are right/wrong. Number 2, you are right/wrong.

(Recorder: Push "Relay Release")

(Slides: Change slide, repeat for all 20 slides)

(Wait)

(Host) As you can see, for the entire test Number 1 got 9/18 correct and 11/2 wrong. Number 2 got 9/18 correct and 11/2 wrong.

As you can see, we have had two unusual performances today. Number 1 is in the Superior/Poor category and Number 2 is in the Superior/Poor category.

If everything is clear . . .

(Pause)

. . . we'll go on.

(Host: Take down Phase I standards and Phase I scoreboard. Give subjects Phase II standards; leave study room.)

Phase II

We are now ready to turn to the second part of our work. In this phase we are going to ask you to work together under a different set of conditions. In this situation we are interested in seeing how well you can work together as a team. We are going to allow you to exchange information with each other on what you think is the correct choice before you make your final decision. That is, we are going first to allow each of you to make a preliminary choice which will be communicated to the other person. Then, after a short period, you will be asked to make a final choice between the two rectangles. We will be concerned with

your team getting as many correct final choices as you possibly can.

After I present a slide, each of you is to make an initial choice as to which you think is the correct answer--the top or the bottom figure. This is for the purpose of letting the other person know what you think is the correct choice. After both of you make your initial choice, you will receive information on your board as to what the other person thinks is the correct answer. Only after you see the other person's initial choice will I repeat the slide for you to make your final choice. Shortly thereafter I will clear your boards.

You are to indicate your initial choice by pressing the appropriate button immediately below the words Initial Choice. This is the top row of buttons on your panel. Once you make your initial choice, this choice will be communicated to the other person and you will be able to see the other person's initial choice on the panel marked "His Choice." That is, the bulb marked "top" or "bottom" corresponding to the other person's choice will light up.

However, this is important. You will not receive information on the other person's initial choice until you have made your own initial choice. After you both make your initial choice, the slide will be presented again for 2 seconds. Immediately after the second presentation, you are to indicate your final choice by pressing the button marked "Final Choice."

(Pause)

Remember only your final decisions are scored in this phase of our work.

(Pause)

Let's try this out. I will present a third demonstration slide so that you can practice with this procedure. This will not count. It is just for the purpose of becoming more familiar with the procedure.

Make your choice immediately after the slide has been presented.

(Recorder: Press buzzer)

(Slides: Present Demonstration slide Number 3)

(Wait 2 seconds after slide)

All right. Now you should have made your initial choice.

(Recorder: Press buzzer)

(Slides: Repeat Demonstration slide Number 3)

(Wait 2 seconds after slide)

(Tape) All right. Now you should have made your final choice.

(Recorder: Push "Relay Release")

(Slides: Change slide)

During the actual test, you will be presented with 25 slides. The procedure for all of them will be as was demonstrated. We will not tell you after each slide which is the correct answer. We will record for each slide whether your final choices are correct or incorrect, and at the end of the test we will tell you how many correct and incorrect final choices the team made.

This is important. We are solely interested in your making as many correct final choices as you possibly can. The only answer that is recorded is your final choice, and you should not hesitate for any reason to change your initial choice in order to make a correct final choice. Let me repeat. Try to make as many correct final choices as you can. Do not worry whether your initial choice and final choice are the same. Let me caution you, however, to make your initial choices with care so as to provide your partner with the best information you can.

I have already mentioned that in this phase of our work we are interested in team performance; that is, in how well two people working together can do a spatial judgment task. We have found from previous studies that the most efficient way for two people to work together on this type of task is to give each member of the team equal responsibility for making the final choices for the team. Regardless of whether your scores in the first test were alike or different, it is our standard practice to give each of you equal responsibility for the team score.

In our work today each member of the team will have equal responsibility for the team's score.

In this phase we are interested primarily in seeing how well you can work together as a team. Therefore, we are allowing you to exchange opinions with one another as to what you think is the correct answer before you make your final

choice. Only your final decision will count on your team's spatial judgment score.

Let me explain how we score final decisions in this phase. Since you have equal responsibility, each time a person makes the correct final decision, your team will get one point. If an individual makes an incorrect final choice, then his final choice adds nothing to the team's score on that trial. Since there are 25 trials in this phase, this means that each person's maximum contribution to your team's spatial judgment score is 25 points and his minimum contribution zero. This means that the maximum score that the team can achieve is 50 and the minimum is zero. Each of you has an equal opportunity to contribute to the team's score.

On the sheet of paper handed to you is a table of team standards for this situation. These standards are also based on previous studies that have been done with college students like yourselves here and elsewhere.

What we find for this set of conditions--that is, where you can exchange preliminary opinions before making your final choice, and where each of you has equal responsibility for the team's score--is that:

31 - 40 is a good team score;

41 - 50 out of a possible 50 is a rare occurrence and clearly constitutes a superior team performance;

21 - 30 is a poor team score; and

0 - 20, which doesn't often occur, would clearly constitute a very poor team performance.

Remember: We are interested in seeing how well you can work together as a team.

(Manipulations)

Before we begin, let me summarize several important points.

1. Each final choice you make constitutes one round or trial.
2. In this phase, you'll be asked to make 25 final choices in all.
3. Since you have equal responsibility for the team's score, for every correct final decision each of you makes the team will get one point. If either of you makes an incorrect final choice, then his final choice contributes no points to the team's score on that trial. This means that each of you has an equal opportunity to contribute to the team's score.

(Turn off tape)

(Slides via intercom)

Is everything clear? Number 1? Number 2? Okay. We will begin with slide Number 1.

(Recorder: Push buzzer)

(Slides: Put slide on screen)

(Recorder: Record responses)

(Recorder: Push buzzer)

(Slides: Repeat slide)

(Recorder: Record responses and push "Relay Release")

(Recorder and Slides: Repeat for all 25 slides)

(Host: Enter study room).

The study is now completed. Before we discuss your scores, we would like to talk to each of you individually to get a further elaboration of your feelings and opinions about the study. Mr. _____ will speak to you, Number 1; and I will speak to you, Number 2.

In addition to the fee which we have agreed to pay you for participating today, you will receive ten cents for each correct response the team makes. For instance, if the team makes forty correct responses, the team will receive an additional four dollars. The amount the team earns in this way will be divided equally between you.

Completion of the Experiments

The following pages contain a listing of the order in which the experimental sessions were completed. In the columns headed "Included" the following code is used: Y means the subject is included, N means the subject is not included, and C means that a confederate participated in that position.

Order in Which Experiments Were Completed

Group Number	Expectation No. 1	Expectation No. 2	Experimental Condition	Included + -	Included - +
1	+ -	- +	Pay	Y	Y
2	+ -	- +	Control	Y	Y
3	- +	+ -	Pay	Y	Y
4	+ -	- +	Pay	Y	Y
5	+ -	- +	Control	Y	Y
6	- +	+ -	Control	Y	Y
7	+ -	- +	Pay	N	Y
8	- +	+ -	Pay	Y	Y
9	+ -	- +	Pay	N	N
10	- +	+ -	Control	Y	Y
11	+ -	- +	Control	Y	Y
12	- +	+ -	Pay	Y	N
13	- +	+ -	Control	Y	Y
14	+ -	- +	Control	Y	Y
15	- +	+ -	Control	N	Y
16	- +	+ -	Pay	Y	Y
17	- +	+ -	Control	Y	Y
18	+ -	- +	Control	Y	Y
19	- +	+ -	Pay	Y	N
20	+ -	- +	Control	Y	Y
21	+ -	- +	Pay	Y	N
22	- +	+ -	Pay	Y	Y
23	- +	+ -	Control	Y	Y
24	- +	+ -	Pay	N	Y
25	+ -	- +	Pay	Y	Y
26	+ -	- +	Control	Y	Y
27	- +	+ -	Pay	Y	Y
28	+ -	- +	Pay	Y	Y

Group Number	Expectation No. 1	Expectation No. 2	Experimental Condition	Included + -	Included - +
29	+ -	- +	Control	N	N
30	+ -	- +	Pay	Y	Y
31	- +	+ -	Pay	N	N
32	- +	+ -	Control	Y	Y
33	- +	+ -	Pay	Y	Y
34	- +	+ -	Control	Y	Y
35	- +	+ -	Pay	Y	Y
36	+ -	- +	Control	Y	Y
37	- +	+ -	Pay	Y	N
38	+ -	- +	Control	N	Y
39	+ -	- +	Pay	Y	Y
40	- +	+ -	Pay	Y	Y
41	- +	+ -	Control	Y	Y
42	- +	+ -	Pay	Y	N
43	+ -	- +	Pay	Y	Y
44	+ -	- +	Control	Y	Y
45	- +	+ -	Pay	Y	Y
46	+ -	- +	Pay	N	Y
47	+ -	- +	Control	Y	Y
48	- +	+ -	Control	Y	Y
49	+ -	- +	Pay	Y	Y
50	- +	+ -	Pay	Y	Y
51	+ -	- +	Pay	Y	Y
52	- +	+ -	Control	Y	Y
53	+ -	- +	Control	Y	Y
54	- +	+ -	Pay	Y	N
55	- +	+ -	Control	Y	Y
56	+ -	- +	Control	Y	Y
57	- +	+ -	Control	Y	Y
58	+ -	- +	Pay	C	Y
59	+ -	- +	Control	Y	C
60	- +	+ -	Pay	Y	Y
61	- +	+ -	Control	N	C
62	+ -	- +	Pay	Y	Y
63	+ -	- +	Pay	C	N
64	- +	+ -	Pay	C	Y
65	- +	+ -	Control	Y	C
66	- +	+ -	Control	N	C
67	+ -	- +	Control	Y	C
68	+ -	- +	Control	Y	C

Group Number	Expectation No. 1	Expectation No. 2	Experimental Condition	Included + -	Included - +
69	+ -	- +	Control	Y	Y
70	- +	+ -	Pay	C	Y
71	+ -	- +	Pay	N	N
72	- +	+ -	Pay	C	Y
73	- +	+ -	Pay	N	Y
74	+ -	- +	Pay	Y	N

The Stimulus

The stimuli in the experiment consist of a series of slides which contain two rectangular figures. Each figure is subdivided into a grid with approximately half of the grid colored black, the remainder white. Figure A1 contains a sample slide. The slides have been pre-tested, and only those with a choice structure of 40-60 per cent are employed in the study.

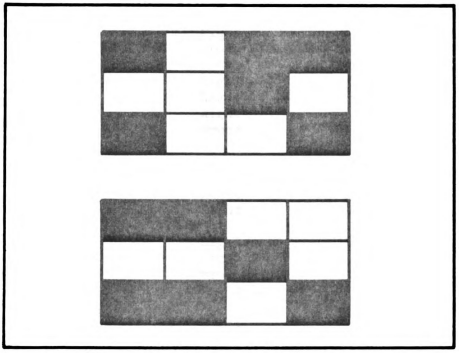

Figure A-1. Sample Slide

Trial Number	Ability [+ -]	Expectation [- +]
1	Right	Right
2	Right	Wrong
3	Right	Wrong
4	Wrong	Right
5	Right	Wrong
6	Right	Right
7	Right	Wrong
8	Wrong	Right
9	Right	Right
10	Right	Wrong
11	Right	Wrong
12	Right	Right
13	Right	Wrong
14	Right	Wrong
15	Wrong	Right
16	Right	Wrong
17	Right	Right
18	Right	Wrong
19	Right	Wrong
20	Right	Wrong

Figure A2.--Schedule of Right-Wrong for Phase I

Results of Choice Analysis of Stimuli

The change in the presentation interval apparently affected the preference for one alternative as opposed to the other. It is doubtful if this had any effect on the principal results of the experiment in that the data consist of the number of changes, and the changes are considered to be socially motivated.

TABLE A1

PERCENTAGES CHOOSING THE TOP FIGURE IN THE STIMULUS

Slide Type	Per Cent Choosing Top	Times Shown	Number of Choices
F	35.0	7	1568
H	59.6	9	2016
I	55.4	7	1568
O	62.2	7	1568
Y	65.7	7	1568
OK	36.1	7	1568

Interview Schedule

Before we discuss the results of the study, I'd like to get your reactions to it. There are a number of things which affect the results, and I want to talk with you about some of them. First, your name is _____? And what is your major field of study, (first name)? And your age?

I. Phase I

1. In general, what are your feelings about the study?

   (Just to get him talking and to determine very suspicious subjects)

2. a. Have you ever participated in a study like this one before?

      (If yes, probe for its description and why it was like this one)

   b. Have you ever read or heard about a study like this one?

      (If yes, probe as in 2.a)

3. a. When the task was first described to you at the beginning of the first test, how well did you expect to do on it? Why?

   b. At that same time, how well did you think the other person would do? Why is that?

4. Do you know the other person at all?

(If YES:	(If NO:
Find out as much about prior acquaintance as possible and probe for its effects on subject's opinions of his own ability relative to the other persons' ability)	Find out what impressions the subject got before the test and what effect these impressions had on his impressions of the ability of the other subjects)

5. a. How did you go about trying to get the correct choice in the first test?

   b. In general, how confident were you of your choices on the first test? Why (not)?

6. Do you think the results of the first test were a good measure of your Spatial Judgment Ability? Why (not)?

II. Now let's talk about the second test.

1. a. After the second test was described--before you began taking it--how well did you as an individual expect to do on it? Why?

   b. At that same time, how well did you expect the other person to do? Why?

2. a. How well do you think your team did on the second test? Why?

   b. How confident were you of your own final decisions on the second test? Why?

3. a. Do you feel that your own ability changed as the second test went along? How? Why?

   b. Do you think the ability of the other person changed during the second test? How? Why?

4. Let's look at your initial choices in the second test.

   a. Do you happen to remember how many times you agreed and disagreed with your partner on your initial choices?

   b. What did you think and feel when you found your partner disagreed with you?

   c. Do you have any ideas why you were disagreeing with him?

   d. Did you begin to feel that someone was usually right or usually wrong? Who? Why?

5. Looking back now, is there anything you could have done differently during the second test that would have improved your team's score? What? Why?

Debriefing

Now, _____, I would like to briefly explain what we were trying to study in today's tests. We are studying the relationships between a person's ability, the responsibility he has to a group, and the decisions he makes. That is, we are studying what effect there is upon a person's changing his decisions if another person with more, less, or equal ability disagrees with him on that decision. We are also studying what effect there is upon changing his decision if that person has more, less, or equal responsibility to the team than the other persons. So, as you can see, we were not interested in testing spatial judgment ability as such. Have I made sense so far?

To set up this type of situation, there were two things we needed to arrange: your ability, and your agreement with the other two persons. Concerning your ability, all of the slides that we showed you had the same amount of black and white area. That is, each rectangle in every pair was fifty per cent black and fifty per cent white. By telling you in Phase I that you got a high (low) proportion of them correct and that the other persons had a high (low) number correct, we hoped you would naturally assume that you had more (less) of this ability than the other persons. But really your high (low) spatial judgment ability and the other persons' high (low) spatial judgment ability were fictions. I'm not even sure there is such a thing as spatial judgment ability. Does this make sense?

Since in Phase II we are interested in the situation where you disagree with the other persons on your initial decisions, the information you received about their initial decisions was controlled so that

you would disagree most of the time. Actually you probably agreed with them about half the time. Is this clear?

So that, briefly, is what we were testing, the things that we arranged, and the reasons why we had to arrange them. Since there are some fictions involved in these tests, we would appreciate it if you didn't tell anybody about it. They might be tested later, and such information might bias their performance and ruin our results. Can I have your word that you won't disclose this information? If anybody asks you about the test, it's all right to tell them that it was a spatial judgment test concerning whether there were more black or white squares on some pictures; but don't tell them about the rest. Okay? Thank you very much.

Reasons for Eliminating Subjects from the Sample

1. Deliberately making wrong initial choices.
2. Misunderstanding instructions.
3. Prior acquaintance with other subject which interferes with process (change of expectation manipulation, friendship determining acceptance of influence, etc.).
4. Status differences based on physical characteristics which interfere with process.
5. Suspicion:
   a. Volunteered information that exchange of information was "rigged" (Phase I or II).
   b. Read previously about deception experiments and thought present study was similar.
   c. Heard from others that there was deception in present study.
   d. Previous participation in deception study and belief that present study was similar.

Recruitment Presentation

I'm _____ from the Sociology Department. We are currently carrying out a series of studies of how individuals and groups solve problems. We need individuals to help us out by participating in one of these studies.

Participation would involve going to Berkey Hall for one time only, for about an hour, and we will pay you at least $2.00 for the time you are there. The studies will be conducted mornings and afternoons throughout this term, so I'm sure there would be some time when you could come. The problems you would be asked to solve are not difficult, and they do not involve mathematics of any form.

These studies are not connected with this course. That is, whether you do or don't participate in a study will not affect your grade in this course. So, participation is on a voluntary basis, but we would appreciate your participation. And, in addition to the $2.00 or more which you will be paid, I think you'll find the experience itself to be interesting as well as rewarding.

So, if you feel you might be able to help us, would you please fill out one of the short forms we're going to hand out. Please fill out the form unless you are absolutely sure you would not be interested. It only obligates you to a telephone call from us and, if at that time you're not interested or you're busy, you can turn us down then. Also, since we try to match the participants in each study, we may not get around to everyone who volunteers, but we'll try to call as many of you as possible. So, if you think you might be interested, will you fill out one of these forms.

Is everything clear?


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