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THE EFFECT OF PERSONAL REJECTION IN ELICITING CONFORMITY WAS STUDIED TO DISCOVER WHETHER THE CONSEQUENCES OF LOWERED SELF-ESTEEM AND THE MOTIVATION TO INGRATIATE ONESELF AFFECT A STUDENT'S RECEPTIVITY TO THE INFLUENCES OF THE SCHOOL. THE TWO SPECIFIC QUESTIONS INVESTIGATED WERE--(1) IS THE INDIVIDUAL WHO EXPERIENCES REJECTION IN AN INITIALLY VALUED RELATIONSHIP RENDERED SUSCEPTIBLE TO INFLUENCE BY THE ONE WHO REJECTED HIM, OR THE ALTERNATE, (2) IS HE MORE SUSCEPTIBLE TO WHATEVER SOCIAL INFLUENCES HAPPEN TO BE PRESENT. THE RESEARCH IS A FACTORIAL DESIGN IN WHICH STOOGES OF THE EXPERIMENTER PROVIDE THE REJECTION OR ACCEPTANCE CONDITIONS. THE FINDINGS WERE AMBIGUOUS. (JK)

A STUDY OF SOME OF THE WAYS IN WHICH REJECTED CHILDREN BEHAVE

Cooperative Research Project No. S-245

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Alfred, New York

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TABLE OF CONTENTS

CHAPTER PAGE

LIST OF TABLES

INTRODUCTION

General Purpose

The general purpose of this experiment was to obtain additional information about a striking relationship observed in several previous studies -- viz., the effect of personal rejection or disesteeming in eliciting conformity. The specific question which this experiment was intended to answer was whether this heightened susceptibility to influence is limited to the rejector-rejectee relationship or whether it extends to relationships with other persons as well. A second purpose of the experiment was to gather some data bearing on the "personality" consequences of rejection or disesteeming (e.g., lowered self-esteem and motivation to ingratiate oneself.)

Significance of the Problem for Education.

The significance of this problem for education lies in its bearing on a more general question -- viz., the factors which affect the student's receptivity to the influences exerted by and within the school. It is possible that one of these factors is the experience of personal rejection or acceptance by someone whose esteem matters. If one makes the assumption that this is so, a number of questions can be asked. The following may be taken as examples:

1. Does personal rejection (or acceptance) by the peer group affect the student's subsequent receptiveness to influence by any of the following categories of people?

   a) member of this peer group
   b) other agemates
   c) teachers of other representatives of the school

2) Does personal rejection (or acceptance by a parent affect the student's subsequent receptiveness to influence by any of the following categories of people?

   a) this parent
   b) the other parent
   c) teachers of the same sex
   d) teachers of the opposite sex
   e) members of the student's peer group
   f) other agemates

The present research is quasi-theoretical rather than applied, but this line of investigation should provide data which would then suggest appropriate designs to for testing questions such as those indicated above.

Related Literature and Commentary

Previous experiments bearing on the rejection-conformity relationship have typically followed a similar format. Four or five subjects appear at the laboratory; and after a period of getting acquainted they are asked to rate each other on same value dimension such as "desirability as a group member." After each subject has rated each of the other subjects, the experimenter collects the ratings, pretends to tabulate them, and then (using bogus ratings prepared in advance) proceeds to inform each subject as to how he has (supposedly) been rated by the others. In this fashion the experimenter can on a random basis give to each subject a certain view as to how the others in the group feel about him. Thus subjects can be randomly assigned to an "acceptance" treatment, "rejection" treatment, or any other treatment the experimenter wishes.

At this point the subject is usually asked to give some judgments on a series of items. Before he does this, however, he is exposed to what is allegedly the unanimous judgment of the rest of the group but what is in fact a contrived concensus with no relation to what the others actually think. Typically also this alleged group judgment is patently

false. Now in a situation like this, one would expect some subjects to go along with the rest of the group and others not to (Asch, 1956; Crutchfield 1955). The question of interest, of course, is whether there is a pattern to the conformity. Do subjects in the rejection condition for example, tend to endorse the alleged group standard to a greater extent that do subjects in the acceptance conditions? As indicated below, some experiments suggest that they do. A further question however, concerns the type of motivation involved in the conformity -- assuming that differential conformity does in fact appear. It is this motivational question which represents the general focus of the present research, although the immediate goals are more limited.

Beginning with the assumption (discussed below) that the experience of personal rejection can produce conformity behavior, the present research was designed to sharpen the contrast between what seem to be two different kinds of explanations for this conformity. According to one of these explanations, the phenomenon of conformity after rejection is the outcome of forces peculiar to the subject's relationship to his rejector and to no one else. According to the other, it reflects a more generalized change in the subject, making him more susceptible to influence by other people as well.

The phenomenon of conformity after rejection has been studied by several investigators, although it has not always appeared where it was expected. Kelley and Shapiro (1954), for example, led half their subjects to believe that they had received "high acceptance" from their co-participants in the group task and half to believe that they had received "low acceptance" from them. It had been predicted that the low-acceptance subjects would conform to the alleged group standard to a greater extent than would the high-acceptance subjects, but such was not the case. The

low-acceptance subjects did, however, show an apparent loss of interest in the group; and the authors suggested that this fact might account for their failure to conform as expected. Consistent with this suggestion was the finding that among the high-acceptance subjects, those with actually low acceptance conformed significantly more than those with actually high acceptance.

In a later study by Dittes and Kelley (1956), the subjects were randomly assigned to four degrees of acceptance--high, average, low and very low. In this study, the subjects who had been led to believe that the group's attitude toward them was one of "average acceptance" subsequently conformed to the fictitious norm in what the authors term "quasi-private" conditions more than did any of the others. Thus, with reported "valuation of group membership" approximately the same for the two groups, the average-acceptance subjects conformed more than did the high-acceptance subjects.

Jackson and Saltzstein (1958) led half their subjects to believe that they had been accepted by their peers as co-members of a particular task group and half to believe that they had not. Later--on a similar task--the subjects were exposed to the alleged unanimous position of their peers but were given an individualistic orientation (as contrasted with the Dittes and Kelley study, in which group unanimity was represented as extremely desirable). In this situation, the non-accepted (i.e., excluded) subjects conformed more than the accepted subjects.

The last two experiments cited above thus provided strong evidence that some individuals do in fact conform when given cause to doubt their acceptance. Why this conformity occurs, however, has not been at all clear. Dittes and Kelley (1956) suggested that their average-acceptance subjects may have been seeking to improve their status and their very-low-acceptance subjects merely avoiding the embarrassment of public rejection.

A third explanation for conformity following rejection (Jackson and Saltzstein, 1958; Jackson and Snoek, 1959; Snoek, 1962) is that individuals who are rejected experience a "need for social reassurance" and that conformity represents a response to that need. Still another possibility (Dittes, 1959) is that conformity is largely a matter of premature cognitive closure--a phenomenon observed in Dittes' low-acceptance subjects.

A somewhat similar explanation was implied in Hochbaum's (1954) suggestion, supported by his experimental data, that the reducing of self-confidence leaves an individual increasingly dependent on other people for the determining of "social reality." It seems possible that one effect of rejection is to reduce the individual's confidence in his own ability to make discriminations and consequently to increase his valuation of those offered by people around him. This possibility is consistent with Dittes' (1959) finding that the low-acceptance subjects scored significantly lower on a self-esteem measure than did the high-acceptance subjects and with the Janis findings (Janis and Hovland, 1959) that low self-esteem is associated with high general persuasibility.

Additional possibilities could be suggested. Perhaps the rejected subject believes implicitly in Heider's P-O-X model (Heider, 1958) and views conformity as a strategy for manipulating his rejecting peers to like him better. Or again, possibly the subject interprets rejection as a sign that his peers are for some reason hostile toward him and that, given a chance, they will do him harm. Finally, the subject may for some reason find agreement with the rejector inherently rewarding. Doubtless there are other possibilities, but the present list should suffice to indicate the extent of our ignorance on this matter.

At least seven of these explanations appear to fall into one of two categories. What is at issue in four of the explanations for conformity

following rejection (avoiding public rejection, avoiding future expression of hostility, manipulating the rejector's esteem, finding agreement with the rejector inherently rewarding) is the particular relationship of the subject to the ones who have rejected him--a relationship which he would have to no one else. In other words, if one or more of these explanations is the correct one, the subject, if given a choice, should conform to the rejector but not to someone else.

A different expectation, however, is implied by the explanations in the second category. What is at issue here (premature cognitive closure, reduced confidence in one's ability to judge, seeking social reassurance) is not the subject's relationship to the rejector *per se* but rather his generalized response to any relevant person who happens to be present. In other words, if one of these explanations is the correct one, the subject, if given a choice should conform more after rejection not only to his rejector but also to anyone else who suggested a solution to his problem. The one remaining explanation, "seeking to improve one's status" (Dittes and Kelley, 1956) perhaps falls into this category also, although its meaning is not entirely clear from the context.

The Question To Be Answered

Consider in the context of a particular experiment the question implied by this categorizing. In the Jackson and Saltzstein (1958) study, the "excluded" subjects subsequently endorsed the alleged position of their peers more than did those who had been "included." Were these subjects simply rendered more susceptible to whatever influences happened to exist in the environment (in this instance, a unanimously endorsed position on a serie of items), or were the conformity forces specific to the person who had rejected them? It is this question of specificity which constitutes the

major focus of the present study, and it may be stated as follows: Is the individual who experiences rejection in an initially valued relationship thereby rendered differentially susceptible to influence by the one who rejected him, or is he simply rendered generally more susceptible to whatever social influences happen to be present?

METHOD

Design of the Experiment

This experiment utilized a 2x2x2x2 factorial design, with subjects randomly assigned to conditions. Only 3 of the 4 independent variables were of experimental interest, however, the fourth (identity of the stooge) having been introduced for purposes of control (see Appendix A). In the discussion that follows, the 3 independent variables which are of experimental interest will be referred to as the "major" independent variables.

Each subject was led to believe that another student (with whom he had just been talking) had formed either an extremely favorable first impression of him (referred to here as "acceptance"), or, conversely, a somewhat negative one (referred to here as "rejection"). Communicating this information to the subject introduced the first of the major independent variables -- the experience of the subject prior to being exposed to influence.

The other student, while representing himself as another subject in the experiment, was in fact a paid accomplice (stooge) of the experimenter. For purposes of control (see Appendix A), two stooges (referred to here as "Stooge A" and "Stooge B") were used in this role; and pairing the subject with one or the other of these two stooges introduced another

independent variable -- the identity of the stooge taking a particular role. As indicated above, however, this particular variable was of no experimental interest.

With appropriate explanations, the subject was then paired with a particular student and asked to give judgments in the autokinetic situation (Sherif, 1936). In half the cases this fellow student was the same person (one of the two stooges) who had, supposedly, just accepted or rejected him; and in half the cases it was somebody else (the other stooge). This manipulation introduced the second major independent variable -- the sameness of the partner during the judging task. The rationale for this manipulation was that if conformity after rejection extends to other relationships, the rejected subject should be equally susceptible to influence by the peer who has rejected him and by another peer who hasn't been involved in the evaluation. If, however, conformity after rejection is limited to the specific person who did the rejecting, the rejected subject should not be unusually susceptible to the second person's influence. The sameness variable should not, in other words, introduce differential effects in the acceptance and rejection conditions.

Finally, each subject was exposed to influence by his partner in the judging task. In half the cases the subject gave his subsequent judgment privately by writing it down. In half the cases he announced his subsequent judgment to his partner after writing it down. This manipulation introduced the third major independent variable -- the publicity attending the giving of judgments. A second-order interaction effect (i.e., unusual conformity by the rejected subject to the rejecting peer but only in the public condition) would have been taken as supporting the "ingratiation" hypothesis (Jones, 1964) rather than the "self-esteem" hypothesis (Dittes, 1959).

The dependent variable was the amount of change exhibited by the subject on his second series of judgments.

The design of the experiment may be summarized schematically as follows:

CONDITIONS				N
Stooge A	Acceptance	Same	Private	5
			Public	5
		Different	Private	5
			Public	5
	Rejection	Same	Private	5
			Public	5
		Different	Private	5
			Public	5
Stooge B	Acceptance	Same	Private	5
			Public	5
		Different	Private	5
			Public	5
	Rejection	Same	Private	5
			Public	5
		Different	Private	5
			Public	5

Hypotheses

The following hypotheses were tested:

1) Rejected subjects will conform more than will the accepted subjects.

This prediction was based on the findings from several previous studies,

cited earlier. It was expected that rejection would heighten the subject's susceptibility to influence by the one who had rejected him, whether or not it affected his relationship with the other person.

> 2) Subjects who announce their judgments publicly after being exposed to influence will show greater change than subjects who do not.

Any concern about one's public image (and there is bound to be some) should increase the attractiveness of conformity in those conditions which provide for surveillance by the partner.

> 3) Rejected subjects will show increased conformity not only to the one who rejected them but to the other person as well.

This prediction is consistent with all of the explanations in category two, mentioned earlier, which suggest that the experience of being rejected makes one generally more persuasible. Such a prediction would be supported by at least two different combinations of treatment effects. The general expectation, however, is that the rejected subjects will not be differentiall affected by the sameness manipulation.

> 4) Announcing judgments publicly will have more of an effect in increasing conformity after rejection than it will have after acceptance.

An assumption is made that one effect of rejection is to make one generally more fearful of his acceptability. If this is so, the effect of publicity whould be to increase the rejected subject's desire to know what is acceptable and to make him more responsive to cues that are provided. This assumption would be supported by an observed interaction of the <u>experience</u> and <u>publicity</u> variables, indicating that publicity makes more of a difference after rejection than it does after acceptance.

> 5) The special effect of publicity in eliciting conformity after rejection will not be markedly different in the "same" and "different" conditions.

This prediction argues against the "ingratiation" hypothesis, which asserts that conformity after rejection represents an attempt to counter-manipulate the rejecting peer to like him better.

Subjects

Subjects were 30 male freshman students at Alfred University who had been recruited from R.O.T.C. classes. Only those volunteers who met the following specifications were invited to the lab for an experimental session: (a) born in the United States and (b) judged to be Caucasian. Altogether, 39 subjects were tested. Of these, 9 were excluded for reasons indicated in Appendix B. Subjects' ages ranged from 17 to 21, the median being 18.

Apparatus

The autokinetic apparatus consisted of a point source of light, a one-tenth watt neon bulb exposed through a one-millimeter aperture in a metal box. The box housed also a small motor, the purpose of which was to enhance the illusion of motion. The apparatus was set to turn the light off and on at regular intervals: on for 60 seconds and off for 20 seconds. Extending from the apparatus was a wire ending in a small button which rested on the table in front of the subject. The button was connected in such a way that pressing it while the light was on would interrupt the cycle, turn the light off two seconds later, and then start the cycle all over again

The box containing the point source of light rested on a table 11 feet in front of the subject and at approximately eye level for him when he was seated. On the table at which the subject and the stooge were seated (in addition to the button previously described) were two cardboard boxes, each containing a ballpoint pen and a small note pad. The pages of the subject's pad were numbered: practice-series pages 1-3 and main-series pages 1-12.

Except for the small amount of light introduced by the experimenter's flashlight while the subject and the stooge were escorted to and from the judging position, the room was in complete darkness throughout the experiment.

Procedure

Getting Acquainted.

At each experimental session three students appeared at the laboratory. One of these students was the real subject, and the other two were paid accomplices (stooges) of the experimenter and were merely taking the role of a second and third subject. In all sessions the experimenter indicated that he was expecting a fourth subject also.

The experimenter escorted the three students to a small room and invited them to have a seat. By prearrangement the two stooges seated themselves approximately opposite the subject and facing him. The experimenter then "called the roll", conspicuously noting the absence of the "other subject." Then he spoke as follows:

> I think I'll wait just a few more minutes for him. While we're waiting, though, why don't you introduce yourselves around. This will speed up what we're doing first. Okay?

The experimenter then left the room for 5 minutes. As soon as he left, the stooges introduced themselves to one another and then turned to the subject and did the same. The stooges kept an informal and innocuous discussion going until the experimenter returned with the announcement "I guess we'd better go ahead."

Manipulating the identity of the stooges

The experimenter first noted that there would be two studies during the experimental session. Then, referring to his list, he identified the subject and one of the stooges by name and told them that they had been assigned together for the first study. Feigning disappointment, the experimenter told the second stooge that "his" partner hadn't shown up yet and that he would like for the stooge to wait outside while the task was explained to the other two students. At this, the second stooge got up and left the room.

Pairing the subject with a particular stooge prior to the evaluation constituted the identity manipulation. While the assignment of a particular subject to a particular identity condition (i.e., "Stooge A" or "Stooge B") had been determined in advance, it was not until the end of the 5-minute getting-acquainted period that the stooges themselves learned which of them had been selected for the role in that particular session.

Prelude to the experience manipulation

The experimenter escorted the subject to a small room, handed him a form (see Apprendix C) to read and fill out, and then asked him to knock on the door when he had finished. The form "explained" the study, stating that its purpose was to find out how people form first impressions. It implied that the two students who had been paired together would both be participating in the study and that both of them would receive the same material. The subject was asked to indicate on a 15-point scale his first impression of the other student (stooge).

When the experimenter left the subject's room, he returned to the stooge and escorted him to another room. Connecting the two rooms (the subject's room and the stooge's room) was an air vent; and a message spoken directly into it in one room would be readily heard in the other. Standing next to the air vent, the experimenter proceeded to "instruct" the stooge in the manner that the subject had previously been instructed. This "instruction," of course, was transmitted to the subject's room by means of the air vent. When the "instructing" was completed, the experimenter left.

When the subject knocked on his door (indicating that he had finished), the experimenter went in and examined the form for possible omissions or evidence of having misunderstood the instructions. As he looked over the

form he asked the subject if he had found the instructions to be clear. He pointed to the subject's rating of the stooge and asked, as though it were a routine confirmation, "And this is your first impression of him?" When confirmation had been received, the experimenter continued as follows:

> Okay. Well, that ends the first study. We're going into the other room now, and I'm going to ask the two of you to make some judgments; and because of the nature of the judgments, I'd like for you not to know how large the room is when you first go inside. It's for this reason that I'm asking you to put on this blindfold (POINTING) when you first go in. But let me check the other man's form, and I'll be right back.

The experimenter left the subject's room and went again to the stooge's room. The experimenter's remarks (and the stooge's replies) were as follows:

> Have you finished? (Yes, I knocked a minute ago) Let's see now. You are Mr. -- (PAUSES AND WAITS FOR NAME TO BE SUPPLIED) -- Mr. (STOOGE'S NAME). (That's right) Were the instructions clear? (Yes) Okay.

The Experience Manipulation (Stage 1).

> ACCEPTANCE CONDITIONS I see you encircled number '15'. Your first impression of him was definitely positive then. (Yes, it was)

> REJECTION CONDITIONS I see you encircled number '6'. Your first impression of him was somewhat negative then. (Yes, it was)

The instructions continued as follows:

> Okay, well, that ends the first study. We're going into the other room now, and I'm going to ask the two of you to make some judgments; and because of the nature of the judgments, I'd like for you not to know how large the room is when you first go inside. It's for this reason that I'm asking you to put on this blindfold (POINTING) when you first to in.

The Sameness Manipulation (Stage 1).

ALL CONDITIONS You haven't taken part in the judging task before, have you?

SAME CONDITIONS (No) Okay, why don't you go ahead and put your blindfold on; and I'll be right back. (All right)

DIFFERENT CONDITIONS (Is that where we go into a dark room and judge how far a light moves?) That's right. (Yes, I did that last week. Do you want me to do it again?) No, I had forgotten that you had already taken it. You came in later one afternoon, didn't you. (Yes) I remember it now. No, there's no point in taking it a second time. Well, I guess that's all for today. (OPENS DOOR) Thanks for coming in. (Okay) Goodbye. (Goodbye)

The experimenter than picked up a previously prepared rating form (Appendix C). This form had supposedly been filled out by this student (the stooge), but in fact it had been filled out by the experimenter in advance of the session. Furthermore, the specific rating it contained born no relation to the identity of subject or the stooge.

The experimenter went once again to the subject's room and spoke to the subject as follows:

The Experience Manipulation (Stage 2)

ACCEPTANCE CONDITIONS It looks as though his first impression of you was definitely positive. I see (POINTING TO BOGUS RATING) he gave you a '15'.

REJECTION CONDITIONS It looks as though his first impression of you was somewhat negative. I see (POINTING TO BOGUS RATING) he gave you a '6'.

The Sameness Manipulation (Stage 2)

ALL CONDITIONS Well...if you'll put your blindfold on now, we'll go out into the other room.

SAME CONDITIONS I'll take you first and go back and get your partner.

DIFFERENT CONDITIONS By the way, the other man just reminded me that he'd taken part in the judging task before. He came in last week -- I'd forgotten that. At any rate, I told him that he could leave. As soon as I take you inside, I'll see if the other man is still out there; and if he is, I'll let him take part in the judging task with you.

With this, the experimenter proceeded(with the aid of a flashlight) to escort the blindfolded subject out of his room and into another, completely dark, room. When the subject had been seated, the experimenter left and returned shortly with the other student -- i.e., the stooge (either the same stooge who had just evaluated him or the other stooge -- depending on whether the subject was in the "same" or "different" condition. The stooge feigned the difficulties on someone being led blindfolded. The experimenter asked the two students to keep their blindfolds on "just a little bit longer -- until I've finished with the flashlight."

General Task Instructions.

When the experimenter reached the front of the room, he told the two students to take off their blindfolds and proceeded to give them the task instructions (Appendix D). They were told that a small light would appear in front of them and that after an interval it would begin to move. As soon as they detected the movement, they were to press the button on the table in front of them. After the light had gone out, they were to estimate in whole inches how far the light had moved.

Actually, of course, the light would not be moving at all. It has been demonstrated many times (Bovard, 1948; Kelman, 1950; Bohrer, Baron Hoffman, and Swander, 1954; Sherif, 1936; Whittaker, 1964) that in this situation the illusion of movement is experienced by most people. It was assumed therefore that after a few trials the subjects would be distributing their judgments within a fairly stable range.

Procedure on the Practice Series.

The two students were given a series of three practice trials and then a series of 12 critical trials. The procedure on these two series was somewhat different. On the practice series, each of the students was

asked (1) to record his own judgments by writing them down on a pad and (2) to remember what he had written down so that the experimenter could find out afterwards how well he was doing. After the completion of the three practice trials, the two students -- first the subject and then the stooge -- announced the three judgments which they had just written down. While the subject's practice judgments presumably reflected a genuine attempt to judge how far the light was moving, the stooge's "practice judgments" had been programmed in advance. The stooge's judgments were distributed around a median exactly 6 inches greater than the subject's practice-series median and varied in approximately the same way that the subject's judgments had varied.

Procedure on the Regular Series

The regular series consisted of 12 trials. On each trial both students indicated their judgments -- but in different ways. The subject recorded his own judgment, each time after the light had gone out, by writing it down on a pad. He tore off the page, put it into a box, and called out "ready". The stooge, on hearing this signal, simply called out his judgment -- allegedly to be recorded for him by the experimenter. Actually, of course, the reason for having the stooge call out his judgments was quite different. The actual reason was to make the subject aware of the discrepancy between his own judgments and those given by his partner. It is this discrepant character of peer judgments which gives them their power to influence (cf. Festinger, 1957; Heider, 1958; Osgood and Tannenbaum, 1955); and it was the announcing of these judgments by the partner, trial after trial, which constituted the influence attempt in this experiment. If on the regular series the subject gave judgments higher than those he had given on the practice series, this change would be taken as indicating that the influence attempt had been successful.

As indicated above, the stooge's judgments had been programmed in advance. They represented nothing more than a prearranged sequence of numbers, distributed symmetrically within a narrow range (S.D. = 1.07) and having a median exactly 6 inches greater than the subject's practice-series median. The stooge had memorized a series of 12 constants (Appendix E) which summed to zero. His procedure in determining his own judgments in a particular session was simply to (1) add 6 inches to the subject's practice-series median and (2) add one of the constants (in prearranged sequence) to this figure.

Additional Instructions for the Public Conditions.

The procedure described in the preceding paragraphs was followed in all the experimental conditions. In the public conditions, however, the following additional instructions were added just before the beginning of the regular series.

> Oh, there's one other thing. We like to give you a chance to compare your judgments with each other as you go along -- you know, after you've already given them; but we've found that doing it this way -- the man who is calling his judgments out is at something of a disadvantage. So let's see. Mr. (SUBJECT), after you have already written your judgment down -- and after the other man has already called out his (in other words, after both of you have finished each time), would you just tell him what you wrote down? (AWAITS FOR ASSENT)

At the end of the regular series, the experimenter turned off the apparatus and asked the two students to put their blindfolds on again. He then escorted them singly -- first the stooge and then the subject -- to their respective rooms. To the subject he gave a questionnaire (Appendix G) which sought among other things to validate the manipulations. When the questionnaire had been completed, the experimenter examined it for errors or omissions, questioning the subject further as appropriate. The experimenter then brought all participants together, explained the deceptions in full, and answered whatever questions were asked. In particular, the subject was questioned with respect to his feelings about the experience. Finally, he was requested not to discuss the experiment with anyone else.

RESULTS

Check on the Manipulations

It is first necessary to inquire whether the experimental variables were successfully introduced.

Experience One of the questions on the final questionnaire was as follows:

> What do you think was his first impression of you?

It was the purpose of this question to determine whether the subject believed the information he had received regarding the other student's evaluation of him. The question, however, proved to be more ambiguous than had been anticipated. Many of the subjects (before responding to the question), asked the experimenter how they should answer it, pointing out that they knew how they had been rated since the experimenter had told them. When the experimenter was asked this question, he told the subject to indicate the rating they would have expected if they hadn't been told. The effect of these added instructions, of course, was to reduce the differences between the evaluations estimated by the "accepted" and the "rejected" subjects. With these instructions, the average estimated rating (on a 15-point scale) by subjects in the acceptance conditions was 12.5, while the corresponding rating for subjects in the rejection conditions was 7.4. Comparison of these means gives an F ratio of 156 -- significant well beyond the .001 level of confidence.

A second bit of evidence is provided by the subjects' replies to questions put to them during the post-session interview. After the general character of the deceptions had been explained, each subject was asked (a) the number corresponding to the rating he had received, (b) the meaning of this number, and (c) whether the other person had in fact evaluated him in

this manner. Without exception, subjects in the acceptance conditions reported that they had received a '15' rating, that this was the highest possible rating, and that (though subjects tended to be a little embarrassed by this) the evaluation had been genuine. Similarly, subjects in the rejection conditions (without exception) reported that they had received a '6' rating, that this was a somewhat negative rating, and that the evaluation had been genuine.

Some indirect evidence that the accepted and rejected subjects believed the ratings they received to have been genuine is found in the responses to the following items on the final questionnaire:

> As a general rule, what do you think of first impressions? That is, how accurate do you think they usually are?
>
> Do you think that (with a longer period of acquaintance) his opinion of you might change?

On the assumption that a negative evaluation is ordinarily more discrepant from one's self-view than is a positive one, attempts to resolve this discrepancy should be more strongly directed toward the low rating than toward the high one. More specifically, rejected subjects should tend to belittle the accuracy of first impressions generally and (in this particular case) should believe that the evaluator would change his evaluation after longer acquaintance -- more so, that is, than the subjects who had received acceptance. The results are consistent with this expectation. Rejected subjects had a lower opinion of the general accuracy of first impressions (4.2 versus 5.1 on a 7-point scale), and the difference is significant ($F = 20.34$) beyond the .001 level of confidence. Similarly, the rejected subjects believed more strongly than the accepted subjects that the evaluator would change his evaluation with a longer period of acquaintance (5.6 versus 4.4 on a 7-point scale). The difference

is significant (F = 12.5) beyond the .001 level of confidence.

Thus (judging from these several pieces of evidence), the rejected subjects believed themselves to have been less acceptable to the evaluating peer than was the case for the subjects who had been accepted.

Sameness. With respect to this variable, the pertinent question on the final questionnaire read as follows:

> In this study you were scheduled to work with (STOOGE WHO HAD EVALUATED THE SUBJECT). Did he take part in the judging task with you as scheduled?

Without exception, subjects in the "same" conditions responded to this question by checking "yes." Similarly, all subjects in the "different" conditions responded by checking "no."

Subjects who had checked "no" were then asked:

> Who took part in the judging task with you?

Without exception, subjects in the "different" conditions correctly named the peer stooge who had been in the judging task with them. Thus, subjects in the "same" conditions were apparently clear that their partner in the judging task was the same person who had evaluated them, and subjects in the "different" conditions were likewise clear that their partner was somebody else.

Publicity. With respect to this variable, the relevant question read as follows:

> Was he able to tell whether you were agreeing with him?

Although the partner was calling out his judgments, the subject was recording his own judgments privately in the dark. Thus there was no way for the partner to know what estimates the subject was making -- except in those conditions in which the subject was instructed to tell him. The

experimental manipulation in this case consisted of instructing the subject to do just this -- tell the partner each time what he had written down. All subjects receiving this treatment complied with the request.[2] The present question sought simply to rule out the unlikely possibility that the subject was unaware of being heard by the partner. Without exception, subjects in the "private" conditions responded to the question by checking "no" (he couldn't tell). Similarly, all subjects in the "public" conditions responded to the question by checking "yes" (he could tell).

Confidence Attributed to Partner. In assessing the success of the experimental manipulations, it is important to consider the possibility that these manipulations systematically introduced extraneous variables. In the present experiment, it is reasonable to ask whether the publicity manipulation differentially affected the subjects' estimate of the partner's confidence. In the public conditions, the subject, in announcing his discrepant judgments to the peer after writing them down, was in effect telling the partner that he disagreed with him. Yet it was obvious that this disagreement was not causing the partner to change his judgments. In other words, the partner was refusing to be influenced. In the private conditions, however, the partner presumably had no way of knowing about the disagreement; and for this reason the subject would not be likely to think of him as having "refused" influence.

One of the items on the questionnaire read as follows:

> How much confidence did the other person seem to have?

If subjects in the public conditions had attributed greater confidence to

---

2 One subject in the "private" condition voluntarily informed the partner each time what he had written down. Data from this subject were not included in the final tabulations.

the partner than had subjects in the private conditions, the difference should be reflected in the responses to this question. Analysis of variance for the main effect of the publicity variable indicates that these two groups did not differ significantly ($F=3.35$, $p > .05$) in the amount of confidence attributed to the partner.

Data on Change Scores

Overall mean change was rather modest (2.64 inches) although significantly greater than zero ($t = 10.3$, $p < .001$). Individual change scores for all 30 subjects, as well as treatment means, are shown in table 1.

Rejected subjects, as predicted, changed more than accepted subjects (2.93 versus 2.80) although the difference is not statistically significant ($F < 1$). The first hypothesis thus receives no support from these data.

Subjects judging privately, contrary to what was predicted, changed more than subjects judging publicly (3.33 versus 2.45). Again, however, the difference is not significant ($F = 3.12$, $p > .05$). Thus the second hypothesis receives no support from these data.

The third hypothesis predicted that rejected subjects would show increased conformity not only to the one who had rejected them but to the other person as well. There is no evidence, however, that this was so (Table 2). An analysis of variance indicated that the interaction of the experience and sameness variables was not significant ($F = 1.02$, $p > .05$)

TABLE I

Change in Median Judgments

From Practice to Regular Series

by Subjects

VARIABLE	EXPERIMENTAL TREATMENTS															
Experience (E)	Acceptance								Rejection							
Sameness (S)	Same				Different				Same				Different			
Publicity (P)	Private		Public		Private		Public		Private		Public		Private		Public	
Identity of the stooge (I)	A	B	A	B	A	B	A	B	A	B	A	B	A	B	A	B
Subjects																
1	3	2	3	4	4	2	3	0	4	6	2	4	2	2	2	3
2	3	6	4	2	6	5.5	9	3	4	4	3	6	3	3.5	2	3
3	2	0	2	5	4	6	4.5	2	1	1	-1	3.5	6	8	5	1
4	0	7	0	1	3	2	1	2	1	3	3	3	5	3	1	2
5	3	1	1	2	-1	2	1	2	8	6	2	1	2	0	-1	2
$\bar{X}$	2.2	3.2	2.0	2.8	3.2	3.5	3.7	1.8	3.6	4.0	1.8	3.5	3.6	3.3	1.8	2.[illegible]

TABLE II

Change Scores Classified by Experience and Sameness with Cell Entries[a] Summed over Levels of Publicity and Identity

Experience	Sameness		
	Same	Different	Total
Acceptance	51	61	112
Rejection	64.5	54.5	119
Total	115.5	115.5	231

[a]Each cell entry is the sum of 20 observations.

The fourth hypothesis postulated an experience-publicity interaction, with publicity doing more to increase conformity after rejection than after acceptance. This hypothesis was not supported ($F < 1$). The relevant figures are presented in Table III.

Finally, the fifth hypothesis postulated that there would not be a second-order interaction effect, and no such effect was observed. The relevant figures are given in Table IV.

An analysis of variance for all treatment effects is summarized in Table V.

TABLE III

Change Scores Classified by Experience and Publicity with Cell Entries[a] Summed over Levels of Sameness and Identity

Experience	Publicity		
	Private	Public	Total
Acceptance	60.5	51.5	112.0
Rejection	72.5	46.5	119.0
Total	113.0	98.0	231.0

[a]Each cell entry is the sum of 20 observations.

TABLE IV

Change Scores Arranged by Sameness (S)

and Classified by Experience and Publicity

With Cell Entries[a] Summed over Levels of Identity

Experience	Same (S_1) Publicity		
	Private	Public	Total
Acceptance	27	24	51
Rejection	38	26.5	64.5
Total	65.0	50.5	115.5

Experience	Different (S_2) Publicity		
	Private	Public	Total
Acceptance	33.5	27.5	61.0
Rejection	34.5	20.0	54.5
Total	68.0	47.5	115.5

[a] Each cell entry is the sum of 10 observations.

TABLE V

Analysis of Variance of Change in Medians

from Practice to Regular Series

SOURCE	SS	df	MS	F[1]
Identity (I)	1.79	1	1.79	< 1
Experience (E)	.62	1	.62	< 1
Sameness (S)	.00	1	.00	< 1
Publicity (P)	15.32	1	15.32	3.12
I X E	1.20	1	1.20	< 1
I X S	9.12	1	9.12	< 1.86
I X P	.05	1	.05	< 1
E X S	5.01	1	5.01	1.02
E X P	3.62	1	3.62	< 1
S X P	.46	1	.46	< 1
I X E X S	.62	1	.62	< 1
I X E X P	4.48	1	4.48	< 1
E X S X P	.01	1	.01	< 1
I X S X P	2.12	1	2.12	< 1
I X E X S X P	2.17	1	2.17	< 1
Within	313.40	64	4.90	
TOTAL	359.99	79		

[1]None of these F ratios are significant.

DISCUSSION

The picture which emerges from these results is twofold: (1) a statistically significant, though modest, amount of overall change and (2) a complete absence of treatment effects on the dependent variable. Since the experimental variables seem to have been successfully introduced, the simplest conclusion is that these variables have no effect on susceptibility to influence.

The problem with this interpretation is that the manipulating of acceptance and rejection has produced differential susceptibility to influence in several previous experiments (e.g., Dittes and Kelley, 1956; Jackson and Saltzstein, 1959; Jackson and Snoek, 1959). Further, announcing one's judgments publicly has usually produced greater influence than giving them privately (e.g., Argyle, 1957; Deutsch and Gerard, 1955; Moulton, Blake, and Olmstead, 1956), although the publicity variable has not always produced this effect (e.g., Pelz, 1958).

In view of this apparent contradiction between the present finding and those reported in other experiments, it seemed worthwhile to investigate the possibility that the expected influence effects were prevented from appearing by some uncontrolled factor in the experimental situation. Two such factors are considered below.

Baseline for computing change. As indicated in the "Procedure" section, the subject's change score was computed from a baseline which was itself determined after three of the independent variables had been introduced. It is of interest therefore to inquire whether the experimental conditions differed with respect to the "magnitude" and reliability of this baseline. The baseline for computing change, it will be recalled, was the median of the subject's three practice judgments. An analysis of variance

of these practice-series medians showed no significant differences among the experimental conditions ($F = 1.2$, $p > .05$). In similar fashion, the the variability of the subject's three practice medians was viewed as a dependent variable; and an analysis of variance was performed on the sigmas. Again, the conditions did not differ significantly from one another ($F = 1.3$, $p > .05$). Thus the absence of treatment effects cannot be attributed to experimental-condition differences in the baseline for measuring change.

Magnitude of the discrepancy. After pre-testing it was decided that the peer stooge would announce judgments which, on the average, would be 6 inches greater than the subject's practice-series median. It is possible, however, that this discrepancy was poorly chosen and that many subjects found their partner's judgments too extreme to be taken seriously. To put the matter differently, if a subject decided that his partner's judgments were simply not credible, he would presumably consider them irrelevant in arriving at his own judgments.

For example, the subject might decide that the partner had misunderstood the instructions or that he was really looking at a different light or perhaps that he believed the light to be farther away than it really was. In each of these cases the discrepancy between the judgments given by the partner and those given by the subject would have been satisfactorily explained; and in the process the partner's judgments would have lost their power to influence the subject's judgments.

Just how extreme a partner's judgment should be in order to exert the most influence is a matter of some controversy in the literature (For a review of relevant experiments and the theoretical issues involved see Cohen, 1964; Whittaker, 1966). The point here is simply that the present

experiment may have employed a discrepancy size that -- for many subjects -- was too large.

In order to examine this possibility, 10 additional subjects were exposed to peer influence in a partial replication of the experiment but, this time, with the partner using a 4-inch discrepancy instead of a 6-inch one. These 10 subjects began the judging task immediately -- i.e., without the prior experience of being evaluated. Five of the subjects were assigned to the private condition, and 5 were assigned to the public condition.

The purpose of this partial replication may be summarized as follows. If the "public" subjects change significantly more than the "private" ones (as is usually the case but was not the case in the original experiment), this finding would lend support to the interpretation suggested earlier -- viz., that the failure of the expected treatment effects to appear is traceable to the discrepancy size used in the influence situation. Put differently, if (with the smaller discrepancy size) the publicity variable has its expected effect, possibly (with a simila change in discrepancy size) the other variables would have the effects expected of them also.

The results of the replication were as predicted: subjects in the public condition changed more than subjects in the private condition (2.4 inches versus .8 inches). These means are different ($t = 2.93$) in a one-tailed test beyond the .01 level of confidence.

Recall of Partner's Average Judgment. As previously noted, the variables manipulated in the original experiment had no observed effect on the subjects' change scores. It has been suggested here, however, that the "rejected" subjects (to take one group) may have been rendered

more susceptible to influence --- but that this greater susceptibility (given the extremeness of the partner's judgments) was not readily expressible by changing one's own judgments. If this is so, one would expect to find some alternative evidence of treatment effects. One such possibility is perceptual distortion. (Steiner and Peters, 1958).

One of the items on the final questionnaire asked the subject to estimate the average of all the judgments announced by his partner. These judgments, it will be recalled, were distrubuted symmetrically within a narrow range. The standard deviation of these judgments was 1.07, and half of them were identical with the mean. It is not surprising, therefore, that the estimates given in response to this question did not differ greatly from the partner's actual mean. Forty-three subjects recalled the partner's mean accurately, while 37 did not. Of the 37 errors, 35 did not exceed 2 inches. When these 37 subjects are classified as "overestimators" and "underestimators," it is clear that there was a strong tendency to report a smaller discrepancy than had in fact existed. Twenty-eight of the errors were errors of underestimation while only 9 were errors of overestimation. These totals depart from chance expectancy (z = 2.96, binomial test for large samples, corrected for continuity) at the .001 level of confidence. On the basis of the evidence at hand, it is not possible to say whether it was the subject's perception, his recall, or merely his report which was faulty. Nevertheless, it seems clear that the peer discrepancy was something which at some point the subjects were likely to underestimate, thus minimizing the pressure they felt during or after the experimental session.

The major question here, however , is not simply whether there is a general tendency to report one's partner's judgments to be less discrepant from one's own than they really are. Identifying this tendency to distort

is rather the first step toward answering the question as to whether there is a pattern to the distorting. Is distorting the magnitude of the partner's judgments, as suggested earlier, an alternative to something else? Is it, for example, an alternative to changing one's own judgments?

To examine this possibility the subjects were assigned to cells in a 2 x 2 table according to the magnitude of the change score and whether the recalled peer mean was lower than the actual peer mean (Table VI). The cell totals depart from chance expectancy (x^2 = 3.26, one-tailed, corrected for continuity), at the .04 level of confidence. It is clear that there is a significant tendency for subjects to exhibit either conformity or distortion but not both.

TABLE VI

Subjects Classified by Amount of Change
and Recall of Peer Mean

Amount of Change	Peer Mean Recalled: Lower than actual mean	Peer Mean Recalled: Same as actual mean -- or higher	Total
Large	10	31	41
Small	18	21	39
Total	28	52	80

x^2 (corrected for continuity) = 3.26

$p < .04$, one-tailed

If these two behaviors (conformity and distortion) are indeed functional alternatives in the type of situation faced by these subjects, it would follow that any given experimental condition which had relatively many

distorters would have correspondingly fewer conformers.

Consider the possibility that these two behaviors (conformity and distortion) are indeed functional alternatives in the type of situation faced by these subjects. Following this line of reasoning, any distorter is a potential conformer; and in choosing to distort, he chooses not to conform. There were, of course, some subjects who both distorted and conformed; and there were others who did neither. The point here is simply that for approximately 61 per cent of the subjects these two behaviors emerged as apparent alternatives. Focussing on these subjects alone, the distorters may be thought of as having unwittingly lowered the conformity score for the particular experimental conditions to which they had been assigned. The immediate question then is whether these distorters appeared in one experimental condition more frequently than another.

To examine this possibility the subjects were assigned to cells in a 2 x 2 table according to the experience they had received (either acceptance or rejection) and whether the peer mean they recalled was lower than the actual peer mean (Table VII). The cell totals were only slightly different from those in Table VI; but since there was not a priori basis for predicting the direction of the discrepancy, a one-tailed test was judged not appropriate. Here the cell totals, while not significantly different from chance expectancy, indicate clearly that -- in this sample, at least -- the rejection condition has more than its share of distorters.

TABLE VII

Subjects Classified by Experience

and Recall of Peer Mean

Experience	Peer Mean Recalled		
	Lower than actual mean	Same as actual mean -- or higher	Total
Acceptance	10	30	40
Rejection	18	22	40
Total	28	52	80

x^2 (corrected for continuity) = 2.69

$p > .05$, two-tailed

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APPENDIX A

Rationale for Manipulating the Identity of the Stooge

Rationale for Manipulating the Identity of the Evaluator

The purpose of introducing the sameness variables was to determine whether the effect of rejection is limited to the subject's relationships with his rejector or whether it extends to his relationships with other people as well. The strategy was to expose half the subjects to influence by the same person who had rejected (or accepted) them and half of them to influence by somebody else. For the subjects who (after being evaluated by one person) were exposed to influence by a different person, it was obviously necessary to use two different accomplices -- given the decision to place the subjects in a face-to-face interaction with the others in the experiment.

A simple procedure would have been to assign a different role to each stooge for the duration of the experiment. One of the stooges could have had the twin roles of evaluator and "same person," while the other stooge had the single role of "different person." Using a different stooge to take each role, however, introduces the possibility that the sameness variable will be confounded with some variable related to the personality of the particular stooge taking that role. For example, in the influence situation the stooge taking the role of "same person" might be perceived as being, say, friendlier or more confident than the stooge taking the role of "different person." If this proved to be the case, the "same" and "different" conditions might show different amounts of change but for reasons that had nothing to do with sameness or differentness. In order to deal with this possibility it is necessary to vary the partner's personality systemmatically. This varying is accomplished, of course, by having each stooge take both roles equally often.

APPENDIX B

Excluded Subjects

Excluded Subjects

The 9 subjects who were excluded may be divided into two groups: 4 subjects whose non-usability may be generally attributed to certain failures by the experimenter and 5 subjects whose non-usability stemmed from other factors. The first category may be broken down as follows: learning the purpose of the experiment prior to taking part in it -- 2; recognizing the illusory character of the autokinetic movement (a junior psychology major who had recently transferred from another school) -- 1; failure of the stooge, through inattention, to play his role in the prescribed manner -- 1. The second category may be broken down as follows; failure to see the autokinetic movement -- 2; reporting of two images rather than one -- 1; use of decimals in indicating judgments (many being less than 1.0) -- 1; voluntarily reporting judgments in a way that put him in the "public" condition rather than the "private" condition to which he had been assigned -- 1.

APPENDIX C

Mimeographed Explanatory Materials

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This is a study about the way people form "first impressions." Although some would say that we shouldn't form first impressions--that we should wait until we know a person better before deciding whether or not we like him--most of us do it anyway. In fact, studies have shown this to be an almost universal human characteristic.

Very simply, we are trying to find out what people "see" when they decide that they like a person or that they don't. To put the question differently, what is it about another person that produces our first impression of him--how friendly he is? Whether he seems reasonably intelligent? Or what?

Perhaps the most interesting thing about first impressions is how extremely durable they are. In study after study, first impressions have continued to stand up--even after years of acquaintance. Members of the human species seem to recognize, almost instinctively, whether a fellow human being is friendly or not--whether he is confident or unsure. As you may know, this ability is also found among some of the lower animals.

In this experiment, we invite pairs of students (usually two pairs) to the lab and give them the following task. First, they rate each other on a series of five traits (e.g., "friendliness"). Naturally, these are not the only traits which could be used. There are hundreds of traits (perhaps thousands) which could be used in a test like this. These, however, were chosen for a particular reason. These traits were chosen because they (unlike some others) are extremely difficult to conceal from another person--even if successfully concealed from oneself. Therefore, when you rate a person on these particular traits, you are (to a large extent) seeing the <u>real</u> person--the person "behind the mask."

INSTRUCTIONS FOR SEMANTICS TEST

The five traits are given on the next page. You will notice that each trait (e.g., "friendliness") is identified by two words which are opposite in meaning. For example:

Unfriendly ___:___:___:___:___:___:___ Friendly

Note also that there are seven spaces between these words. For each set of opposites, use one of the spaces to describe the person with whom you were assigned. For example, if he seemed to be <u>extremely friendly</u>, you would put a check mark in the space nearest the word "friendly" as shown here:

Unfriendly ___:___:___:___:___:✓___ Friendly

If he seemed to be <u>definitely friendly</u> (but not extremely so), you would place a check mark one space farther away from the word "friendly" as shown here:

Unfriendly ___:___:___:___:___:✓___:___ Friendly

If he seemed to you to be <u>somewhat friendly</u>, you would place a check mark on the "friendly" side of the middle:

Unfriendly ___:___:___:___:✓___:___:___ Friendly

If he seemed to be <u>neither friendly nor unfriendly</u>, you would place a check mark in the middle:

Unfriendly ___:___:___:✓___:___:___:___ Friendly

If he seemed to you <u>somewhat unfriendly</u>, <u>definitely unfriendly</u>, or <u>extremely unfriendly</u>, follow the same procedure--using the other end of the scale.

Turn now to the next page, and rate your partner on each of the five traits listed.

SEMANTICS TEST

My first impression of _____

Unfriendly ____:____:____:____:____:____:____ Friendly

Undependable ____:____:____:____:____:____:____ Responsible

Unsure ____:____:____:____:____:____:____ Confident

Unintelligent ____:____:____:____:____:____:____ Intelligent

Sexually maladjusted ____:____:____:____:____:____:____ Sexually well-adjusted

Now that you have rated the other person on the five traits (be sure to do this before proceeding further), we want you to give your "overall impression" of him. Keeping the five traits in mind, what is your overall impression of the person with whom you were assigned--completely positive? Mostly positive? Somewhat negative? Or what?

You will notice that the scale contains five broad categories--from "all negative" to "all positive." Within each category there are three numbers--each one representing a different degree or amount. The higher the number, the more positive or favorable your overall impression. Look over the entire scale, and encircle the one number which best indicates your overall impression of that person.

My overall impression of _____

was. . .

1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
All negative			More negative than positive			Both negative and positive (about equally)			More positive than negative			All positive		

APPENDIX D

Speech Explaining the Judging Task

Speech Explaining the Judging Task

Okay, you can take your blindfolds off now. There is a cardboard box on the table in front of each one of you -- slightly to your right. Just put your blindfold in your cardboard box so that you can locate it again later. There is also a pad and a ballpoint pen in your box, but don't worry about these right now. And would you let me know when you've finished that (WAITS FOR CONFIRMATION). Now I pulled the table out from you so that you could slide in easily. Why don't both of you just pull the table back toward you now (WAITS UNTIL THIS IS DONE).

During the next few minutes I'd like to explain this second study to you in detail and answer any questions about it before we start. During this time, of course, your eyes will have a chance to become adapted to the darkness. What we're doing in this second study is this: we're studying the ability of people to make judgments in the absence of reference points. Let me give you an example of what I mean. Suppose someone were to point toward a house and ask you to judge how far away it was. What most of us would do in a situation like this is to look in the vicinity of the house for something familiar -- another house or building that we recognized, a tree, an automobile, the crest of a hill, or something of this sort. In other words, in making judgments about things we don't know, we typically refer to things that we do know.

But what would happen if these things in the vicinity were removed? What would happen if we didn't have these other things to refer to? Well, that's what we're studying in this second study: the ability of people to make judgments in the absence of reference points...and that's simply the reason for the darkness.

More specifically, what will happen is this: in just a few minutes, a small light will appear in front of you. Then, after an interval, it will begin to move. It will move for a certain distance, and then it will stop and go out again. Your task each time will be to judge how far the light moved that particular time...And that's basically what you'll be doing during this second study: judging the distance a small light moves without the usual reference points for making this kind of judgment.

Now before I go any further, I'd like for you to have a chance to see the light so that you'll know what it is that I'm talking about... And I might mention that the apparatus can move the light in any direction: left, right, up, or down --- or off at an angle. In fact, it can change the direction of the light while it's moving. Now this won't be a problem for you, of course; it will be obvious to you which direction the light is moving. Your task will simply be to judge "how far". Just now, however, when I show you the light, don't bother about trying to judge the distance it moves; we'll do that later. All I want you to do now is to see the light and get some idea about it. Just watch it; and when it starts to move, follow it -- as I say, to get some idea about it. (LIGHT COMES ON FOR 60 SECONDS AND THEN GOES OUT AGAIN)

Now, I can't see the light from where I'm sitting; so let's be sure we're all on the same wave length. Which direction did the light move that time? Mr. (SUBJECT)? (WAITS FOR DIRECTION TO BE SUPPLIED AND THEN ASKS FOR CONFIRMATION FROM PEER STOOGE)[1] Okay, now I think we're ready to get started. Each of you has on the table in front of you another box -- with a small plastic button sticking out from it. You can take the button easily in your hand and compress it with your thumb. There is a piece of tape over

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[1]If the subject reported seeing no movement, the peer stooge would be asked whether he also saw no movement. The peer stooge would agree that no movement had occurred. Then the experimenter would make vague reference to the possibility that the light was stuck and would present the light for an additional 60 seconds. If the subject reported no movement after 3 presentations, the session was discontinued. Two sessions were discontinued for this reason.

the end of the button. Would you let me know when you've found your small plastic button. (WAITS FOR CONFIRMATION FROM SUBJECT AND PEER STOOGE) Now let me just check that. Mr. (SUBJECT), would you press your button, please. (OBSERVES A PANEL LIGHT, HIDDEN FROM SUBJECT' VIEW, INDICATING THAT SUBJECT'S BUTTON HAS BEEN PRESSED). Thank you. And now, Mr. (PEER STOOGE), would you press your button, please (PAUSE). Thank you.

Now what I want you to do with the button is this. As you noticed a minute ago, when the light comes on it doesn't come on moving. There is an interval before it begins to move. One of the things we're interested in is how readily you can detect the beginning of the movement. Can you detect it as soon as the light begins to move, or has it been moving for awhile already before you're aware of it? The way you'll tell us that is this: press the button as soon as the light starts to move.

What sometimes happens is that a person will become so engrossed in watching the light move that he'll forget to press the button. If this should happen to you, would you just let me know, please, and I'll make a notation up front. In other words, this is part of our score on you; and it doesn't make any sense to give you a low score if you just happen to forget to press your button. So...anytime you do forget, as soon as you think of it, just let me know.

Okay. Now let's have a few practice trials before we get started. Each one of you reach inside your cardboard box and get out your pad and ballpoint pen...And would you let me know when you've got them (WAITS FOR CONFIRMATION FROM SUBJECT AND PEER STOOGE). Now what I want you to do with the pad and ballpoint pen is this. We'll take three practice trials; and on each of these practice trails I want you to make a judgment as to how far the light moved that time, write your judgment down on the pad, tear it off, and put it back inside the box.

I have already numbered the pages for you; so don't worry about their getting mixed up inside the box. We can sort them out later. So that's what I want you to do. Each time, after the light goes out, make a judgment, write it down on the pad, tear it off, and put it back in the box; and you'll be all ready for the next one.

Now I might mention that, from here on out, the light movements that you'll be seeing will be less extensive than the one you saw a minute ago. I deliberately gave you a light movement a minute ago that was more extensive simply to let you see what the possibilities were. As I say, from here on out the light movements will be less extensive. Each time, then, make an estimate in whole inches of how far the light moved that time -- one inch, two inches, five, ten, twenty, whatever you think it is. Write your estimate down on the pad -- and just write the figure -- the figure standing for whole inches. Tear it off, put it back in the box; and we'll go on to the next one. And we'll take three practice trials --- just like that. Now are there any questions before we begin? (UNLESS THE SUBJECT ASKS FOR CLARIFICATION ABOUT USE OF THE BUTTON, THE PEER STOOGE QUESTIONS THE EXPERIMENTER AS FOLLOWS:)

"We press the button when the light first starts to move?"

That's right. As you recall, after the light comes on there's an interval before it starts to move. Wait until the light starts to move, and then press the button. As I may have said a minute ago, that will register on the apparatus and tell us (when we look at it later) at exactly what point you detected the movement -- whether you detected it as soon as the light started moving, or whether it had been moving for awhile already before you detected it or maybe you jumped the gun that time. We'll be able to tell that when we look at it later...Yes, press the button when the light begins to move. Are there any other questions?

Oh, there's one other thing. Try to remember what you write down for your three practice trails so that I can ask you about them to see how you're doing. Don't worry about these three -- they're just practice -- but, as I say, try to remember what you write down for these three practice judgments. (THE APPARATUS WAS TURNED ON AND THE LIGHT APPEARED THREE TIMES).

Okay. Let's see how it went. Mr. (SUBJECT), what did you write down for your three practice judgments? (WHEN THESE HAD BEEN REPORTED, THE EXPERIMENTER "DOUBLECHECKED" THE SUBJECT'S MEDIAN AS FOLLOWS:) And the (e.g.) first one again was [illegible]? (WAITS FOR CONFIRMATION) And Mr. (PEER STOOGE), can you recall what you wrote down for your three practice trails? (PEER STOOGE REPORTS, ACCORDING TO STANDARD SCRIPT, JUDGMENTS AVERAGING 6 INCHES GREATER THAN SUBJECT'S MEDIAN)

All right. Now I'll give you the correct answers when we finish, but let's go ahead with this now. As I may have mentioned earlier, this part of our work is still fairly new; and we're still trying to work out the best way to have people give judgments when they come into the experiment. One way of course would be to let both of you call them out and for me to record them for you. The problem there, I've found, is that it's easy for me to make a mistake recording for two people. So we're experimenting with letting you record your own judgments -- just as you did during the practice trials. Someone suggested the other day, however, that some people may have difficulty writing in the dark; and if that's so, we'd want to find it out before we went on the next stage in your work. What this boils down to is that we're currently trying it both ways. We'll divide it up, and at the end of the session I'll ask you if you had any difficulty doing your particular way. Let's see. Mr. (SUBJECT), why don't you just continue

recording your own judgments -- the same way you've been doing. Okay? (WAITS FOR CONFIRMATION). And Mr. (PEER STOOGE), if you'll call yours out each time, I'll record them for you. Okay? (WAITS FOR CONFIRMATION). Now, Mr. (SUBJECT), each time after you've made your judgment, would you just call out "ready" -- indicating that you're through? (WAITS FOR CONFIRMATION) And, Mr. (PEER STOOGE), I want you to wait until Mr. (SUBJECT) has finished each time -- until he has called out "ready" before calling out judgment -- you know, so there won't be any influence. Do you follow me on this? (WAITS FOR CONFIRMATION). Okay then. Mr. (SUBJECT) will record his own judgment, tear it off and put it in the box and then signal "ready". Mr. (PEER STOOGE) will call out his judgment for me to record -- but each time waiting until the other man is through.

Sometimes people tell us that they have a temptation to move their heads -- you know, in following the light; and we've found that this reduces their accuracy a good bit. So I'm going to ask you to let your head rest gently against the wall directly behind you. And if you'll do this, you'll be able to control your own head movement -- in other words, keep it down to a minimum. The point is, of course, that if you'll follow the light only with your eyes and not with your head, you'll be considerably more accurate.

Now I want you to be as accurate as you can. Are there any questions before we start? (ANSWERS ANY QUESTIONS WHICH ARE ASKED)

(IN THE PUBLIC CONDITIONS, THE FOLLOWING COMMENTS ARE ADDED:)

> Oh, there's one other thing. We like to give you a chance to compare your judgments with each other as you go along -- you know, after you've already given them; but we've found that doing it this way -- the man calling

his judgments out is at something of a disadvantage. So let's see. Mr. (SUBJECT), after you have already written your judgment down -- and after the other man has already called his out (In other words, after both of you have finished each time) would you just tell him what you wrote down? (WAITS FOR ASSENT)

APPENDIX E

Constants Used by the Partner in Determining His Own Judgments in a Particular Session

Constants Used by the Partner

in Determining His Own Judgments in a Particular Session

(The partner adds the constants to a figure exactly 6 inches greater than the subject's practice-series median.)

JUDGMENT #	CONSTANT
1	-2
2	0
3	0
4	1
5	-1
6	0
7	2
8	0
9	0
10	1
11	-1
12	0

APPENDIX F

Final Questionnaire

Department of Psychology
Alfred University
Alfred, New York 14802

TO: Students taking part in psychological experiments

FROM: Chairman of the Committee on Research Activities

RE: Evaluation

We are seeking your help in evaluating the experiment(s) in which you have just taken part. We would like to know, for example, whether you enjoyed the experiment, how clear the instructions were -- things of that sort. Needless to say, this evaluation will be useful only if you tell us how you really felt.

May I thank you in advance for your help with this evaluation.

Walter Burdick

Walter Burdick, Ph.D.
Chairman of the
Committee on Research Activities

STUDY OF FIRST IMPRESSIONS

1) If you recall, we asked you to give your first impression of another student who was taking part in the experiment with you. Think back and tell us how clear you found the instructions for this task? (Place a check in the appropriate space)

Not clear at all ____:____;____:____:____:____:____; Very Clear

2) As a general rule, what do you think of first impressions? That is, how accurate do you think they usually are?

Completely inaccurate ____:____:____:____:____:____:____: Completely accurate

3) In this study, you were assigned with the following person:

_____

Before coming to the lab today, how well did you know this person?

Not at all ____:____:____:____:____:____:____: Extremely well

At the time you rated him, how well did you feel you knew him?

Not at all ____:____:____:____:____:____:____: Extremely well

4) The next question is a memory question. What was your first impression of him? (Encircle the appropriate number -- just as you did on the form given you during the experiment)

1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
All Negative			More negative than positive			both negative and positive about equally			more positive than negative			All Positive		

5) Do you think that (with a longer period of acquaintance) your opinion of him would change very much? (Place a check in the appropriate space)

No, probably wouldn't change very much ____:____:____:____:____:____:____ Yes, could easily change a great deal

6) Some people are easier to rate than others. How easy did you find it to rate this particular person?

Extremely difficult ____:____:____:____:____:____:____: Extremely easy

7) What do you think was his first impression of you? (Encircle the appropriate number)

1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
All negative			more negative than positive			both negative and positive about equally			more positive than negative			All positive		

8) How well do you think he got to know you before he rated you? (Check the appropriate space)

Not at all ____:____:____:____:____:____:____: Extremely well

9) Do you think that (with a longer period of acquaintance) his opinion of you might change?

No, probably wouldn't change very much ____:____:____:____:____:____:____ Yes, could easily change a great deal

STUDY OF

JUDGING WITHOUT REFERENCE POINTS

1) How clear did you find the instructions for this task?

Completely confused ____:____:____:____:____:____:____ Extremely clear

2) Many people are uncomfortable when they go into a completely dark room. How uncomfortable did you feel in the dark room?

extremely uncomfortable ____:____:____:____:____:____:____ Completely at ease

3) In this study you were scheduled to work with _____

Did he take part in the judging task with you as scheduled?

____ yes ____ no

If not, please answer the next question. Otherwise, ignore it.

Who took part in the judging task with you? (If you don't remember his name, simply write as much as you recall)

_____

4) Before coming to the lab today, how well did you know the person who took part in the judging task with you?

not at all ____:____:____:____:____:____:____: extremely well

5) Did you care whether you did well on the judging task?

no, didn't care at all ____:____:____:____:____:____:____ yes, cared a great deal

6) How far was the light from where you were sitting?

_____ feet

7) On the whole, how confident were you about the judgments you gave?

not confident at all ____:____:____:____:____:____:____ extremely confident

8) On the whole, how confident did the other person seem to be?

not confident at all ____:____:____:____:____:____:____ extremely confident

9) Think back to the set of judgments you gave. As accurately as you can, estimate the average of all your judgments (Give your answer in whole inches.)

_____ inches

10) Now try to estimate the average of all the judgments given by the other person (again, in whole inches).

_____ inches

11) Did you record your own judgments, or did someone else record them for you?

_____ recorded my own judgments

_____ someone else recorded them for me

12) Whichever method you used, did you have any difficulty with it?

_____ yes _____ no

13) During the judging task, were you able to tell whether the other person was agreeing with you?

_____ yes _____ no

14) Was he able to tell whether you were agreeing with him?

_____ yes _____ no

15) Can you think of any special advantage you may have had? If so, what was it?

16) Can you think of anything that may have handicapped you (in comparison with other subjects)? If so, what was it?

17) Did the other person have any handicap that you knew of?

18) Do you know of anything that might have given a special advantage to the other person? If so, what was it?

GENERAL INFORMATION

1. How old are you (to nearest birthday)? _____ years old

2. Do you have any brothers or sisters? _____ yes _____ no

   If so, give ages of each:

ages of brothers	ages of sisters
_____	_____
_____	_____
_____	_____
_____	_____
_____	_____

3. In what country were you born? _____

4. In what country did you spend your early years? _____

5. In how many studies did you take part during this lab session?

   _____ one _____ two or more

6. If you took part in two or more studies during this lab session, please answer the following question. Otherwise, ignore it.

   To what extent, during the second study, did you find yourself thinking about the first study? (Place a check in the appropriate space below)

   Never thought about it at all ____:____:____:____:____:____:____ Thought about it a great deal

7. Before coming to the lab today, what had you heard about the experiment? Try to recall in as much detail as possible?