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OF AGGRESSIVE AND CONSTRUCTIVE BEHAVIOR.

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by

ROBERT P. NUSSBAUM

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A B S T R A C T

AN EMPIRICAL TEST OF RECIPROCITY THEORY IN A
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by

ROBERT P. NUSSEBAUM

Adviser: Professor Thad R. Harshbarger

In recent years, a number of researchers have been interested in testing hypotheses based on Gouldner's reciprocity theory. One postulate of this theory is that in large numbers of social systems, people help those who help them, and harm those who harm them. The theory further postulates that at least a "rough equivalence" of the amount of help or harm received will be returned. Finally, the theory postulates that such reciprocity behavior is enforced by a norm of reciprocity.

Several researchers concluded, in analyzing their own data and that of other researchers that people do consistently and predictably reciprocate aggression and constructiveness. Their analyses have also implied support for reciprocity theory's prediction that a rough equivalence of exchange would take place, and its postulation of a norm of

ABSTRACT (continued)

reciprocity as being responsible for the observable behavior.

Despite the impressive consistency of the research they cited, our opinion was that due to certain methodological features of that research that tended to lower restraints, create an artificial situation, and eliminate possible sources of intersubject behavioral variability, the reciprocity hypotheses might still fail to be supported in a more naturalistic interaction between strangers and as such, the simple postulates of reciprocity theory might prove to be inadequate in fully understanding people's behavior in such situations.

To empirically assess this possibility and to test some hypotheses we developed, we designed a simulated naturalistic office waiting-room setting. Each subject sat alone in the room with one of four confederates who administered one of nine pre-selected social stimuli representing nine different levels of the aggressiveness-constructiveness variable. The interaction took place either under a "normal restraint" condition, where the real life atmosphere was minimally tampered with, or a "low restraint" condition where, via a prior set of manipulations, we encouraged a less inhibited laboratory-type atmosphere.

All interactions were video-taped from behind a one-way mirror, and the behavior of the confederates and subjects were rated by college student judges for aggressiveness-constructiveness and normality-abnormality. These ratings were then intercorrelated and became the focus of our interest.

We put forth several hypotheses concerning the effect of context on reciprocal behavioral change, as indicated by obtained Pearson correlations between magnitudes of aggressive-constructive stimuli, and subjects' responses to them. Consistent with expectation, a low restraint context--similar to that in previous laboratory studies--fostered as high a degree of reciprocal

ABSTRACT (continued)

behavioral change as that found previously, while the normal restraint context inhibited reciprocal behavioral change to the point where no significant amount was produced. Also consistent with expectation, abnormally intense instigations were accompanied by reciprocal behavioral change under low restraint conditions but not under normal restraint conditions, and normal instigations were accompanied by equal amounts of reciprocal behavioral change regardless of context.

Regardless of context, inspection of quantitative magnitudes of aggressiveness-constructiveness received and exchanged showed that the only magnitudes of instigation which led to a "rough equivalence" of exchange, were the "normal" constructive ones. Also regardless of context, there was a great deal of intersubject response variability in what was reciprocated.

Inspection of the self report data showed little evidence that subjects' decisions concerning their behavior were consciously motivated by normative feelings of obligation.

Theories to account for these findings and our overall findings were explored in depth. Variants of reactance theory seemed the best explanatory framework for our data.

Implications of the data for reciprocity theory and research, and for everyday life behavior, were explored in depth. The discussion also pointed out the general inapplicability of previous laboratory findings to real life normal restraint contexts and discussed the reasons for this.

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CHAPTER 1

INTRODUCTION AND LITERATURE REVIEW

Introduction

For the past twenty years, there has been increased study of variables affecting the manifestations of constructive and aggressive behavior. The focus on these areas of human behavior seems to stem from society's growing concern about violence and the need to predict and control it, as well as the increasing concern of society about insuring the continuance of constructive behavior among its members.

Previous laboratory findings have stirred the hope that such behaviors are predictable and understandable as functions of antecedent aggressive or constructive acts and other personal and situational variables. The present study was undertaken in the light of these findings. It is hoped that this research will shed more light on the possibilities for prediction and control of these types of behavior.

A body of literature supported the view that people predictably reciprocate aggression with aggression and helpfulness with helpfulness (Marshbarger 1974). The results of this research seemed consistent with, and have been interpreted as supporting Gouldner's reciprocity theory (1960), which posited that such reciprocity of behavior would occur and would be governed by norms of reciprocity.

However, a review of other literature also suggested that the low restraint laboratory context of such studies may have produced results that showed significantly greater reciprocity behavior and greater support for Gouldner's theory than is actually found in real life. Accordingly, the question of how powerful and relevant the norm of reciprocity really is - and the degree to which people do reciprocate - was an empirical question that remained to be further explored and answered.

We believe that a more comprehensive answer to this question could be obtained through studies derived from the creation of a simulated naturalistic setting where the degree of social restraint can be experimentally manipulated and observed.

Definition of Concepts

For purposes of this study, we conceived the terms aggressiveness and constructiveness as two opposite qualities of human behavior. We considered aggressive and constructive acts as lying along a continuum. Harshbarger (1974) had pointed out that "In the past there has been a tradition of studying either one or the other side of this continuum" (p. 1). However, he felt that "every interpersonal action can be classified along a continuum from helpful to harmful depending on the consequences to the recipient of the action" (p. 1). As such, he conceived the idea of studying "the entire constructiveness - aggressiveness continuum in its application to action by one person against another" (p.1).

Harshbarger's approach has been extended and adapted for use in this study. As such, it was markedly divergent from most previous studies which had not allowed for and/or included in their analyses, possible constructive responses to aggressive stimuli, or possible aggressive responses to constructive stimuli. In thus narrowing the field of possible responses to particular stimuli, these previous studies may have obtained an incomplete understanding of people's responses to aggressive or altruistic instigations.

The definitions of constructive and aggressive acts, with minor modifications, likewise followed Harshbarger. He defined an aggressive act as one which "resulted in pain to someone, or in partial or total destruction of something valued by the person aggressed against" (p. 1). Implicit in Harshbarger's notion, and stated explicitly in the

present study was the idea that the "something" which was destroyed might be tangible (one's house) or intangible (one's peace of mind). Likewise, we followed Harshbarger in defining constructive acts as those which "help another person physically, emotionally, financially, or in some other way" (p. 1). In both our definitions, whether or not the act was intended, was irrelevant, except insofar as the intent made the act more or less harmful or helpful.

Literature Review

A Theoretical View of Constructive Behavior

In recent years, one line of research into constructive behavior stemmed from hypotheses derived from reciprocity theory. Basically, the results showed that on an empirical level, the changes in the magnitude of laboratory constructive behavior of subjects were a reasonably predictable linear function of corresponding changes in the magnitudes of antecedent constructive acts of others. This predictability was understood in terms of reciprocity theory which was practically the only theory that offered a convincing explanation of these findings. It did this by postulating that a norm of reciprocity was responsible for the observable empirical reciprocity. The following presents our review of the theory itself as well as the research it generated with a view toward obtaining a better understanding of exactly what the theory is, and how it and the studies generated from it relate to actual every-day life situations.

Reciprocity theory was originally proposed by Alvin Gouldner (1960), a sociologist. Gouldner's theory interestingly enough, was not a theory of constructive behavior; rather, he wished to "suggest concretely, ways in which the...concept provides new leverage for analysis of essential problems of sociological theory, namely accounting for the stability and instability in social systems" (p. 162). Thus, he proposed a general

sociological theory and in his discussion "deliberately collapses distinctions between institutional, interpersonal, group or row reciprocities, treating them....under a single rubric for reasons of space" (p. 164).

Gouldner theorized that at many levels of social and psychological analysis, stable interaction patterns emerge based on the consensually agreed upon rights and duties of each participant. However, above and beyond the duties that a person or group may perform for another person or group, "by virtue of the socially standardized roles that they play" and which are "incumbent on all those of a given status simply by virtue of its occupancy...the generalized norm of reciprocity evokes obligatory behavior towards others on the basis of their past behavior" (p. 170)

Gouldner thus theorized the existence of a universal norm of reciprocity. He felt that such a norm served to counterbalance the "egoism" (p. 173) of both parties. While any interaction might break down if one person refused to acknowledge the other person's rights as his own duties, this norm thus serves to make it right and proper that the second person should reciprocate the services of the first and compels him to do so. Gouldner also felt that this norm actually "mobilizes egoistic motivations and channels them into the maintenance of the social system" and is buttressed by them. For among the sanctions attached to not obeying the norm are "if you want to be helped by others you must help them" (p. 173). Thus, Gouldner pointed out that the norm of reciprocation is buttressed by the egoistic hope of obtaining future fulfillment of one's rights.

Gouldner went on to state the norm's more specific consequences. He felt the norm's "minimal demands" (p. 171) were that "people should help those who have helped them" and "people should not injure those who have helped

them". (p. 171) Gouldner could not theorize an exact exchange of behavior but rather theorized that a "rough equivalence" (p. 171) of the amount of harm or help was to be returned in each case.

Gouldner's theory, albeit of a general nature, lent itself readily as a theoretical framework for the understanding of socially constructive behavior, and Gouldner himself discussed his theory with reference to such constructive acts. Furthermore, Gouldner's theory seemed to readily lend itself to hypotheses concerning the relationship between constructive responses to antecedent constructive acts at several different levels of analysis. On one level - the empirical analysis of overt behavior - it suggested the hypothesis that reciprocation of constructive acts could be demonstrated in a scientific study; on another level, that of explaining the dynamics motivating the observable empirical reciprocity relationships, it suggested the hypothesis that a norm of reciprocity was indeed behind the behavioral reciprocation that must be observed.

Gouldner's theory attracted the attention of psychologists who sought to test some of the hypotheses derived from it. As described below, the data derived from these tests seemed to support the theory, while the theory seemed to be a promising explanatory framework for the predictability that emerged in these studies. However, while there was experimental support for some of the theoretically derived hypotheses mentioned previously, the data seemed, upon my careful review, to be open to alternative theoretical interpretations. Moreover, these alternative interpretations of the observed data, if valid, would greatly limit the generalizability of conclusions based on the data, far more than was implied by a simple statement of reciprocity. This possibility, in turn, led us to speculate that conclusions based on this data might not be applicable to naturalistic social context and that the question of the degree of linear reciprocity or pre-

dictability of behavior under naturalistic social conditions remained to be put to an empirical test. A detailed presentation and critical review of the research into constructive behavior based on reciprocity theory appears below.

Review of literature relevant to the reciprocity theory of constructive behavior

Goranson et al (1963) addressed themselves to the hypothesis that subjects operate as though "people should help those who help them" (p. 228). In this study, subjects were led to believe they were taking part in a test of supervisory ability. The subjects were asked to work on a tedious task, but half of them were helped by a confederate-peer. Following this, they were told the confederate was to supervise their work. The researchers found that the subjects who had received prior help, worked harder for the confederate-supervisor than did subjects who did not receive prior help. While these results seemed to support reciprocity theory on an empirical level, the authors themselves felt that differences between the two groups may not have necessarily been due to the operation of a norm of reciprocity. Two alternative interpretations of the data were proposed. One hypothesis was that instead of the positive norm of reciprocity being involved, a negative norm may have been involved, that is, the non-help group may have felt angered at the lack of help and reciprocated thus. The other hypothesis was that, rather than there being a norm of positive reciprocity motivating the resultant behavior, the behavior might have been motivated by a heightened "norm of social responsibility" (p. 228), which made the subjects feel that they should help not only the person who helped them, but instead, that they should help anyone who is dependent on them, that is, that they should be "socially responsible" in their behavior. Berkowitz and Daniels (1964) had previously demonstrated that indeed, subjects would work harder for a person, simply because they are told success

depends on their efforts, even without being previously helped by this person.

To see if this theoretical explanation accounted for the observed data, they set up a study similar to the one just described. The study, (Goranson et al (1958) had several different conditions. The help offered was either "voluntary", "compelled by the experimenter", or was suggested by the experimenter to the confederate in the subject's presence but the confederate refused to cooperate. Following the study, a questionnaire was administered asking the subjects "to what extent would most people have expected you to work hard to help your supervisor". Results showed that subjects who received voluntary help felt "more expected to help than subjects receiving compulsory help or subjects who had dealt with a confederate who refused to help". The latter two groups answered the question similarly. With regard to productivity increases, the group that received voluntary help worked harder than the compulsory help group, which in turn worked harder than the refused help group. These results suggested the reciprocity norm was at work here and was stronger than the "responsibility norm".

Other groups of subjects underwent the same conditions described above, but had to help a different confederate-peer from the one who had or had not previously helped them. Here results indicated a moderately high level of productivity with no differences attributed to the type of help previously obtained from the first confederate-peer. These findings suggested that the responsibility norm evoked by help previously received also could have been operating, as evidenced by the subject's willingness to maintain a moderately high level of productivity even though they were working for a new confederate-supervisor who had not helped them previously at all.

The experimenters concluded that both hypothesized norms were at work in their studies. Thus, the results of their studies seemed not only to support reciprocity theory on the empirical level by showing that people would return help offered, but also provide evidence that a norm of reciprocity indeed was one factor which accounted for these findings, and that the findings were not just the product of a norm of responsibility.

However, while this study took into account some alternate theoretical interpretations of its findings, we felt that it did not take some important alternate interpretations into account, which could have drastically different implications for the prediction whether or not reciprocity of constructive acts would occur in real life.

Pruitt's (1963) study also provides support for reciprocity theory on both empirical and theoretical levels. In his study, "a two person, mixed motive game was employed, technically speaking an expanded decomposed prisoner's dilemma game" (p. 144). The procedure was introduced to the participants as "analogous to real life situations where people exchange favors" (p. 145). Money was to be exchanged between pairs of participants using the experimenter as an intermediary. The participants were told they could keep 5% of the monies they had accumulated at the study's end. Pruitt's data supported reciprocity theory on an empirical level. He reported that "more reward was provided to the other person when he had previously sent out 80% of one dollar than when he had sent out 20% of one dollar." Moreover Pruitt evaluated several different alternate theories in regard to their explanatory power in accounting for his data. Based on questionnaire results and other data he concluded that although his subjects' behavior seemed to have been motivated by the desire to reciprocate, the "process mediating reciprocity" seemed more

unclear. Pruitt said that Gouldner's hypothesis that people reciprocated benefits to elicit even further benefits, could not be supported by his data. He felt that while the norm of reciprocity might account for the data, the data could also be accounted for by theorizing that people who "give" are better liked and therefore more is given to them. Or, alternately, Pruitt suggested that while reciprocity did not seem to be used to encourage giving by the other, it did seem to be a response to people who on the basis of past experience, could be expected to be givers.

To sum up, the research reported thus far concluded that evidence did demonstrate that people did reciprocate benefits, at least in the laboratory. Moreover, people did think in terms of reciprocating what was given to them and did seem to feel expected to reciprocate.

However, the question of whether it was simply a norm of reciprocity that motivated people to reciprocate remained somewhat unanswered. For reasons to be discussed later, it seemed to be an open question as to how powerful and relevant the norm of reciprocity was for real life behavior and whether reciprocity behavior would be as clearly expressed in a naturalistic social context as it was in these studies. Specifically, we reasoned that the contextual factors, to be discussed later, which were built into the previous design may have been responsible for the obtained results. These points also have relevance for the study of aggressive behavior where there had been few tests of alternate theoretical interpretations of the obtained data and similar questions might be raised regarding the limitations of the findings. We will return to this point and its relevance for studies of aggressive and constructive behavior in a later section. However, let us now review the theory and research on aggressive behavior.

A Theoretical View of Aggressive Behavior

Most of the hypotheses tested and most of the research done to explore the relationship between aggressive behavior and antecedent events and to gain an understanding thereof, to date, had stemmed from two main theoretical approaches. The first was the theory of "frustration aggression" proposed by Dollard et al (1939) and a second was the theory of reciprocity, proposed by Gouldner (1960). We will now look at each of these theories in turn.

Until recently, Dollard and Miller's theory that "the occurrence of aggressive behavior always presupposes the existence of frustration and contrary-wise that the existence of frustration always leads to some form of aggression" (Dollard & Miller, 1939, p. 1) had dominated psychologists' thinking in this area. Dollard and Miller additionally hypothesized more specific relationships between frustration and the aggressive behavior that was hypothesized to follow from it. They proposed that the strength of tendencies leading to aggressive behavior should vary with "the strength of the instigation to a frustrated response....and the number of frustrated response sequences" (Dollard & Miller, 1939, p. 13). They also hypothesized that aggressive responses that might normally be made following a frustration, would be less likely to occur if punishment was anticipated for them. Substitute delayed or displaced responses will occur instead.

Dollard and Miller marshalled a great deal of anecdotal evidence in support of their hypothesis. They and their colleagues also carried out a series of laboratory studies which likewise seemed to provide support for the hypothesis previously mentioned. However, more recently, many workers in this area seemed to have become dissatisfied with the frustration aggression hypothesis as originally stated (Mussen et al 1975).

They have pointed out that the link between frustration and aggression is not nearly as strong as was implied in the original theoretical formulation, and some writers have questioned whether frustration, without harmful intent on the part of the frustrator, will produce an aggressive response (Epstein & Taylor, 1967).

In the last sixteen years a rival explanatory framework of how aggression relates to antecedent events came to the fore, perhaps in part due to the aforementioned misgivings regarding the frustration aggression hypothesis. Arnold Buss must be credited with providing real impetus for this new look in aggression theorizing. His analysis (1961) of previous aggression studies led him to conclude that "the antecedent event most likely to elicit aggression is attack" (Buss, 1961, p. 33), not necessarily frustration. He also suggested a shock apparatus which he thought might be useful to test the hypothesis based on the notion that attack comprises the crucial antecedent event in eliciting aggression.

Roughly around the time Buss' book went into print, Gouldner (1960) proposed his reciprocity theory. As will be recalled, Gouldner's theory was meant to be of a highly general nature; however, as we have seen, he did suggest its application to constructive behavior. Moreover, in passing, he did also mention the possibility of its application to aggressive behavior. This possibility seemed to have been seized upon by present workers in the field as a way of conceptualizing the relationship between antecedent aggressive attack and antecedent and subsequent aggressive behavior. As such, reciprocity theory presented researchers with an alternate way of explaining the occurrence of aggressive behavior.

Gouldner, in a passing comment, proposed the existence of a "negative norm of reciprocity" (Gouldner, 1960, p. 172). He invoked this norm in the process of pointing out that in real life, he believed there

to be a reciprocation of aggressive acts with aggression. He spoke of this reciprocation being of "rough equivalence" (Gouldner, 1960, p. 171), rather than necessarily an eye for an eye and a tooth for a tooth. Inherent in this passing reference was the hypothesis that it can be shown empirically that aggressive acts are reciprocated with aggressive acts by the average person. Also inherent in Gouldner's statement was the hypothesis that a universal negative norm of reciprocity was responsible for this hypothetical empirical relationship. Gouldner himself never clarified exactly what he felt the "negative norm" was, so we could only conjecture what processes he felt underlay the reciprocation process. However, despite the ambiguous nature of this aspect of his theory, it served to stimulate a considerable amount of research.

We will now look at research generated by hypotheses derived from reciprocity theory as well as research which, though not directly generated from this theory, can be said to support the theory. The body of this data seemed to previous researchers, to not only provide evidence supporting the existence and importance of the negative norm of reciprocity, but moreover indicated that there is a very strong linear reciprocal relationship between antecedent aggression and subsequent aggressive responses, thus giving hope to the idea that aggressive behavior might indeed be highly predictable.* However, our critical review of this literature on aggression suggested to us that there are several possible alternate theoretical interpretations of the body of data supporting reciprocity theory and its relation to both aggressive and constructive behavior. These different interpretations of why these empirical findings have occurred would have drastically different implications for the degree to which one could feel comfortable in generalizing from these empirical findings to naturalistic social

* e.g. a linear function of instigation magnitude

contexts. A detailed presentation of previous research together with our critical review of these studies appears below.

Review of literature relevant to reciprocity theory and aggressive behavior

While earlier research (Sears, 1939; Doob, Sears & Miller, 1939) generated data which could be interpreted as supporting reciprocity theory, the antecedent stimuli in those cases was frustration rather than aggressive attack. As such, Taylor's study is one of the first cited, as clearly showing a reciprocal relationship between antecedent aggression and subsequent aggression.

Taylor (1967) carried out one of the first studies specifically designed to investigate the relationship of antecedent aggression to subsequent aggressive responses. He was obviously influenced by Buss, and used a version of Buss' apparatus as part of his methodology. While Taylor did not mention reciprocity theory in his paper, in the minds of subsequent researchers, his data was interpreted as supporting the hypothesis of reciprocity of aggression.

Taylor's study was based on Buss' observation that "the antecedent event most likely to elicit aggression is attack" (Buss, 1961, p. 38). Taylor wanted to study physical aggressive responses to a physically aggressive stimulus as a function of the level of stimulus aggression and personality of the subject. Shock was used as the physically aggressive instrument. Buss had previously (1961) developed a technique for measuring intensities of psychological aggressive inclinations by having the subject shock another person. Taylor in turn felt that this constituted "the answer to manipulating physical attack in the laboratory. Besides allowing the subject the opportunity of shocking another person, that

person can be allowed to shock the subject back" (Taylor, 1967, p. 298). Using this technique, Taylor set up a competitive reaction time situation between a given subject and what the subject thought was another peer but who in reality was a confederate. Subjects were told that if they were quicker than the "other subject", they could shock him at any level of intensity; they were told that their opponent had the same prerogative. In actuality there was no opponent; his "responses" were in reality pre-programmed by the experimenter.

Subjects were classified based on a self-rating inventory into over-controlled, under-controlled and controlled subjects. All subjects were male undergraduates. Different levels of shock provocation were used with subjects in a random order. For any given shock setting for their opponent, the mean response was obtained for each of the different subject groups.

Taylor found that the higher the level of shock the "opponent" selected, the higher the mean level of shock the subject gave to the "opponent" when he lost. He also found that there were significant differences in the mean magnitude of shocks delivered by the different groups of subjects. Subjects who were considered having under-controlled hostility on the basis of their self-report responses to a questionnaire, tended to give higher mean levels of shock to any given stimulus, than the over-controlled subjects did. He also found that the subjects tended to increase their aggression at a less rapid rate than did their opponent. That is, a one point increase in their "opponent's" shock level was matched with much less than a one point increase in the shock they delivered to their "opponent". The under-controlled group came closest to matching their "opponent" on a one for one basis and generally showed

a positively accelerated curve of mean responses to a linear increase in the "opponent's" aggression . The over-controlled group showed a negatively accelerated curve of mean responses as a function of provocation and showed the least change of responses to their "opponent's" behavior. Taylor's finding that subjects seemed to reciprocate increments of harm received or anticipated, with increments of harm delivered, was replicated by Taylor and his colleagues in other studies using similar methodology (Shortell & Taylor, 1970; Macci, et al., 1971; Hendrick & Taylor, 1973).

Similar data was also obtained by Berkowitz and his colleagues (Berkowitz, 1962, 1964, 1967), using a somewhat different set of procedures. However, as Helm (1972) pointed out, Berkowitz likewise did not interpret these findings within the framework of reciprocity theory and moreover did not focus on them. The procedure used in Berkowitz's studies involved having subjects and confederate-peers write essays and having them evaluate each other's essays by delivering one to seven shocks to the other writer. The confederate went first. Contained in the data was once more the finding that subjects returned more shocks than they were shocked, and that for every unit change in the amount of shock delivered, there was less than a unit change in the amount of shock received.

Helm, Bonoma & Tedeschi, (1972), were the first to review all these studies and the data obtained, and pointed out their consistency with the negative norm of reciprocity. They then made their own experimental tests of the reciprocity hypothesis using a third experimental procedure.

In their procedure unlike the previous procedures, participants were told that while they could shock the other person, they had the option not to do so. The researchers pointed out that one alternate theoretical interpretation of the previous data was that rather than a

negative norm of reciprocity being responsible for the obtained results, perhaps the demand characteristics of those studies were. (Orne, 1960).

To remedy this, they then offered subjects the option of not shocking back, and pretended to offer the confederates the same option. In the study itself, subjects were asked to estimate the probability that the other "subject" (confederate) would shock them on a given trial. Out of ten trials a given subject got shocked either one, five or nine times. Subjects and confederates then reversed roles; the subject now being the "operator" and the confederate now being the "estimator". Male undergraduates were used as subjects with female experimenters being employed "to decrease subjects' trepidation and subsequent refusal to participate in an experiment employing electric shock" (Helm et al, 1972, p. 91). Helm's results also indicated that the frequency with which the confederate delivered shocks to the subjects produced the expected effect on the subjects' frequency of counter-aggression. Further results indicated that subjects delivered more shocks than they received when the frequency of shock received was low, and less shock than they received when the frequency of shock was high. Helm et al pointed out that the Berkowitz and Taylor data likewise showed similar results. They suggested that this finding could be accounted for by hypothesizing that subjects avoid making extreme responses even when these are called for by the stimulus. They felt this held true "whether subjects were making perceptual judgments, attitudinal judgments, etc." (Helm et al, 1972, p. 93). Thus, Helm et al not only provided data lending further support to reciprocity theory, but the suggestion also implied that subjects would also try to keep their responses within the limits of what they thought were not extreme.

Helm et al, in their discussion, concluded that results supported

the notion that "the reciprocity norm applies not only to the magnitude of harm but also to the frequency of harm" (Helm et al, 1972), in the case where the initial aggression was non-provoked or non-contingent. Despite this conclusion, we noticed that while these researchers maintained that their results supported the notion of a reciprocity norm, they provided no data bearing directly on the question of whether this norm was uppermost in the subjects' minds when they responded as they did.

While Helm et al, called attention to the consistent reciprocal relationship found in previous research, and extended these findings, more recently Harshbarger (1974) pointed out that there were specific quantitative consistencies in the data too. In all these studies, he pointed out there was a very strong consistent linear relationship between antecedent aggression and subsequent aggressive responses, and between antecedent constructive behavior and subsequent constructive responses. The strengths of these relationships suggested to him that aggressive and constructive behavior might be highly predictable from antecedent aggressive and constructive acts. Specifically, in his re-analysis of Taylor's study (1967) he found a correlation of .97 to .99 between mean levels of aggressive response, and different levels of aggressive shock stimulation. Moreover, Harshbarger determined that the slopes of lines relating the variables in this study were about .45, with the exception of Taylor's under-controlled hostility group, where the slope was about .63. When Harshbarger (1974) similarly re-analyzed the results of Helm's (1972) research, he came up with highly consistent results.

Based on such promising findings, as well as the highly consistent reciprocity that had been demonstrated in studies of constructive behavior as well, Harshbarger undertook to further extend these findings by

"quantifying these (reciprocal) relationships, previously expressed only by inequalities, by developing a regression equation for predicting the aggressiveness of a response from the aggressiveness of prior instigating behavior by another person" (Harshbarger, 1974, p. 1 - 2), using self-report data as a first step in this direction. He also included constructive behavior in this quantitative analysis. Moreover, unlike previous researchers, he conceptualized constructive and aggressive behavior as being opposite poles along a continuum, and studied them in this light.

In this unpublished study by Harshbarger (1974), college students were presented with descriptions of a number of actions that were relatively unambiguous such as "to shake somebody's hand"; "to punch somebody in the belly" (Harshbarger, 1974, p. 2). Each student was to judge each of these descriptions as to its aggressiveness or its constructiveness along a nine point equal appearing interval scale discussed by Edwards (1957). One pole of this scale was labelled: aggressive, painful, antagonistic, harmful, injurious, obstinate, destructive. The other pole was labelled: constructive, pleasant, helpful, creative, productive, friendly, and loving. Scale values went from extremely aggressive at one end of the scale to extremely constructive at the other. The other scale values were: strong, moderate, mild, and neutral. For each action described, the experimenter thus obtained a rating from one to nine, on the scale, from each student judge. The numbers for each action were summed and averaged, giving a mean value for each.

Following this procedure, multiple choice, self-report inventories and questionnaires were set up using these actions. To construct each multiple choice question, one of the actions was used as a hypothetical stimulus situation. Likewise, the multiple choices of possible hypothetical responses to this situation were selected from the pool of

"action" items so as to represent divergent positions along the scale. Harshbarger (1974) gave an illustration of one of these multiple choice items as follows:

If one were to spread malicious gossip about you what would you most likely do? (stimulus situation) (possible response alternative selected from pool of rated items as above)

- a. Break his nose.
- b. Yell at him.
- c. Ask "why did you do that?"
- d. Tell an intimate embarrassing story about him in front of him.
- e. Tell him that he disgusts you.
- f. Call him a stupid jerk.
- g. Plan a party in his presence but not invite him to it.

Once the creation of several forms of these multiple choice inventories had been accomplished, the forms were administered to new groups of college students. Each student was asked to pick out the response in each stimulus situation that he thought he (self-report analysis) or the average person (questionnaire analysis) might make. Since each of the stimulus items had been previously rated as to its aggressiveness and constructiveness and since each of the multiple response alternatives had likewise received a rating on the same scale, it was now possible to calculate for each stimulus action the mean magnitude of response to it along the aggressive, constructive dimension, by averaging the response of the students to each item. Once this had been done, a correlation and regression line could be obtained relating the magnitude of the stimulus action to the mean magnitude of the subjects' response to them.

Results of this procedure yielded very strong, highly consistent correlations of about .8 to .9 depending on the form of inventory used. A consistent slope of about .4 was also calculated for this data. Moreover, like previous research, Harshbarger found the subjects' responses avoided extreme values.

Harshbarger's results thus suggested that aggressive-constructive behavior was a consistent, highly predictable linear quantitatively stable function of previous aggressive-constructive instigation. However, Harshbarger noted that his study had several limitations, particularly the fact that the questionnaire method merely taps what the subjects said that they would do in a situation and not what they actually would do. Harshbarger suggested the need to replicate these findings in a naturalistic, social, interpersonal, interaction situation, and this was the basis of the present research.

Do People Reciprocate?---The Limitations of Past Research

As we have seen, data from recent research showed a high correlation between antecedent, aggressive or constructive acts and subsequent aggressive or constructive responses. The impressive consistency of this data, together with our own "armchair" observations of human behavior, might easily have convinced at this point that reciprocal responses to unprovoked aggressive or constructive acts are characteristic of the naturalistic, social interactions in every day life and moreover, that the responses to such unprovoked instigations are highly linearly predictable.

On the basis of the following review of the literature however, our conclusion was that this is not necessarily the case and that it would be overly simplistic to make such a generalization. There were three reasons for our opinion. First, very recently, there have been a few studies and articles pointing out weaknesses in the methodology of previous studies which by virtue of these weaknesses allow for alternate interpretations of the results. While some of these methodological weaknesses have been pointed out, there have been little or no hypotheses put forth in the literature with regard to exactly how these methodological weaknesses alter the quantitative, and more importantly from the standpoint of being able

to demonstrate the predictability of aggressive and constructive behavior, the qualitative findings of reciprocity obtained in the studies and/or the generalizations that might be made from them. However, we felt that these weaknesses had drastic implications for the findings previously mentioned and we decided to hypothesize what these implications were.

The second reason for our opinion that reciprocity was not necessarily characteristic of behavior in a naturalistic setting was that we concluded that there were several alternate possibilities for the statistical treatment of the response variable. We hypothesized that these alternate statistical treatments might importantly affect the interpretations of the resulting data.

The third reason for our opinion was the fact that since many factors had been shown to exert a facilitating or inhibiting effect on aggressive behavior, if these factors were differentially present in the previous laboratory contexts opposed to real life contexts (as we came to believe they were), then the possibility existed for this reason too, that the empirical reciprocity relationship shown to exist in laboratory studies, between antecedent aggressive and constructive acts and responses to them, might be drastically different in real life contexts.

We will now detail below the observations we made and the data and theory which led us to postulate the foregoing limitations of previous research findings.

The first two sections of the review deal with observations and research which raised the issue in our minds of what effect certain methodological features of previous laboratory research had in producing the empirical reciprocity behavior observed. The third and final section concerns some of the research and theory that provoked us to ask about the effect that that factors, differentially present in real life and laboratory contexts,

might have on the empirical reciprocity relationships obtained.

Within each section, we have indicated the basis for some of the hypotheses formulated by us concerning the limitations of previous research and the type of reciprocity to be expected in real life. The present study was designed to test and/or evaluate the hypotheses and to get a clearer idea of the kind of reciprocity behavior, if any, to be expected in real life contexts.

Section 1: Observations concerning methodological features of past research and suggestive evidence as to how they may limit the generalizability of findings concerning empirical reciprocity.

The first methodological problem we discovered in earlier studies was that a number of studies of behavioral reciprocity had lacked self report information from the subjects, as to what they thought happened to them, what they thought about, and what they did about it. Studies often assumed that what the experimenter thought was an aggressive stimulus indeed was one; what the experimenter thought was a constructive stimulus was one too. Due to the lack of self report measures, we do not know whether subjects accepted the experimenter's definitions of the situations. If they did not, and had the subjects been given the opportunity to inform the experimenter about this, the conclusions drawn about their reciprocity behavior may have been very different, as would the conclusions drawn about the generalizability of this behavior.

For example, while one tended to assume the shock given or received constitutes aggressive stimulus, it could be hypothesized that subjects--in such studies as Taylor's (1967)--may have seen it simply as a device to produce better learning. To the extent that this was so, it would raise the possibility that subjects may have seen the shocks they had received as being a constructive stimulus, and the shocks they delivered back were likewise being constructive in nature. If this were true, it

would mean that while Taylor's data indicated that constructive acts are reciprocated, it would also mean that Taylor had not really provided any data bearing on the reciprocation of aggressive acts, thus weakening the argument that aggression is reciprocated and strengthening the argument that constructiveness is reciprocated.

Baron et al, first called this problem to our attention. Baron & Eggleston (1972), using Buss' (1961) procedure, demonstrated that the magnitude of shock delivered by "teachers" to "learners", who were asked to learn word lists, was significantly related to the subjects' reported desire to help the "learner". Since Taylor's research as well as Berkowitz's were based on similar sets of instructions, one could argue that the subjects in these studies were not only motivated by desires to harm the other "students" (confederates), but may also have been motivated by the desire to help them. Further, they may have seen the behavior of these opponents as helpful and not harmful and reciprocated accordingly. To the extent that this was true, it would severely undermine the evidence for the negative norm of reciprocity and the empirical reciprocity of aggression as well.

However, Baron & Eggleston (1972) felt that the research to date suggested that both motives seem to be at work in both studies. Based on the above, we conjectured that while the aggressive acts were probably accurately perceived and acted on, the alternate interpretation that they were perceived as helpful acts and reacted to as such could not be completely ruled out. As such, this might greatly affect whether or not the same type of responses may be observed in a more naturalistic, social context where one party is not trying to teach the other party the word list, and aggressive stimuli are not readily misinterpreted as being helpful. By designing a more naturalistic study and by collecting self report data, we hoped to

see what sort of subjective experiences underlie empirical reciprocity behavior of subjects in a more realistic life setting and thus avoid the mistakes some previous authors had made of automatically inferring subjects' perceptions of the situation from observations of their behavior and possibly making other wrong inferences as a consequence.

A second methodological problem with earlier studies concerned whether or not the demand characteristics of the study (Orne 1960) communicated to the subjects the responses that were hypothetically expected of them. If this were the case, then the resultant reciprocity behavior might or might not be observed in a naturalistic social context where these demand characteristics are not operating. Influence of demand characteristics in this research was raised by Schuck & Pisor (1974), who used the standard Berkowitz design involving essay ratings, except that they added a simultaneous group of subjects who were disconnected from the electrodes at the point they were to receive the other students' shocks. They were then told how many shocks they would have received had the electrodes been connected to them to receive them. These subjects were told that they were controls in the study. They were asked to rate how they felt on several mood scales. Following this, a new experimenter took over the procedure and had them evaluate the other "students" (confederates) essays via electric shocks. Results indicated that the simulating subjects' aggressive responses to different levels of prior provocations were indistinguishable from those of the real subjects. With regard to the mood scales, while both groups showed similar changes on these scales as a function of a level of provocation, the simulating subjects reported themselves to be significantly more angry than the real subjects for any given shock frequency. The researchers felt however these differences could easily be erased by increased shock

intensity. They concluded that real and simulating subjects behaved alike.

It was our opinion that the questions that these authors raised would probably be equally applicable to the other research previously cited. As such, there were now three possible theoretical alternate interpretations to account for the data they obtained. Subjects may have been operating on the basis of their feelings as Taylor implied. They may have been operating on the basis of internalized norms that tell them what they should do when they are shocked, or as Helm has implied, they may have been operating on the basis of subtle "demands" issued by the experimenter independent of such norms. If, in fact, the latter is primarily the case, laboratory findings then have become the figment of the experimenter's imagination, and the true relationship between aggressive stimuli and responses in real life must be considered totally untested. Unfortunately, there was no clear answer as to which of these theoretical interpretations was correct. However, by designing a naturalistic study and collecting self report data on why subjects really behaved the way they did, we would be able to see how subjects really behave in real life settings and also, would avoid the ambiguities in the interpretation of why subjects behaved the way they did, which we encountered in the research just cited.

A third methodological issue was hinted at in Helm's paper (Helm, 1972), in which he had criticized early studies as restricting the range of subjects' responses to aggressive provocations by requiring subjects to make some aggressive responses to the aggressive provocation of the confederate. Helm's design allowed subjects the option of not responding and likewise made subjects feel that the confederate also had that option.

However, neither Helm nor previous researchers allowed the subject to make responses that were constructive in nature to the degree that subjects might have wished to make those responses, but were prevented from doing so. Resultant data would give an artificial impression of the way subjects behaved following such provocations and the results of such studies would surely not be generalizable in real life situations where the subject has other response options. This point is equally relevant to the study of constructive behavior. Just as the early research on the frustration-aggression hypothesis ignored non-aggressive responses to frustration, artificially boosting its apparent predictable power, the same might be said for the recent laboratory work on aggression and constructiveness.

Some evidence that subjects may in fact, make unaggressive or even constructive responses to aggressive stimuli was suggested by the research of Nacci et al (1971) and McClelland and Apicella (1945).

Nacci et al, argued that implicit in the positive and negative norms of reciprocity were the demands that "one should not help those who harm you" and "one should not harm those who help you" (Nacci, 1971, p. 263). The researchers used Helm's design (Helm, 1972) where subjects were initially supposed to estimate the probability that the other "student" confederate would act on them by shocking them. The confederate of course was programmed to shock different subjects with different shock frequencies. However, unlike Helm's study, when roles were reversed, the confederate was now to estimate the frequency of benefits he would received from a given subject. And each subject was to give the confed-

erate benefits as he so chose. Another variation involved the subject's first being the "estimator" of benefits delivered and then having to be the "operator" giving shocks.

Results showed that subjects who were harmed provided as many benefits to the harm doer as those who were not harmed. Further, subjects who were benefited displayed as strong a tendency to administer harm as those who were not benefited. The researchers noted that these findings were contrary to their interpretation of Gouldner's hypothesis that this would not occur and they suggested that the "norms of reciprocity must be limited to statements of exchange only within positive and negative modes of reinforcement" (Nacci, 1971, p. 268). What these findings suggested to us was that if subjects did indeed feel free to be helpful to aggressors and if they likewise felt free to be harmful to people who have benefited them, they might in fact choose to enact such behaviors. Accordingly, the behavior of the subjects following provocations would be far less predictable than the previous cited research would lead one to believe, provided, of course that subjects were allowed the freedom to make whatever responses they deemed appropriate.

McClelland and Apicella (1945) did a study pointing to a similar problem in the data collected bearing on the frustration-aggression hypothesis. They pointed out that most of the research based on this hypothesis had given "very little idea of the total range, either in content or emotional tone of possible responses and still less of the relative importance of the various types of response within that range" (McClelland & Apicella, 1945, p. 376).

They tried to "create in the laboratory a.....situation in which reactions of normal individuals could be recorded as they occur and in which social restraint could be reduced to the minimum so as to allow for the widest, possible responses" (McClelland & Apicella, 1945, p. 377). Two groups of subjects were run, differing in the degree of frustration (insult) the peer experimenter heaped on them for poor performance on a task. An "uninhibited atmosphere" (McClelland & Apicella, 1945, p. 379) was maintained by virtue of the experimenter peers' uninhibited insults, the fact that the subject knew of the peer before the study, and the fact that being a peer he carried minimal ability to physically or psychologically punish the subjects for any actions.

The researchers made up a classifications scheme to classify the observed responses. Responses were grouped together "if they served the same end in the subject's struggle to adjust to the frustration" (McClelland & Apicella, 1945). A given response was only to be classified in one place in this scheme, although the authors noted theoretically, a response could serve several functions. In practice, they did not find the decision, as to which end was predominant, a hard one.

Results of this procedure showed that 14% of the responses of what we would call moderate attack were "anger"; 32.08% were "anger" under severe attack. These were the most common responses percentage-wise and obviously there were increased anger responses as attack increased. "Attack aggressive" responses showed a similar pattern but with much lower percentages. Under moderate attack, 6% of the responses were "aggressive attacks" against the task and none comprised "aggressive attack" against the experimenter. Under severe attack, 9.05% of the responses constituted "aggressive attacks" against the task and 9.0% constituted "aggressive attacks" against the experimenter.

At first glance, the increased percentages of "anger" and "aggressive attack" as a function of increased provocation would seem compatible with the recent data from shock studies and seemed to support the reciprocity hypothesis. However, examination of all responses that could be considered aggressive in the broadest sense versus all responses made, showed that even in the severe provocation condition they only made up about 40% of all responses emitted by subjects. Of the other responses emitted by subjects in the severe provocation condition, 12.98% involved subjects' attempts to get the experimenter to let them quit the task. 17.55% represented either attempts by subjects to rationalize their failures to the experimenter or to accept the experimenter's evaluation of them while isolating their feelings about it or

making fun of their failures. If the narrowed definition of aggression or aggressive attack is utilized, direct aggressive attacks against the experimenter made up a very, very small minority of the responses made by subjects, despite the "uninhibited" atmosphere in which the study was conducted.

In other words, the subjects' responses to what was considered severe provocation were overall, remarkably non-aggressive or neutral in nature. While overall, these responses may have been slightly more aggressive than those obtained following moderate provocation and the change may have been a predictable, linear reciprocal function of increased provocation, the overall response pattern to both provocation levels would be described as neither constructive nor aggressive but basically a neutral one with considerable variability. In view of the fact that despite the "uninhibited" atmosphere, when subjects were given the freedom to respond as they chose, their overall response pattern was so neutral, the possibility arose that in a real life situation where such response freedom is likewise available, subjects might likewise show slight overall reciprocal changes in their behavior as a function of provocation level, while still keeping their behavior basically neutral. As such, while their behavior might be a predictable function of antecedent aggressive acts, it still might not support reciprocity theory's contention that people in real life reciprocate aggression with aggression.

In any event, the above observations pointed out to us the peril of focusing only on aggressive responses in analyzing the data of an aggression study, as well as the peril of restricting the options open

to the subjects. Since the shock studies must be judged weak in this and in other respects, we thus had no way of knowing how consistent, linearly predictable, and reciprocal, subjects' responses to aggressive provocations would be in real life. Moreover, it did not seem unreasonable to infer that the same criticisms may apply to the studies of constructive behavior we have already cited. To get a more realistic and accurate picture of people's responses to all such instigations, it would thus seem necessary and desirable to create a simulated naturalistic setting which would allow subjects a normal amount of behavioral freedom. We hoped to design such a setting in the present research.

Section 2 - Observations concerning how previous methodology used in statistical analysis of empirical reciprocity data may have limited the generalizability of the findings.

The second set of observations which questioned how consistent, predictable and reciprocal responses to aggressive or constructive provocations may be in real life, involved the different possibilities for the statistical treatment of the results of previous studies. These studies previously cited focused on the implications of subjects' mean response to various levels of provocation. While of course, one may do this if one chooses, Guilford (1965) in effect, points out that the correlation statistic obtained through such procedures may be far higher than one obtained by correlating each individual subject's discrete response with the instigation magnitude he received across all subjects. This is because, by using mean values, intersubject variability has been markedly reduced by using the mean value to represent the performance of a number of subjects. Which statistical treatment is most appropriate, of course depends upon the use one wants to make of the results. The point that occurred to us however, was that the predictability of any given subject's behavior might be far less than what is implied by the

results previously reported which are based on the subjects' mean responses to given levels of provocation. By using both types of statistical analysis in our study, we hoped to get a clearer idea of what the impact of these methodological differences on the reported findings might be.

Section 3

Observations concerning factors differentially present in laboratory and naturalistic social contexts suggestive of evidence as to how they limit the generalizability of laboratory reciprocity data.

The third reason we questioned the degree to which responses to aggressive and constructive provocations in real life are consistent, highly predictable and reciprocal in nature, involved the proposition that there may be important mediating factors differentially present in the laboratory context or the real life naturalistic social context. Many factors which display a great deal of variability in their operation in real life have been generally held constant in laboratory studies. For example, some of the factors that have been shown to (or have been theorized to) have an inhibiting effect on aggressive behavior include the degree of threat of retaliation (Baron, 1971), intent of the instigator (Epstein et al, 1967), sex roles (Taylor et al, 1974), anonymity of the instigator (Baron, 1970), etc. Other such factors have also been shown to affect constructive behavior (Krebs 1970).

While factors such as the above may differentially affect the empirical reciprocity of behavior in lab and real life contexts, due to the variability of their operation in real life, they were not made the central focus of this paper. Rather we were more concerned with the possibility that, if factors were found which importantly encourage or inhibit aggressive or constructive acts, and if such factors were to be inherently part of one context but not necessarily part of the other context, this would logically suggest that the generalization of findings in one context cannot be made to the other context. The question of how truly consistent, linearly

predictable and reciprocal people's responses really would be to aggressive and constructive acts, then, would become an empirical one.

Based on the theory and data below, we came to the tentative conclusion that such factors differentially inherent in one context and not the other did indeed exist. As such, we became further intrigued by the question of whether people do really reciprocate in real life, as well as the question of how such contextual factors might operate to alter reciprocity behavior in real life from that observed in the labs. Our study was designed to provide a clearer answer to these questions.

While we will focus on the study of aggressive behavior in the subsequent analysis, much of what will be said is equally applicable to the study of constructive behavior. Rather than be repetitive, I will leave it to the reader to make the necessary inferences.

One factor that clearly seemed to be differentially present, in the two contexts, involves conditions in the laboratory designed to produce "low social restraints" (Taylor, 1967). Most of the laboratory studies we have already cited have involved the use of shock. Taylor, the researcher who pioneered the use of shock in the area of aggression, did not decide to study physical aggression using shock by accident. It seems that he wanted to provide the right atmosphere where subjects would be free to behave the way they felt. That is, an atmosphere of "low social restraints". Taylor said he chose to use physically, aggressive stimuli and responses because he felt that -

"to study aggressive behavior in the laboratory, one must place subjects in a situation which permits them to transgress the powerful social prohibitions against acts of aggression or one in which aggression appears to be justified. A situation that fits this requirement is one in which subjects receive direct, physical aggression, for physical attack encourages physical counter-aggression, viewed as self-defense and sanctioned by society." (Taylor, 1967, p. 297)

Unfortunately, Taylor never theorized the exact nature of these "powerful social inhibitions" nor did he discuss their relationship to reciprocity theory. However, it was obvious that Taylor did not want to study how people might actually respond to an aggressive stimulus but rather he wished to study how people might be inclined to respond if it were not for these "powerful social prohibitions". He felt that by creating an environment conducive to allowing a clear psychological pathway for aggressive inclinations to manifest themselves in behavior, he could study the inclinations better than through "unnecessary links in the chain of inference" (Taylor, 1967, p. 297) such as tests. He felt he had his best chance of succeeding in this through the use of physical aggression as his variable and apparently designed the study to minimize "social prohibitions". Thus, Taylor's statement led us to conjecture that one can see that while his methodology might be a fruitful one, for studying psychological inclinations to physical aggression following a physical provocation, if Taylor were correct in his views on the presence

of reduced social restraints in the shock studies, his methodology would be ill-suited for studying whether or not aggression, particularly verbal social aggression, is really reciprocated under naturalistic social conditions. Despite Taylor's comments, most researchers since Taylor, have used variances of his methodology and have cited his data as supporting reciprocity theory's prediction of behavioral reciprocity.

We not only conjectured that the lab studies provided an atmosphere of "low social restraints", but moreover, based on the reports of those researchers, we felt we could assume that the procedure of the laboratory experiments served to legitimize both the behavior of the instigator and the behavior of the recipient of the aggressive and constructive acts as furthering the experimental tasks. In these studies, a ready justification was provided for the participants' behavior by the experimenter's rationale for the use of the shock board. In addition, we also conjectured that the behavior of the peer-confederate served as a model for the subject's subsequent behavior, which would probably be a most potent influence on subjects who were uncertain what they were to do in this novel situation in the first place.

Milgram (1965) and Larson et al (1972) have provided data suggesting how powerful such subtle and not so subtle influences can be in making subjects behave in ways they normally wouldn't in typical, naturalistic context.

Prior to the Larson research, Milgram (1965) had shown that the orders of an authority figure constituted a highly potent elicitor of aggressive behavior. Larson et al (1972) extended these findings using a Milgram-like apparatus. Like Milgram, they told their subjects that

they were going to be in a study of the role of punishment on learning, thus providing them with the rationale for the use of the shock board. Subjects were led to believe that they were going to be "teachers" and were to punish a "learner" confederate who gave wrong responses. However, unlike Milgram, they did not request the subject to use any given level of shock but rather told the subject to use whatever levels they thought would produce the best learning. They were told that the shock board would deliver slight to severe shocks. They were told that the severe shocks could be severely painful but not permanently harmful. 0 to 120 volts were labelled mild, 121 to 240 labelled moderate, 241 to 390 were labelled dangerous. Of course, unknown to the subjects, the confederate was not connected to the equipment and behaved throughout as though he had not improved.

When college men were described this study and asked to imagine what shock levels they would have used if they were the subjects, results showed that they said the maximum voltage they would administer to the subjects was on the average 147 volts. Yet male subjects in the situation delivered a mean maximal voltage of 187 volts. Having the opportunity to observe a model, they raised the mean maximal voltage to 267 volts.

Based on such results, Larson et al (1972) concluded that with regard to such shock designs, "when a person is actually in the situation with no ready escape, the individual is under pressure from situational factors" and since "certain expectations of permissible voltage administration might be elicited from the simple fact that the machine is labelled up to 390 volts", therefore for these reasons, "what people say they might do and what they actually do might be highly discrepant because of situational factors that change the individual's behavior" (Larson et al, 1972, p. 294).

Larson et al, did not speculate as to how such situational influences might affect the generalizability of the consistent, predictable, reciprocal relationships found in these laboratory studies. However, it did seem to us that the points those researchers made were applicable both to the shock studies we have already cited as well as the cited research on altruistic behavior.

When we looked at the few non-shock studies of aggression which seemed to show that aggressive responses to antecedent aggression are consistent, linearly predictable and reciprocal in nature, we saw that these studies were open to similar criticism. For example, we have seen that the McClelland et al study (1945) tried to set up "an uninhibited atmosphere" capitalizing on the modeling effect of the experimenter himself. Another naturalistic study by Mosher (1963) which has been cited as supporting reciprocity theory, likewise instructed pairs of boys to try to interfere with each other's performance on a task by distracting each other verbally and angering and frightening their opponents. Thus, not only were these boys sanctioned for their behavior and given justification for it but the boys involved were delinquent youths in reform schools - perhaps the last people who would be likely to be swayed by the "powerful social prohibitions" Taylor discussed in his article.

Aside from the factors we have mentioned which seem to be an inherent part of the laboratory context but are not necessarily inherent in social context, there are likewise factors which we hypothesized to be part of naturalistic social context but which are not necessarily part of the laboratory context previously discussed. As mentioned previously, if in fact such factors were differentially present, they might weaken the generalizability of the reciprocity relationships obtained in naturalistic setting.

The first factor which seemed an inherent part of the naturalistic social context is the "social prohibitions" that Taylor spoke of. An unpublished pilot study by the present author (Nussbaum, 1975), supports the idea that there are norms governing how aggressive or constructive one may be in particular situations. The more one deviates from these standards, the more one's behavior seems to be deemed irrational, abnormal, etc. and apparently the more one is negatively sanctioned for such behavior. As Milgram (1965) and others have pointed out, it is very difficult for anybody to violate these norms without feeling great strain, whether they be experimenters, confederates, or subjects and whether they have or do not have personal justification for their actions. That subjects may indeed be inhibited in responding to a provocation even when they feel justified in doing so, is suggested both by the data of Baron (1971) and Moriarity (1975). Baron has reported data showing that such inhibitions will occur whenever bystanders are present, even if the bystanders have witnessed the antecedent instigation. One may infer from this that even if bystanders are potentially present in a situation the same inhibitions would occur. Moriarity has demonstrated that people are willing to put up with and accept a great deal of obvious abuse and he described the average person as being a "willing victim" (Moriarity, 1975).

Based on the above, we hypothesized that in a naturalistic setting where normal restraints operate, such as the one we intended to design, most subjects contemplating violating norms of acceptable conduct in order to reciprocate an instigation would be severely inhibited in doing so, in proportion to the contemplated magnitude of violation. Accordingly, we expected that subjects in such naturalistic settings would show weaker tendencies to reciprocate (as evidenced by $r < .9$) than those

previously reported in the literature. We further expected that if we designed a naturalistic social setting that had artificially lowered restraints, as strong a degree of reciprocity behavior would be displayed as that found in the previous lab studies.

It also seemed likely that under naturalistic social conditions, reciprocity behavior would be limited by the "acceptable" behavior in that situation. Since the "acceptable" behavior may be very narrowly constrained, we expected to find reciprocity behavior shown only within very narrow limits, as a function of this factor. This is in contrast to the laboratory studies where the range of acceptable behavior has been broadened by the experimenter, through such techniques as presenting subjects with a shock, and urging them to make use of it, or alternately sanctioning the exchange of benefits by subjects, and giving subjects an acceptable rationale for such behavior.

We further supposed that not only were there standards of acceptable aggressive and constructive behavior in naturalistic situations, but there might be in fact, other social norms that exist in our society that directly counter any reciprocity norm. For example, many people in our society believe that one should "be cool", "avoid hassles", "be courteous and tolerant", "not talk to strangers". Further, as Legant and Mates (1973) have pointed out, the New Testament urges people to "turn the other cheek" (p. 244). Such behavioral guidelines and the sanctions that might be attached to them, might effectively serve to inhibit reciprocity behavior in naturalistic social context. These norms and guidelines might also be reinforced by the fact that under certain conditions of aggressive interaction, such responses as outlined above have been

shown to make aggressors like their victims more (Legant (1973), and become less exploitive towards the objects of their aggression (Marwell, 1972).

In addition to the norms thus far mentioned, it is likely that in naturalistic social context, there are also normative standards that dictate the degree of magnitude of reciprocation that is considered appropriate for any given level of instigation and which thus affect reciprocity. Depending on the population sub-group one is studying, these norms might dictate either "no response", "revenge", "overkill". Someone who departs too far from the accepted standards of his reference group might possibly be considered "sick" or "abnormal". We further supposed that someone from one reference group who interacts with a person from a different reference group might be seen by the latter as behaving abnormally, even though his behavior is normal for this group.

On the basis of Harshbarger's analysis of previous research (1974), we supposed that the average college student would feel .45 of the aggression or constructiveness he receives as an appropriate amount to return. If this is the case, extreme divergences from this value would constitute abnormal amounts of reciprocation in the college students' eyes. Further, we supposed that college students in real life would feel

constrained to keep their reciprocation from exceeding the .4 mark in order to avoid being considered deviant. Since the shock studies showed a similar magnitude of reciprocation, we assumed that this particular norm was not greatly counter-acted by situational contacts of the laboratory. On the other hand, we assumed that while students may feel that it is appropriate to reciprocate 40% of what is delivered to them in a naturalistic social context, we should not necessarily find that they will do what is appropriate when this conflicts with the other norms we have mentioned previously regarding the acceptability of constructive and aggressive acts. We therefore expected that in a naturalistic context where normal restraints are operating, such as the one we proposed to establish, subjects would reciprocate less than 40% of the magnitude of aggressiveness or constructiveness received, while in the naturalistic context we established where the normal restraints were counteracted, we would expect approximately 40% of the instigation magnitude to be reciprocated consistent with the findings of previous lab research.

We further supposed that if the aforementioned norms operate as we described in a naturalistic social context, and are not counteracted by situational factors as they may have in the shock studies, then these norms are likely to not only affect the behavior of the recipients of various instigations, but moreover, would affect the behavior of the instigators themselves the way these instigators are perceived by the recipients of their behavior and whether or how strongly the recipients reciprocate. We theorized that when an instigator, in a naturalistic social context, acted in an unprovoked aggressive or constructive way towards a stranger, his behavior was likely to be deemed abnormal in proportion to the magnitude of aggressiveness or constructiveness and the degree to which this behavior violates the norms we have mentioned

previously. We considered it likely and took further steps to determine whether this norm violation will in turn affect the way the recipient sees this behavior and the instigator and deals with him. In turn, this would affect how and whether reciprocity actually will take place in a consistent and linearly predictable manner.

Jones et al (1959) provided data suggesting a relationship between aggressive attack, the perceived maladjustment of the attacker, and retaliatory behavior. While some of their findings were only trends and were not statistically significant, the data suggested that a person under personal attack by a maladjusted individual would tend to retaliate less and like him more than would another person who was not personally on the receiving end of the instigator's attack. From this, we inferred that if people feel threatened by abnormal behavior, they would tend to focus on its abnormality rather than how hostile it is. Moreover, they would display less aggressive behavior than they otherwise would in response to an individual judged to be normal. In fact, as Jones et al (1959) have suggested, they might even feel sympathetic towards the instigator. Further, Berkowitz (1962) has suggested that, for many people, aggressive behavior towards maladjusted individuals is prohibited. Moreover, he suggested that many people do not see the maladjusted individual's behavior as harmful since they discount the attacks and do not take them personally.

We also theorized that the instigator's behavior by virtue of its abnormality would make subjects excessively fearful of reciprocating. The recipients of such acts might decide that the perpetrator is "sick" and his behavior unpredictable by the ordinary laws of intercourse. Alternatively, they might think that "if not acting aggressively towards this person resulted in this behavior, then what will happen if I act

aggressively?" or "I better not give him an excuse" to reciprocate.

We further conjectured that not only may the recipient feel that the instigator has an abnormally high level of reciprocity but moreover, if the instigator has been defined as "sick", as opposed to merely being "tough", the recipient may feel that the instigator's curve of reciprocation is likely to be positively accelerated rather than merely a linear function of the recipient's possible retaliation, especially since the instigator has already violated social norms. In laymen's terms, the recipient may be concerned that the instigator may suddenly whip out a knife or a gun and terminate the interaction violently. As such, the recipient may feel that because he is not physically or psychologically prepared to accept and deal with the costs that are involved in reciprocation, he had better withdraw from the interaction, or attempt to alter it to a neutral level.

A similar set of events were conjectured to occur following excessively constructive acts on the instigator's part. In this case, the reciprocation of constructiveness would be blocked by a desire to avoid feeling obligated to respond in a highly helpful way to a person one distrusts by virtue of the abnormality of his behavior. The subject may think that "this individual is nice to me for doing nothing, and if I feel obligated to him already, think how obligated I will be if I reciprocate further and encourage him". Again, the costs involved in reciprocating may be more than one wants to accept and can deal with, and the subject may thus try to reduce these costs by discouraging the instigation.

Brehm's (1966) reactance theory deals with the reluctance of people to reciprocate when important behavioral freedoms are threatened. These include the freedom to behave normally and stay out of trouble which we

hypothesized are threatened by abnormal instigation intensities, for a variety of reasons. He postulated that to the degree one feels that his personal freedom is threatened, a person would react in such a way as to reestablish his freedom and would thus probably behave differently than reciprocity theory would predict.

Worchel et al (1972) have shown this is especially true when the parties involved feel that they may have to continue an interaction in ways other than those involved in the original instigation, for an extended period of time and need to keep their options open. He also observed that one way this behavioral freedom could be restored was by cognitively altering the instigation by the recipient, so as to make it more neutral and decrease the pressures toward reciprocation. While the shock or benefits used in laboratory studies are not easily defined as non-harmful behavior, in naturalistic context, where much of the harm or help is symbolic or intangible, the possibilities for such cognitive alteration in real life abounds. It has been shown by Berkowitz et al (1972), that such cognitive reinterpretation of events can serve to relieve stress and can alter emotional responses to situations. Moreover, Worchel et al (1972) had shown that such cognitive alterations may be made particularly in situations that arouse maximal reactance to them. Based on this, we supposed that in a naturalistic context, as opposed to a laboratory study, a subject might easily and more readily reinterpret an instigation, either accidentally or on purpose, and then make responses consistent with that reinterpreted situation, thus lowering the empirical reciprocity correlation (r) between instigation and response magnitudes.

In sum then, for all the reasons given above, it seemed probable that in a naturalistic setting, powerful social restraints normally would inhibit people from making "rough equivalent" reciprocal responses to abnormally intense aggressive or constructive acts. As stated, this

inhibition in part would result from the perceived illegitimacy of such abnormally intense instigations and in part would stem from the recipient's unwillingness to violate the same accepted standards of normal and acceptable behavior that the instigator had violated, in order to produce the necessary magnitude of aggressiveness or constructiveness to constitute such a "rough equivalent" in exchange.

On the other hand, no such inhibition of reciprocity of behavior should occur under normal circumstances if the instigation levels involved constitute normal and acceptable behavior in such situations. We felt this was true because under these circumstances the instigations involved would be seen as legitimate and at the same time, the responses to them needed to constitute a "rough equivalence" of magnitude would not be considered deviant.

Based on this thinking, we expected that if we designed a naturalistic setting where normal restraints were allowed to operate, overall, little or no consistent, linear predictable, or reciprocal relationship would be found between a wide range of antecedent aggressive-constructive instigations and aggressiveness-constructiveness of the recipients' responses.* However, if we divided the instigation levels employed into those magnitudes judged to be normal and acceptable and those magnitudes judged to be abnormal, we further expected to find that responses to abnormally intense instigation levels would, for a variety of reasons, show little evidence of being linear predictable reciprocal functions of changes in instigation levels and there would be little evidence of "roughly equivalent" reciprocal responses being made. On the other hand, we expected to find that responses to normal instigations would be a predictable reciprocal function of them and a "rough equivalence" of the instigation level indeed would be returned.

* though a non-linear relationship, not supporting reciprocity theory, might be found as an alternative to no relationship.

It also seemed probable that if we designed a similar naturalistic setting to the one above, but artificially lowered the restraints against abnormal behavior by justifying and legitimizing it, we would find as strong a linear predictable, reciprocal relationship between instigation level changes and reciprocal response changes as those found in previous studies. However, despite this prediction, in view of the McClelland & Apicella (1945) and Harshbarger (1974) studies which seemed to indicate it was questionable whether people will reciprocate aggression with its "rough equivalent" when given the option of making neutral or constructive responses, even when the atmosphere is an "uninhibited" one, we expected that it was possible that people might likewise not return the "rough equivalent" to that received. That is, while their behavior under low restraint conditions would indeed be a reciprocal function of antecedent aggressive or constructive acts, the exact magnitudes of harm or help reciprocated might not constitute a rough equivalence of that received.

The Present Research

Goals to be Achieved

Our literature review thus left us with several hypotheses. One was the possibility that the context under which aggressive and constructive instigations occurred would markedly affect people's tendencies to predictably reciprocate increments or decrements in the amount of aggression or constructiveness received with reciprocal behavioral increments or decrements in aggressiveness-constructiveness. Our second observation was that it was possible that even when such a predictable reciprocal relationship might be observed, it would still be an open question whether a "rough equivalence" of aggressiveness or constructiveness was exchanged, as reciprocity theory predicts it should be.

Based on those and other observations the present study of reciprocity was designed with the following goals in mind:

First and foremost, we wanted to see what kind of reciprocity behavior was likely to occur in a naturalistic setting. Secondly, we wished to gain a better idea as to what effect certain methodological weaknesses may have played in limiting the generalizability of previous laboratory studies, and wished to better assess the effect that correction of these weaknesses might have on the findings of strong empirical reciprocity obtained in those studies. Thirdly, we wished to gather evidence supporting our view that there are factors differentially present in laboratory and naturalistic contexts and that these factors can dramatically affect the amount of empirical reciprocity shown in a naturalistic context. Finally, we wished to be able to explore how these factors affected the subjects by interviewing them about this. Through these interviews, we also wished to see if norms of reciprocity were at work in the naturalistic contexts, and to understand how these norms related to empirical behaviors observed.

Rationale for Design of Present Research

Having theorized that the question of whether people consistently, linearly and predictably reciprocate aggressiveness and constructiveness in real life had not been satisfactorily answered by previous research to date, the next step was to devise a study aimed at providing at least part of the answer.

To empirically determine whether or not reciprocity behavior was characteristic of real life interactions between strangers, a simulated naturalistic encounter was set up between a given subject and given confederate peer stranger. We decided that for ethical reasons and other pragmatic considerations, we would simulate such an encounter rather than send confederates "out into the field" to seek out naive subjects. Within

the limits of these constraints though, the encounter was designed to be as realistic as possible and as representative of real life interactions with strangers as possible. Since the hypothesized normal restraints that prevail in real life were left intact in our design of this condition, and since our goal was to determine how such normal restraints would affect reciprocity behavior, we termed this condition the normal restraint condition.

Included in this condition were certain methodological features not present in previous laboratory research on reciprocity of aggressive and constructive behavior, but which we decided to include to get a more realistic idea of the kind of reciprocity data characteristic of real life as well as to get a better idea of what motivated it.

One such methodological feature included in this design and not included in much previous research, was the provision of not restricting subjects' response options. Subjects were free to behave in any way they normally would. Furthermore, their behaviors were video-taped and whatever behavior they produced was then given a global rating for aggressiveness and constructiveness.

A second methodological feature included in the design was a two-fold statistical analysis of our data. For purposes of comparison with reciprocity data from previous studies, we correlated the mean response to each given instigation across instigations, and used this mean response data correlation as one measure of reciprocity. A second correlation procedure called the discrete response data procedure was also carried out, e.g. a correlation was obtained between the given instigation each subject received and the response each subject made across all subjects in the group. As mentioned earlier, we felt that such an analysis would give a more accurate idea of how strongly subjects' responses related to

antecedent provocation.

A third methodological feature included in our design which had been lacking in many previous studies, was the inclusion of detailed self report procedures in order to get an idea of why subjects did or did not reciprocate, whether or not they really felt harmed or helped, etc.

Since we expected normal restraints operating in real life to inhibit linear predictable reciprocal behavioral changes as a function of instigation level changes in a real life context, as compared to that observed in the laboratory, we made the following predictions:

First, we hypothesized lower Pearson correlation (r) and slope (b) values relating the mean response data to instigation levels than those obtained in previous studies. Second, we predicted that using the discrete response analysis, the Pearson correlation (r) value would be even lower. Third, we predicted that while the normal restraint findings would be inconsistent with findings of previous research where these inhibitions were counteracted, we hypothesized that subjects' self reports should reveal a high correlation between the instigations they received and their desires to return the help or harm received as well as their reported happy or angry affect. Fourth, we predicted that based on the theoretical relationship outlined earlier between normal restraints and abnormal behavior, subjects receiving instigation levels judged abnormal would show little reciprocity behavior while subjects who received normal and acceptable magnitudes of instigations would show a high level of reciprocity (as evidenced by a high Pearson correlation (r)) equal to that obtained in analysis of previous research. Last, we theorized that within the normal restraint condition, the more directly the confederates attempted to draw the subjects into interaction, the higher the pressure to reciprocate. Accordingly, we hypothesized that if we divided the subjects' responses in half, and compared those responses made when the

confederates were directly attempting to provoke the subjects (hereafter referred to as direct response data) to those made once the confederates had finished and/or were not verbally pressuring the subjects to respond, (hereafter referred to as spontaneous response data) we would find stronger reciprocity behavior shown in the direct response data than in the spontaneous response data.

With regard to the effect of the methodological procedure of allowing subjects complete freedom of response, we expected that in view of the influence of normal social restraint, many subjects might make neutral responses or responses opposite to those predicted by reciprocity theory. Accordingly, we expected more intersubject response variability, more neutrality of behavior, and less reciprocation of a "rough equivalence" to that received, than previous lab research would have led us to expect.

A second condition was also included in our study, for control and comparison purposes. This condition, called the low social restraint condition, was to be as similar as possible to the first condition in methodology and procedure, except that as we hypothesized was the case in previous lab studies, attempts would be made to artificially reduce the normal restraints inherent in a real life context. A variety of techniques would be used aimed at legitimizing, justifying and sanctioning almost any and all behavior emitted by the confederates and subjects.

Through the inclusion of this second condition in our design, we could accomplish several objectives. First, we could rule out the possibility that if little or no reciprocity was observed in our normal restraint group, some methodological flaw in our design, or some of the changes in methodology from previous studies that we introduced, was responsible. By holding these factors constant and by varying only the amount of normal restraint present, if we obtained a strong reciprocity

relationship under low restraint conditions and little or none under normal restraint conditions, we could be more confident that it was the normal restraint context that reduced the strength of the reciprocity relationship.

A second objective that could be accomplished by having such a low restraint condition is that, by assuming or demonstrating that once the normal restraint conditions were removed, this condition then approximated those found in previous laboratory studies, we could then compare the results found in each case to see what effect our new methodological procedures may have had on the results we obtained and provide further support for our view that the absence of such procedures in previous research severely limited the findings, even to another low restraint context such as this, by not allowing subjects the option of not reciprocating the "rough equivalence" of that received.

There were two sets of hypotheses we entertained about behavior of subjects under these conditions. The first set concerned the comparison of subjects' behavior under low restraint conditions to their behavioral response to instigation under normal restraint conditions. The second set of hypotheses concerned our expectations that the reciprocity relationship (as evidenced by Pearson's correlation (r) and slope (b) values) obtained under low restraint conditions, should approximate those obtained in previous laboratory research.

With regard to the first set of hypotheses, we expected the low restraint direct data to show stronger evidence favoring reciprocal behavioral changes (higher correlation (r) and slope (b) values) than the normal restraint direct data should, since the normal restraints inhibiting reciprocity were counteracted. In addition, because of the low restraint in this condition, we expected that subjects would be much more prepared to make an otherwise abnormal response to an otherwise abnormal instigation. Accordingly, we

hypothesized that levels of instigations judged abnormal should show the same levels of reciprocity instigation levels judged normal would; this compared to our hypotheses of differential reciprocity to normal and abnormal instigation levels under normal restraint conditions.

With regard to the second set of hypotheses, we expected that since much previous laboratory research under what we hypothesized to be "low restraint conditions" had shown that subjects' behavior was a consistent, reciprocal, predictable linear function of antecedent instigation levels, (as evidenced by the highly stable correlation (r) and slope (b) values of .9 and .4 respectively obtained), we expected that despite procedural differences, our subjects' behavior would evidence similar correlation (r) and slope (b) values.

On the other hand, in view of the fact that in our study subjects were not restricted in their response options as they had been in much previous research, it seemed to be an open question whether, despite the above predictions, subjects would reciprocate with a "rough equivalence" to that received. Rather than making a prediction on this point, we hoped to try to address the question in our discussion based on the empirical findings we obtained.

Lastly, regardless of which condition was being looked at, we did expect the following similarities in the data: we expected that for normal and low restraint conditions as well, the spontaneous data would yield lower correlation (r) and slope (b) values than the direct response data obtained in each case. We also expected in each case that the discrete response correlation r_d for the direct response data would be lower than the mean response analysis correlation r_m . Finally, we expected that under both conditions, a correlation between instigation levels and subjects' self reported affective and motivational states would be obtained which was at least as strong in magnitude as the correlations obtained in previous research under low restraint conditions between instigation levels and subjects' responses to them.

Outline of the Design for the Present Research

As stated, the design called for the creation of normal and low restraint conditions. To accomplish this, a simulated naturalistic encounter was set up between a given subject and a given confederate stranger. We kept the general nature of this simulated naturalistic encounter constant for all subjects. However, prior to the subject's exposure to this encounter, different groups of subjects went through one of two procedures designed to differentially affect their interpretation and their reaction to it. One of these conditions, which we called the "normal social restraint" condition was designed to minimally influence the subject's normal restraints or interpretation of what would follow. The second condition, called the "low social restraint" condition was designed to be similar to this one in many respects except that it was designed to lower restraints giving the participants a justification for expressing their feelings and desires in behavior during the subsequent interaction, etc.. It was also designed to lower the social restraints prohibiting such expression and was designed to lend legitimacy to such expression. At the same time, like in the previous laboratory studies, this procedure was designed to deceive subjects into thinking that their behavior in the subsequent interaction was of interest but was not the crucial focus of the study. To the extent that we succeeded in this regard, we could expect to find behavior in subsequent encounters similar to that obtained in the laboratory studies of behavior.

Male college students were used in this study, in keeping with most previous research and for matters of convenience. For similar reasons, this study was limited to a study of unprovoked instigations.

Regardless of which prior procedure a subject was exposed to, he interacted with an assigned confederate in a small room seated side by side on a couch. In all cases, subjects were given a deceptive rationale for the time they would be alone together, and were invited to fill out some forms and wait for the "experimenter" to return. Both subjects and confederates were video-taped from behind a one-way mirror.

As much as possible, similar confederates and standardized instigations were used, regardless of the prior procedure the subject was exposed to. The social instigations that were employed ranged from extremely constructive to extremely aggressive in nature. Each subject was exposed to one such social instigation and was free to respond as he chose to.

The confederates' instigations and the subjects' subsequent interaction with the confederates were video-taped. The ratings of their behavior on video-tapes on several scales comprised the main independent and dependent variables of this study. Additionally, we had the subjects fill out MAACLs (Mood Affect Adjective Checklists) before and after the instigations took place, had them fill out eight (8) self report scales assessing their affect and motives following the instigation and had them fill out a detailed self report and biographical questionnaire.

With regard to the video ratings, the behaviors of the subjects and that of the confederates were rated by college students judges viewing the video-tapes as to how constructive or aggressive each was. Other groups of judges rated how normal or abnormal the behavior of each was. Still other groups of judges rated the emotional honesty of each person's behavior.

The video-taped interaction was broken up into two periods to obtain a more methodologically clean analysis and to obtain some more detailed

data. Each was separately judged and rated. One period (direct response) was a period where a subject made his immediate direct response to the confederate. The second period (spontaneous response) was a period of time following this first interval, where the subject had the opportunity to instigate further interaction in the absence of further direct instigation by the confederate himself. It should be noted that non-verbal instigations were not dealt with in this manner for reasons which will be apparent later but instead, were included in both sets of data for purposes of analysis.

The ratings of the video-tapes we obtained were subjected to two types of statistical analyses. One type, called the mean response analysis, selected all instigations of a given level of aggressiveness-constructiveness and then calculated the mean level of aggressive or constructive response made in return. Correlations were then obtained between a given instigation level and the mean response to it, across all instigation levels, and as such, the procedure was similar to that used in most previous studies.

The alternate procedure, called the discrete response analysis, did not group the instigations into levels but rather, correlated each individual instigation a subject was judged to have received with the judged aggressiveness-constructiveness of his responses across all subjects. While we believed that the discrete response analysis would yield a more realistic view of behavioral reciprocity than the mean response procedure, the latter procedure was the major one used in our research, so that we could make quantitative comparisons with previous research. However, the results of both types of procedures are reported in our "Results" section.

CHAPTER 2

HYPOTHESES

In accordance with the theory of reciprocity (Gouldner, 1960) and the empirical evidence supporting it (Epstein et al., 1967; Helm et al., 1972; Harshbarger et al., 1974; Shortell et al., 1970; Taylor, 1967) as well as in accordance with the hypothesized role of contextual factors, we derived 19 hypotheses to be tested statistically. These hypotheses concern the degree to which reciprocity of aggression and constructiveness would be predictably found as a function of context and other factors. The prediction of and the finding of a high positive linear correlation between changes in instigation magnitude and changes in the subjects' responses is a necessary step to establish such reciprocity.*

Some of these hypotheses were not stated formally as such, but instead were stated informally as comparisons. The decision of whether an hypothesis was stated formally or not depended on whether we could provide a meaningful test of its significance. Where we concluded that the N was too small to even use a given statistical formula, or where we thought the predicted difference would be too small to be detected from our present data, given the power of the test available, or where we thought the hypothesis could not be tested for significance for other reasons, the hypothesis was not formally stated. However, it was stated as a comparison, and the data we did obtain in support of it is presented together with whatever tests of significance could be made.

*though not necessarily a sufficient step, if one has not established that a "rough equivalence" of exchange took place. This point will be discussed later. It should also be noted that even where a high positive linear correlation is not reported, the responses may still be a predictable, though not linear and non reciprocal function of instigation levels.

Overview of Hypotheses and Comparisons

The hypotheses and comparisons have been grouped into several sections for ease of inspection. The hypotheses and comparisons, 1 - 3, in the section entitled "Main Experimental Hypotheses" were derived from the major theoretical issues under test. They were based on the theory of reciprocity and previous research in this area, and using the direct mean response data, predicted that while subjects should on average reciprocate strongly when in a low restraint context similar to that found in the laboratory, subjects would show little or no reciprocation of aggression and constructiveness under normal restraint conditions, as evidenced by lower Pearson's correlation and slope values.

The next section entitled "Hypotheses and Comparisons of Similarities and Differences Between Direct Response Data Sample Statistics and Previous Population Values", contain Hypotheses Comparisons 4 - 7. These quantitative hypotheses hypothesized the degree to which subjects should, on average, reciprocate under different restraint conditions. For low restraint conditions, they hypothesized both the exact numerical strength of the Pearson correlation to be obtained (r) and the amount of increased aggression or constructiveness subjects should make for any given 1 point increase in instigation level (the slope (b)). These r and b values hypothesized were derived from the approximate values obtained from previous researchers, as outlined by Harshbarger. These values were ($r=.9$, $b=.45$). We expected that our subjects would show about the same given quantitative amounts of reciprocity under low restraint conditions as those reported in previous studies. On the other hand, we also hypothesized that subjects would show significantly less reciprocity behavior and lower increments of reciprocation under normal restraint conditions.

The next section "Hypotheses and Comparisons Concerning (Non-Demanded) Spontaneous Mean Responses" contain hypotheses 8 - 10. These hypotheses were also derived from reciprocity theory and proposed that under low social restraint conditions, a significant degree of reciprocity behavior (as reflected by r) would be shown in response to aggressive or constructive acts even in the analysis of the mean "spontaneous" data, where the subject was no longer obliged to make a response. However, we predicted that this obtained r would be less than the r obtained as a measure of the subjects' mean direct responses to aggressive or constructive acts under the same low restraint context because of decreased stimulation of the norm of reciprocity in the subject by the confederate, by ceasing his direct elicitation of the subject's responses. No predictions were made for the mean "spontaneous" behavior of subjects under normal restraint conditions since we expected the \bar{x} to be minimal and expected it to differ very little from the mean direct response \bar{x} . However, the data has been presented in a later section of this paper, (see Table 2, p.141) and the reader is invited to inspect it.

The next section "Hypotheses and Comparisons Concerning Self Reported Emotional and Motivational Dispositions to Reciprocate Following Aggressive or Constructive Acts", contains Hypotheses and Comparisons 11 - 13. These hypotheses were again derived from reciprocity theory and previous research. They were based on the assumption that even if subjects are inhibited by the context of the aggressive or constructive act from responding, the context would not substantially affect their desires to reciprocate, provided tremendous cognitive distortion or interpretation of the instigations by the subjects did not occur. We further hypothesized the exact magnitudes (.9) of the strength of the linear relationship that would exist

between the instigation levels received and the mean self reported desire to reciprocate, as well as between instigation levels and mean self reported happy or angry affect, and predicted significant results regardless of context. They hypothesized magnitudes of these correlations were derived from previous research where social restraint is assumed to be minimal and therefore, where the interaction norms of reciprocity were allowed to freely manifest themselves in behavior.

The next section "Hypothesized Relationships Between the Magnitudes of Instigations, Judged Levels of Abnormality, and the Mean Responses to Them Under Different Contexts", contain hypotheses and comparisons 14 - 17. We found in our pilot work that, in general, the more extremely aggressive an action was, the greater its judged abnormality, and the more extremely constructive an action was, past a certain point, the more abnormal it was judged to be. We went on to assume, based on this data, that while this statement was true in general, the exact specification of what magnitude of behavior are considered normal and acceptable depends on the situation.

However, for any given every day life situation, we predicted, based on an analysis of this data and previous research into people's responses to abnormal instigations, that where normal restraints are operative, the degree of reciprocity shown to abnormal magnitudes of instigation as evidenced by the Pearson correlation statistic (r) would be less than that shown to similar abnormal magnitudes of instigation under low restraint conditions, since it was assumed that low restraints encourage people to disregard the abnormality of the instigation. We also predicted that there would be no difference between the degree of reciprocity (r) shown to normal magnitudes of instigation under normal restraint conditions and that shown to normal magnitudes of instigation under low restraint condition - this because we assumed that normal restraints would not weaken the degree of reciprocity behavior in response to normal instigation levels.

The last section entitled "Comparisons Bearing on the Effect and the Differences in the Statistical Treatment of Reciprocity Data on Subsequent Findings" includes "Hypotheses and Comparisons 18 - 19." We hypothesized, for reasons mentioned previously, that when direct responses to each instigation received across subjects were looked at, we would still obtain data supporting reciprocity theory under low restraint conditions though the correlation would be much weaker than the value obtained using the mean response analysis. The former correlation would however, give a more accurate picture of our subjects' behavior, and by inference, the behavior to be expected under low social restraint conditions. We did not hypothesize what the normal restraint data would look like using the direct response analysis. However, we expected that the correlation obtained would be less than that obtained using the mean response data and that therefore, the relative positions of the correlations obtained under normal and low restraint would be similar, regardless of analysis done. In another part of this paper we have presented our central findings as obtained through both levels of analysis and the reader is invited to inspect them.

The 19 Hypotheses and Comparisons we made now follow:

Main Experimental Hypotheses - Direct Response Data

Hypothesis 1. In a 'low social restraint naturalistic condition, a strong significant reciprocity relationship will be observed between magnitudes of constructive and aggressive instigations and subjects' mean level of "direct response" to them.

Rationale: This prediction was based on the laboratory research

cited previously. It was also based on more naturalistic studies such as McClelland and Apicella (1945) where "low social restraints" were likewise hypothesized to be present. It was also based on the self-reports of Harshbarger's (1974) subjects as to what they would do in an aggressive or constructive situation.

Hypothesis 2. Under normal social restraint conditions there will be significantly less direct reciprocity behavior displayed than under low social restraint conditions. As such, the correlation between the degree of aggressive or constructive instigation and the mean "direct" responses to them, will be lower under normal social restraint conditions.

Rationale: There were many theoretical reasons and empirical support for this hypothesis that any observed reciprocity behavior under normal restraint conditions would be constrained and inhibited compared to that observed under low social restrained conditions (see Introduction).

Comparison 3. The slope of the regression line for predicting mean "direct responses" to aggressive or constructive stimuli will be less under conditions of normal social restraint than the slope obtained under conditions of low social restraint.

Rationale: Since Taylor (1967) had provided data showing that under-controlled individuals have higher slopes of reciprocation of aggression than over-controlled individuals, we inferred that not only were these slopes of reciprocation influenced by the personality of the subject, but moreover, we speculated that situational variables had served to arouse these personality characteristics and might therefore affect the slope of the regression line relating the responses of subjects to

different levels of provocation. We had also hypothesized that factors present under conditions of normal social restraint would function to inhibit people's behavior to extreme aggressive and constructive acts. In accordance with these assumptions, this comparison therefore would follow. However, since previous data had not produced marked changes in slope as a function of personality and since the probability of Type II error was not low enough, no formal hypothesis was stated.

Hypotheses and Comparisons of Similarities and Differences Between Sample Statistics and Previous Population Values - Based on Direct Response Data

Comparison 4. In "low social restraints" naturalistic conditions, the strength of the correlation reflecting the reciprocity relationship obtained above will be approximately .9.

Rationale: Since the "low social restraint condition" was to be designed to provide a context for the subsequent interaction similar to that hypothesized to be present in previous research, we expected r and b data that was quantitatively consistent with previous results. However, since for statistical reasons we could not demonstrate, the sample was indeed drawn from a population having r and b values of .9 and .45 respectively, but could demonstrate our obtained values were not inconsistent with this assumption, no formal hypothesis was stated.

Comparison 5. Under "low social restraint" naturalistic condition, the slope of the regression line relating the independent and dependent measures will be approximately .45.

Rationale: See Comparison 4.

Hypothesis 6. The strength of the correlation reflecting the reciprocity relationship under normal social restraint conditions will be significantly less than .9.

Rationale: In accordance with the hypothesized inhibitions, the normal restraint context imposed on reciprocity (as measured by r and b) and in accordance with our prediction of a .9 correlation between the independent and dependent measures under low social restraint conditions, this hypothesis logically followed.

Comparison 7. Under normal social restraint naturalistic conditions the slope of the regression line relating the independent and dependent measures will be less than .45.

Rationale: In accordance with the rationale given for Hypothesis 6 and in accordance with our prediction of a .45 slope for the regression line relating to the independent and dependent measures under low social restraint, this result should have been obtained. However, since the theoretical slope value was not too big to begin with, it was unlikely that we would obtain a slope value small enough to significantly differ from the .45 value obtained in prior research. Therefore, no formal hypothesis was stated.

Hypotheses and Comparisons Concerning
(Non-Demanded) Spontaneous Mean Responses

In analyzing this set of data, we would be looking at whatever mean responses are made to aggressive or constructive instigations when the confederate is no longer directly eliciting them through his verbalizations. As such, the behavior would be much more spontaneous and would be less controlled by any reciprocity norms. For these reasons, as well as for others we will outline later, we predicted similar rs and bs to those predicted earlier, though somewhat weaker in strength. Accordingly, we hypothesized that:

Hypothesis 8. Under low social restraint conditions a significant correlation will be observed between magnitudes of constructive aggressive instigations and subjects' mean level of spontaneous response to them.

Rationale: The rationale for this hypothesis was the same as for Hypothesis 1.

Hypothesis 9. The correlation reflecting the reciprocity relationship in the "spontaneous" data obtained under low social restraint conditions will be significantly less than that observed in the direct response analysis under low social restraint conditions.

Rationale: In accordance with reciprocity theory, we still expected a significant amount of reciprocity behavior to be displayed, even though the immediate elicitation of that behavior had ceased. (Gouldner, 1960). However, by making the assumption that direct instigation serves as a cue for norm arousal in subjects, we inferred that when that direct instigation is no longer present, the degree of norm arousal would be somewhat lessened, resulting in a somewhat weakened reciprocity relationship compared to that observed for people's direct responses to the immediate instigation.

Comparison 10. Under normal social restraint conditions analyzing the spontaneous response data, the correlation relating this variable to the magnitude of aggressive or constructive instigations will be less than that observed in the direct response analysis.

But since we reasoned that both correlations would probably be small due to the postulated influence of normal social restraint, no formal Hypothesis of statistical significance was made.

Hypotheses and Comparisons Concerning Self-Reported Emotional and Motivational Dispositions to Reciprocate Following Aggressive or Constructive Acts.

Hypothesis 11. Under both normal and low social restraint conditions combined, a positive correlation will be observed between the magnitudes of aggressive and constructive instigations and mean magnitudes of harmful and helpful desires towards the instigator, as reported by subjects following the instigations.

Rationale: These predictions were based on reciprocity theory. We reasoned that subjects should develop desires to help or harm in accordance with the magnitude of the aggressive or constructive instigations they have received and this would be true even if subjects are inhibited from reciprocating.

Comparison 12. Under low social restraint conditions, the correlations mentioned in the previous hypotheses would not be significantly different from .9.

Rationale: This prediction was based on reciprocity theory and previous research which had shown that when subjects are allowed to do as they will, a .9 correlation between their behavior and the antecedent instigation was obtained. By assuming, as Taylor did (1967), that behavior under low social restraint reflects motivational tendencies, on a self report measure of such tendencies, we likewise expected to obtain a similar .9 correlation between the magnitude of the instigation and the mean magnitude of resulting affect. However, since we could not show statistically that the sample was drawn from population having a $r = .90$ but could only show our obtained sample was not inconsistent with this assumption, no formal hypothesis was stated.

Hypothesis 13. Even under conditions of normal social restraint,

significant correlation will be obtained between the magnitude of aggressive or constructive instigations and the mean magnitudes of subjects' self-reported emotional and motivational reactions to them, following the instigations.

Rationale: Even if subjects were inhibited in displaying reciprocity behavior as a result of normal social restraint, we reasoned they should still develop the desires to help or harm which reciprocity theory would predict that they would.

Hypothesized Relationships Between Magnitudes of Instigations, Their Judged Levels of Abnormality and the Mean Responses to Them under Different Contexts.

Hypothesis 14. Treating aggressiveness and constructiveness as a unipolar dimension for purpose of this analysis, if one looks at those instigation levels judged to be neutral to extremely aggressive there will be a strong significant linear relationship across actions between their judged degree of aggressiveness and mean judged level of abnormality.

Rationale: Pilot work where college students rated the abnormality of various aggressive acts described on paper, led us to believe that judges' ratings of actual behavior along these two dimensions could produce similar findings. (see Appendix A, p. 237).

Comparison 15. Treating aggressiveness and constructiveness as a unipolar dimension for purpose of this analysis, if one looks at those instigation levels judged to be neutral to extremely aggressive there will be a curvilinear relationship between how constructive behavior is and its mean judged level of abnormality.

Rationale: Pilot work, where college students rated the abnormality and normality of various constructive acts described on paper, led us to believe that judges' ratings of the actual behavior of our confederates along these two dimensions would produce similar findings. (see Appendix A,

p. 237). However, since our pilot data did not produce marked curvilinearity at the most extreme levels utilized (similar to those used here) no formal hypothesis was stated.

Hypothesis 16. In contrast to the prediction made in Hypothesis 2, in an analysis of only those instigations that were judged to be "normal" under condition of normal social restraint, the correlation between given levels of instigation and the mean direct responses to them, will not be significantly different from the correlation obtained from all instigations in the low social restraint conditions.

Rationale: While we previously expected to find significant differences in the correlations of these measures under conditions of normal and low social restraint, we further hypothesized that this was due to the lack of legitimacy and justification provided for extreme aggressive and constructive instigations and responses under the condition of normal social restraint. It was argued that the lack of legitimacy and justification provided for these behaviors under normal social restraint, would lead to a weakening of reciprocity behavior in relation to them.

However, by the same token, it was hypothesized that for those instigations that are judged normal and are expected in such situations, we would find strong reciprocity behavior displayed, the strength of which rivaled the reciprocity behavior displayed under low social restraint for all instigations, not just for those which were deemed to be normal. Thus, we reasoned that under conditions of normal social restraint for the range of instigations which were judged to be normal, the expected factors leading to inhibit reciprocity would be minimal or non-existent and we would obtain similar reciprocity data to that obtained in previous studies or that obtained under conditions of low social restraint, where we hypothesized the abnormality of the instigations was nullified by the contextual manipulations.

Hypothesis 17. Under normal social restraint the correlation between instigations

judged to be "abnormal" and the mean direct response to them will be significantly less than the same correlation achieved using the same magnitudes of instigations under conditions of low social restraint.

Rationale: This readily followed from the rationale given in Hypothesis 16.

Comparisons Bearing on the Effect that Differences in the Statistical Treatment of Reciprocity Data have on Subsequent Findings.

Hypothesis 18. Using the analysis of the direct response data under low social restraint conditions, there will be a significant correlation between the magnitude of aggressive or constructive instigations each person received and the actual magnitude of response he returned. (Versus mean response for all subjects receiving that instigation.)

Rationale: This prediction was in accord with reciprocity theory and the results supporting it.

Comparison 19. The above correlation will be less than that obtained using mean responses to given levels of instigation as a dependent variable.

Rationale: If we were right in assuming that using mean response data produces stronger correlations than using the response of each subject individually in correlational analysis, then this result would be obtained. However, since no test of significance was readily available and/or necessary in this case, this prediction was stated as a comparison rather than as a formal hypothesis, since both correlations were based on the exact same data.

CHAPTER 3

METHOD

Essential Elements of the DesignGeneral Design

In the current research, nine different stimuli presenting different levels of the variable aggressiveness-constructiveness were administered to college students by a confederate-peer in a simulated naturalistic setting. The manipulation of this variable took place under two different contextual and situational conditions. One condition was termed "normal social restraint" and the second we have termed "low social restraint". The effects of the stimuli and the two conditions were observed on the subjects' responses, mood and motivation. The variables and their levels are as follows:

Main independent variable-degree of helpfulness and aggressiveness of the stranger (confederate).

Nine different stimuli were chosen which represented points along this dimension as follows:

9. Extremely aggressive - stranger orders subject to move over on couch and curses him vehemently for not moving enough.
8. Strongly aggressive - stranger deliberately engulfs the subject in cigarette smoke.
7. Moderately aggressive - stranger angrily tells subject to move over on the couch after he has already moved.
6. Mildly aggressive - stranger chews gum loudly while subject attempts to concentrate.
5. Neutral - stranger sits quietly.
4. Mildly constructive - stranger asks for change of a quarter for the parking meter.

3. Moderately constructive - stranger offers subject gum and starts friendly conversation.
2. Strongly constructive - stranger engages the subject in personal conversation and compliments him highly.
1. Extremely constructive - stranger compliments subject and invites him and his girl-friend for an all expense paid dinner.

NOTE: For each of the stimuli listed, the a priori approximate value of aggressiveness-constructiveness is attached. The exact value (judged value) of each individual instigation was obtained by having student judges view and rate video-tapes of them. The medians of these judgments formed the actual independent variable.

Contextual variables - two levels

Normal social restraint - No attempt made to interfere with normal hypothesized restraints on reciprocity occurring in naturalistic context.

Low social restraint - Attempts made to alter social restraints and context to that theorized to exist in previous research.

Dependent variables

Several dependent measures were obtained as follows:

Judged aggressiveness and constructiveness of the subject's video-taped direct responses.

Judged aggressiveness and constructiveness of the subject's video-taped spontaneous responses.

Self-report of hostile or happy affect on a nine point scale.

Self-report of hostile or helpful motivation.

MAACL (Mood Affect Adjective Checklist) (See Appendix E) self-ratings of anxiety, depression, and hostility, (as scored by

MAACL Manual) obtained before and after the experimental manipulations.

In addition to the above variables, behavior of both subject and confederate were also judged and evaluated along the dimensions of normality-abnormality. Using this variable, a break-down analysis of the data was done. Biographical and other data were also collected but will not be analyzed in the present research.

The flow chart presented in Figure 1, pictorially displays the manipulations performed in this study, the point at which they were performed, and the points at which data were collected. The reader is referred to this flow chart to get a clearer idea of the overall design. (See Figure 1).

In this research, subjects were randomly assigned to 1 of 4 confederates of differing heights, who enacted 1 of 9 randomly selected aggressive or constructive acts, under 1 of 2 randomly selected situational contexts. As can be seen a 72 cell matrix can be constructed based on the above factors.

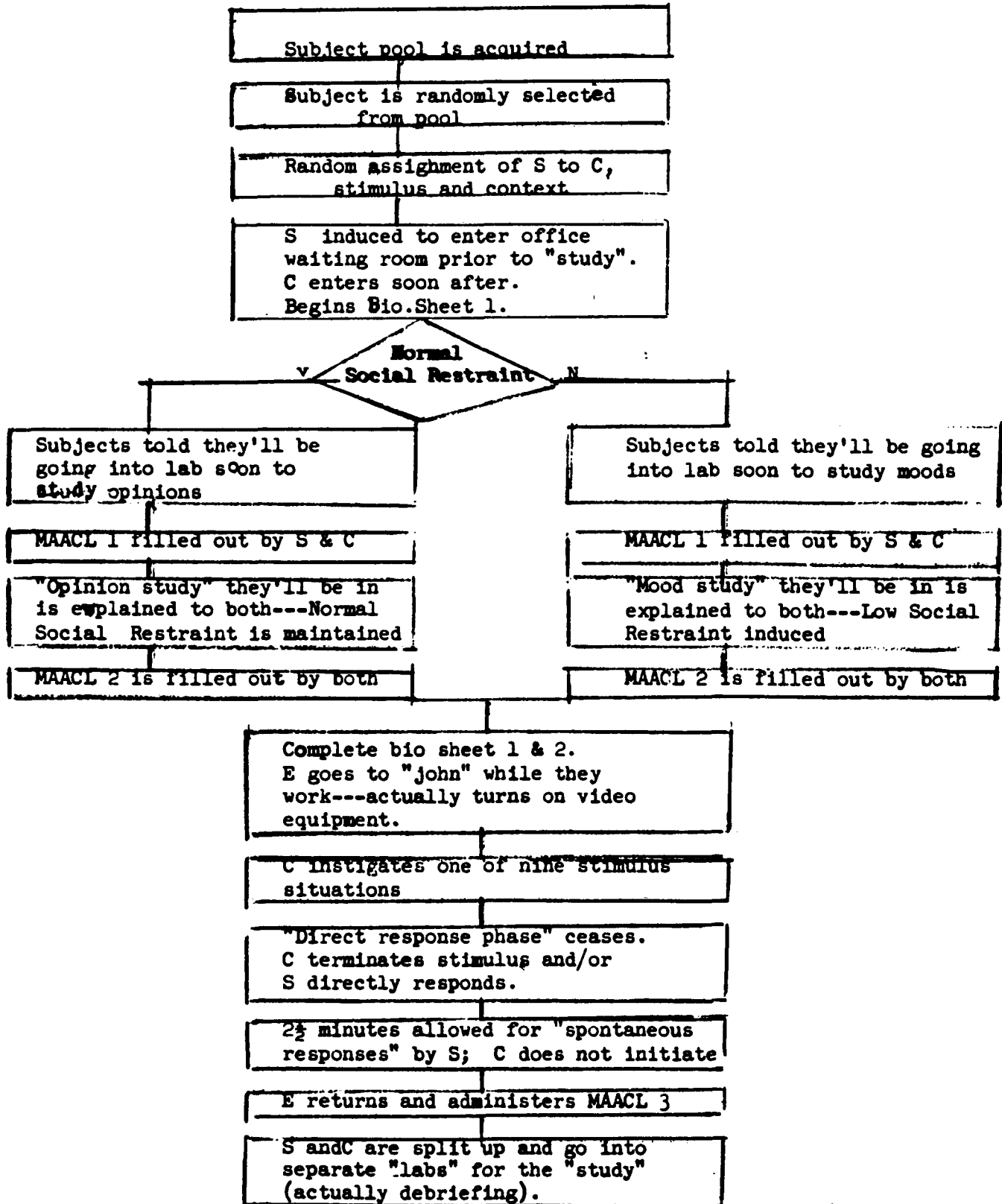
It was planned to randomly assign one subject to each cell in this matrix. In cases where a subject's data would have to be eliminated from the study due to equipment failure etc., a new subject was randomly assigned to that cell in his place, if possible. Close inspection of the cases where this was made necessary revealed that they occurred essentially at random and thus, such deviations were considered random error.

Definition of Stimulus Dimension and Selection of Stimuli

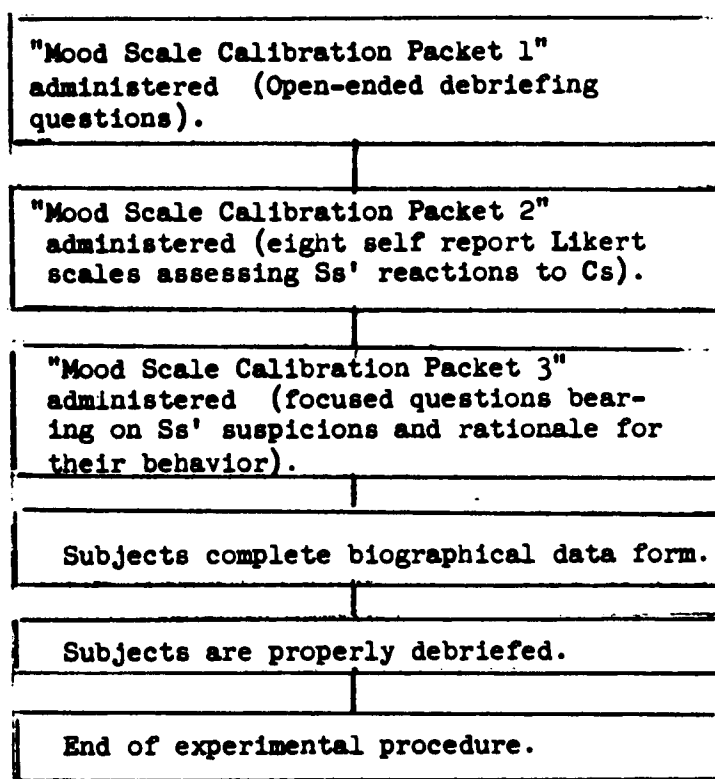
To study the effect of different intensities of aggression, and helpfulness on a subject's behavior, following Harshbarger (1974) a stimulus dimension was hypothesized, defined on the one end by stimuli that could

FIGURE 1

Flow Chart of Experimental Procedure in Study



(Go to next page)

Flow Chart of Experimental Procedure in Study (Contd)

be said to be extremely aggressive and on the other end by stimuli that could be called extremely constructive. The several social stimuli were thought to fall in different places along this stimulus dimension, depending upon how aggressive or constructive they were.

To define the dimension in more detail, we needed to define the terms of "aggressive" and "constructive" and to define the extremes of the scale. We define aggressive actions as actions that in general "destroy, harm or threaten to harm something physical or psychological that is valued by others", such as a valued state of mind, a valued asset, valued feelings, etc. In turn, we define constructive actions as those which "help" others maintain or secure something that they value", such as a valued state of mind, a valued asset, a valued object, or valued feelings. Implicit in these definitions, was the idea that actions could be physically or psychologically harmful or helpful.

Having thus defined our relevant stimulus dimension, the next step was to construct an appropriate scale on which acts, differing in the intensity along this stimulus dimension could be rated. Basically, the method of equal-appearing intervals was utilized in the construction of this scale (Edwards, 1957). The two extreme ends of this scale were defined using specific examples of what the author meant by "extreme" aggressive or constructive acts. The first reason the end points were defined using these specific examples, was that it was desired to set up stable reference points for the ratings. We felt that just using the words "extremely aggressive" or "extremely constructive" would be much too ambiguous and might lead to spurious scale ratings. The second reason for giving specific examples of the extreme points was that due to ethical considerations, it was anticipated that the stimuli finally

selected for use in the study, would be less harmful or helpful than the full range of actions that one human being could perpetrate on another. In defining our extreme anchor points, using specific examples of what was meant by an extreme act in terms of the study, we insured that the ratings thus obtained from raters, would be made with reference to these examples rather than with reference to some other definition of extreme, for example, murder. It was hoped that by providing raters with these definitions of the term "extreme", we would encourage them to make finer distinctions in their ratings and utilize more of the nine point scale than would otherwise be the case if they had relied on their own perception of what the term extreme meant.

The anchor points were defined as follows: an action was to be rated "extremely aggressive" if it was equivalent in harm to being cruelly insulted or forcibly pushed. An action was to be rated "extremely constructive" if it was equally as helpful as being lavishly and sincerely complimented, or being offered a lift home in the rain when you needed one. The scale dimension that was constructed appears in Figure 2. Raters were told that they were to make all ratings with reference to the defined anchor points. They were also told that all other points and descriptive adjectives on the scale were with reference to the anchors and constituted equal intervals along the scale. If a rater felt that any given action was more extreme than could be encompassed within the limits of the anchor points, they were instructed in procedure which allowed them the option of extending reference. (See Appendix A for sample scale instructions). Since all ratings of behavior were made utilizing the same scale, it was possible to compare the magnitude of any instigation to the magnitude of the resulting response (Harshbarger, 1974).

FIGURE 2

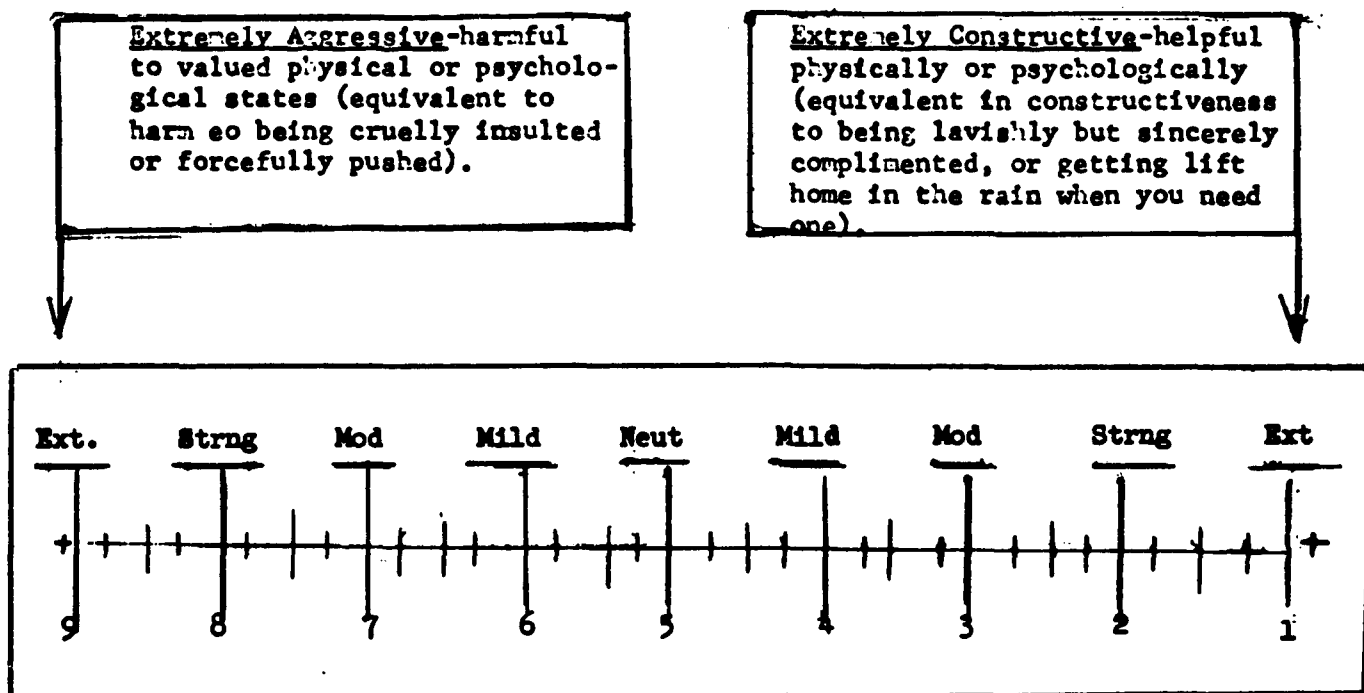
Diagram of Aggressiveness-Constructiveness Scale

Figure 2. Aggressiveness-Constructiveness scale dimension used for all ratings in the present study

Criteria for the selection of stimuli

Having constructed our relevant scale dimension, the next job was to select a number of stimulus actions which confederates could be trained to instigate. Several criteria were kept in mind during the selection process. One set of criteria concerned the desire to provide for maximal generality of findings. In the desire to obtain stimuli that would cover the full range of our scale dimensions, we decided to select nine different stimuli which would fall at roughly even spaced points on the scale. We decided to use this number of stimuli because more than this number would have created problems in the training of confederates and fewer stimuli would decrease the generality of the findings. In this regard, pilot work had indicated that certain meaningful interactions between stimuli and contexts might be ignored if fewer than this number were used. To further provide for maximum generality of our findings, we decided to select stimuli that varied not only in intensity but also in content. That is, we decided to include several different types of stimuli, verbal and non-verbal, etc. other than just one type of stimulus.

Another set of criteria used in stimulus selection were the APA guidelines on ethics. It was desired to keep the intensity of the aggressive instigation within those guidelines.

In addition to the above criteria, it was also desired to select stimuli that were relatively unambiguous and that did not lend themselves to varied interpretations, particularly in different contexts. It was also desired to select stimuli that were as appropriate and realistic as possible for any given level of unprovoked aggression.

Finally, it was desired to select stimuli that could be easily performed and that were practical given the facilities available.

Method of stimulus selection

Student volunteers were presented with the scale described previously and were asked to think up stimuli that were appropriate, practical, and that varied in intensity along this scale. Based on these pooled items, items were then eliminated a priori, if they looked like they fell short of meeting any of the criteria mentioned previously. We were then left thirty promising items. These items were put on a mimeographed sheet for the final elimination process. The judgments of two classes of psychology students were utilized in the final elimination process. Classes received the mimeographed list of potential stimuli. One class was merely told that these stimuli were initiated by one person toward another. The other class was given a more specific context which came close to the actual experimental setting. Half of each class were asked to rate the thirty actions as to how aggressive or constructive they thought the actions were. The other half were asked to rate them as to how normal or abnormal they thought they were. Using this data, we were able to get a better idea of which stimuli fell at roughly equal points along the scale.

Using the standard deviations of the raters' judgments as a guide, it was further possible to get an idea of which stimuli were least ambiguous and least subject to personal distortion. Looking at a scatter plot of correlation between the abnormality of the stimulus and its level of aggression or helpfulness, it was also possible to get a better idea of which stimuli were the most appropriate at any given level of instigation. Based on this information, the final selection of stimuli was made.

Once the stimuli were selected their exact nature had to be concretely specified. The first step in accomplishing this was to role-play the instigations on video-tape. A number of assistants were utilized in the making of the tape. In the course of this production, we learned that some modification in the stimuli was needed. We also learned to anticipate and deal with the various contingencies that might arise as each assistant continually play-acted "naive subjects" responding to the stranger-confederate. Out of this role-playing, we developed specific procedures and dialogues for each of the stimuli and developed procedures to handle contingencies that might occur in their production. Once these problems had been ironed out a final video-tape was made. A training manual incorporating a transcription of the verbal and non-verbal behavior of each stimulus situation was likewise prepared, as was an audio-tape. These latter materials were utilized by confederates in their training process.

As can be seen (See Appendix C) the training manual consists of a set of procedures and principles that the confederate was to follow at each point in the study. It also contains the dialogue he was to use, the facial and bodily postures he was to assume, the affect he was to emit, etc. for each of the stimulus instigations. All this was spelled out specifically to insure as much uniformity and consistency in the experimental production of the stimuli as possible.

Subjects and Experimental Personnel

The selection of subjects is an important variable that must be taken into consideration in doing a study of this type, since previous research by Taylor (1967) suggests that different groups of subjects may show a very different response curve to increasing gradients of aggression.

Although it limits our findings, we decided to use male college student volunteers who were fulfilling their introductory psychology requirements through their participation in this study. This population was decided on for several reasons. First, most previous research in this area has employed males from introductory psychology classes and it was decided to use similar subjects to allow for some comparisons. Second, we felt that for ethical reasons, volunteers rather than unknowing participants would have to be used, since we were presenting potentially "harmful" stimuli to some subjects. Third, this particular population was readily available and immediately accessible. Of all subjects we contacted who indicated their availability for our research, some were eliminated on the grounds we suspected that they were not "naive", some were eliminated due to technical malfunctions in the recording of their video-tape data, and the data of some subjects were eliminated on the grounds that they reported not merely being suspicious, but being "convinced", of the deceptiveness of our study. The remaining subject pool consisted of 81 subjects and these were the focus of our research.

Since each volunteer filled out a detailed biographical questionnaire, it is possible to accurately describe the demographic characteristics of our sample. Some of these characteristics are given in Table 1 to which the reader is referred.

The experimental personnel consisted of myself and several male college student confederates. Criteria we had in mind in the selection of these confederates, were that they come as close as possible to being representative of the student body, in terms of height and appearance, the latter being necessary to avoid arousing undue suspicion. Potential

TABLE 1DESCRIPTIVE CHARACTERISTICS OF THE SAMPLE USED IN THE PRESENT STUDY

Median Age	18.8
Percentage of males	100.0
Mean Height	5'9"
Mean Height Difference from Confederate	.537 "
Percentage in each college year	
Freshman	39.3
Sophomore	11.1
Junior	6.1
Senior	23.5
Median reported income of parents	17,667
Percentage of each race	
Caucasian	87.7
Black	6.2
Other	6.1
Percent of S's life lived in United States	
100%	90.1
90%	2.5
60%	1.2
less	6.2
Ethnic background listed by S	
Jewish Unspecified	28.0
Jewish European	17.0
Italian	12.0
Irish	10.0
Mixed and other	33.0

confederates were also screened as to their ability to enact the different situations required, their believability as actors, and the amount of time they had available for the project.

Five confederates were finally obtained, selected according to the criteria mentioned above. Of the five confederates, four of them successfully reproduced each of the nine stimulus situations under each of the two conditions. The fifth confederate had to drop out during the course of the study but since the stimuli he had already produced had been selected at random, it was decided to include his data in the analysis. The heights of the four remaining confederates were 5'6", 5'9", 5'10", and 6'4". All confederates were from medium to light build and were Caucasian.

The "experimenter" was the writer of the present paper. I am of average height and build, and am Caucasian. As in the case of confederates, there was a standard set of procedures that I was to follow during the course of the study.

Apparatus

Three rooms were selected and made available for the research. The factors that led to the selection of the rooms that were used included availability, room size and the presence of one-way mirrors. A 9x7 room was eventually decided upon. This room had two large one-way mirrors, approximately 2½' x 6' each on one wall and baffles concealing the hidden microphones in the ceiling. To minimize the possibility that subjects might be curious about the mirrors, large metal chests were placed against the half of the mirror facing the subjects. In addition, three quarters of the surface of the mirrors were covered by four metallic foil diamonds (1'x1') with posters placed in the center of these diamonds.

The hoped-for effect was to make the mirrors seem to be a modern mirror art deco piece. Facing the mirror, we placed a 3½' couch, judged to be comfortable enough for two people but small enough so that the confederate could easily maneuver himself so that he was sitting close enough to the subject without arousing undue suspicion on the subject's part. A desk was also present in the room propped up against the wall. A coat rack, colored posters, waste basket helped complete the impression of the room as an office-waiting room.

To supply the lighting necessary for the video-tape through the one-way mirror, the fluorescent ceiling fixtures were utilized. In addition, a 200 watt wall lamp directed toward the general position of the couch, served to give proper illumination for taping.

Two other rooms were also utilized in our research. One room was adjacent to the "office-waiting" room and had a conspicuous sign on its door saying "Lab 1 Do Not Enter". All subjects were met outside of this room, and only then were invited into the "office-waiting" room. The purpose of doing this was to help further the impression that the real experiment would be taking place in this latter room and the "office-waiting" room was not connected with the study in any way. Another function of this second room was to provide a place where subjects could be questioned and debriefed.

The third room contained our video-tape equipment. A Sony portapack unit was used to tape the subject on half inch tape. Since tape is quite expensive, we decided to use used video-tape. Although there is some "drop-out" in the use of such tape, the amount was very minimal. A portapack was used with a standard Sony camera with its lens iris opened wide.

With regard to the audio it was found that the ceiling microphones were inadequate. To deal with this problem, a Lafayette Model ML-1 condenser microphone was hidden in the middle of the couch between the back cushions at shoulder level. The effect of this was to bring a highly sensitive microphone within one to two feet of the subject's and confederate's mouths. This microphone was fed into a solid state amplifier located beneath the couch whose output was fed to a five inch speaker located in the taping room. Acoustical coupling was then used between the speaker and the Sony auxiliary dynamic microphone connected to the portapack. A 12" Sylvania monitor was used to monitor the audio as it was being taped, and to monitor the video as well.

All video tape equipment in the taping room could be turned on and off through a master in line remote switch. This switch was hidden directly outside the "office-waiting" room so that I could turn on all equipment immediately upon leaving the subject and confederate alone together.

As will be described, following the active phase of experimentation, the resulting video-tapes were edited and re-copied. The Sony video-tape units, Model 3600 and 3650 were used back-to-back. Sony monitors were used to monitor input and output of each. During the judging and rating of these video-tapes by students a Model 3600 machine was used together with a 12" Sylvania monitor. A piece of cardboard attached to a curtain rod arrangement, allowed me to keep part of the screen masked as required while allowing me to shift the mask or entirely remove it as indicated. Instructions to ignore the masked individual were impressed on the cardboard with metallic lettering. To improve the audio quality during the judging and rating process, and in addition to the output of

of the 12" monitor, we also obtained audio through an auxiliary polyplanar 12" speaker which was fed by a small amplifier and acoustically coupled to the Sylvania monitor. To improve the quality of the audio, this speaker was placed in the back of the room so that subjects seated there would be able to hear better.

Procedure

Procedure for Assignment of Subjects

Since we planned to compare the reciprocity correlations between the confederates' and subjects' behavior under two different situational contexts, it was necessary to insure that several other factors besides the contexts, which might have produced similar comparisons between such correlations, were controlled in our design. First, it was necessary to insure a rough equivalence of the subject groups participating under any given context, with respect to any subject variables that might mediate their responses to aggressive or constructive stimuli. To insure this rough equivalence of subject groups, subjects were assigned to each context in a random manner. Secondly, it was necessary to insure that the same confederates enacted the stimulus situations under each of the two situational contexts, and any deviation from this be random error. To insure this, the same confederates were used throughout the study under both situational contexts. Lastly, it was necessary to insure that the same type of instigations would occur within each situational context and that the range of their aggressiveness or constructiveness was similar - any deviation from this plan being random error. Our strict training procedures outlined in our training manual, were devised in part to insure this would take place.

In our design, each subject was randomly assigned to one of four confederates who differed widely in height and personality. The confed-

erate then enacted one of nine aggressive or constructive acts under one of two situational contexts. The particular instigation and context that each subject received was determined by a table of random numbers.

It was planned to randomly assign one subject to each of the 72 cells generated by combinations of different levels of the above variables. In cases where it was clear that a given subject's data would have to be eliminated from the study, due to video-tape equipment failures, etc., a new subject was randomly assigned in his place. In actual practice, the data that forms the basis for the present research was collected from 81 subjects, nine more than the number to fill all the cells. Three of these nine subjects had interacted with a fifth confederate who did not complete all the 18 cells assigned to him. Four more of the nine subjects had been running cases where the confederate did not seem to have accurately reproduced as strong a stimulus situation as required to fill a given cell. However, instead of substituting the new data for the old, both sets of data were included in our analysis. The last two of the nine additional subjects were not included in our main analysis because video data was not complete on them.

It should be noted that despite the excess data included in our analysis three of the 72 cells were emptied, as a result of a post hoc decision to adopt a more stringent criteria for the elimination of excessively suspicious subjects. Subjects who claimed that they were "convinced" that they were being deceived in any essential fashion were now eliminated. Since the criteria for additional deletion of subjects was applied uniformly regardless of context and affected the samples in an essentially random manner, they would not affect the comparisons made between the results obtained under the two different contexts.

Beginning the Experimental Procedure.

Subjects were recruited through a sign on the Introductory Psychology Subject Pool bulletin board. It promised them two credit hours for participating in a study of various mental processes and asked them to leave their phone number.

Shortly thereafter subjects were called. I introduced myself and set up an appointment. Once this was done, the subject was asked to meet me outside the "lab room" at a specified time. He was cautioned to be on time so as not to interfere with the study in progress before he arrived and so as not to hold up his participation in it. He also was casually informed that other students would also be arriving and was told that the instructions would be explained to all of them at the same time before they went into the Lab to begin the study. They were also told the study would take an average of two hours to complete.

When they arrived, I greeted them outside the door of the "lab". I informed them that the Lab was not ready yet and the other subjects had not yet arrived. I indicated that I wanted to wait until everyone had arrived to explain to everybody what they would be doing in the laboratory. I told the subject that he could either return in five minutes or could come into the waiting room and begin working on the first page of the biographical material, which they would need to do anyway, as part of the study. In actual fact, I subtly persuaded the subject to come in and begin working on the biographical form.

Once they began working, I propped the door open and wandered out into the corridor where I signaled the confederate to enter a minute and a half later.

I wandered back, sat down and asked the subject how he was doing with the form. Shortly thereafter the confederate knocked on the door and asked "if this was where the Psych 2 experiment was". I asked him his name, invited him in and gave him a biographical data sheet to fill out. I then casually asked each person if they had seen anyone else looking for the room. When they said they hadn't, I propped open the door and went out for a quick look, returned and said I didn't want to hold them up any further so I would begin explaining to them what they would be doing when they went into the laboratory. Up to this point, the purpose of this procedure was designed to have the subject think he was not in the actual laboratory, to have the subject think he was sitting with another student and to deceive the subject into thinking it was just a coincidence that there was just another student present. The biographical data sheet was used not only to collect some data but also to occupy the subject so he would not be forced to visually inspect the room.

Manipulation of Context

At this point in the procedure, just prior to the instigations by the confederate, it was desired to manipulate the contexts in which this instigation would take place so as to test the hypothesized effects of this contextual manipulation on the degree of reciprocity behavior that would result.

Prior to the actual instigation, one group of randomly assigned subjects, the normal social restraint group, were led to believe the actual study would be taking place in another laboratory much later on, and concerned a topic totally unrelated to their interaction with each other in the "office-waiting room". As such, instigations of the confederates for this group were seen by subjects as occurring in the contexts of a normal social situation in an office-waiting room setting, prior to going into the laboratory for the supposed real study. Thus, the contexts under which these instigations were to take place come closest to that found in naturalistic social setting where two strangers find themselves together for a short while, with no other people immediately present on the scene.

For the second group of randomly assigned subjects, the low social restraint group, the confederates' subsequent instigations were to be seen within a context similar to that hypothesized to be present in previous research. Like the first group of subjects, this group was also led to believe that the actual study would be taking place in another room. However, unlike the first group they were told that this would be a study of people's moods. As we said, they were led to believe the actual study would be taking place much later in another room. However, they were told that to make this study a success, it would be a good idea to get used to the idea of doing and saying just what they felt before they went into the "lab" for the "actual study". As such, when the instigations of the confederates began, they were seen within the contexts we had previously encouraged both the parties to do and say just as they pleased. In other words, prior to the confederates' instigations, we had produced a context where any subsequent norm vio-

lating self-expressive behavior was sanctioned, legitimized, and justified as being part and parcel of one's practice for the actual study itself which was to take place in another room. Thus, subjects were not only induced to see these instigations of the confederate and their reactions to them as occurring under conditions of "low social restraint" but, like in the previous research, they were deceived about the exact nature of the behaviors that were the focus of my interest.

Normal restraint procedures

Let us now look at the actual procedures for the manipulation of the contextual variable. At the start of the procedure (see Appendix D), designed to maintain a context of normal social restraint, it was explained to the confederate in the presence of the subject, that they would be participating in a study of "different types of opinions that people have on topics such as social issues, politics, and sex", and how people's opinions "form and change over various periods of time". They were told that they would be going into the "lab" across the hall for this study. But before that, I wished to get "an initial estimate of their mood stability and changeability". The deceptive rationale was provided for this. Subjects were told that we needed to get some idea of what their base line mood was before they went into the laboratory, since "sometimes the opinions that people give are colored by their mood".

Both the subject and confederate were then given a copy of the MAACL and the following instructions were then read to them. These instructions were based on the standard MAACL instructions with slight modifications for purpose of this research. They were told:

"This is a scale with words which describe many different kinds of moods and feelings which people have. What we'd like you to do is put an (x) next to all those words which describe your mood

and feelings, now and during the last five minutes. Some of the words may sound alike but please (x) all the words that apply.

Please work rapidly". (See MAACL , Appendix E) .

Once subjects had completed the form, we collected each person's MAACL. (The real purpose of giving the MAACL was not only to get some idea of what their mood was like at this point in the study, but moreover, to avoid making subjects excessively suspicious when the MAACL was administered for the third time, just after the confederate had instigated one of the nine stimulus situations. By that time, they were used to the MAACL and had been informed that it was necessary to give this checklist several times in order to insure the stability of its ratings.)

After the MAACL was administered, I went on to explain to the subjects that this was to be a study of people's opinions on significant important and political issues. I said that I had asked two or three students to come down before the start of the experimental sessions in the laboratories so that I could save some time and not have to repeat myself. I pointed out that this way I could introduce myself to them all at once, have them fill out the preliminary forms at the same time and explain to them all at once what the study was about before they went into the laboratories.

Following this introductory explanation, I then reiterated the goals of this study and said that in designing a study of opinions I had to deal with the fact that since opinions are inner states, it would be necessary to have subjects in the study give their accurate, considered opinions on a variety of topics. I pointed out that one problem I faced in designing this study was that some students are vehemently opposed to participating in any study but come down simply to fulfill their course

requirements. I noted that I had to rely on the student's cooperation and so, if either of the two "students" felt like leaving at any point, they should say so, since I would rather give each student the credit slip than have him participate against his will.

The real reason for this speech was first to lessen any feelings of being coerced to participate on the part of the subjects and second to ethically inform subjects that they could leave at any time, without penalty. A similar speech was given to the second group of subjects too. The real reason for the lengthiness of the introduction was to control the amount of time we would be spending with the second group of subjects, in order to try to "lower their restraints".

As anticipated, all subjects agreed to stay. I then pointed out that another problem I had to deal with was that even when one has cooperative participants, sometimes people are flippant in their answers, and this can confuse experimenters as to how they really feel. I reiterated my hope that they would try to give their carefully reasoned opinions.

I went on to briefly discuss with the subject and confederate the fact that in order to study people's opinions, the best way to do it was to use scales. I asked if either student was familiar with the Likert scale and the confederate feigned ignorance. I then said that since both students were not familiar with this scale, I wanted to show what it was and how to use it before they went into the lab for the study. I pointed out that most Brooklyn College students in fact, had not heard of this scale either. However, before I went on to show them the scale, I wanted to raise certain issues with them that I thought might have come up in their minds and affect their ability to indicate their accurate, considered opinions. I pointed out that some students might be concerned

about the confidentiality of their opinions, whether or not their opinions would be used without their written permission at the end. I assured them that any opinions they might give on social or political issues would be kept confidential and they were free not to answer any question they found offensive, etc..

I then got back to the issue of the Likert scale saying that since I had found that most students were unfamiliar with the Likert scale, I prepared some sample Likert scales and questions related to them and would now like to show this particular pair of students how to use the scale.

I gave out a sheet to each student with six questions on it and six blank scales, and showed each "student" how to use the scales. I asked them to complete the sheet so they could be sure they knew how to use the scales. The confederate again appeared naive and asked some seemingly naive questions about the Likert scales.

Following this, I collected the sheets and appeared satisfied. At this point, I announced that, once again, it would be necessary for each person to fill out another MAACL form. I indicated that the reason for this was that some people's moods naturally fluctuate and I would need to get several estimates of their mood to take into account these fluctuations and so obtain an accurate reading. The new MAACL was re-administered with similar instructions as before, only now the students were told to indicate how they felt "now and during the last five minutes".

As mentioned before, the real purpose of readministering the MAACL was to get subjects used to the idea that it had to be administered several times. Through the readministration, it was also possible to get an idea of what their mood was just before the actual instigation was to

take place.

Once the MAACLs were finished, they were then collected. I asked the students to try to finish the preliminary filling out of the biographical form and indicated, following this, they would be going into the lab for the study. As they began finishing up the biographical information sheet, I said that I might as well use this opportunity to go out "to the john". I then left the room and turned on the video-tape equipment. Approximately a minute thereafter the confederate initiated one of the nine stimulus situations in the manner described in the following section:

Low restraint procedures

Under the condition of low social restraint, subject and confederate were told that they would soon be going into the "laboratory" to participate in a study on different types of moods that people have and how people's moods form and change over various periods of time.

As in a normal social restraint condition, they were told that an initial estimate of their mood was needed. Again, the rationale offered was that it was needed to get an idea of what their pre-experimental mood was like before they went into the laboratory. MAACL was then administered with identical instructions to those described previously and following their completion, the MAACL forms were then collected.

As in conditions of normal social restraint, the real purposes of administering the MAACL was to obtain an early indication of what each subject's mood was like, and to get subjects used to the readministration of the MAACL.

Following the subject's completion of the first MAACL, I then introduced myself and, as in the condition of normal social restraint, explained why I had had them come down together and reiterated what the

study was about. At this point, however, in contrast to the normal social restraint condition, I added that the two people would not be together in the laboratory when the actual experiment took place, but in fact, one would work with me and one would allegedly work with my assistant. The purpose of telling the "students" this was to make them feel less concerned about what the other "student" might think of them later in the study.

At this point in the procedure, I again reiterated the alleged goals of the study. I pointed out that I recognized that achieving these goals would not be easy for several reasons. The first was that some students were vehemently opposed to participating in any study but came down simply to fulfill their course requirement. As described previously, I offered to give students their credit slips and have them leave at any time rather than remain in the study against their will. Following the expression of willingness to continue, on the part of the "students", I pointed out that I also realized that even though they had both now expressed willingness to cooperate, another problem was that in order to study people's moods it would be necessary to have subjects who can feel relaxed enough when they go into the experimental lab to freely express what they are feeling through their behavior or to freely communicate and act on their desires through their actions "verbal and non-verbal".

I pointed out that this would be necessary because psychologists can only study people's moods in so far as they are reflected in people's behavior. I indicated that as far as I was concerned, when they went into the laboratory for the actual study, any honest expression or action on either participant's part was not only to be tolerated but in fact was to be considered helpful and was to be encouraged, no matter how "mature or immature, reasonable or unreasonable, appropriate or inappropriate,

normal or abnormal, such feelings might be". I stated that my interest was in "understanding" students' feelings, not judging them."

This acceptance and encouragement of honest emotional expression was reiterated several times throughout the procedure. Obviously its real intent was to convey to the subject the idea that any behavior on the part of either "student" was not only going to be tolerated but was in fact sanctioned, provided it was an honest expression of the "student's" feelings.

Following the initial statement of this position, I went on to point out to both "students" that I understood that what I was going to ask them to do later in the laboratories, might be something most people might find it difficult to do. I said that it was my contention that people often do not act the way they feel in real life. I then again asked both participants if either of them agreed with this contention, and if so, did they have any ideas why this might be? The confederate answered that "sometimes people think that if they did anything they wanted or even told others how they felt, they would be regarded as weird, abnormal, etc." I then asked what he thought and adding the subject's comments together with those of the confederate's, I pointed out some major reasons why people don't always do what they want. I pointed out that many people might be afraid of the consequences; some people might feel embarrassed or ashamed if others really knew what was on their minds; some people might think they were sick if they did just as they pleased; and finally some people might feel guilty as a result of doing what they felt like.

Having brought up these issues, I then presented the other side of what was in effect, a two-sided argument in an attempt to convince the

"students" that if they were honestly to express themselves, they had really nothing to worry about. As previous studies have shown, a two-sided argument would be very effective with this type of population (Musseneta: 1975). In debating these points, I mentioned that the laboratory situation they would be going into comprised a situation where factors that might normally inhibit someone from honest emotional expression would not be operating. I pointed out that everybody involved in these studies would be strangers to each other, and that each of the participants was not dependent on the other for the attainment of any realistic goals. I further pointed out that each of the participants was not emotionally involved with the other or with me or with my assistant and would not be seeing each other again when the experiment was over. Moreover, I pointed out that any data they provided for me or my assistant was confidential and would not be used without their written permission. Finally, I stated flatly, that as far as I was concerned, there would not be any adverse consequences from me or from the school for any actions, feelings, etc. that were expressed, providing no laws were broken. In fact, as far as I was concerned, I expressed the feeling that I felt it was unfortunate that people cannot be themselves more in real life. I stated that in any event, the more that "students" could feel free to be themselves in this laboratory, the purer the results of this study would be.

At this point, I stated that I felt that even though the two "students" had some intellectual appreciation for the unique lab situation they would eventually be going into, I felt it would take some getting used to because it was not like most every day life situations. I said that for this reason, I would like them to try to practice being

emotionally honest with me and with each other before they went into the laboratory. By the time they went into the actual laboratory for the study they would "feel free to do and say whatever they wanted". I stated further, that I realized that in order to allow them to get used to being honest, as far as I was concerned, from this point on, the "students" were free to do, say and act whatever way they wished even before they went into the actual lab for the study.

The real purpose for the statements, of course, was to further sanction, justify, and make legitimate, any subsequent interaction that might take place between the "students". At the same time, of course, I made it seem that their behavior in the "laboratory" and not in the "office-waiting room" would be the real focus of interest in the study. At this point in the procedure, I said I did not believe that a mere statement on my part, that it was okay to "be themselves" would be enough for them to take advantage of the unique opportunity they would find in the laboratory or to get used to the unique setting prior to going into the laboratory. For this reason, I stated that I had decided to engage them in a verbal discussion with the hope that they could use the occasion to loosen up a bit before they entered the laboratory. I pointed out that if they honestly felt they did not wish to participate in the discussion or answer any question, then that's exactly what they should do and say. I went on to throw out some personal questions and open-ended statements. These standardized questions were, "I am here now because"; "I like psychology because"; "I like privacy because"; "My worst habit is"; "I think it's too bad that"; for each of these questions, I gave my own, alleged personal answer which seemed to be highly candid and forthright in nature. I sought to elicit answers

from the confederate and subject. In fact, both I and the confederate, in reality, had a set of prepared monologues to follow and the order of rotation in answering the questions was set in advance. Moreover, during the discussion session, it was preplanned that I would put my feet up on the couch and that the confederate would object to this, pointing out that while ordinarily he would not say anything to an instructor, in this case it bothered him and he decided to say something. I apologized, removed my feet and reinforced the apparent self-assertion on the part of the confederate by saying I was glad that he, the confederate, was able to utilize and feel free enough in this atmosphere to tell me just what he felt. (For a full statement of this scenario, (see Appendix D).

The purpose of this discussion was, of course, to concretely induce the context of low social restraint. Several deceptive elements were present which were aimed at fostering this objective. Through my "personal" answers it was hoped that I would convince the subject that not only was I sanctioning the possible violations of norms but in fact, I practiced what I preached.

Through sanctioning and reinforcing the confederate and subject for "honest" behaviors, I hoped that we could put the subject on a new reinforcement schedule where "honesty" would be a valued goal. Further, through the preprogrammed responses of the confederate, it was hoped that the confederate would be used as a model for "honest" behavior. It was hoped that the acceptance I offered to the confederate for his "honest" behavior would have the effect of persuading the actual subject to test out the limits of the situation and see that on a concrete level there was in fact no realistic reason for not doing and saying just what he

wished. Thus, it can be seen that I brought to bear upon the subject the influence of modelling (Bandura, 1967, 1972), enforcement (Skinner, 1953), group process (Asch, 1951) and unconditional regard (Rogers, 1959), in order to break down normal social restraints and induce a context of low social restraint prior to the actual instigation by the confederate. Following the active inducement of low social restraint, I then said that I hoped that everyone "loosened up" and that they would all use the few minutes remaining before they went into the laboratory to practice being as emotionally honest as they cared to. I then stated it would be necessary to readminister the mood scale to get a more stable, accurate estimate of each person's initial mood state prior to going into the laboratory.

I then administered the MAACL once more, telling them to indicate how they felt now and during the last five minutes. When completed, these forms were then collected. At this point, under the condition of normal social restraint, I asked them to try to finish filling out the preliminary biographical forms and indicated that following this, they would be going into the labs for the study. Likewise, as they began finishing up the biographical information form, I said that I was going to use the opportunity to go out "to the john". I then left the room and turned on the video equipment. Shortly thereafter, the confederate initiated one of the nine stimulus situations. We will now discuss the details concerning the administration of these nine stimuli.

Stimulus Situations and Their Presentation

At this point in the procedure, the confederate initiated one of the nine aggressive or constructive stimuli in random order. As mentioned previously, these nine stimuli ranged from extremely aggressive to extremely constructive in nature. We have previously listed the nine stimuli,

and the reader is referred to the Appendix for a detailed presentation of them (see Appendix C, p.255).

For purpose of the present discussion, the period of time where the stimulus situation was presented and the subject's response was observed and video-taped to it, can be broken down into three sub-phases. The first phase consisted of a short period of time which began when I left the room to go to the john and ended when the confederate initiated one of the stimulus situations. The second phase, began with the initiation of one of the stimulus situations by the confederate and ended with the termination of the subject's direct immediate response to it. The third phase began with the end of the subject's direct response and ended with my return to the room. Each of these phases will now be discussed in some detail, but the reader is referred to the training manual in the Appendix for a fuller elaboration of them. (Appendix C).

Phase 1

This phase consisted of a one minute interval, beginning when I left the room and ending when the confederate began his planned stimulus behavior. The rationale for the inclusion of this phase was to avoid having the confederate's behavior contemporaneous with my leaving the room and arousing undue suspicion in the subject's mind as a possible result. An additional reason for allowing this short period of time, prior to the instigation by the confederate, was to allow time for him to maneuver closer to the subject on the couch on which they were both seated. During this period of time, the confederate was instructed to avoid any interaction on a verbal or non-verbal level with the subject that would in any way affect the subsequent interactions. Specifically, he was to keep his behavior as uniformly neutral for each subject as

possible and to continue to act like a naive subject would. In order to accomplish this, the confederate was instructed to appear to be concentrating on the biographical information form that they had both been encouraged to fill out and was not to make any spontaneous comments. He was to answer any question that was put to him by the subject in as neutral and non-committal manner as possible.

In order to get closer to the subject on the couch, the confederate was instructed to take out a piece of scrap paper, get up and throw it in the trash can, and then sit down again as close to the subject as possible without touching him. When the confederate sat down again, the video-tape equipment was to be turned on and Phase 2 would begin. It is to be noted that the one minute length of Phase 1, was not a hard and fast rule but was a guideline given to the confederates to follow. While we wished to make the Phase 1 period as uniform in length for all subjects as possible, it was realized that there were possible situations where the Phase 1 period might have to be slightly lengthened. However, the confederate was urged to limit the Phase 1 period as close as possible to one minute in length.

Phase 2

This phase began with the initiation of one of the nine aggressive or constructive acts on the part of the confederate. It was deemed to have ended when the confederate ceased his preplanned stimulus behavior and the subject had been allowed to cease any direct immediate response to it he may have made. I made the judgment as to where the direct response period ended as I observed the ongoing interaction from behind the one-way mirror. If the confederate had ceased his preplanned insti-

gation and the subject showed no further direct response to it, the Phase 2 period was deemed to have terminated. An important goal for the confederates during this period was not only to accurately reproduce each of the nine stimulus situations, but moreover, to reproduce each of the situations in a uniform manner. The need to maintain uniformity in the acting out of the various stimulus situations, was necessary to avoid the possibility that the actor's behavior would become contingent on the subject's responses or behavior, rather than visa versa. If the actor's behavior did indeed become contingent on the subject's behavior, it was possible that any tendencies to reciprocate on the subject's part might be wiped out as a result. For example, if confederates chose to be more aggressive to less intimidating subjects, and less aggressive to more intimidating subjects, very little aggression on the part of the subjects would probably result.

To insure uniformity of the confederates' presentation of the various stimulus situations, the confederate was trained to consistently produce the same stimulus regardless of the subject who was assigned to him. The importance of his doing this, was continually stressed to the confederate. Furthermore, confederates were instructed that they were to produce the same stimulus situation regardless of the response, verbal or otherwise, of the subject. Insofar as they seemed believable, they were to attempt to carry out the same instigation, regardless of the subject's responses to it. To insure further uniformity in their behavior, the training manual specified the exact lines and facial and bodily posture the confederates were to assume for the stimulus situation (see Appendix C). Furthermore, each stimulus situation had a time period

allotted to fit it for its production. These periods of time ranged from between one and a half to five minutes depending on the stimulus. To further insure uniformity in the behavior of the confederates, they were trained to adhere to the time limits specified in the training manual for the production of each stimulus situation. This was to avoid having confederates unnecessarily prolonging or shortening their instigations as a function of the particular subjects they were with. It also served the added function, of keeping the stimulus situations roughly equivalent in length, and keeping the length of the stimulus situation short enough to be practical for the subsequent judging of the video-tapes made of them by college students.

Phase 3

I deemed the Phase 3 period to have begun when I judged the confederate to have finished his preplanned instigation and the subject to have finished making any immediate direct response to it. From this point, we continued video-taping the behavior of the participants for the next two and a half minutes. During this two and a half minute period, the confederate had been instructed to become passive and was not to instigate any further actions. However, he was to respond and to facilitate any actions the subject might choose to initiate. While the behavior occurring during this period of time was not our major focus of concern, we thought it would be interesting to see the degree to which subjects would freely and spontaneously reciprocate behavior in the absence of further instigation by the confederate.

It was deemed unnecessary to include this period for some of our stimulus situations. These situations were primarily non-verbal instigations on the part of the confederate and involved three of the nine

stimulus situations. These were stimulus 5, 6, and 8 (see Appendix C, p. 258). The reason for the non-inclusion of a Phase 3 period for the stimuli, was that due to their non-verbal nature, we felt that the subjects had ample opportunity to make a spontaneous response to them and because they required a relatively long period of time to enact. As such, the non-verbal instigation data was similar to the verbal spontaneous data since the subject had similar freedom to respond, and to the verbal direct response data in that the behavior was elicited by the confederate. Accordingly, because of this dual similarity, I decided to have the subject's responses made during the actual instigations considered for purposes of analysis, as either direct responses or spontaneous responses. For this reason, no special period of time needed to be allotted in these cases to obtain possible spontaneous responses.

Getting back to the more verbal stimuli, mentioned above, we allowed two and a half additional minutes following the subject's direct response to the instigation, for any spontaneous responses on the part of the subjects. The instructions to the confederates indicated that once they had terminated their preplanned instigation, they were not to spontaneously initiate any further action but instead were to remain responsive to any verbal or non-verbal behavior that the subject might initiate. They were further instructed to behave in such a way as to allow the subject the easy opportunity of initiating such behavior. For example, the confederates were instructed not to seem preoccupied with any element of the room but to look around the room in a casual manner as a naive subject would do. Additional detailed rules for accomplishing this were spelled out for each confederate (see Appendix C, p.255). For example,

if the subject seemed to be giving the confederate a "dirty look" or seemed to be displaying some unusual behavior, the confederate was allowed the option of inquiring in an open-ended manner as to the reason for the behavior.

At the end of the two and a half minute period, I terminated the interaction by coming back into the room. It should be noted that in some cases, due to mechanical difficulties with the video-tape equipment or because of ethical considerations, we were unable to collect the video-tape data for the full two and a half minute period. However, (since upon close inspection of the cases where this occurred, the problems were deemed to have occurred randomly) this deviation is to be considered random error in our analysis.

Training Procedure for the Confederates

As mentioned previously, we had produced a training video-tape of each of the nine stimulus situations, as well as an audio-tape of them and a training manual (for Manual, see Appendix C). As can be seen, the training manual described for each confederate exactly what he was supposed to say, how he was to position his body, what affect he was supposed to display in enacting each stimulus, what he could or could not do, how much time he was supposed to spend at each point in the procedure, etc. In addition, to insure that the actor would not unexpectedly alter his behavior in response to the behavior of a given subject, the training procedure also briefed the actor on what he was to do and say if the subject tried to become a "stimulus" for him. After each potential confederate had reviewed this material in depth, he was asked to role-play these stimuli with a volunteer, who improvised unplanned responses. Those potential confederates who could not adequately produce

the range of aggressive and constructive behavior or effect that was required, or who were otherwise deemed unable to enact the stimuli properly, were eliminated. Confederates who were finally selected were then asked to review the training video-tape. Following this, we spent considerable time rehearsing each actor as to what he was to do and say from the time the subject entered the room for this study, to the time I came back into the room after the instigation. After we were satisfied that the actor was competent and could satisfactorily perform his role, the confederate was then asked to perform one or more of the extreme stimulus situations with an actual naive subject, prior to his participation in the study. Upon satisfactory completion of this task, our confederates were now ready to serve as social stimuli.

Procedure for Leaving Waiting Room

When I returned, I asked both "students" if they had completed filling out the biographical sheet I had given them. I informed them that there would be ample time to complete it later. At this point, however, I stated that I was interested in getting them into the laboratory to begin the study. I said that they would be leaving for the laboratory in just a minute or two but, before they did, I wanted them to fill out just one more copy of the MAACL (MAACL 3). The rationale for filling out the third MAACL was similar to that offered previously. The instructions were also similar, except the subjects were asked to check off all the adjectives that described their mood and feelings "during the last five minutes". Once this form had been completed and returned to me, I then announced the subjects would now be splitting up to go into the laboratory. I said that one of the subjects would work with me and the other would work with my assistant. It was pre-arranged

with the confederate that the real subject would be allowed to get up first and that I would ask him to come with me, while the confederate was to stay behind and would be "introduced" to my supposed assistant who would work with me. At this point, the subject went with me into the adjacent lab-room, where the collection of self-report data occurred. Let us now turn to a discussion of the self-report data that was collected.

Procedure for Collection of Self-Report Data

One problem in the methodology of the experimental research is to be reasonably sure that the experimental manipulations have had the desired effects on the subjects and have affected the subjects in the ways that were intended. To help ascertain the effect of our manipulations on the subject sample, it was deemed desirable to collect self-report data from the subjects as to their perception of the various elements of the situation they found themselves in, their perception of whether or not the situation had any effect on them, their perception of how the different elements of the situation affected them, and finally their perception of their own behavioral response to the situation and the factors that may have affected it.

Several goals were kept in mind in designing the procedure for the collection of this data. It was deemed desirable that the data be collected as soon as possible after the experimental manipulations had been performed so that the subjects would have to rely as little as possible on memory. At the same time, we wished to avoid the possibility that demand characteristics of the experiment would influence the subject's self-report responses. Finally, we wished to avoid the possibility that the collection of the self-report data would arouse excessive suspicion

concerning the true nature of the experiment or would be conducted in a manner that would make subjects excessively defensive about answering the questions honestly.

Bearing these considerations in mind, several different types of self-report data were collected. One way we were able to obtain self-report data was through the three administrations of the MAACL as discussed previously. The actual reason for the use of this scale was to provide for self-report data on the subject's mood and feelings, immediately following the two main experimental manipulations (Context and Stimulus Situation). As such, we were able to get a self-report from the subject less than a minute following each of the experimental manipulations. By providing subjects with a strong, deceptive rationale for the use of the MAACL, and by administering the MAACL two times prior to the onset of the confederate's behavior, we felt that it would be possible to gain this data without excessively arousing the subject's suspicions about the true nature of our study, and the real reason for the use of the MAACL. Additionally, by having three administrations of the mood scale, we were able to obtain three post-measures of each subject's mood change following each of the two main experimental manipulations.

As for the collection of other self-report data, we felt the safest time to collect it would be after all the experimental manipulations had taken place. As such, we would have to rely somewhat on the subject's recall of the previous events and experiences but we traded off this limitation for the alternate choice of interfering with the experiment itself.

To collect this self-report data, a three-part self-report questionnaire was constructed. Its contents and the procedure for data collection will be described shortly.

In designing this self-report questionnaire, the main goal was to provide for independent assessment of the effects of our experimental manipulations on the subjects. However, in order to insure that the demand characteristics of the experiment minimally contaminated the subject's self-report data, a deceptive rationale was provided to the subjects for the use of this questionnaire. They had been deceptively told that before they could begin the study, (they were still under the impression that there was a study going to be taking place in the "lab" that they had just entered), it would be necessary to have a basic idea of their overall mood. We explained as mentioned previously one way of establishing this has been to administer the MAACL three times. However, it was now stated that sometimes people's scores on this scale might be affected by external factors as well as their general mood state. Several examples of such external factors were cited. Subjects were told because of the susceptibility of the MAACL to these influences, we needed to know about any "external" influences that may have affected their MAACL scores so that we could compensate the MAACL scores for them. They were told that for this reason, we would be administering the "mood scale calibration packet" which was specially adapted for this study. They were told that as soon as they finished the packet, the study itself would begin and take just five or ten minutes to complete. I indicated that some colorful pictures, left on the table, were part of the study they would be involved in after they finished filling out the packet.

In reality, "the packet" was a self-report questionnaire, designed

to assess the influence of our manipulations on the subject. As can be seen in Appendix (see Appendix F, (p. 276), G (p. 283), H (p. 293), this self-report questionnaire basically consisted of three parts.

The questions in the first part of the self-report questionnaire, were designed to be open-ended questions such as "Describe your impression of the experimenter" or "Describe any thoughts you had before coming down for the study today". The reason for using such open-ended questions initially was to try to minimize the influence of demand characteristics on the subject's responses. Through the use of part one, I hoped to gather a "spontaneous" self-report of the subjects, while insuring that I was not subtly suggesting the responses to the subjects that they didn't really feel. Moreover, since the questions covered the period of time from the point where the subject had signed up for this study to the point where he entered the "lab" room and began filling out this self-report questionnaire, the subject was kept minimally aware of what behaviors were the real focus of my interest.

The questions, in part, attempted to elicit what impressions the subject had about me and the confederate, what each did and said, how each behaved, what the subject's mood was like and what affected it, and any other miscellaneous comments the subject cared to make. Subjects were encouraged to answer all items and I asked them to elaborate honest clear statements. Each of the questions was basically to be answered four times, each time with reference to one of four delineated time periods.

The first time period began when the subject signed up for the study and lasted until five minutes after he walked into the "office-waiting room" (and filled out MAACL 1). The second time period began

where the last one left off and ended when the practice Likert scales were given out in the normal social restraint condition, or when the personal discussion began in the low social restraint condition. The third time period was the period of time during which the Likert scales were being filled out or the discussion was taking place. This period was terminated by the filling out of MAACL2. The last time period began when the subject filled out MAACL 2 and ended just before they came into the "lab" for the supposed study.

As can be seen from the Appendix, the subject answered all the questions concerning time period one first, and then proceeded to answer questions concerning time periods two, three and four in that order, as discussed; it was hoped that this procedure would avoid giving subjects definite clues as to what behaviors were the real focus of my interest.

When part one was completed, the subjects went on to fill out part two of the self-report questionnaire. Part two consisted of eight self-report scales that the subjects were to use. As will be seen, these scales basically asked subjects to rate the actions of the other "student" who had been in the room with them and to also rate their emotional reactions to the other "student's" actions. Subjects were told that we were including these scales in the "mood calibration packet" on an experimental basis to see if in the future they could be substituted for the "older procedure" of asking subjects to give their written descriptions of external factors that may have affected their mood scale responses. Subjects were told that since the scales in part two were experimental in nature, we were not going to have them fill out the scales for all four time periods, nor were we going to have them fill out the scales with reference to all possible external factors that may

have affected their mood scale data. Rather, they were told, that it merely would be necessary to confine their ratings to the last period of time (in actuality the period of time during which the instigation by the confederate took place) and were to merely indicate ratings for the behavior of the other "student" and their emotional reaction to it, if any. Subjects were told that it was decided to confine their ratings to this particular time period, because it was probably the period of time that they remembered most clearly since it was the most recent. They were also told that to assess the validity of these scales, we would be correlating their ratings with the written information they had given us previously.

The eight scales used and their accompanying instructions are located in the Appendix (see Appendix G, p. 283). The first two scales dealt with the subject's evaluation of the behavior of the "other student"; they were asked to indicate on scale one, how aggressive or constructive they found his behavior to be; on scale two, how normal and rational or how abnormal or irrational they found his behavior to be. Scales three, four, and five dealt with their emotional and motivational reactions to the "other student". Scale three asked them to indicate how helpful and constructive they really wanted to be to the "other student" after he did whatever it is they previously described him as doing. Scale four, asked subjects to rate how much aggression they really wanted to carry out toward the other subject following the other subject's behavior. Scale five asked them to indicate what they felt as a direct result of the "other student's" actions on a scale going from extremely happy and gleeful to extreme anger and rage. Scales six, seven and eight dealt with the consequences they anticipa-

ted, or thought they anticipated, would ensue if they did what they really felt like doing or wanted to do to the "other subject" at that time. Scale six asked them how harmful they thought the "other subject" would have been to them, if they had done what they really wanted to do to them during the last time period. Scale seven asked them how aggressive they thought I would have been towards them if they had done what they really wanted to do to the "other subject". Scale eight asked them how aggressively they would have behaved towards themselves, if they had done what they really wanted to do to the "other subject".

With regard to the construction of these scales, scale one, scale two, and five were nine point bipolar scales with a neutral point, similar in construction to the aggressive constructive scale mentioned previously. The other scales were five point scales. Scale values ranged from none to extreme. The extreme points for these scales were given concrete definitions and examples, identical to those used to define dimensions. Subjects were given the option on all scales of extending them as far as necessary to make a satisfactory rating, if the defined limits of the scale proved unsatisfactory. They were also allowed to make more than one rating on a scale if they felt the need to do so. For those few subjects that did this, averages of the two ratings made were computed and used as representative of their overall ratings.

After each scale was used by the subject, we asked each subject to indicate the rationale and basis for his rating on this scale and this was written down in the "comments" section next to each scale.

Following the completion of these scales in part 2, part 3 was administered. (see Appendix H, p. 293) This part began with another set of open-ended questions that basically asked subjects if there was

anything they felt they left out in their previous answers that they felt might be related to their performance on the mood scales given to them previously.

After they finished completing these questions they were told that to further improve the validity of the mood scale, we were going to look at each of the subject's previous scale responses to see if the mood they had indicated they had been in was indeed recognized by the "other student" in the room, based on his self-report about them. Under this pretext, they were then asked a series of questions having to do with whether or not they thought their mood feelings and impulses were fully reflected in their behavior. If the subject felt he had not done or expressed what he really felt like doing or expressing, he was asked what he would like to have done, and how intense his behavior would have been. He was also asked specifically whether he thought the instructions to his group in any way affected his behavior and whether or not he would have behaved similarly or differently if he was not waiting to be in a study or was in a different situation with a different student, or a different experimenter. The subject in each case was encouraged to elaborate his answer. For some of these questions, the subject was asked to give a "yes", "no" or "maybe" answer. These latter discreet responses could then be quantified on a one to three scale for purposes of later analysis.

Following these questions, the next question asked subjects if they believed the "other subject" had behaved in such a way as to affect his mood and if so, if he felt the "other subject" was aware of what he was doing, intended to do it to him, or intended to harm him through this action. The question was of the multiple choice variety and allowed for five different choices. A sixth space was provided for subjects who

felt none of these choices were adequate. For the few subjects who wrote in their response in the sixth space, a judge read them and picked a category that best seemed to represent their answer. As with the previous questions, a way was developed to translate the subject's choices into quantitative form. The choices the subjects picked were seen to reflect the subject's perception of how intentionally harmful they thought the "other student" had been to them, and the multiple choices were conceived as lying along equal intervals of this scale dimension.

Following this question, more open-ended miscellaneous questions were asked. Following them, the last section of part three told subjects that "sometimes subjects come into a study with certain preconceived notions of what the study is about; it further stated that in order to calibrate the mood scale properly before the study began we would like to know if they had any suspicions or preconceived ideas that might have affected their mood scale response during the last time period." It was mentioned that since sometimes the suspicions might affect the person's mood without him being aware of it, it was important that they reported any suspicions or preconceived notions they had had about the procedure thus far. They were then specifically asked if they had noticed that there were mirrors in the "office-waiting room" and if they were suspicious that they were one-way. They were also asked if they were suspicious that they were being observed or recorded during the time when I wasn't in the room. They were asked if they had any suspicions about my expressed reasons for leaving the room to go to the john. Finally, they were asked if they were at all suspicious that the "other subject" was really a confederate. Subjects were given the option of "no", "maybe" or "yes" in answering each question. In addition, for the last

three questions, subjects were asked to rate how suspicious they were on a six point scale running from "not at all suspicious" at one end to "convinced" at the other. Whenever subjects reported any degree of suspicion, they were then asked to state what was the reality or fantasy basis for these suspicions and were encouraged to indicate when, in the procedure, they thought they had first become suspicious.

The self-reports of degrees of suspicion lent themselves to ready quantification. For purposes of the present study, the data of any subject who reported being convinced that one of the aforementioned deceptions had indeed taken place, had been deleted from our analysis. In such cases, a new subject was run in his place under similar conditions and his data was included instead.

With the completion of part three, we were now ready to actually debrief the subject. We will now look at the debriefing procedures involved in the study.

Procedures for Debriefing

At this point in the procedure it was necessary to inform each subject as to the true nature of the study and to elicit his reactions to it. There were several reasons for this. First, it was felt that it would be unethical to have subjects leave with bad feelings about themselves, the experiment, etc.. Second, it was hoped that some additional information might be gathered from the subjects that they had not previously thought of or mentioned prior to a full explanation of the procedure. Third, it was desirable to secure the cooperation of the subjects in not discussing the study with any of their classmates who might participate in it in the future. Lastly, we needed to get the written permission from each subject to allow the video-tapes to be used in our

study.

In order to carry out the debriefing, each subject was informed as to the true nature of the study. His feelings were explored with him briefly and we made a considerable effort to point out the essential normality and/or rightness of his actions. He was told about the video-tape procedures, the one-way mirror, and was told that the other person was a confederate, etc. He was offered an opportunity to review the video-tape and to get the complete results of the study. The use we would be making of his data was explained to him and we obtained his written permission to use it. Following this, we introduced him to the confederate who shook his hand and reassured him there were no hard feelings.

The subject then came back into the lab room with me and finished filling out the biographical data sheet. He was also asked if he had any comment or questions about the procedure and these were explored with him. Following this, he was thanked for his participation in the study, given my telephone number should he wish to contact me further, and dismissed.

Procedure for Rating of the Video-Tape Data

As will be recalled we had made a video-tape of each subject and interaction with one of the confederates during the period of time when I left the room. As will also be recalled, it was during this period of time that the confederate had instigated one of the nine aggressive or constructive acts and the subject had been given the opportunity to respond to it. A procedure now had to be developed to extract some numerical information based on the video-tape data allowing us to assess how aggressively or constructively the confederate and subject had behaved towards each other during this period of time so as to provide data

relevant to the hypothesis under test. In order to extract this numerical information, it was necessary to employ the aid of judges who were asked to view each video-tape segment and to rate the behavior of the participants along one of several scale dimensions.

In developing the procedure for judging of the video-tapes, one main goal was to insure that the judgments of the video-tapes by the raters were as objective as possible. To achieve the most objective ratings possible, we enlisted the aid of judges who were not connected in any way with our research project. Furthermore, several different judges were asked to make each rating. Finally, each judge only made a few ratings in order to make it less likely he would guess the hypotheses under test and thus be swayed by "experimenter bias" when making his ratings.

A second goal which we kept in mind during the design of the judging procedure was to insure as much randomness in the selection and assignment of raters as possible. This randomness was needed to minimize the possibility of selecting an unrepresentative sample of raters or systematically assigning certain raters to rate certain stimuli, use certain scales, etc.

A third goal that we had in mind in designing the procedure for the judging of these video-tapes was to keep certain ratings as independent as possible from others, to avoid the possibility that raters would bias one set of ratings as a result of having made another set of ratings. To achieve maximal independence of these ratings, it was deemed desirable to assign different groups of raters to make the ratings in each case. (see Figure 3, p.121). Thus, although the behavior of each participant was rated along several different scale dimensions, any given rater was

only asked to evaluate the behaviors in question along one scale dimension. This effectively prevented the ratings of the behaviors along one scale dimension from contaminating the ratings of the behaviors made along the other scale dimensions. Similarly, for any given interaction sequence, one group of raters was asked to rate the behavior of the confederate on a given scale dimension while another group of raters was asked to rate the behavior of the subject on the same scale dimension. In this way, the ratings made of the confederate's behavior could not directly contaminate the ratings made of the subject's behavior. Finally, to make the ratings of the behaviors occurring during Phase 2 (the direct response phase) independent of the ratings of behaviors occurring during Phase 3 (the spontaneous response phase), different groups of raters were to rate the behaviors occurring in each of these two phases. (see Figure 3).

While it was desirable to make our ratings as independent of one another as possible, there are certain practical limitations that limit one's ability to do this. For example, one issue that we have just mentioned concerned the need to keep the ratings of the confederate's behavior and the subject's behavior in any given interaction independent of one another. While assigning different raters to make each of these ratings was a positive step in this direction, there was still the problem whether the video-tape data that the raters were to use as a basis for their ratings contained the behavior of both the subject and the confederate on the tape. In order to insure that the ratings of the confederate's behavior be made independently of the ratings of the subject's behavior in each case, it would not only be necessary to have different raters making each set of ratings, but moreover, it would be necessary to insure

FIGURE 3Diagram of Breakdown of Sample Video Segment for Judging Purposes

	<u>(a) Verbal Instigations</u>	<u>(b) Non-Verbal Instigations</u>
Direct r subsegment	<u>Item 1</u> Active instigation of one of nine stimuli by the confederate.	<u>Item A</u> Prolonged active instiga- tion of non-verbal act by the confederate. (This Item included among all Item 1's for purposes of analysis in this paper).
	<u>Item 2</u> Subject's direct response to the instigation.	
Spont. r subsegment	<u>Item 3</u> Passive-facilitating behavior of the confederate for 2½ minutes following the end of subject's direct response to the confederate's instigation.	<u>Item B</u> Response made by subject to the instigation during the entire period the insti- gation takes place. (This Item included among all Item 2's and all Item 4's for purposes of statistical analysis in this paper).
	<u>Item 4</u> Any spontaneous behavior on the subject's part occurring during the 2½ minute period just mentioned.	

NOTE: Each Item was rated by different groups of judges to insure independence of ratings.

Groups rating Items 3 and 4 of any given segment were not exposed to Items 1 and 2 of that segment. Items A and B were also rated by different groups of judges, as were Items 1 and 2; 3 and 4.

Each Item was rated as to how aggressive-constructive the behavior on the videotape was, how normal and abnormal it was, and how emotionally honest or dishonest it was by a given group of judges. One-third of the group rating that Item used each of these scales.

that each group of raters was only exposed to either the confederate's behavior or the subject's behavior, but not both. The technological procedures needed to accomplish this were not only highly impractical, but the product that would thus be obtained would be so cut up and dissected that the behavior of each of the participants would be reduced to unintelligible gibberish. Instead of this, an alternate procedure was decided upon, whereupon each group of raters would be given a set of instructions designed to make them focus on the behavior of one of the participants and to ignore the behavior of the other participant. At the same time, they were helped to do this by a cardboard masking which was placed over the video-tape image of the person whose behavior and verbalization they were to ignore.

A second problem that emerged as a result of practical limitations, concerns the need to keep the ratings made of one action, independent of the ratings made of all the other actions. While ideally, we would have as many groups of raters as we had different actions to rate, in practice, we had to have each group of raters make several ratings of different subjects in interactions with different confederates under different stimulus situations. Since we could not insure that the ratings would be independent of one another in this regard, the issue was dealt with by randomizing the presentation of different actions to different groups of raters, and furthermore, by randomizing the order of presentation of the actions to any given group of raters. Any contamination of ratings present would thus tend to cancel itself out, and could then be treated as random error. Through the use of this procedure, as well as through the instructions to raters to try to be as objective and non-biased as possible, it was felt that any contamination of some ratings by other

ratings made by the same rater would be minimal.

Description of procedures for video-tape judging

Now that we have reviewed some of the goals that were kept in mind in our design of the procedure, let us describe the judging procedure in some detail.* As mentioned previously, it was decided it would be necessary to secure the services of a large number of judges. In order to secure as large a pool of judges as possible, it was decided to enlist the volunteer aid of college instructors and their students.

To keep our pool of judges relatively homogenous and to insure that they were reasonably well-motivated, as well as for other practical reasons, it was decided to contact the staff and students of the social science departments of colleges within the greater metropolitan area. It was possible to enlist the cooperation of three social science departments at three different community colleges. Individual instructors at these schools were contacted and arrangements were made to come into their classes for one contact hour. All in all, 41 classes of students were utilized. In terms of numbers of students, 883 students of both sexes served as judges for the video-tape data.

This pool of judges seemed satisfactory for several reasons. First, they were not connected in any way with our research project, but yet, because of their interest in social science, were reasonably well-motivated. Further, because of the large number of judges we enlisted, it was possible to have several different judges rate the same video-tape segment, thus allowing any personal biases on the part of these judges to cancel out. Further, due to the large number of college students participating as judges of these video-tapes, we could be reasonably confident that the judgments we obtained would be fairly representative of those that

Note: See Appendix L for evidence supporting the validity of the judging procedure.

might be made from the parent population of community college students. Moreover, by having such a large pool readily accessible, the problem of intrarater contamination of ratings would be minimized. Finally, by having this large pool we would be able to achieve maximal independence of the ratings obtained, by assigning different classes of judges to make different ratings. We could also keep our raters minimally aware of the hypothesis under test, by exposing them to relatively few videotape segments to rate, thus making it much more unlikely that subtle demand characteristics of the study would creep into and affect their ratings.

In order to keep the ratings we obtained maximally independent of one another, the next job was to devise an orderly procedure for determining which ratings would be made by which class of raters, and for insuring that ratings we wished to keep maximally independent from one another were indeed made by the different student raters. In other words, a procedure needed to be developed to determine which portions of which video-tape segments would be shown to any given class of raters, prior to going into that class. Once this determination was made, a further procedure needed to be developed to most efficiently present these portions to the particular class that was doing them.

To further clarify the discussion of the judging procedure, it would be useful for the reader to refer to Figure 3, (p.121). As can be seen, ratings needed to be obtained for either two or four subdivisions of each video-tape segment, depending on the situation involved.

The first step in our judging procedure was to recopy the video-tape segments onto new video-tape. It was decided to recopy these video-tape segments in random order. This was to insure that among all the groups

of judges that would be participating as raters, any given group was equally as likely as any other to view any given confederate, any given subject, any given stimulus situation, etc.. It was also done to insure that for any group of judges, insofar as possible, the order in which they would be asked to rate segments, and the stimulus situations which were represented in them, would be as random as possible. As mentioned earlier, it was felt that this procedure would have the effect of treating any intrarater order effects as random error.

Once the video-tape segments were randomized in this manner, they were then catalogued and indexed so that they could be easily identified prior to each classroom viewing. One or more of these recopied video-tapes was selected at random for use with that particular class. Following this viewing by that class, the tape was then returned to the pool of those available for viewing. Those portions or segments that already had been rated of course, were deleted from the index of that tape and were not subsequently rated by any other class.

Once we had recopied these video-tape segments onto new tape in random order, the next job was to insure that the ratings of the confederate's behavior and the subject's behavior in interaction with them, were made independently of each other. To help insure this, the principle was adopted that any given class of judges would be asked to rate either one or the other person shown in any particular video-tape segment or sub-division, but not both parties. A table of random numbers was used to determine which of the two individuals on each of the given video-tape segments or sub-divisions, would be rated by the particular class that was to rate them. Of course, when the tape was later shown to another class, the individual that had not received a rating previous-

ly, was now rated.

It will also be recalled that we wished to insure that the ratings of the behavior during the direct response phase, and the spontaneous response phase, were made independently, when those two phases were present on any video-tape segment. In order to insure this independence of ratings, a procedure was adopted whereby any given class of judges would only be asked to rate one of the two phases but not both. As above, a table of random numbers was used to determine which of the two phases would be rated by any given class. It was likewise planned, that the phase that was not rated at first, would be rated by another class at a later point in the judging procedure. For those video-tape segments that did not require separate ratings for direct and spontaneous behavior, the behavior of either confederate or subject, was rated in total, the first time it came up in random sequence. The other party of course, was later rated the second time the segment came up to be rated. It is to be noted, that using this procedure, raters of the spontaneous phase had no way of knowing what had transpired during the direct phase, since we prevented them from seeing the latter video-tape sub-segment.

To help minimize the possibility that the raters would be able to determine the hypothesis under test, and thus be influenced by subtle demand characteristics in making their ratings, three final guides were adopted in the preparation of the video-tapes for presentation to these classes. Basically, these guides were devised to prevent the judges from becoming aware that the confederates were indeed confederates and the behavior they initiated was set-up.

The first guide adopted was that insofar as possible, any given

group of judges was not to view the visual image of the same confederate enacting two different stimulus situations. The second guide was that insofar as possible, the same group of judges was not to be able to view the visual image of the same experimental stimulus being enacted by two different confederates with the exception of the neutral stimulus situation. The third guide was that any group of judges was to view certain pairs of aggressive stimuli which seemed somewhat similar in content. Thus, no given group of judges was to view both stimulus 7 (telling someone to move over on the couch in an annoying fashion) and stimulus 9 (same as 7 but cursing and yelling at the subject for his failure to comply). These three guidelines were super-imposed on the random orders established previously. Where one of these guidelines was in danger of being violated, the video-tape sub-segment that was to have been rated at that point in the sequence was skipped over and the next segment was shown. However, in actual practice, it was not always possible to strictly adhere to these guidelines and on a few occasions they had to be violated. In most cases where this was done, it seemed likely that the judges would not remember the previous segments in question and would thus probably not draw any unwanted conclusions about the behavior they observed.

Classroom judging procedure

Having prepared the recopied tapes for judging, and having established an order for the judging for various sub-segments on them, it was now time to begin the judging procedure itself. A Sony 3600 playback machine was used in conjunction with a 12" Sylvania monitor. While a bigger monitor would have been more desirable, it was found that the use of a bigger monitor was too impractical to transport from college

to college and the 12" monitor was deemed to be adequate.

Audio was provided through a dual speaker arrangement. The TV speaker was utilized as well as another speaker that was fed from the output of a small Lafayette amplifier. A microphone, taped to the TV set, provided the input to the amplifier and also doubled as a P.A. system for giving instructions to the class.

Each teacher's individual classroom was utilized as a viewing room since it was deemed impractical and unfeasible to transport the students to other rooms. Ten minutes prior to each given class, I entered the classroom with my assistant and began setting up the equipment. The TV was mounted on a stand placed on top of the desk so that it was easily visible. Seats were arranged in a partial semi-circle as close as possible to the set. When the students came in, we handed each student a paper called "Preliminary Instructions" which explained the rationale of what they would be doing. (see Appendix J, p. 304). These questions were designed to deceive the raters into thinking that all the people they would be asked to rate were other college students who had been candidly filmed.

After we had handed out these instructions, I para-phrased them for the raters, explaining to them that I was a researcher at another college who was planning to do a study requiring the use of volunteers. I explained to them that I had obtained a large number of potential volunteers but had to screen them to select those that I needed for my research. To get the information needed to do this, I had invited each of the volunteers to come down, alone or in pairs, to fill out a biographical questionnaire. While they were doing this I had also secretly made a video-tape of them to get a sample of their behavior. I then explained

to the class of raters that it was required that I get an objective opinion from a number of intelligent, perceptive people such as themselves as to the exact nature of the behavior that these potential volunteers had displayed.

The class of raters was then told they did not have to participate in helping me with the screening process. However, I urged them to lend me their cooperation and pointed out that some of the tapes might be very interesting to watch.

At this point, the raters were cautioned they might possibly recognize some of the people of the tapes. It was explained to them that since we had made these tapes secretly, some of the "volunteers" might have felt embarrassed to be caught off guard like this. The volunteers had been nice enough to let us use these tapes as part of the screening process and therefore we did not want to cause any potential embarrassment by having a friend or acquaintance view them. It was explained to the raters that for this reason, if they thought that they recognized somebody on one of the tapes, they should leave the room for that particular segment.

With this introduction out of the way, the group of raters were then told how the judging would proceed. It was explained that we would be showing them a small sample of tape segments out of all those we had available. They would be asked to watch each segment carefully and at its conclusion, would be asked to make a judgment about it, using a scale we would give them, and would be asked to indicate this judgment through the use of an IBM sheet and a #2 pencil.

The raters were also informed that generally there was more than one person on any given tape segment. Since we wanted to obtain an

objective judgment for each person's behavior, the raters were cautioned that it was extremely important that they ignore the behavior of the person who was not to be rated, and only focus on the behavior of the person that they were asked to rate. To help them do this, they were told that a simple apparatus had been devised, made up of a curtain rod, a piece of cardboard, and a large clip that would allow us to mask the visual image of the person we wished them to ignore. They were assured that other classes of students would rate the masked individuals and the importance of focusing on and objectively evaluating the behavior of the visible person only, was repeatedly stressed to them. In contrast to the rationale given to these judges however, the real rationale for these instructions was of course, to try to impress on the raters, the need to avoid having their evaluations of the visible individual influenced by the behavior and actions of the person they could not see, or by some other ratings they had previously made. By instructing the raters in this manner, as well as by masking the visual image of one of the two individuals on any given video-tape segment, it was felt that the ratings we obtained from these judges would be maximally independent of one another.

Having explained the instructions to our particular group of judges, the instructions for the rating scales they would be using were then handed out. Three different rating scales were used in each class and the judges were so informed. Each judge received a pencil and an opscan sheet to record their ratings on. The procedures for the use of the opscan sheet were also explained to them and questions were answered regarding the three scales.

The three scales used to rate the video-tapes were those of aggress-

iveness constructiveness; normality abnormality; and emotional honesty, emotional dishonesty. (see Appendix J, p. 304).

As can be seen from the appendix, the aggressiveness-constructiveness rating scale given to these judges was almost identical to the aggressiveness constructiveness scales used previously in the course of the research. The data obtained through the raters' use of this scale, comprised the main independent and dependent variables of the present study.

The normality and abnormality scale was also practically identical to that used previously in this research. The judges' rating of how normal or abnormal the behavior of the confederate or subject was, was used to shed some light on the relationship between aggressive and constructive behavior and the degree of its abnormality.

The emotional honesty, emotional dishonesty scale was included to provide some interesting material for future analysis; it will not be discussed at length in the present paper.

Each classroom of judges was asked to rate behaviors, that they would view on the video-tape, on all three scales. However, to keep the ratings on each scale uncontaminated by ratings on the others, any given judge used only one of the three scales. Further, they were cautioned about talking to each other or discussing their ratings out loud. To decide which judges would use which rating in any given classroom, a table of random numbers was used to determine which section of the room, left, middle, or right, got which scale. Since the aggressiveness-constructiveness scale was the most important scale, we tried to give out a few more of these than the others, so as to increase the number of raters using this instrument.

The number of student raters using any given scale varied with the normal size of the class and its attendance on that day, but was roughly one third of the average size class. Once the scale instructions had been understood, the showing of the video-tape segments could begin. Prior to beginning the video-tape viewing the raters were advised to raise their hands if they were unable to make out any aspect of the audio-visual presentation or if they had any questions of what they were to rate.

Where the audio-visual was not, on occasion, distinct enough for a given set of judges, I either repeated the segment and/or gave a verbal account of what was said or clarified what was done, trying to keep the quality of my voice similar to that on the tape.

Following these instructions, the video-tape showings began. After any given video-tape sub-segment had been shown, the equipment was temporarily turned off, and the judges were asked to think about the scale instruction and to make their rating on this scale provided for this purpose. A new video-tape segment was then queued up and shown and the process was repeated. As many segments and sub-segments as possible were shown during the class period, leaving some time at the end for an explanation of our research and for answering the questions of the students.

It should be pointed out that during the showing of these video-tapes, I found it necessary to make some verbal comments when the segments involving non-verbal instigations were shown for rating purposes. One reason for this, was that I anticipated that the cigarette smoke in the "obnoxious cigarette smoking instigation" might not be clearly made out by the judges. To handle this problem, I decided to add my own verbal

non-committal comment each time the instigation was shown, in order to draw the judges' attention to the cigarette smoke. I commented that "I hope that all of you can see the person is smoking and his smoke seems to be going in the other person's direction". A second reason that I decided to make some verbal comment was that in the case of those instigations that were primarily non-verbal in nature, judges who were asked to rate the behavior of the subjects receiving these instigations had absolutely no idea of what instigation was occurring, since the visual image of the confederate on the screen was masked. This was in contrast to the other instigations where the judges were unable to see the confederate, but were able to hear his voice, and thus could obtain some idea of the context in which the subject's response was made.

To hold this factor constant, and give judges some idea of the context in which the subjects' responses to the non-verbal stimuli were made, it was decided to allow judges, rating the subjects' responses to these non-verbal stimuli, to see the visual image of the confederate's non-verbal behavior. For three five-second periods, the cardboard mask was temporarily removed. The rest of the time, it was in place so that the judges could once more concentrate on the behavior of the subjects that they were called upon to rate.

Once the judging procedure was concluded, judges were asked to write in their age, sex, height, weight and race on the top of the opscan sheets. After passing back all materials, they were properly debriefed, their questions were answered, and they were given my phone number for possible future reference.

Treatment of Video-Tape Data

Once all of the video-tape ratings had been made, the next question was to how statistically evaluate and analyze this data. It will be recalled that for each subject that had received verbal instigation, his video-tape had been broken into two sub-segments for rating purposes: the "direct" response phase and the "spontaneous" response phase. Each phase was to be rated by a different classroom of judges and each of the two people in interaction in any given phase, were likewise to be rated by different classes. Thus, for each subject's data, one would obtain four video item ratings on any given scale (see Figure 3). Two ratings would be of the confederate's behavior - one for each sub-segment, and two ratings would be for the subject's behavior - one each for each sub-segment. Since more than one judge made each rating, the median of the ratings made by these judges, for each one of these four video item ratings would need to be obtained. Since each video item was rated on three different scales, by three different sub-groupings of judges within each classroom, this procedure resulted in our obtaining median judgments of how aggressive or constructive, normal or abnormal, emotionally honest or dishonest, the behavior displayed by the confederate or subject in each one of these four video items was.

For those subjects who had received a "non-verbal" instigation the video-tape was not broken up into sub-segments, since due to the nature of the instigation, there could not be any distinct direct or spontaneous response phases in this case. As such, only two median judgments were obtained for each of these video-tape segments on each of the three scales. One of the sets of median judgments consisted of ratings of the confederate's behavior throughout the whole video segment; the other set consisted of ratings of the subject's behavior throughout the whole video segment (see Figure 3).

Since different classrooms of judges were called upon to rate different video items, within the same segment, the next job was to collate all the median judgments belonging to the same segment - each segment representing in turn the video data collected for each subject. Not all the ratings obtained were analyzed in the present paper. For each subject the following data was collected for use in the present paper:

1. Median judged aggressiveness-constructiveness of confederate's instigation.
2. Median judged aggressiveness-constructiveness of subject's "direct" response to them.
3. Median judged aggressiveness-constructiveness of subject's "spontaneous" response to them.
4. Median judged normality-abnormality of the confederate's instigation.
5. Median judged normality-abnormality of the subject's "direct" response to it.

Other judgments that were collected but are not the focus of interest in this paper, include the judgments of how aggressive or constructive the confederate was during the spontaneous response, how normal or abnormal the confederates or subjects were judged to be during the spontaneous response, or how emotionally honest or dishonest the confederates or subjects were during each of the two response phases.

With regard to those subjects who had received the more non-verbal instigations, since we did not break up the data into phases during the judging procedure, we did not have two separate ratings for direct and spontaneous responses of the subjects on each scale but instead had one lump sum rating on each of the scales. It was decided to treat this lump sum data as both "direct" responses and "spontaneous" responses for purposes of analysis, since this data had seemed to have elements of both. It has therefore been included in the analysis of both kinds of data in this paper.

Approaches Used in the Calculation of Correlations Between Independent and Dependent Variables

As mentioned in our introduction we decided to subject the data to two types of correlational procedures. For our purpose, we labeled the correlational data obtained through the first procedure mean response data and the correlational data obtained through the second procedure, discrete response data.

Mean response procedure

This statistical procedure was very similar to that employed by past researchers in analyzing their results. To allow ready comparisons between their results and ours, we focused on this approach in our study as well.

In this procedure, the 9 point scale of aggressiveness-constructiveness was broken down into nine intervals of one unit apiece. In analyzing any of the stimulus response relationships of interest, all ratings of the actual stimulus instigations were first sorted into one of the nine categories or levels. Then, the mean responses on the relevant dependent measure of all subjects who received an instigation falling into a given level was calculated. Thus for any given dependent measure, for each of the nine instigation levels, we could obtain a mean rating of subjects' responses (behavioral self reports, etc.) to each. Then, using the midpoint of each level as representative of all instigations in that level, we could correlate the 9 midpoints with the 9 mean responses to them. The correlations thus obtained could then be compared using standard statistical techniques.

Discrete response approach

This approach was also included since we felt that the results thus obtained would yield more accurate statistical descriptions of the reci-

proximity relationships obtained. It was not the main focus of our attention however, because this would then preclude our ability to compare our results to previous research.

Basically in this approach, the ratings of the instigations were not grouped into levels. Rather, for all subjects in any given set of data involving any given dependent measure, the instigation each subject received was correlated with the response each subject made across all subjects. The statistics thus obtained could likewise be tested for statistical significance as before.

CHAPTER 4

RESULTS

Description of General FindingsOverview

Table 2 (p.141) displays the main experimental findings. For each social restraint condition, correlation, slope and intercept data is provided for all data and analysis done as listed below. The data and analyses are as follows:

1. Mean direct response data.
2. Mean direct response data - verbal instigations only - this analysis includes only those subjects who received a primarily verbal stimulus. (see Stimuli 1 through 4, 7, and 9 in Appendix C).
3. Mean "spontaneous" response data.
4. Mean "spontaneous" response data - verbal instigations only.
5. Discrete direct response data.
6. Discrete direct response data - verbal instigations only.
7. Discrete "spontaneous" response data.
8. Discrete "spontaneous" response data - verbal instigations only.

All correlations tabled were tested for significance and the results are displayed. The .01 level of significance was chosen. Borderline significance is also indicated ($.05 > p > .01$)

In looking at these tabled correlations it should be remembered that in each case, they basically indicate the degree of linear relatedness that was found between given instigation magnitudes and subjects' responses. As such, they indicate the consistency and predictability with which increases or decreases of instigation levels along the dimension of aggressiveness-constructiveness were met with corresponding increases or decreases

of subjects' responses along the same scale.*

The demonstration of such a relation, as evidenced by a high positive correlation (+r), confirms the presence of one aspect of reciprocity behavior, e.g. that the more a person's instigation moves toward one end of the scale, the more he can expect the recipient's behavior to linearly change in the same direction. It does not however, confirm the second aspect of reciprocity behavior, e.g. that aggression was always met with its "rough equivalent" (aggression) and constructiveness with constructiveness. For example, it might be found that subjects' responses only vary with the constructiveness of the continuum.

In one sense, the demonstration of a high positive correlation without demonstrating that a "rough equivalence" of what was received, was returned, was not necessary to demonstrate reciprocity behavior in our study. This is because in this study we defined an aggressiveness-constructiveness continuum such that the more aggressive a given behavior is, by definition, the less constructive it is and vice versa. As such, for example, behavior that becomes increasingly less constructive by our definition, simultaneously becomes more aggressive and vice versa. Thus, built into our conceptual framework is the insurance that positively correlated change in instigation responses with instigation levels, automatically constitutes such "rough equivalence".

However, since this conceptualization may not be persuasive to some, we have decided to do the following: We will use the term "reciprocity" and reciprocity behavior" in the broadest sense, in analyzing the statistical results. In our discussion of these results, we will examine the limitations of any positive findings in light of the question of "rough equivalence".

*See footnote p. 184, Chap. 4 "Discussion"

A detailed presentation of the main experimental findings contained in Table 2 now ensues. This will be followed by a presentation of evidence obtained in attempts to test each of the hypotheses and comparisons mentioned earlier. For those tests, the .01 level of significance was used throughout.

TABLE 2

Summary of Main Experimental Findings

Data Analyzed	Normal Restraint	Low Restraint
Mean Response Data		
Direct Response		
All Data (N=8)	r=0.50 b=0.13 a=4.33	r=0.92**** b=0.23 a=3.62
Verbal Instig Only (N=7)	r=0.52 b=0.13 a=4.38	r=0.96***** b=0.37 a=2.70
Spontaneous Response		
All Data (N=8)	r=0.73* b=0.14 a=3.99	r=0.72* b=0.14 a=3.89
Verbal Instig Only (N=7)	r=0.84** b=0.15 a=3.93	r=0.90*** b=0.23 a=3.31
Discrete Response Data		
Direct Response		
All Data (N=40nr) (N=391r)	r=0.31* b=0.14 a=4.27	r=0.53***** b=0.25 a=3.50
Verbal Instig Only (N=27nr) (N=231r)	r=0.35* b=0.14 a=4.41	r=0.63***** b=0.37 a=2.80
Spontaneous Response		
All Data (N=40nr) (N=391r)	r=0.34* b=0.13 a=4.01	r=0.41*** b=0.16 a=3.76
Verbal Instig Only	r=0.45** b=0.15 a=3.95	r=0.57*** b=0.25 a=3.22
*p < .05	****p < .001	
p < .01	***p < .0005	
***p < .005		

Low Social Restraint - Direct Response DataMean response analysis

As can be seen from Table 2 under conditions of Low Social Restraint, an extremely strong, significant correlation was obtained between different levels of aggressive or constructive instigation and mean levels of direct aggressive or constructive response to them, ($r = .92$, $N = 8$, $p < .00069$). The slope of the regression line for the prediction of mean response of subjects to given levels of aggressive or constructive provocation under these conditions was .23. A confidence interval was set up using the standard error of the slope indicated the probability was .99, that the population slope falls between .077 and .3769.

The obtained regression equation for the prediction of the average subject's response under conditions of low social restraint was written as follows: $Y = .23 x + 3.62$. The standard error of the estimate was .26.

A graphic representation of the regression line is shown in Figure 4. A value of 0 represents extreme aggression; a value of 8, extreme constructiveness. As can be seen the means of the subjects' responses to given levels of instigation fell neatly on this line. Inspection of the data also revealed that while the range of the instigations delivered to subjects varied from being judged strongly aggressive (1) to strongly constructive (7), the range of the mean direct response was more constructed and centered around the neutral point. The strongly aggressive stimulus was judged to have been responded to, on the average, in a neutral to mildly aggressive manner (value 3.45). The strongly constructive stimulus was judged to have received a mild to moderate constructive response (5.35). All other average levels of response under low social restraint fell within those 2 points.

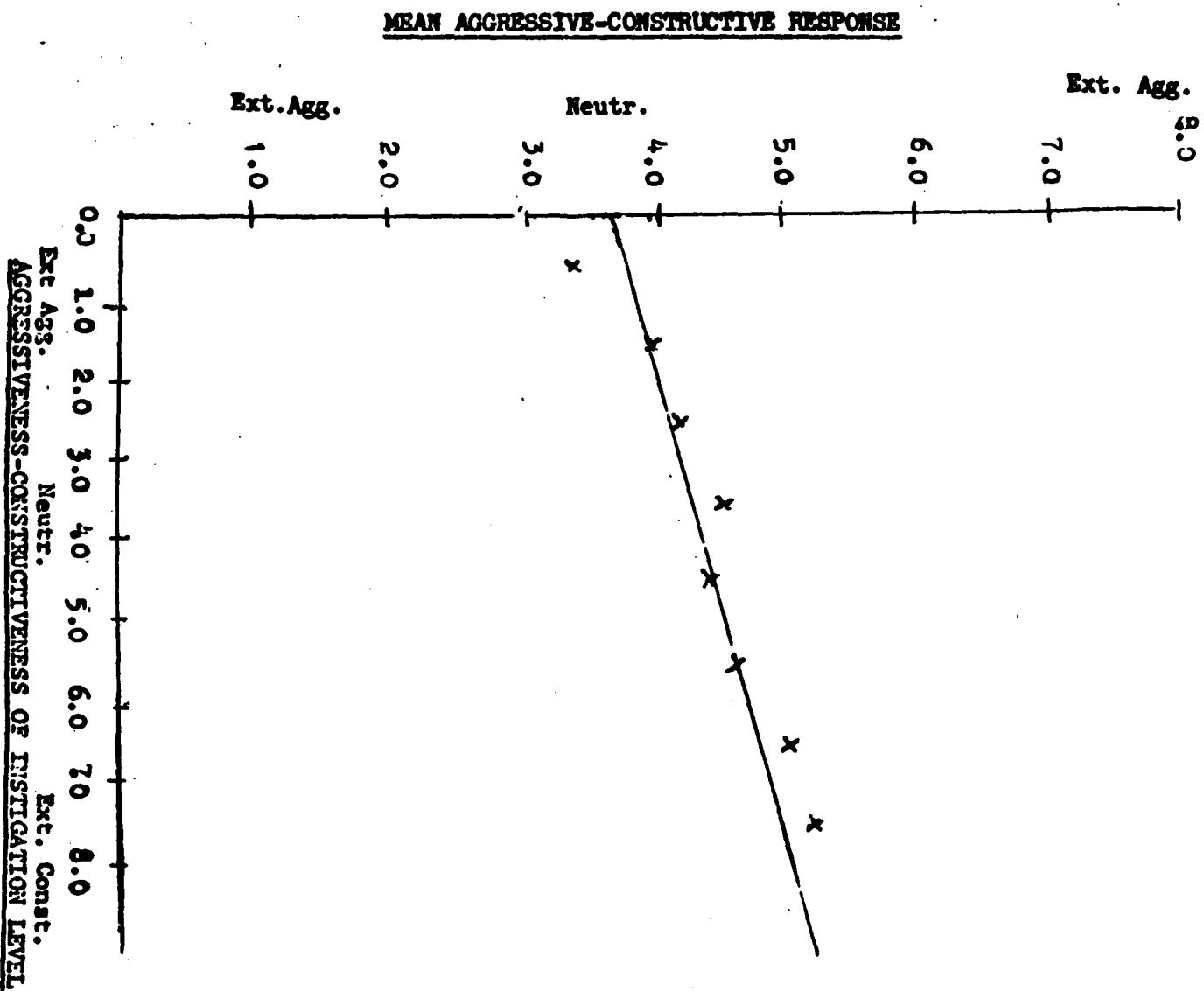


Figure 4. Plot of mean direct responses made by subjects to each judged level of instigation under low Social Restraint conditions.

Figure 5 shows a regression line, and scattergram obtained by Harshbarger in previous research using self report questionnaires as described previously and a similar procedure for data analysis. As expected, the results obtained under this condition seemed highly comparable to data reported by Harshbarger.

Discrete Response Analysis

Table 2 gives the results obtained under this condition as analyzed by the procedure of correlating the level of instigation each subject was judged to have received, with the exact magnitude of response he was judged to have made. As such, this analysis showed how well we could predict the actual responses of subjects to instigations, as opposed to predicting the average response of subjects to given levels of instigation. As one can see, inspection of this data revealed that the obtained correlation, while significant, was considerably smaller in magnitude ($r = .53$, $N = 39$, $p < .00027$) and the standard error of the estimate was considerably larger (.88 vs .26). Slope and intercept value stayed basically constant regardless of which procedure was used ($b = .25$, $int = 3.5$).

Looking at Figure 6, the scatterplot of subjects' responses in this condition, we found, as expected, that the data was much more variable than one would be led to expect from the first analysis. Inspection of the data revealed that, a good proportion of the responses made in response to aggressive action turned out to be judged neutral or helpful by our raters. As previously predicted, very few subjects made out and out aggressive responses, even under conditions of low social restraint.

Many subjects apparently preferred to try to ignore or be polite to the instigator rather than to reciprocate aggressively. Even when the subject did reciprocate aggression with aggressive behavior, the nature

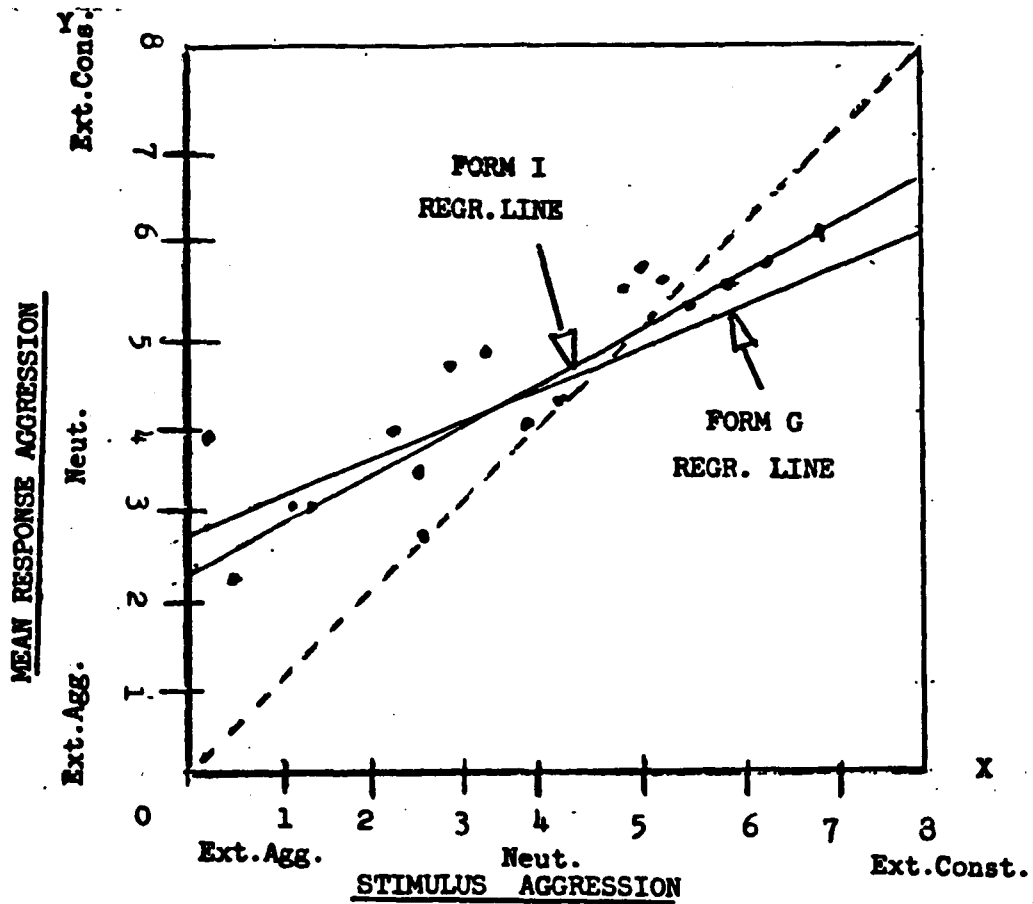


Figure 5.

Note: Graph reprinted by author's permission.
Graph inverted and renumbered to allow ready comparison.

AGGRESSIVENESS-CONSTRUCTIVENESS OF S_s' DISCRETE RESPONSES

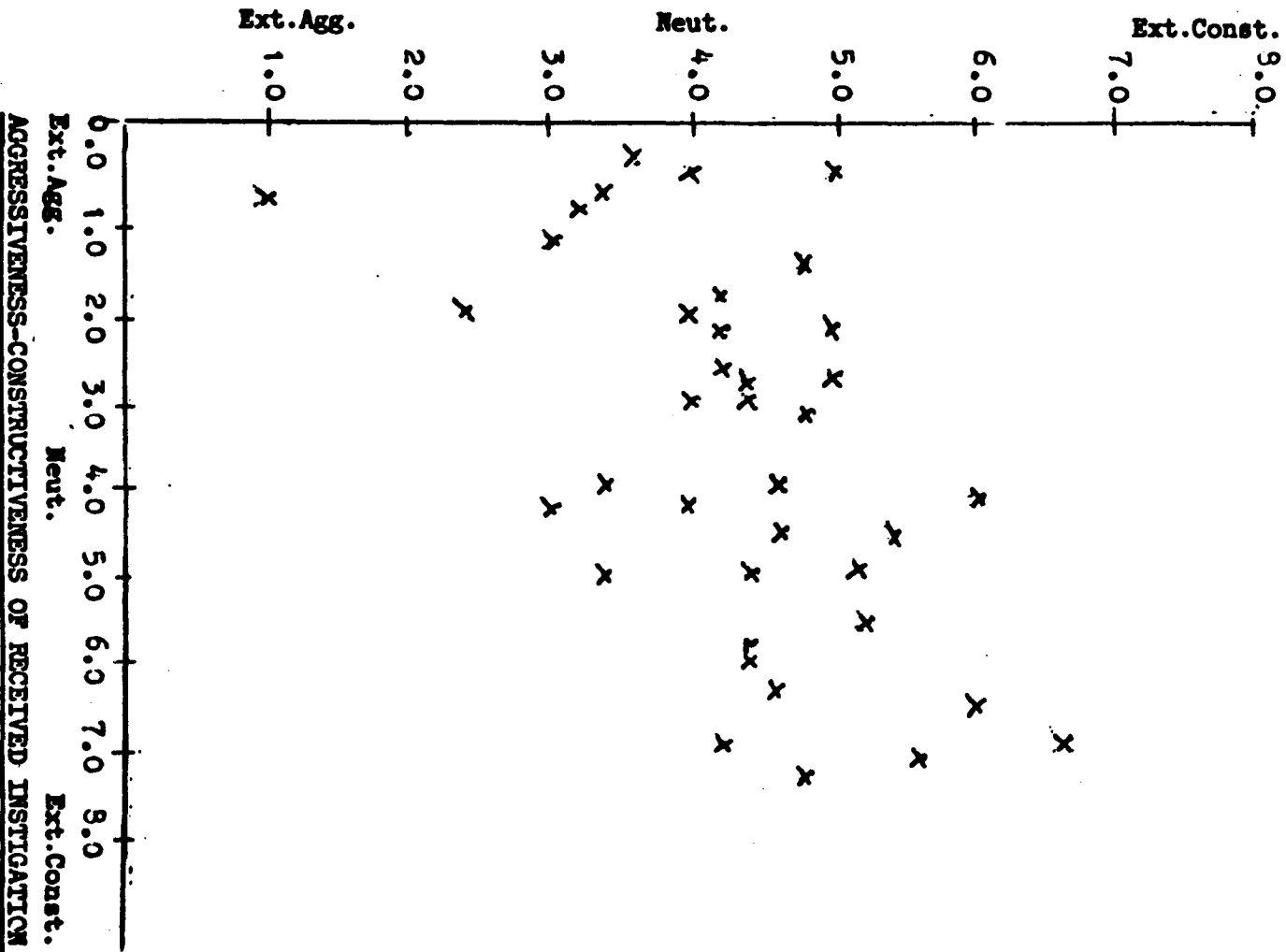


Figure 6.

Scattergram of subjects' discrete direct responses made to the magnitude of instigation each received under Low Restraint Conditions

of the behavior was often so restrained that some raters viewed it as helpful behavior and evaluated it in this light lowering its overall rating. This was reflected both in the variability of the ratings made by the raters and in remarks made by some raters at the end of the rating procedure to the effect that the subjects' restraint was seen as "helpful".

In contrast to most subjects' reactions to aggressive instigations, one can also see that a few subjects were seen to be moderately to strongly aggressive, a finding which concurs well with the author's observation that a few students in this condition became so enraged at the confederate and had so little control over their actions, that the confederate and I feared strongly for the confederate's physical safety.

At the other end of the scale, we noted that most constructive acts were reciprocated with constructive behavior. There were only one or two exceptions to this however, mostly for those acts which were mildly constructive in nature.

Normal Social Restraint - Direct Response Data

Mean Response Analysis

As can be seen from Table 2, under conditions of Normal Social Restraint, a non-significant correlation was obtained between different levels of aggressive and constructive instigation and mean levels of direct aggressive or constructive responses to them ($r = .5$, $N = 8$, $p > .05$). As can be seen this correlation was considerably smaller than that obtained under low social restraint conditions and previously reported. Further, the obtained slope for the prediction of mean response of subjects to different provocation levels was likewise smaller than that obtained under low restraint conditions ($b = .13$). A confidence interval for the slope, using the standard error of the slope, indicated the probability is .99

that the population value falls between $-.209$ and $.46454$.

The obtained regression equation under normal social restraint conditions was $Y = .13 + 4.33$.

A graphic representation of this regression line is shown in Figure 7. As before, a value of 0 represents extreme aggression; a value of 8, extreme constructiveness. As can be seen, the range of means of subjects' responses to the different instigations was more constricted than under low social restraint conditions and they did not fall neatly on the regression line. The response means centered around the neutral point and varied from neutral (value 4.04) to mild to moderately constructive (value 5.39). Mean responses to aggressive stimuli under this condition tended to be a curvilinear function of the level of instigation with strongly aggressive instigations receiving neutral to slightly helpful responses. At the constructive end, a curvilinear pattern could also be discerned in this condition in subjects' responses to constructive instigations.

Discrete Response Analysis

As under low social restraint conditions, a second analysis of the data was done which involved correlating the level of instigation each person was judged to have received with the exact magnitude of response he was judged to have made, in order to see how well we could predict the actual responses of subjects under normal social restraint vs average responses of subject to provocation. As one can see from Table 4, the correlation obtained using this procedure in normal restraint conditions was considerably smaller than that obtained using the same procedure with low social restraint data ($r = .31$ vs. $.53$) and the standard error is slightly larger ($.92$ vs. $.88$).

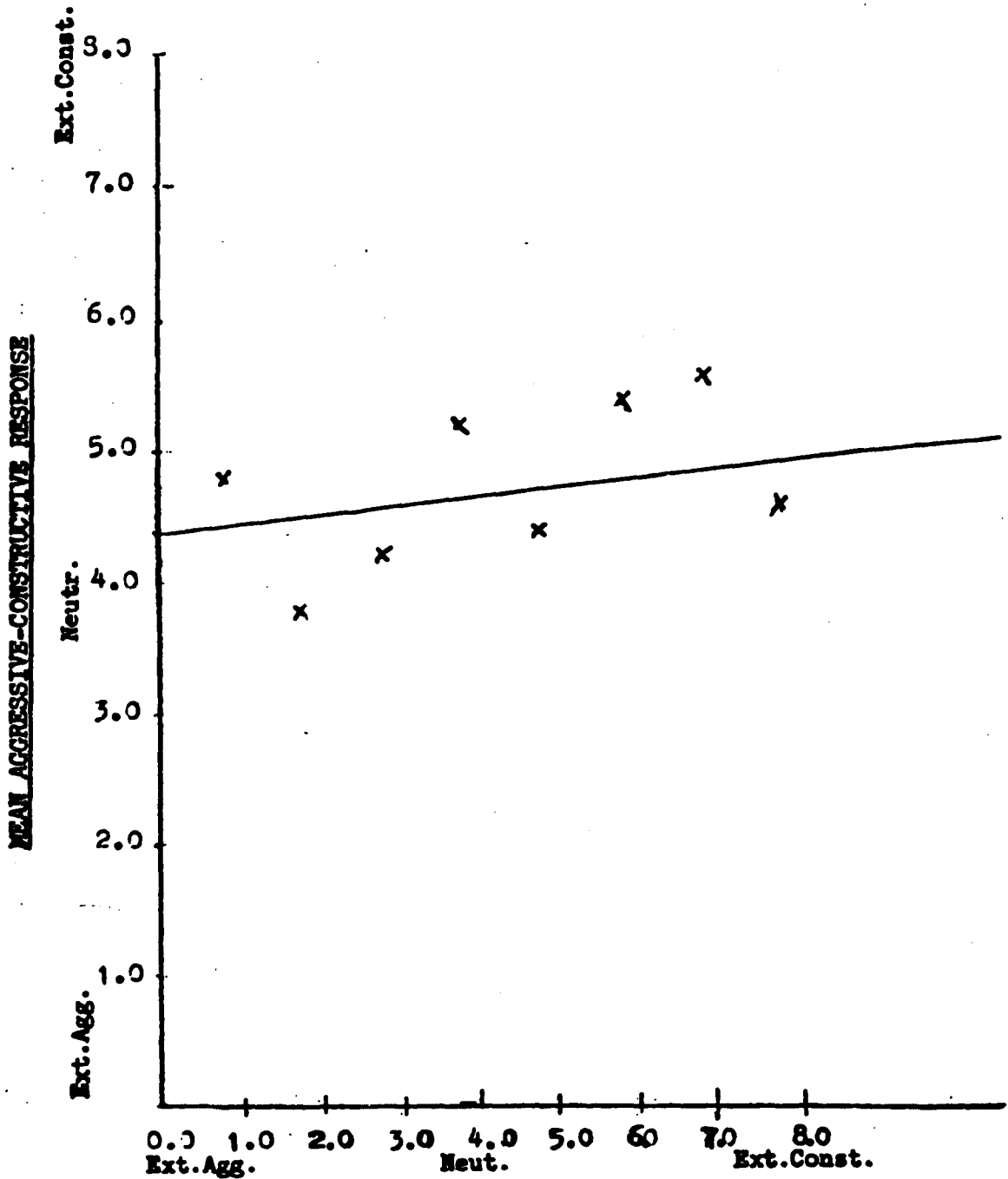


Figure 7. Plot of mean direct responses made by subjects to each judged level of instigation under Normal Social Restraint Conditions

Figure 8 presents the scatterplot of subjects' responses in this condition. As expected the data was more variable than one would be led to believe it would be by the inspection of the mean response data. As for the scatterplot itself, one notes that as with the scatterplot for the low restraint - direct response data, subjects' judged responses were quite variable in response to aggressive instigations. As before, most subjects were judged to have responded to these instigations in a neutral to slightly helpful manner. In contrast to the low restraint - direct response data though, even fewer subjects made a response judged to be aggressive in nature in response to an aggressive instigation.

Looking at subjects' responses to the constructive stimuli, we found that under normal restraint conditions, most of the direct response to constructive acts were judged to be constructive in nature. We also detected a curvilinearity in the response data as a function of level of constructive instigation that was not present under low social restraint conditions. Inspection of the subjects' written self-report for those subjects who received the two most extreme instigations revealed an increased tendency under the normal restraint condition to report themselves as "annoyed" by the behavior of the confederate and to mistrust him. We will discuss the meaning of this observation in a later section of this paper.

Low Social Restraint - "Spontaneous Response Data"

Mean Response Analysis

As can be seen from Table 2, under conditions of low social restraint, a substantial though nonsignificant correlation was obtained between different levels of aggressive or constructive instigations and mean levels of "spontaneous" aggressive or constructive responses to them ($r = .72$, $N = 8$, $p < .025$).

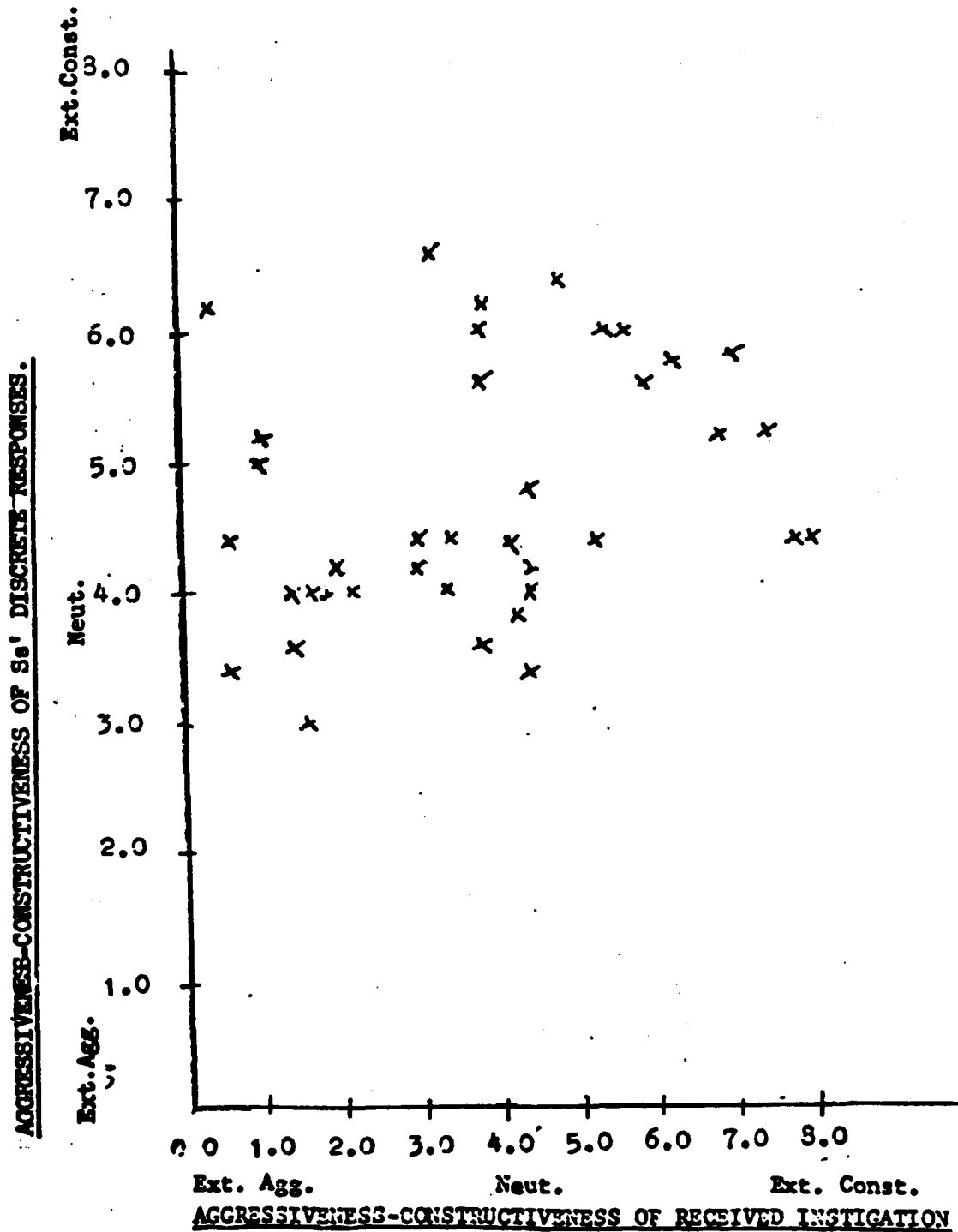


Figure 8. Scattergram of subjects' discrete direct responses made to the magnitude of instigation each was judged to have received under Normal Social Restraint Conditions

The slope of the regression line for the prediction of mean responses of subjects to given levels of aggressive or constructive provocations under these conditions was .14. A confidence interval that was set up using the standard error of the slope indicated the probability is .99 that the true population slope falls between -.07 and .36.

The obtained regression equation for the prediction of the average subject's "spontaneous" response under conditions of low social restraint was written as follows:

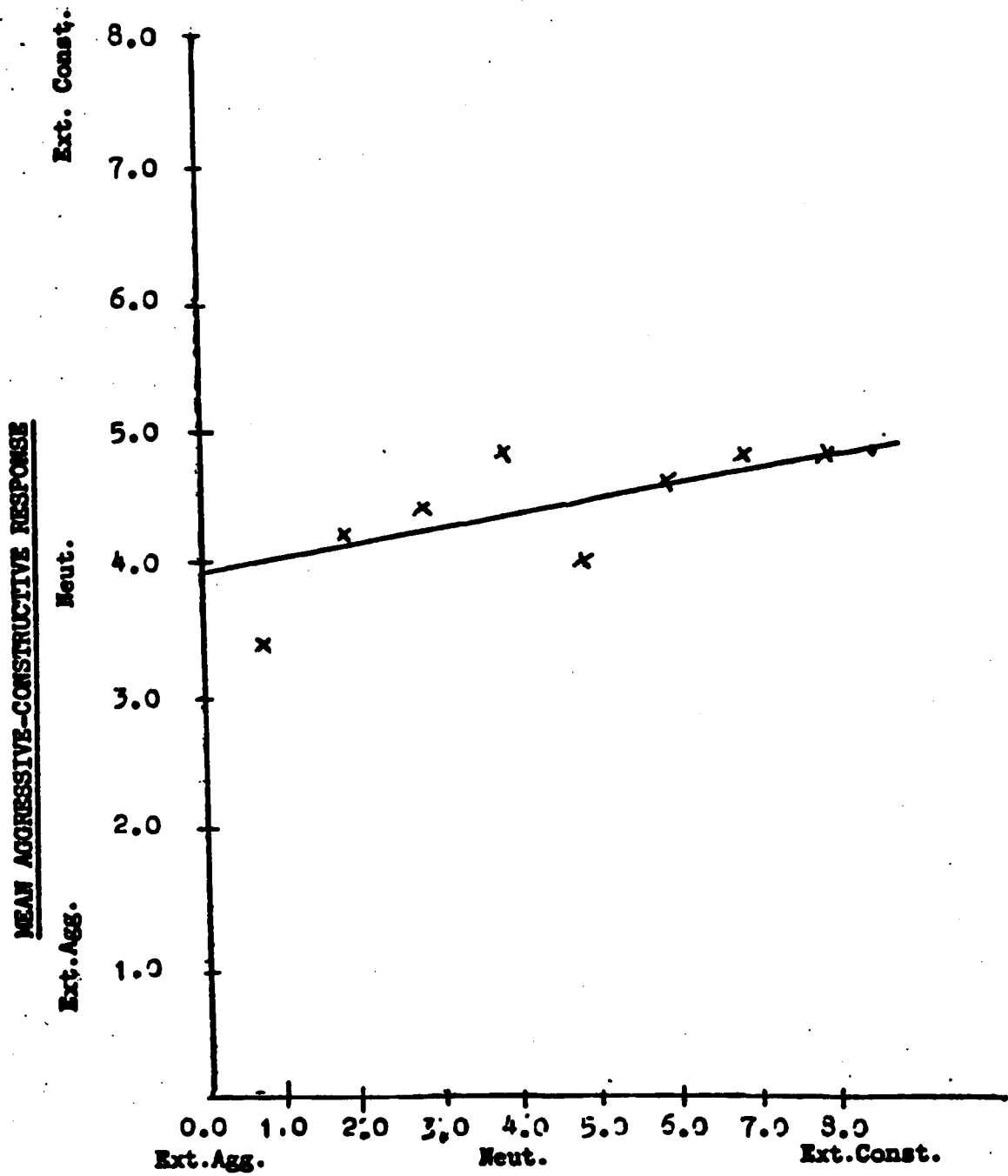
$$Y = .14 X + 3.89$$

A graphic representation of this regression line is shown in Figure 9. A value of 0 represents extreme aggression, a value of 8 extreme constructiveness.

Inspection of this data revealed that, as before, while the range of instigations delivered to the subjects varied greatly in how aggressive or constructive they were judged to be, (from extremely aggressive to strongly constructive) the range of the mean spontaneous responses to them was much more constricted and centered around the neutral point. The strong extreme aggressive stimulus was judged to have been spontaneously responded to in a neutral to mildly aggressive manner ($\bar{X} = 3.49$). The strong extreme constructive stimuli were judged to have received a neutral to mildly constructive response ($\bar{X} = 4.8$); other mean values fell between these points.

Discrete Response Analysis

Looking at the scatterplot of each subject's "spontaneous" discrete response to the actual instigation he received (see Figure 10), we again note the marked variability of the data. However, we note that in



AGGRESSIVENESS-CONSTRUCTIVENESS OF INSTIGATION LEVEL

Figure 9. Plot of mean spontaneous responses made by subjects to each judged level of instigation under low restraint conditions.

AGGRESSIVENESS-CONSTRUCTIVENESS OF S_B' DISCRETE RESPONSES

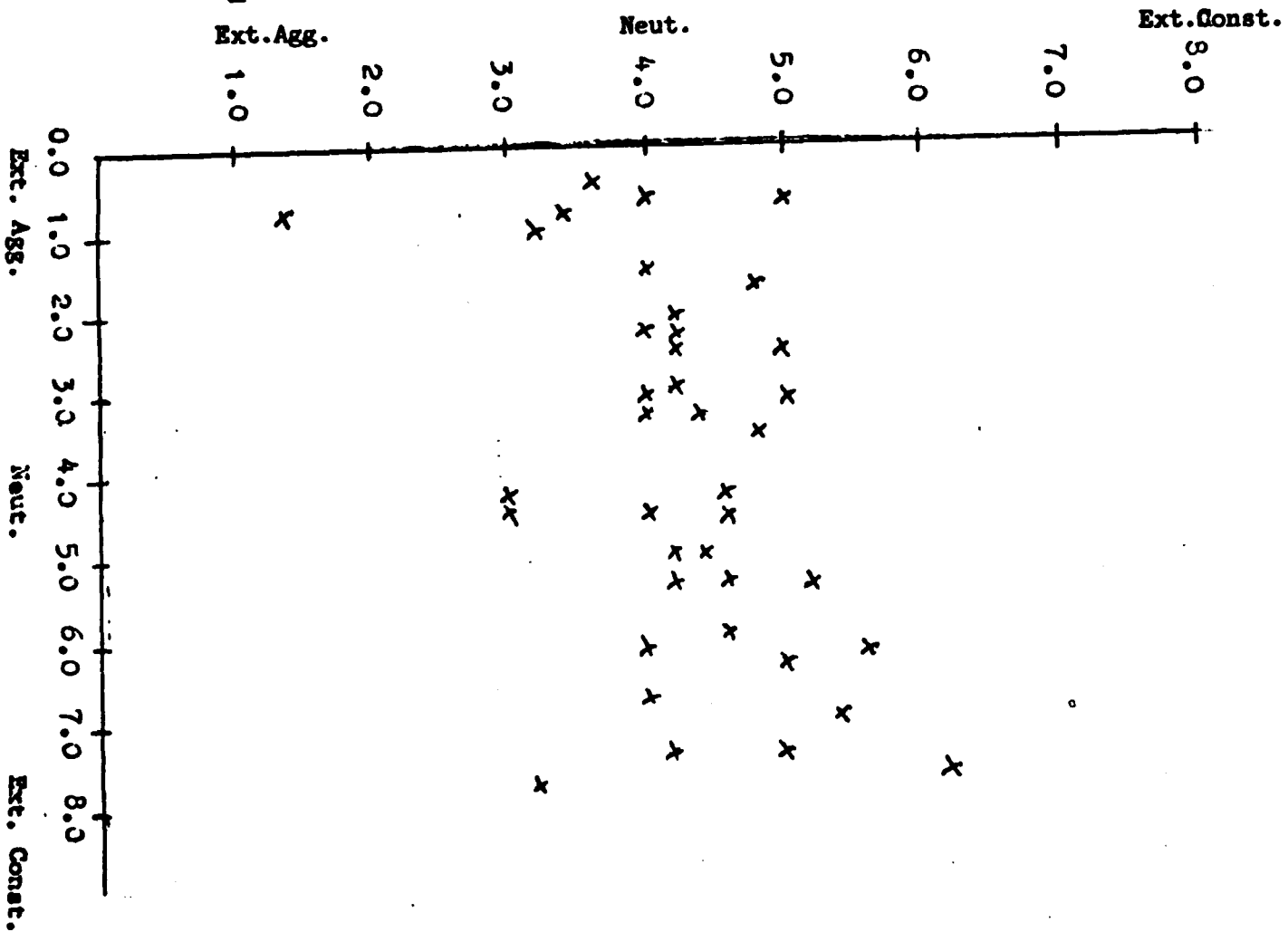


Figure 10. Scattergram of subjects' discrete spontaneous responses made to the magnitude of instigation each was judged to have received under four social restraint conditions.

AGGRESSIVENESS-CONSTRUCTIVENESS OF PERCEIVED INSTIGATOR:

contrast to the scatterplot of the discrete direct response data under the same restraint condition, this scatterplot tended to have slightly less variability and people's reactions are more nearly neutral. Again there were very few aggressive responses to aggressive stimuli, while most of the responses to constructive stimuli were constructive in nature.

Normal Social Restraint - "Spontaneous" Response Data

Mean Response Analysis

Under conditions of normal social restraint a good sized though non-significant correlation was again obtained between different levels of aggressive or constructive instigation and mean levels of "spontaneous" aggressive or constructive responses to them ($r = .74$, $n = 8$, $p < .02$) (see Table 2). The slope of the regression line for the prediction of mean responses of subjects to given levels of aggressive or constructive provocations under these conditions was .14. A confidence interval that was set up using the standard error of the slope indicated the probability is .99 that the true population slope falls between -.055 and .333 .

The obtained regression equation for the prediction of the average subjects' "spontaneous" responses under normal restraint condition was written as follows:

$$Y = .14 X + 3.99$$

A graphic representation of this regression line is shown in Figure 11. 0 represents extreme aggression; 8 represents extreme constructiveness. Inspection of this Figure shows that as before, the range of the mean responses to various instigations was more constricted than the range of the instigation levels themselves and centered around the neutral point. The strong extreme aggressive instigation level was responded to on the average in a neutral manner. Less extreme aggressive instigations were on the average responded to in a helpful manner.

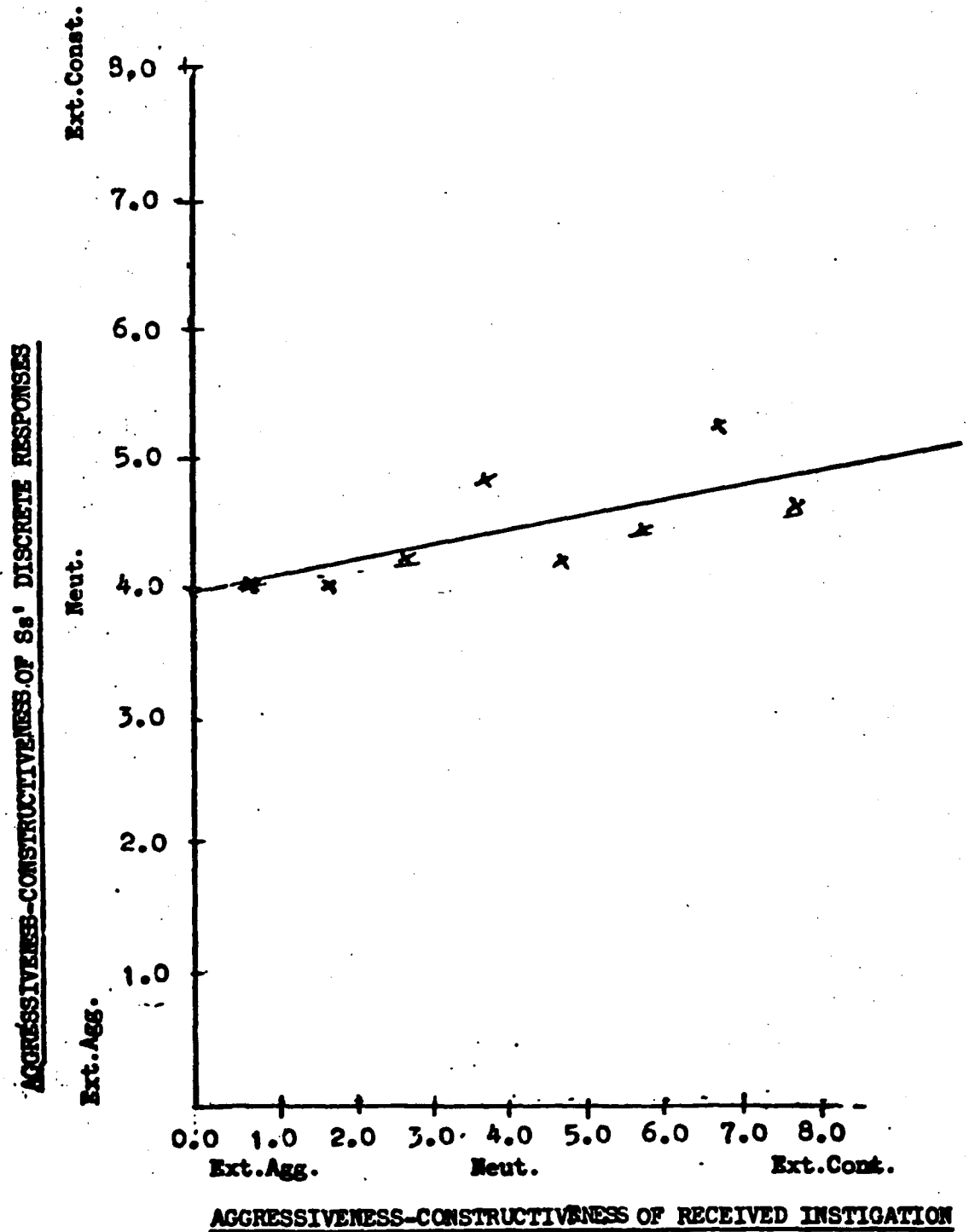
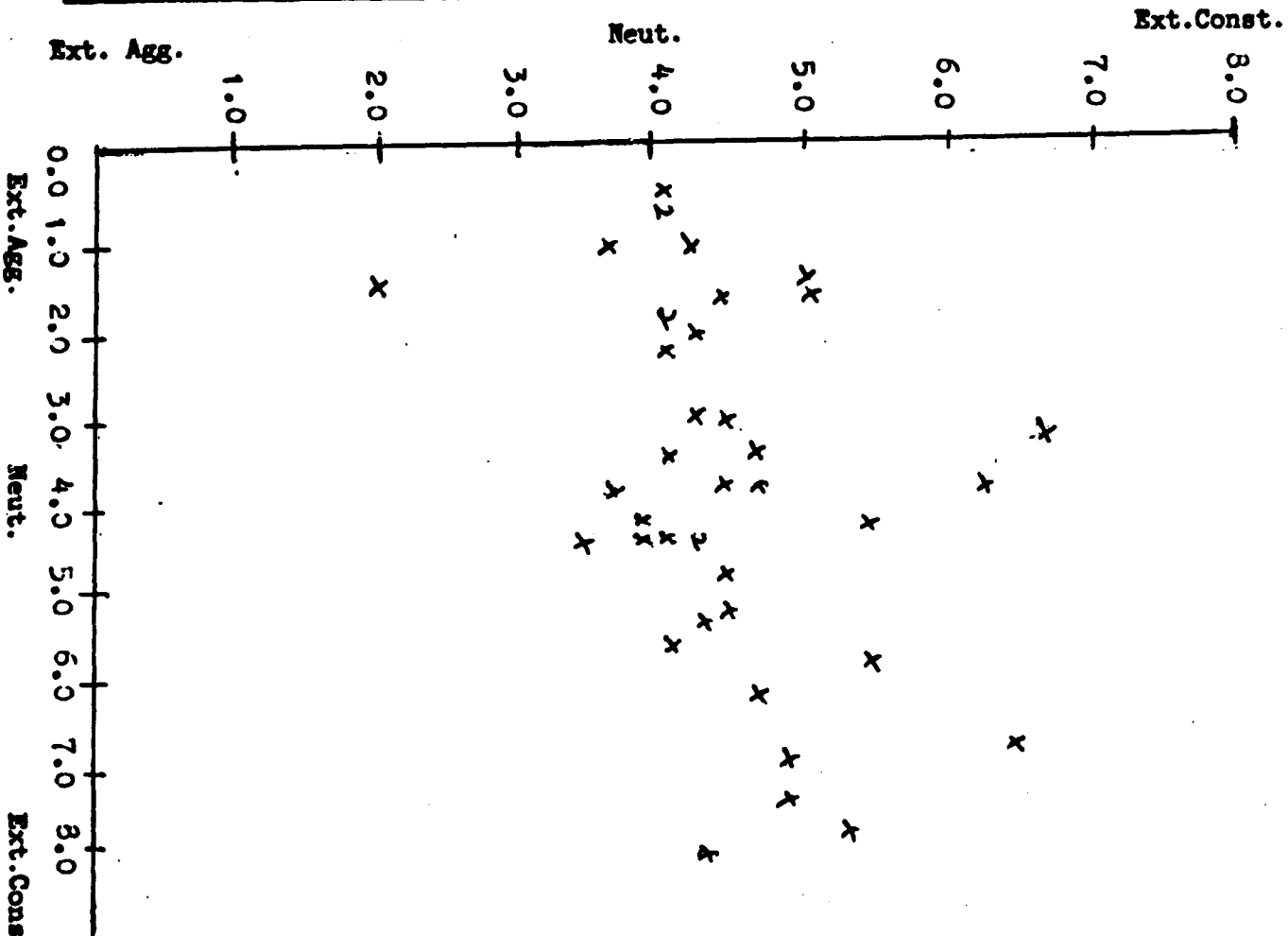


Figure 11. Plot of mean spontaneous responses made by subjects to each judged level of instigation under normal restraint conditions.

AGGRESSIVENESS-CONSTRUCTIVENESS OF Ss' DISCRETE RESPONSES



AGGRESSIVENESS-CONSTRUCTIVENESS OF RECEIVED INSTIGATION

Figure 12a. Scattergram of subjects' discrete spontaneous responses made to the magnitude of instigation each was judged to have received under normal social restraint conditions.

Mean responses to constructive instigation levels were all neutral or helpful in nature. As in the mean direct response data obtained under the same (normal) restraint conditions there was a slight curvilinear trend in subjects' mean spontaneous response to constructive acts as well.

Discrete response analysis

Looking at the scatterplot in Figure 12 of the subject's spontaneous discrete responses to the actual instigation he received, one noted that in contrast to the scatterplot of each subject's direct responses, the data is not quite as variable and subjects' behavior was judged more nearly neutral. We also noted that there were fewer aggressive responses made to aggressive stimuli, while most of the responses made to constructive stimuli were judged to be constructive in nature. (see Figure 12, p. 157).

Tests of the Experimental Hypotheses

Tests of Central Hypotheses and Comparisons

The first three hypotheses and comparisons concerned predictions that while linear reciprocal changes in subjects' responses to the confederates' instigations behavior should be displayed in some contexts, they would not be displayed as much in others. We predicted that under conditions of low social restraint where inhibiting factors working against reciprocity are minimal, a significant degree of linear reciprocal behavioral change would be displayed, as evidenced by a significant correlation and slope. We further predicted that under normal social restraint conditions, the degree of reciprocal behavioral change would be significantly less, as evidenced by a decreased correlation and slope. In other words, under normal restraint conditions, people's average behavior should be less linearly bound by the stimulus instigation and additionally, the amount of change in the aggression and helpfulness they would on the average reciprocate for any change in the amount of aggression or help-

fulness received, would be correspondingly reduced.

Data bearing on these hypotheses appear below:

Test of Hypothesis 1

Hypotheses are predicted that under low social restraint conditions an extremely strong correlation would be observed between different levels of judged aggressive or constructive instigation and subjects' mean level of direct response to them. A summary of the data bearing on this point as well as results of a t test for the significance of the obtained correlation appears in Table 3. A bivariate graph of this data has already been presented (see Figure 4).

Table 3

Mean Levels of Aggressive and Constructive Direct Response
Reciprocated for Different Levels of Judged Aggressive or
Constructive Instigation, under Low Social Restraint Conditions

Instigation Level	Value	Mean Response	N	Computed r	t
Ext - Strong Aggressive	(0.5)	3.45	6	r=.92, N=8	5.75*
Strong - Mod. Aggressive	(1.5)	4.06	3		
Moderage - Mild Aggressive	(2.5)	4.26	9		
Mild - Neutral Aggressive	(3.5)	4.90	1		
Neutral - Mild Constructive	(4.5)	4.53	7		
Mild - Moderate Constructive	(5.5)	4.61	6		
Mod. - Strong Constructive	(6.5)	5.08	3		
Strong - Extreme Constructive	(7.5)	5.35	4		

NOTE: All values are based on a 9 point scale, where 0 = extremely aggressive, 8 = extremely constructive, 4 = neutral (see Appendix J, p. 301) a r is based on 8 mean responses to each of 8 instigation levels shown.

* $p < .005$

As can be seen, the data provided experimental support for this hypothesis and the obtained correlation was significant at the .005 level. Thus, under low social restraint a significant degree of reciprocal behavioral change as a function of instigation magnitude was obtained.

Test of Hypothesis 2

According to Hypothesis 2, under normal social restraint conditions, the correlation relating the different levels of aggressive constructive instigations to mean levels of direct aggressive or constructive direct response to them should be significantly less than the correlation obtained for the same variables under low social restraint conditions.

Data and results bearing on this hypothesis appear in Table 4 along with a test of their significance using Fisher's test for differences between independent sample r's. (see Appendix K, p. 311).

Table 4

Comparison Between Pearson Statistic Found for Direct Response Data Obtained under Normal Social Restraint Conditions and Pearson Statistic Found for Direct Response Data under Low Social Restraint Conditions

Low Restraint Direct	Normal Restraint Direct	Difference	Z
r = .92 N = 8**	r = .50 N = 8	.42	z = 1.65*

* $p < .05$ - significance approached at .01 level.

** $p < .005$

As can be seen, (as predicted), there was a considerable difference between the correlation of these variables under low social restraint conditions ($r = .92$) and the correlation obtained under normal social restraint conditions ($r = .50$).

A test for significance of this difference using Fischer's transformation formula for testing the difference between sample correlations (see Guilford, pp. 189-190, and Appendix K), shows that with a significant level of .01, the difference approaches significance ($p > .05$). While this difference does not reach significance at the .01 level, test of significance of the correlations obtained under normal and low restraint conditions showed that while the correlation demonstrating reciprocal behavior change is extremely significant under low restraint conditions, under normal restraint conditions it does not differ significantly from 0. Thus, on balance, the hypothesis that the context can determine whether or not reciprocal behavior change is displayed, is supported even if the difference in correlations does not reach significance.

• Comparison 3

Comparison 3 stated that for theoretical reasons mentioned previously, the slope of the regression line relating the mean "direct response" to given levels of aggressive or constructive stimuli should be less under normal social restraint conditions than under low social restraint conditions.

While we make this prediction for theoretical reasons, no formal test of significance of any difference obtained was proposed because the hypothesized difference would not be too big and we would run a great risk of making a Type II error. However, a comparison of the slopes obtained under the two conditions is presented in Table 5 along with results of a test of significance using a Cohen derived formula (1975) for the test of significance of difference between two slopes obtained for independent samples. The derived formula did not require computation of the sums of squares as he did, but instead utilized the standard error of the estimate (see Appendix K for formula and documentation).

Table 5

Comparison Between Slope Statistic Found for Direct Response Data Obtained under Normal Social Restraint Conditions and Slope Obtained from Direct Response Data under Low Social Restraint Conditions

Low Restraint - Direct Data	Normal Restraint - Direct Data	Diff.	t
b=.23, N=8*	b=.13, N=8 N.S.	.10	t = .22 N.S.

* $p < .01$

As can be seen from Table 5 the difference in slopes was in the predicted direction. Using this Cohen-derived formula to test the difference, significance could not be established. However, despite this, whenever we tested each obtained slope value for significance, the slope obtained under low restraint conditions proved to be significant while the normal restraint slope did not. Thus, on balance, the more general hypothesis that the context can affect the obtained slope of the regression line received experimental support despite the fact that the difference in slopes does not achieve significance.

Tests of Similarities and Contrasts Between Sample Statistics Based on Mean Direct Response Data and Previous Population Values Established in Previous Research

We had previously pointed out that previous research has demonstrated that under low restraint conditions people show a strong degree of reciprocity of harmful or helpful behavior. Previous data indicated roughly a .9 correlation between aggressive or helpful insitgations and corresponding changes in people's responses to them with a slope of .45 e.g., on average people reciprocate a one unit increase in aggression with a .45 increase in their own aggression, etc. We theorized that this data had been collected

by researchers under low restraint conditions and that therefore we should obtain similar data in our own low social restraint group, provided we did create such a climate. We further theorized that under normal restraint that are more representative of average real life contexts, significant decreases from the values should be obtained.

The hypotheses relevant to those points and the data supporting them appears below.

Comparison 4

Comparison 4 concerned the expectation that the correlation obtained in our sample under low social restraint conditions was drawn from a population having a .9 correlation between the variables of interest.

While we couldn't demonstrate that this was the case, it could be demonstrated statistically that the data was not inconsistent with this hypothesis by showing the sample correlation was to not significantly differ from .9.

While there was no readily available formula for testing the difference between a sample correlation and a theoretical population value, Fischer had developed a formula for testing the difference between two independent sample correlations involving his Z transformation procedure (see Guilford pp. 189-190). Based on a personal communication with Harshbarger, a formula for the required test was derived from Fischer's formula (see Appendix K for derivation and formula).

Results obtained using this formula appear in Table 6 below.

Table 6

Comparison Between Pearson Statistic Found in Previous Research and Obtained Value of Pearson for Direct Response Data under Low Social Restraint Context

Obtained Value of r	Previous Values Obtained	Diff.	Z
.92 N = 8	.9	.2	Z = .26 N.S.

As can be seen, our obtained sample value of .92 was very consistent with the theoretical population value of .9 we expected to approximate under those conditions.

Comparison 5

Comparison 5 stated that the sample slope of the regression line relating the different levels of aggressive or constructive instigations to mean levels of direct responses to them under low social restraint conditions should not be significantly different from a population value of .45 found in previous research.

The comparison was not stated as a hypothesis because one could not demonstrate that the population value was .45. One could only demonstrate that the evidence was not inconsistent with this position by showing that the sample value did not significantly differ from the theoretical population value.

Again, there was no readily available formula for testing the difference between a sample slope value and a theoretical population value. The only readily available formula for differences between slopes was Cohen's (1975) t test, which tests the difference between two slope

values for two independent samples and which involved the calculation of the sums of the squared residuals as a step in the procedure for obtaining it.

Based on a personal communication with Harshbarger, a formula for the required test based on the standard error of the estimate rather than the residuals, was derived from Cohen's formula (see Appendix K for derivation and formula). Using this Cohen-derived formula, the following results were obtained. (see Table 7 below):

Table 7

Comparison Between Slope Statistic Found in Previous Research and Obtained Value of Slope for Direct Response Data under Low Social Restraint Context

Obtained Value of Slope	Previous Values Obtained	Diff.	t
b = .23, N = 8	b = .45	.22	t = .84 N.S.

As can be seen, the evidence was consistent with the view that the sample was drawn from a population whose slope = .45 as expected. However, since the power of the test is very low the evidence cannot be considered conclusive on this point.

Test of Hypothesis 6

This hypothesis predicted that the correlation obtained for the aforementioned variables under conditions of normal social restraint should be significantly less than the .9 population correlation obtained in previous research.

A test of this hypothesis was made using the Fischer-derived test for significant differences between theoretical population R 's and sample r 's as outlined previously (see p. 163). Results of this test are presented in Table 8 below.

Table 8

Comparison Between Pearson Statistic Found in Previous Research and Obtained Value of Pearson for Direct Response Data under Normal Social Restraint Context.

Obtained Value of r	Previous Values Obtained	Diff.	Z
r = .50; N = 8	.9	.4	Z = 2.06*

* $p < .02$ - significance approached at .01 level.

As can be seen, there was considerable difference between the .9 correlation obtained for the aforementioned variables in previous research and the obtained correlations under these conditions ($r = .5$). The differences between them approached significance at .01 level. Since a theoretical population value was used, the degrees of freedom and the power of this test were greater here than for Hypothesis 2, thus accounting for the increased significance in the obtained results. However, probably due to low power of the test, these results per se could not conclusively establish that the value estimated from previous research were not characteristic of results to be obtained under conditions where the normal restraints of everyday life were operating, although there was a strong trend in support of this conclusion.

Comparison 7.

Comparison 7 stated that for theoretical reasons, the slope of the regression line relating the mean "direct" responses to given levels of aggressive or constructive stimuli under normal restraint conditions, should be lower in magnitude than the previously obtained population slope values.

A comparison of the obtained slope value with the previously obtained population B is presented in Table 9, along with results of

test of significance, using a formula derived from Cohen's formula (1975) for the test of significance of difference between two slopes obtained from a sample and a theoretical population value (see p 308).

Table 9

Comparison Between Slope Statistic Found in Previous Research and Obtained Value of Slope for Direct Response Data under Normal Social Restraint Context.

Obtained Value of Slope	Previous Values Obtained	Diff.	t
b = .13, N = 3, N.S.	.45	.32	t = .54 N.S.

As can be seen, the difference was substantial and in the predicted direction, though non-significant.

Thus, while normal restraint had been demonstrated to differentially affect the correlations, no such effect could be conclusively demonstrated for the slopes of the regression line relating aggressive or helpful instigations and subjects' mean responses to them through this procedure. However, since the normal restraint slope proved non-significant, while the low restraint slope value was significant on balance, we could conclude that our general hypothesis about the effect of context on slope values is substantially supported.

Tests of Hypotheses Concerning Non-Demanded (Spontaneous) Mean Responses.

As will be recalled, we theorized that less reciprocal behavioral change as a function of instigation magnitude should be exhibited when subjects are not forced to respond to instigation, as compared to when they are. It was also theorized that a significant degree of reciprocal behavioral change should be observed under low restraint conditions, as demonstrated by a significant r between instigational levels and mean responses to them. Data bearing on these points is below.

Test of Hypothesis 8

Hypothesis 8 predicted that under conditions of low social restraint, a significant correlation should be obtained between different levels of aggressive or constructive instigation and subjects' mean level of "spontaneous responses" to them. A summary of the data bearing on this point, as well as a t test for significance of the obtained correlation appears in Table 10.

Table 10

Mean Levels of Aggressive or Constructive Spontaneous Response
Reciprocated for Different Levels of Judged Aggressive or Constructive
Instigation under Low Social Restraint Conditions

Instigation Level	Value	Mean Response	N	Computed r	t
Ext - Strong Aggressive	(0.5)	3.49	6	r = .72; N = 8	2.54*
Strong - Mod. Aggressive	(1.5)	4.39	3		
Moderate - Mild Aggressive	(2.5)	4.40	9		
Mild - Neutral Aggressive	(3.5)	4.90	1		
Neutral - Mild Constructive	(4.5)	4.09	7		
Mild - Mod. Constructive	(5.5)	4.76	6		
Mod. - Strong Constructive	(6.5)	4.91	3		
Strong - Ext. Constructive	(7.5)	4.80	4		

Note: All values are based on 9 pt. scale where 0 = extremely aggressive; 8 = extremely constructive; 4 = neutral (see Appendix p 304). The r is based on mean response to each of 8 instigation levels shown.

*p < .025

As can be seen, substantial correlation was found between the 8 instigation levels and the mean levels of spontaneous responses to them under those conditions. However, the correlation approached but failed to reach significance at the .01 level ($p < .025$). Thus, the data supporting the hypothesis that reciprocal behavioral change would be demonstrated

under low social restraint conditions where the responses are not demanded (spontaneous) did not receive sufficient experimental support to be considered significant and the hypothesis was not considered to be conclusively supported

Test of Hypothesis 9

Hypothesis 9 predicted that the correlation obtained under low social restraint, between different levels of aggressive or constructive instigation and mean levels of "spontaneous" responses to them should be significantly less than the correlation obtained using the "direct" response data as the dependent measure.

Since the two correlations were based on data from the sample and since both correlations had the same independent variable in common, the Fischer test was not appropriate. Hotelling (1940) (see Appendix K) had devised a statistic to test the significance of a difference in r 's obtained under these conditions.

The obtained correlations and the results of a test of significance using the Hotelling technique appears in Table 11.

Table 11

Comparison Between Pearson Statistic Found for Direct Response Data Obtained under Low Social Restraint Conditions and Pearson Obtained for Spontaneous Response Data under Low Social Restraint Conditions

Low Restraint Direct	Low Restraint Spontaneous	Diff.	t
$r = .92, N = 8$	$r = .72, N = 8$.20	2.43*

* $p < .05$ approaches significance at .01 level.

As can be seen, the difference between correlations was in the predicted direction and approached significance at the .01 level. However, once again, the difference could not be considered significant and the hypothesis was not adequately supported.

Comparison 10

Comparison 10 stated that for theoretical reasons under conditions of normal social restraint, the sample correlations between different levels of aggressive or constructive instigation and mean levels of "spontaneous responses" to them should be less than the sample correlation obtained using the "direct" response data as the dependent measure. However, since the correlations obtained under normal social restraint should be rather small, no formal hypothesis was proposed since the probability of a Type II error would be quite high. The sample correlations, relevant to this comparison are presented in Table 12 along with results of a Hotelling test for significance of correlation based on the same sample as having one variable in common.

Table 12

Comparison Between Pearson Statistic Found for Direct Response Data Obtained under Normal Social Restraint Conditions and Pearson Obtained for Spontaneous Response Data under Normal Social Restraint Conditions

Normal Restraint Direct	Normal Restraint Spontaneous	Diff.	t
r = .50, N = 8	r = .74, N = 8	- .24	t = 1.03 N.S.

As can be seen, the difference in correlations was in the reverse direction to that expected. Thus, this prediction received no empirical

support. The obtained reversal is rather interesting and will be discussed in the last section of this paper. It should also be remembered, in leaving this section, that the r 's obtained for the mean spontaneous responses to given instigation levels under normal and low restraint conditions were both about the same value (.74 vs .73) and approached significance. ($p < .025$). This pattern of results will be discussed in a later section of this paper.

Tests of Hypotheses Concerning Self Reported Emotional and Motivational Dispositions to Reciprocate Following Aggressive or Constructive Acts.

As mentioned previously, we have theorized that while the context of instigations may inhibit reciprocity, it need not inhibit desires to reciprocate. We predicted that a high correlation should be observed between given levels of instigation and subjects' desires to reciprocate. Further, we predicted that the correlations should approximate .9, the correlation obtained between instigations and subjects' responses under conditions such as those found by Marshbarger in Taylor's study (1967), where social prohibitions against the translation of feelings into behavior were assumed by Taylor to be minimal.

Data relevant to these hypotheses appears below.

Test of Hypothesis 11

This set of hypotheses predicted that strong correlations should be observed between given levels of aggressive and constructive instigations and mean magnitudes of hostile or happy affect reported by subjects, (Scale 5) (See App. G) It also predicted that a strong correlation should be observed between given levels of aggressive instigations and mean levels of desires to be harmful toward the instigator in return (Scale 4), and between given levels of constructive instigation and desires to be helpful toward the instigator in return (Scale 3). No

significant correlations were expected to be obtained between different levels of aggressive instigation and desires to be helpful in return; or between different levels of constructive instigation and desire to be harmful in return.

Data bearing on these hypotheses and tests of its significance are contained in Table 13.

Table 13

Comparison Between Correlations Obtained Between Harmful or Helpful Instigation Levels and Self Reported Mean Magnitudes of Emotional and Motivational Reactions to Them.

Self Report Index	Helpful Instigation	Harmful Instigation
Desires to be Helpful (Scale 3)	$r = +1.00, N = 4$ ***	$r = +.56, N = 4$
Desires to be Harmful	$r = -.41, N = 4$	$r = +.94, N = 4$ *
All Instigations Combined		
Angry-Happy Affect (Scale 5)	$r = +.89, N = 8$ **	

* $p < .05$

** $p < .005$

*** $p < .00001$

As can be seen, the obtained data clearly supported this set of hypotheses, and/or were consistent with them.

Comparison 12

Comparison 12 stated that we expected the correlations hypothesized to be significant in Hypothesis 11, not to differ significantly from .9.

As before, while we could not demonstrate that the correlations in our samples were really drawn from a population where a .9 correlation would be found, it could be demonstrated that the data was not inconsistent with this hypothesis by showing that the sample correlation did not significantly differ from .9. The results of tests of significance between these obtained correlations and the theoretical population value of .9, using the Fisher derived formula for the test of differences between an obtained r and a theoretical population r (see p. 308), appear in Table 14.

Table 14

Comparison Between Correlation Obtained for Harmful or Helpful Instigation Levels with Self Report Scales and Correlation Value Theoretically Expected

Nature of Obtained Correlation	Value Obtained	Predicted Value	Diff.	Z
Helpful Instigations- Helpful Desires	$r = + 1.00, N = 4$.9	.1	1.82 N.S.
Harmful Instigations- Harmful Desires	$r = + .94, N = 4$.9	.06	.27 N.S.
All Instigations Angry-Happy Affect	$r = + .89, N = 8$.9	.11	.12 N.S.

As can be seen, the obtained sample values were very close to .9. As predicted, the obtained evidence was entirely consistent with our

hypothesis that as the instigation levels increase, people's self-reported desires to be helpful and and harmful increase and moreover, the correlation between the two variables was approximately .9, as predicted on the basis of previous research.

Test of Hypothesis 13

Hypothesis 13 predicted that significant correlations, between the measures mentioned in Hypothesis 12, should be obtained under conditions of normal social restraint .

Data bearing on these hypotheses is contained in Table 15 along with the results of a t test for significance of the obtained correlations.

Table 15

Correlations Obtained Between Self Report Scales and Levels of Harmful or Helpful Instigation under Normal Restraint Conditions

Nature of Obtained Correlation	Value Obtained	t
Helpful Instigations- Helpful Desires	r = .59 N = 4	1.03 N.S.
Harmful Instigations- Harmful Desires	r = .98 N = 4	6.96*
All Instigations- Angry-Happy Affect	r = .88 N = 8	4.54**

* $p < .01$

** $p < .005$

As can be seen, two of the three correlations were significant at

the .01 level. The obtained correlation between helpful instigations and desire to reciprocate helpfully was not significant. It must be noted that due to the small numbers of degrees of freedom, the power of the test was not high.

Tests of Hypothesized Relationships Between the Magnitude of Instigations, Their Judged Levels of Abnormality, and the Mean Responses to Them as a Function of the Context Wherein it Occurs.

As discussed earlier, pilot data suggested that increasing magnitudes of aggression were seen as increasingly abnormal by college students, resulting in a high correlation (r) between the two dimensions (see Appendix A). We also found that a curvilinear trend existed between increasing constructiveness and judgments of its abnormality.

The first two hypotheses in this section were derived from this pilot work and the data represented an attempt at replication. The next two hypotheses took the replication as given and concerned its theoretical implications for reciprocity under different contexts. Specifically, we hypothesized that for those instigation levels judged normal, positive correlation should be observed between them and the mean response to them, under normal social restraint, and the r would be equal in magnitude to the r obtained between all instigation levels and mean responses to them under low social restraint. This hypothesis was made because we theorized that the normal restraint would not inhibit normal responses to normal instigations. We also predicted that under normal restraint conditions for those instigation levels judged abnormal, the r between them and the mean responses to them should be significantly less than the r obtained for instigations similarly deemed abnormal under low social restraint. This hypothesis was made because, under normal social restraint, we theorized that powerful inhibitions would prevent people from making abnormal responses to these abnormal instigations, while under low social restraint,

the inhibitions were counteracted.

Data relevant to these hypotheses appear below.

Test of Hypothesis 14

Hypothesis 14 predicted that there would be a strong linear relationship between any given level of judged aggressiveness of instigations and the mean judged level of the normality-abnormality of such instigation.

Data bearing on this hypothesis is presented in Table 16 together with results of a t test for the significance of the correlation thus obtained. As can be seen, this hypothesis was strongly supported.

Table 16

Table of Instigation Levels and Mean Rated Judgments of Their Normality-Abnormality

Instigation Level	Value	\bar{x}	Normality-Abnormality	a Computed r	b
Strong-Extreme Aggressive	(0.5)		7.02	r agg = .56, N=41	**
Moderate-Strong Aggressive	(1.5)		5.39	Eta = .64*	
Mild-Moderate Aggressive	(2.5)		5.00		
Neutral-Mildly Aggressive	(3.5)		5.55		
Neutral-Mildly Constructive	(4.5)		2.94	r const = .12, N.S., N = 38	
Mild - Moderately Constructive	(5.5)		2.39	Eta = .37 N.S.	
Moderate - Strongly Constructive	(6.5)		2.09		
Strong- Extremely Constructive	(7.5)		3.84		

a based on rater Scale 2 (see Appendix J, p. 308) where 0 = extremely normal; 8 = extremely abnormal; 4 = neutral.

b based on correlation of magnitudes of aggressiveness-constructiveness of each instigation with its rater normality-abnormality.

*p < .005

**p < .0005

Test of Hypothesis 15

Hypothesis 15 predicted that there would be a curvilinear relationship between given levels of judged constructiveness of instigations and their mean levels of judged abnormality.

The data bearing on this hypothesis appears in Table 16 together with computed Eta. (see Table above)

As can be seen, while there was a tendency toward a curvilinear relationship between the variables, it was not significant. Thus, despite the slight trend observed in the data, due to great variability in subjects rated responses to any given level of constructive instigations, the hypotheses failed to receive statistical support.

Test of Comparison 16

Comparison 16 stated that if we looked at only those aggressive or constructive instigations judged to be normal under conditions of normal social restraint, the correlation between given levels of aggressive or constructive instigations and mean direct responses to them, should not significantly differ from the correlation obtained between all levels of aggressive or constructive instigation and mean direct responses to them under conditions of low social restraint. While it was not possible to demonstrate that it was highly likely that both sets of data were drawn from the same population, we could show that the evidence was not inconsistent with this assumption by showing that the two correlations were not significantly different from each other.

In order to select those instigations judged to be "normal and reasonable and justified" under normal social restraint conditions, we obtained the raters' mean judgment of normality and abnormality of instigations (rater Scale normality-abnormality, see Appendix I, p. 308) for each level of instigation, under normal restraint conditions.

Those instigations whose mean judgment was less than 4 (0 = extremely "normal", etc., 8 = extremely abnormal) were included in the analysis. The obtained correlation under normal restraint conditions between only those levels of instigation and the mean direct responses to them was then compared with that obtained between all instigations and the mean direct responses to them under low restraint conditions. The comparison between these correlations appears in Table 17. No test of significance could be made due to the small N used in the normal restraint condition.

Table 17

Comparison Between Pearson Statistic Found for Direct Response Data to Instigations Judged Normal under Normal Social Restraint Conditions and Pearson N Found for Direct Response Data for all Instigations under Low Social Restraint Conditions

All Low Restraint Direct Data	Normal-Normal Restraint Direct Data	Difference
$r = .92; N = 8$	$r = .91 N = 3$.01, N.S.

As can be seen, and as predicted, the obtained correlation hardly differed from one another. The results therefore are very consistent with the hypothesis that under normal social restraint conditions, there would be a correlation between different levels of acceptable, normal behavior and the mean direct response to them equivalent in magnitude to the correlation observed where social restraints are minimal.

Test of Hypothesis 17

Hypothesis 17 stated that under conditions of normal social restraint, for only those aggressive or constructive instigations judged to be abnormal, the correlation between different levels of aggressive or con-

destructive instigation and mean direct responses to them, should be significantly less than for the correlation obtained between given levels of aggressive and constructive instigations and mean direct aggressive or constructive responses to them, under condition of low social restraint for only those instigations likewise judged to be abnormal. This prediction was in keeping with the theory that under normal social restraint, behavior judged abnormal due to its excessiveness would not be reciprocated compared to a situation of "low restraint" where the instigations and the reciprocity behavior was legitimized.

In order to select those instigation judged to be abnormal in each of the two restraint procedures, mean rating of their abnormality were utilized in a manner similar to that outlined previously. (see p. 178)

The correlations obtained and a Fisher test for the significance of their difference are shown in Table 16.

Table 16

Comparison Between Pearson Statistic Found For Direct Response Data to Instigations Judged Abnormal under Normal Social Restraint Conditions and Pearson N Found for Direct Response Data for Instigations under Low Social Restraint Conditions.

Low Restraint - Abnormal Only	Normal Restraint - Abnormal Only	Diff.	Z
r=.99 N = 4	r= .24 N = 5	.75	1.97*

* $p < .026$; approached significance at .01 level.

As can be seen, the obtained correlations and the difference between them accorded fairly well with our theoretical expectations. The obtained sample difference between these two correlations approached significance

at the .01 level.

Comparisons Bearing on the Effect that Differences in the Statistical Treatment of Reciprocity Data Have on Subsequent Findings.

As discussed earlier, we conjectured that the type of statistical procedures used in analyzing the data from previous research produced results that might not fully characterize the behavior of the subjects under test.

While we have analyzed our results similarly to allow for some comparison, we also reanalyzed our results to see what the statistics would be like using the alternate procedure of correlating the instigation each person received with the response each person made. We predicted that the correlations would be substantially reduced but the r for our low restraint direct data would still be significant.

Results bearing on these predictions are given below.

Test of Hypothesis 18

Hypothesis 18 predicted that, for conditions of low social restraint, if one correlated the judged magnitude of aggressiveness-constructiveness of the instigation each subject received, with the judged magnitude of the actual direct response that he made, the result should be a significant correlation.

The obtained sample correlation and the results of a t test of its significance are presented in Table 19.

Table 19

Pearson r Obtained by Correlating Each Individual's Response to Each Discrete Instigation for Low Restraint - Direct Response Data

Obtained Correlation	Test of Significance
$r = .52$ $N = 39$	$t = 3.70^*$

* $p < .0005$

As can be seen, this alternate statistical procedure of correlating each person's discrete response with the actual instigation received, produced a significant correlation under low restraint conditions. This correlation however is lower than the one obtained for the same data by correlating the mean of the subjects' responses to each of the 8 levels of instigation and implied substantially less stimulus control of response behavior than was implied by the latter procedure. A personal communication with Harshbarger indicated that he too carried out this alternate analysis and got a similar r for his data.

Comparison 19

Comparison 19 stated that under low social restraint conditions the correlation obtained through the procedure mentioned in Hypothesis 14, should be substantially less than that obtained by grouping the subjects by level of instigation received and correlating their mean direct responses to each given level of instigation as represented by the mid-point of each interval used in the grouping process.

A comparison of the obtained correlations is shown on Table 20.

Table 20

Comparison Between Pearson r Obtained by Correlating Each Individual's Response to Each Discrete Instigation with Pearson Statistic Obtained by Correlating Mean Responses to Each of Eight Levels of Instigation for Low Restraint - Direct Response Data

Mean Response r	Discrete Response r	Difference
$r = .92, N = 8$	$r = .52, N = 39$.40

As can be seen, the values of the correlations obtained accorded well with our theoretical predictions. No test of significance was made since both correlations were based on the same raw data obtained from the same subjects and no statistic was readily available to test such a difference.

A Note About Our Results

While for the most part our results proved to be consistent with our original hypotheses, in several cases (particularly in tests of Hypotheses 2, 6, 8, 11 and 17) only a borderline level of significance was achieved. Despite the borderline significance of these latter results, we will not consider them to be non-significant in our discussion of them. There are several reasons for this decision:

First, the power of the test in these cases was quite low. Sample size was necessarily small since only eight (8) instigation levels were studied. A conservative .01 rejection region was chosen, since it was entirely possible that due to the number of hypotheses we tested, one or two might be significant just by chance. As a result of such factors, the possibility of committing a Type II error by rejecting those borderline results as insignificant, must be considered.

Secondly, those results while not significant at the .01 level, were for the most part, significant at the .05 level. Again, the possibility that a Type II error might be committed must be considered, in view of the fact that these differences approached significance so closely.

Thirdly, and most important, a finding of non-significance would be not only inconsistent with our hypothesis but moreover, would be inconsistent with the overall pattern of the data, as well as inconsistent with other data that was judged significant. As Cohen (1975) pointed out, such consistency or inconsistency is an important factor to be taken into account when considering data. As he put it, citing Tukey:

" 'Data Analysis' (as opposed to statistical analysis) accepts 'inadequate' data, and is thus prepared to settle for 'indications' rather than conclusions. It risks greater frequency of errors in the interest of a greater frequency of occasion when the right answer is suggested. It compensates for cutting some statistical corners by using scientific as well as mathematical judgment, and by relying on self-consistency and repetition of results. Data analysis operates like a detective searching for clues, rather than a bookkeeper seeking to prove out a balance." (p.15).

In short, while we have generally tried to be like Cohen's bookkeeper in reporting our results, we intend to be more like Cohen's detective in interpreting our borderline significant and "non-significant" findings, given the consistency of these finding with the rest of the data.

CHAPTER 5

DISCUSSION

The Effect of Context on Reciprocity

Our main hypotheses concerned our contention that the context under which aggressive and constructive instigation occur can importantly affect the degree to which behavioral responses of the recipients, are a predictable reciprocal linear function of changes in instigation magnitudes.*

As hypothesized, the results of the present research, taken together, showed that the context can importantly affect the degree to which people will reciprocate changes in instigation magnitude with corresponding behavioral changes.

Normal Restraint and Reciprocity

As hypothesized, the results of the present research further indicated that under normal restraint conditions, people's direct responses to instigations, on average, not only show less tendency to consistently and linearly vary with changes in instigation levels than that obtained in previous laboratory studies, but moreover, the data fail to support the hypotheses of reciprocity entirely. This finding was expected in line with our view that powerful social restraints normally operative in a naturalistic context inhibit reciprocity behavior.

Findings Which Support Qualifications to be Placed on the Negative Report of Reciprocity under Normal Restraint Conditions.

As discussed, our data indicated that where the normal restraints

* Throughout our discussion, we will be talking about linear relationships and predictability, since such relationships and such predictability must be demonstrated as a necessary step in showing reciprocity. The reader should be cautioned that the absence of such relationships in the data, while indicating that reciprocity was also absent, would not necessarily mean responses were not a predictable nonlinear function of instigation level.

theorized to operate in a real life situation are minimally interfered with, the hypothesis that people will reciprocate changes in aggressiveness or constructiveness with same, failed to receive experimental support in analysis of the "direct response data". However, the evidence obtained under these normal restraint conditions, also demonstrated that while in general these normal restraints inhibit reciprocity behavior, there are some circumstances where such reciprocity behavior may be displayed even where normal restraints are operative in the situation. A discussion of these qualifying circumstances proceeds below.*

Reciprocity behavior shown under Normal Restraint Conditions in response to Instigation Levels Judged Normal

Empirical support for the reciprocity hypothesis was obtained as hypothesized, under normal restraint conditions where only those instigation levels judged to be "normal reasonable and justifiable" were included in the analysis. Under those conditions, the data indicated that an extremely strong positive relationship was found between the levels of instigation included and the mean direct responses to them. This correlation ($r = .91$) was as strong as that obtained in previous laboratory research and was entirely consistent with previous values in magnitude.

Inspection of those instigation levels judged to be "Normal" shows that they ranged between neutral and moderately constructive. For all other levels of instigation under normal restraint conditions, as expected, no significant degree of reciprocity behavioral change was obtained. Thus, the evidence obtained indicated that in a naturalistic setting where normal restraints are operative, the range of instigations magnitude employed and their associated normality-abnormality

*It should be noted that in this discussion, we have purposely omitted the question of whether such positive reports of reciprocity involve a "rough equivalence" of exchange. This question, will be addressed in a separate section of this paper.

may play a crucial role in determining whether reciprocal behavioral changes as a function of instigation magnitude is displayed. I will have more to say on this later.

Reciprocity behavior displayed under Normal Restraint Conditions
When subjects no longer are verbally forced to respond

Qualified support for the reciprocity hypothesis was also obtained when the "spontaneous" responses of subjects to instigation levels was analyzed. It will be recalled that the data labelled "spontaneous" consisted of subjects' responses to the instigator's behavior when the instigator was not attempting to instigate a verbal interaction with them; either the confederate was instigating a non-verbal stimulus or had ceased his verbal comments and waited for subjects to indicate any further interaction. In either case, we hypothesized that since the cues eliciting reciprocity would be weaker under these conditions, a weaker reciprocity relationship should be obtained. Accordingly, we had hypothesized that analysis of the "spontaneous" data should reveal a lowered tendency to reciprocate, compared to the strength of the tendency to reciprocate shown for the direct response data where the cues were stronger. It was therefore surprising to learn that not only was this hypothesis not supported by the data obtained under normal restraint, but moreover, in contrast to the overt "direct response" data where no significant degree of reciprocity was displayed, the spontaneous data revealed an increased tendency to reciprocate which approached significance. While the finding was not statistically significant, it suggested that under normal restraint conditions where subjects' responses are no longer verbally elicited, a substantial amount of reciprocity behavior may on average be displayed, at least under some circumstances. Some reasons for these findings will be discussed in more detail a little later.

Support for reciprocity obtained under normal restraint conditions based on analysis of subjects' self reported emotional and motivational dispositions following instigations

Self reports by subjects also provided some support for the reciprocity hypothesis under normal restraint conditions.

While overall, people may not reciprocate increased magnitudes of aggression with increased aggressiveness under normal conditions, we have obtained evidence showing that as hypothesized, they develop strong emotional desires to reciprocate the "rough equivalent" of that received. The strength of the relationships between various levels of aggressive instigations and the mean amounts of harm subjects desired to reciprocate, approaches in magnitude the strength of the relationship between instigation levels and overt mean responses to them found in previous research.

In contrast to these findings though, a substantial but non-significant correlation was found between constructive instigation levels and mean amounts of constructiveness. Inspection of the data reveals that a number of factors could account for their lack of desire to reciprocate. However, we will have more to say about this later.

Low Restraint and Reciprocity

To make sure that any difference from previous results was really due to the normal restraint context, for another group of subjects, we attempted to induce a low restraint context similar to the contexts subtly induced in previous research. In all other respects, the simulated naturalistic nature of the study was maintained under these conditions of low social restraint. As hypothesized, under low restraint conditions, it was found that subjects not only showed a strong significant tendency to reciprocate increases or decreases in aggressiveness or constructiveness with corresponding changes in their own behavior, but moreover, the strength of this tendency was not significantly different from that obtained in pre-

vious research. Further, slope statistic representing the actual magnitude of increase of aggressiveness or constructiveness reciprocated for any one point increase in aggressiveness or constructiveness, while somewhat lower in value, was significant.

The strong positive finding of reciprocal behavioral change under low restraint conditions, in contrast to the lack of reciprocity found under normal restraint conditions, thus demonstrated that the normal restraints of real life strongly inhibit reciprocity behavior. However, our data also suggested some qualification which needs to be placed on the report of reciprocity under low restraint conditions. These qualifications are discussed below.

Findings Which Support Qualifications to be Placed on a Positive Report of Reciprocity under Low Social Restraint Conditions*

Possible Lack of reciprocity found under low restraint conditions in analysis of spontaneous response data

We had hypothesized that the magnitude of reciprocity behavior displayed in the spontaneous response should be significantly less than that obtained in analysis of the direct response data. The obtained results do show a trend in this direction that approaches significance at the .01 level. However, not only was the magnitude of the reciprocity relationship obtained for the spontaneous data lower, but its value proved to be of only borderline significance. Ordinarily, we would have to conclude that the question of whether subjects reciprocate under low social restraint conditions, when not directly drawn into verbal interaction

*Again, it should be noted that the question of whether the evidence indicated that subjects' reciprocated with a "rough equivalence" is not addressed here but will be addressed in the next section of this paper.

with the confederates could not be discarded, based on the present "spontaneous" data analysis. However, since an almost identical magnitude of reciprocity, again of borderline significance, was obtained under normal restraint conditions as well, we feel confident that a Type II error is being made and feel confident in interpreting the magnitude of reciprocity displayed in the analysis of the spontaneous data under low social restraint conditions as weaker than that obtained in analysis of the direct response data, but no less substantial or real. If this conclusion is warranted, which we believe it is, we need only conclude that under low social restraint conditions, when subjects are not drawn into verbal interaction by a stranger, subjects on average will still display reciprocity behavior, but the strength of the reciprocity relationship will as predicted be appreciably diluted.

General Qualifications to be Placed on All
Positive Reports of Reciprocity Cited

The Question of Rough Equivalence

The preceding discussion suggested that under the following conditions, subjects' behavior consistently increased or decreased in magnitude along the 9 point aggressive-constructive scale as a function of the instigation magnitude received:

1. Normal restraint data - direct responses to normal and acceptable instigation magnitudes.
2. Normal restraint data - "spontaneous" responses to all magnitudes.
3. Low restraint data - direct responses to all instigation magnitudes.
4. Low restraint data - "spontaneous" responses to all magnitudes.

While such reciprocal changes in subjects' responses as a function of instigation levels constitute one necessary criterion of "reciprocity behavior", in many people's view, this may not be a sufficient criterion.

They may feel reciprocity behavior is only demonstrated when subjects not only tailor their behavioral response magnitude to the magnitude of instigation received, but in addition, reciprocate with at least the "rough equivalent" of that received.

In the present study, we did not at first deem it necessary to evaluate our correlational data from the standpoint of determining whether such a "rough equivalence" took place, in order to determine if "reciprocity" took place. This was because in designing the study, we set up a scale dimension which conceived of aggressiveness and constructiveness as lying on a bipolar continuum. As such, constructive responses could be viewed as very unaggressive acts (see Harshbarger's data in Figure 3) and constructive instigations could be viewed as very unaggressive instigations. Since from this perspective we would always be correlating aggression with aggression, regardless of data analysis done, the criteria of "rough equivalence" would always be satisfied (as it was in Harshbarger's analyses) any time a significant correlation is obtained.

However, in view of the fact that many readers may not share this conceptual perspective, we have analyzed our obtained correlations from the perspective of attempting to see if any of them represent the more everyday notions of "rough equivalence" e.g. aggression with aggression and constructiveness with constructiveness.

The results of this analysis indicate that for each of the situations cited above where reciprocal behavioral change was found, true reciprocity in this latter sense was only shown in people's responses to constructive instigations, particularly those of normal and acceptable magnitudes. Thus, using this more stringent definition of "rough equivalence" the results show that regardless of restraint conditions and strength of cues

eliciting reciprocity, normal and acceptable magnitudes of constructive behavior are on average, always reciprocated and aggressive behavior is on average almost never reciprocated. Abnormally constructive behavior may or may not be reciprocated depending on the context and other factors.

Question of Response Variability

A second qualification that needed to be placed on all positive reports of reciprocity cited is that despite the high correlation between instigation levels and the mean responses to them, there was great variability in subjects' responses to any given instigation level.

As expected, when we looked at the correlation between the magnitude of instigation each subject received with the discrete response he actually made, across all subjects, it was considerably lower than the correlation statistic based on the mean response to each instigation level. While the former correlation was as statistically significant as the latter, its lower value reflects the fact that knowing the instigation level only allows us to explain approximately 25% of the variability in the subjects' behavior. As such, the strength of the reciprocity relation, even under low social restraint, did not seem to be anywhere near as strong as previous research implied, and the behavior of such subjects is therefore far less predictable a function of instigation level than previous research would lead us to believe.

Is a Norm of Reciprocity Relevant to Understanding People's Everyday Social Aggressive and Constructive Behavior?—A Post hoc Inspection of Subjects' Rationales for Their Behavior

In our introduction we raised the question of how characteristic the laboratory findings of empirical reciprocity were for everyday life social behavior. Our data indicates that in answer to this question, such reciprocity is much less a feature of everyday social interactions

than reciprocity theory and previous research would lead us to believe.

In our introduction we also raised the question of whether or not a norm of reciprocity really underlay the empirical reciprocity data obtained in previous studies. More generally, the questions could be raised whether such a norm really exists, whether it is a universal norm as Gouldner implied and whether it is as controlling and guiding a force over recipients' aggressive and constructive behavior as previous results suggested it was.

In answer to these latter questions, concerning the norm of reciprocity, the data we reported thus far indicates that certainly the norm is not as dominant a force in determining the behavior of recipients of aggressive and constructive social acts in naturalistic settings as previous research data implied it might be. This is because if it were a dominant force, subjects' behavior would have been more predictable and consistent than it actually was.

The more important question however, still needs to be addressed. Namely, does a norm of reciprocity really exist and does it play any role in controlling aggressive and constructive behavior in naturalistic settings?

Such a question cannot be answered simply by observing subjects' behavior. For just as we pointed out earlier that reciprocity behavior does not necessarily indicate that a norm of reciprocity exists and is responsible for it, so too, a lack of reciprocity behavior does not necessarily negate the existence or the influence of the norm of reciprocity. Practically the only way to determine if such a norm really exists is to examine subjects' self reported rationales for their behavior to see if they made their behavioral decisions based on such a norm.

As we have pointed out already, while previous researchers and my-

self have postulated that a norm of reciprocity was involved in mediating reciprocity behavior, there had been little attempt by previous researchers to determine whether in fact, an internalized norm of reciprocity determined the results previously obtained. Krebs' (1970) criticisms of the current research on altruism are equally applicable to aggression and to the behavioral reciprocation of aggressiveness and constructiveness as well when he stated:

"Most behavioral research has skirted the problem associated with the specification of altruism by employing operational definitions. It has examined antecedents of behavior that seemed altruistic and assumed that motivations were congruent. Although the antecedents of operations with undefined motivational bases can be elucidated, it is important to make sure that the conclusions that are drawn about them do not relate back to the motivation implied by the category of behavior in question...it must be realized that although terms such as prosocial behavior...imply self sacrificial other directed behavior, they do not establish it." (p. 250)

While Krebs' comments applied to the study of altruism, they seemed equally applicable to the study of aggression and the "norm of reciprocity". In fact, Krebs in reviewing the research on the norm of reciprocity of constructive acts, remarked that:

"...the postulation of social norms can...end in tautology. A particular response, for example, can be predicted on the basis of a norm. If it occurs a norm is said to have an effect. If it does not occur, the situation is said to fall outside the range of this norm. In cases where the norm is established on the basis of the behavior it is supposed to predict, it is in the same position as Skinner's reinforcer...its existence is established by the effect it produces...the methods used to study social norms should be different from the operations used to study their effects. And once norms are identified, focus should shift to the dynamics of their internalization and the conditions of their effect."

Thus, to see if "norms" indeed were operating and to see the "condition of their effect", an independent post hoc inspection of the subjects' stated rationales was done. This inspection was carried out by grouping the nine stimulus actions used in the study into four different groups based on my judgment of how aggressive or constructive each was. These groupings were Neutral to Moderately Constructive; Strong to Extremely Con-

structive; Neutral to Moderately Harmful; Strong to Extremely Harmful.

A description of the rationales for the behavior given by subjects who received each group of instigations, as revealed by our inspection, appears below. Also included are various theoretical formulations which should explain this data, and a discussion of the implications of these theories for the "norm of reciprocity".

Neutral-Moderately Constructive

It will be recalled that subjects receiving instigations falling in this range were on average likely to reciprocate increased constructiveness with same, regardless of context. Moreover, these instigations were the most likely to be judged as normal. Inspections of subjects' rationales for their behavioral responses to these instigations, however, did not produce much evidence in support of the idea that a norm of reciprocity mediated their behavior. Rather, the subjects seemed to feel that a certain degree of helpfulness was expected from anyone in such situations, and they wished to respond similarly. The more helpful the confederate was, the more likely subjects were to feel at ease, happy, content, etc. and the more they felt inclined to respond in kind- not because they felt they ought to return what was given but rather because they wished to lower their anxiety level by affiliating with the confederate and as a result of the confederate's helpful behavior, felt more at ease to do just this and felt more positive toward him. The more neutral the confederate's behavior was the more subjects seemed to feel either that there was no need to interact with the confederate or no opportunity to do so. A few subjects felt upset, annoyed and frustrated by the neutral confederate's lack of reciprocity on his part, when they attempted to affiliate with him and his lack of initiative in instigating an interaction when they did not.

Theoretical explanatory frameworks for the data and implications for the norms of reciprocity.

Despite the reciprocity behavior shown to these instigations, on the surface the self report data does not support the reciprocity theorists' contention that such reciprocity is motivated by an individual's desire to pay back past obligations or feelings on his part that he must do so.

It is possible to theoretically explain the self report data while still allowing for the existence of the norm of reciprocity. One could theorize that the failure to find feelings of obligation expressed in the self report data was because the instigations involved here were considered expected behavior in this situation and hence not behavior that would induce a feeling of indebtedness. Or, one could theorize that these instigations were just not constructive enough, or tangible enough, to induce a conscious feeling of obligation in these subjects.

However, while these explanations seem reasonable it seems equally reasonable to theoretically explain the behavioral and self report data without recourse to a norm of reciprocity.

For example, consistent with Iserl et al findings (1970) that when people are in a good mood they are apt to be helpful, one could theorize that the more constructive the confederate's behavior, the more of a good mood he put the subjects into, the more ready they were to be helpful.

Alternately, based on Schacter's affiliation theory, we could theorize that the confederates gave these anxious subjects an increased opportunity to decrease their anxiety by relating to them, which the subjects then took advantage of in order to decrease their anxiety.

While based on just this data alone, we cannot say conclusively which theoretical explanation has validity here, the important point is that despite the high level of reciprocity behavior shown in response to these

instigations, no clearcut evidence for the operation of a norm of reciprocity was found.

Strong-Extreme Constructive

It will be recalled that subjects receiving these instigations in some cases showed slightly decreased helpfulness as the judged helpfulness of the confederate's behavior increased, in contrast to the behavior predicted from a simple statement of reciprocity theory.

Reciprocity theory would also predict that subjects receiving strong to extreme constructive acts would feel indebted to the instigator. Our data did not show this. Inspection of the self reports of subjects receiving strong-extreme instigations showed little evidence that the subjects felt indebted.

Instead, some subjects felt helped by the confederate and expressed willingness, but not feelings of obligation, to help him if he needed it. The self reports of others revealed that a number of the subjects were taken aback by the unexpectedness of the confederate's behavior and distrusted or distorted the confederate's intentions. Subsequently, these subjects experienced feelings of anxiety or annoyance at the confederate. Moreover, they reported they were reluctant to express their feelings because they said they wished to avoid embarrassing themselves or the confederate, hurting him, or creating an unpleasant situation.

Implicit in their rationales however, was their feeling of being obligated to respond positively to the confederate and to avoid expressing what they felt were in effect, harmful emotions. Thus, rather than feeling morally indebted to the confederate, the subjects felt extremely constrained in their responses.

On the surface then, the self reports of subjects receiving strong-extreme constructive instigations did not show much evidence of the

feelings of indebtedness that the simple statement of reciprocity led us to believe they would experience. On the other hand a number of the subjects did report feeling externally constrained. We therefore entertained the possibility that for one reason or another, these subjects may have externalized their own moral feelings of indebtedness. If this were true, our data would therefore indicate that a norm of reciprocity was at work but was not consciously recognized by our subjects. As such it would lend support to a modified version of reciprocity theory. However, in order to hold this as a tenable assumption, we would have to understand why subjects did not abide by or report their internalized norm. We would also have to understand why instead of feeling indebted to the confederates, many felt anger and annoyance at them.

Reactance theory as an explanatory framework for this data - a review of the literature and implications for the norm of reciprocity.
Review of the literature

In attempting to understand our data, we looked at the literature to see if similar data had been obtained. In fact, Brehm (1966) and others had similarly found evidence which simultaneously supported reciprocity theory's contention that there is a norm of reciprocity while contradicting the simplistic notion that this norm would lead to behavioral reciprocity. The evidence they produced suggested that behavioral reciprocity might be lessened if people felt too obligated. Brehm postulated reactance theory to deal with this phenomenon.

According to Brehm's theory of psychological reactance (1966) :

"for a given individual at a given time, there is a set of behaviors in which he believes he is free to engage. Any reduction or threat of reduction in that set of free behaviors arouses a motivational state "reactance", which is directed toward reestablishment of the lost or threatened freedom. The amount of reactance experienced from any given threat to, or reduction of freedom is a direct function of how important it is to the individual to have that freedom." (Brehm & Cole, 1966 , pp. 420-421).

While Brehm & Cole originally applied their theory to attitude change, they suggested its particular application to constructive behavior and carried out a number of studies on its applicability to this area.

Consistent with their theory, they demonstrated that when people deem it important not to become partial to a person, if that person attempts to go out of his way to do something constructive for them, they are less likely to do a favor for that person than if he hadn't offered a favor in the first place, e.g. they behave contrary to what reciprocity theory would predict. On the other hand, when maintaining their impartiality was not important to the subjects, results supporting reciprocity theory were obtained, e.g. under these conditions, subjects were more apt to help someone who had previously helped them than someone who hadn't. Worchel & Brehm (1971) and Andreoli, Worchel, & Folger (1974) extended findings similar to these by showing that the restriction of freedom need not actually occur but may only be implied by the situation.

Other writers have extended the finding still further through tests of hypotheses bearing directly or indirectly on Brehm's contention that the magnitude of reactance increases with the importance of the threatened freedom. Andreoli et al (1974) have shown that as long as the subject valued the threatened freedom to some degree, this hypothesis was supported. By inference, to the degree a person values maintaining given levels of behavior, for example, behavior judged to be normal or expected to that degree, reactance would be increased if their freedom to continue to act normally was threatened.

Other studies have dealt with the psychological outcomes that ensue following the arousal of reactance. Worchel (1971), Worchel & Andreoli (1974) found that when people are in direct interaction with each other a person who arouses psychological reactance will tend to be derogated.

However, when people know that they are committed to future interaction with each other, the derogation as well as the reactance is likely to be felt but only expressed covertly if at all, according to the data of Pallak & Heller (1971). In fact, the data of Kesler & Pallak (1967) demonstrated that people in such positions may even do what they think they are obligated to do in order to avoid "future disagreement or unpleasant interaction with the other", in a situation where other alternatives are impractical. However, despite the unwillingness of people in such situations to display their feelings of reactance, the very fact that people feel committed to the interaction may intensify the reactance even more. Presumably in such situations some people may experience intense cognitive dissonance and deal with it through a variety of dissonance reducing procedures. In this regard Worchel & Andreoli (1974) theorized that when people cannot restore their lost freedoms by behaviorally violating the norms of reciprocity they may attempt to restore it by "reducing the strength of the norm... (it) may be diminished by the individual attributing the actor's norm eliciting behavior as being situationally determined rather than caused by the individual's own traits". According to this view, once a person has made this attribution, he would no longer feel as compelled to reciprocate, and his reactance would decrease, as he thus regained his lost freedom. In turn, as a result of the reduced reactance, less derogation and dislike for the other person should occur.

Worchel et al, also suggested however, that if people are prevented from attributing another person's behavior to the environmental press of what is expected in the situation by strong evidence that the other person did in fact freely choose his behavior, then increased reactance, increased overt and covert derogation of the confederate, and increased overt or

covert hostility would eventuate instead.

Worchel et al presented evidence that supported their theory. In their report, they mentioned that while derogation was a natural derivative of reactance they did not feel it was a means through which reactance was reduced. However, I believe that certain types of derogation may indeed lower reactance through a similar technique of altering the focus of responsibility and rationale for the act. One example of this is Schopler & Thompson's (1970) theory, and the evidence marshalled for it, that when favors are perceived as selfish vs selfless, decreased constructive reciprocation would occur, presumably due to a lessening of the normative pressure to reciprocate. While Schopler & Thompson did not believe reactance accounted for these results, it is reasonable to assume that if reactance is a viable phenomenon and does lead to attribution, then an attribution of selfishness would lessen both the normative pressure to reciprocate and reactance as well. Similarly Cherulnik & Citrin (1974) demonstrated that there are individual differences in the degree of reactance subjects will experience in different situations, dependent on the prior degree to which they felt they indeed had control over the threatened freedoms in the first place. This in turn, they showed to not only be a function of the situation but also, of the personality of the subjects. In sum, it does not seem unreasonable to assume that reactance may encourage some individuals to attribute derogatory characteristics to the instigator which in turn would decrease reactance.

One such type of attributed derogation may be that of insanity.

As Kruglanski & Cohen (1973), Kruglanski & Cohen (1974) have demonstrated, acts that are seen as both inconsistent with a person's personality and the situation they are in, e.g. acts considered irrational, are not viewed by people as the personal responsibility of the actor, even

where he may still be perceived to have freely formed such acts. Further, a person is not seen as having freely behaved in a given manner, where his acts are not at least consistent with what we know of his personality or we may add, at best potentially consistent - our knowledge of his personality being gained either through extensive past experience or "focal cues conveyed by the ambient context and characteristics of the actor's behavior" (Kruglanski & Cohen 1974, p. 178), such as the degree of the actor's uncertainty prior to his behavior, the degree of commitment to it, etc. In any event, we would conjecture that if the attribution of personal freedom for the act or probably more importantly, perceived responsibility for the act cannot be made, the norm of reciprocity would be just as weakened as if the act were attributed to the environment and as a result, reactance and reciprocity would both be reduced at the expense of the necessity of maintaining the belief in the other person's insanity.

Schopler & Thompson's finding that a favor done in an informal context produced more constructive response than no favor, while one done in a formal context showed the reverse pattern, is an example of this. As these authors explained, they did not feel reactance produced these findings but instead felt that favors done in a formal context are seen as inappropriate and interpreted as selfish, while favors done in an informal context being perceived as appropriate, were in turn seen as genuine. While these attributions, in turn, were assumed by these authors to have weakened the norm of reciprocity rather than aroused reactance, we feel it can be reasonably assumed that where reactance is aroused and this type of derogation of the instigation subsequently occurs, lowered reciprocity and lowered reactance would occur. However, the price would be increased derogation of the instigator.

Relation of reactance-reciprocity theory to the present self report data.

In relating the theory and research on reactance theory to our present data, we first note that our data demonstrated that people have very definite ideas about how much aggressiveness or constructiveness on their part and on the part of others, is considered normal or acceptable between strangers. This range of behavior goes from approximately neutral to approximately moderately constructive. Further, we can note that our data shows that almost all of the subjects' responses fall within this range. If we assume that people are strongly motivated to keep their behavior within this range for a variety of reasons and expect others to do the same, a lot of the self report data can be readily understood in terms of a modification of reciprocity theory in light of reactance theory.

Subjects desiring to confine their behavior to this range would be apt to be well disposed toward someone who allowed them to do this and would like that person more. This might explain the self reports of subjects who received the neutral-moderately constructive responses. They became more constructive the more the subjects did what they expected from "another student" and less constructive, annoyed and frustrated when he did not, not necessarily because of the presence or absence of obligation, but because the "other student" aroused a genuine altruistic response.

On the other hand, we could infer that subjects receiving unexpectedly strong or extreme incitations, suffered a threatened loss of the freedom to behave in the way they felt most comfortable, by virtue of the implied normative obligations imposed on them by the confederate's institution of the norm of reciprocity through his behavior. This loss

would be paramount in their minds, accounting for the externalized constraints many subjects reported following this instigation. As in previous research, this threatened loss would have produced "reactance" aimed at restoring this freedom. Also, as in previous research, we may infer that this reactance aided and abetted the derogation of the confederate and his sanity by some subjects, which we hypothesized would then result in a lowered motive to reciprocate and less behavioral reciprocation than what would normally be expected. We may also infer that the reactance also was responsible for the subsequent dislike of and annoyance covertly felt toward the confederate. Since the amount of reactance an individual will experience in any given situation has been shown to be in part a function of his personality, this would help explain the great variability in subjects' behavioral response to these instigations. Since reactance also varies with the importance the threatened freedoms hold for the individual, it would also help explain why the correlations in the normal and low social restraint conditions differed from each other. In the normal restraint conditions of our study and of real life, maintaining normal expected behavior in oneself and others would be important and hence maximal reactance would occur resulting in decreased reciprocity. On the other hand if we assume that in the low restraint conditions of our study and in the previous laboratory studies, the need to maintain expected behavior was artificially lowered, then minimal reactance and maximal reciprocity would occur.

It would thus seem that the interrelation of reciprocity theory and reactance theory yields a fruitful understanding of the self report data and behavior of subjects receiving extremely constructive instigations. However, while this may be the case, it is important to note that while reciprocity-reactance theory can account for our data, other aspects of

a social situation besides reciprocity norms can also arouse reactance. Further, other theoretical approaches may also offer plausible explanations for our data.

For example, Kiesler et al, have theorized a relation to exist between norm violation and dislike for the violator. Without recourse to reactance theory, Kiesler (1976) has shown that where a person values predictability in a relationship, norm violations will produce dislike of the violator, since it makes the violator seem unpredictable. Further, Kiesler et al (1967) had shown that when one anticipates future interaction with the violator, appropriate behavior will lead to overt liking of the other person while inappropriate behavior leads to covert disliking of the other person. Furthermore, these authors have shown that when several norm violations occur, attempts will not be made to bring the violator back into line if future interaction is expected with him. Thus, presumably one would dislike such a norm violator yet try to keep the "peace" with him.

In terms of the present study, the strong-extreme constructive instigations and the aggressive instigations were judged to be abnormal. As such, Kiesler et al's theory would predict that on a behavioral level, subjects receiving such instigations would try to keep the "peace" and would try to keep their behavior within normal limits. On an emotional level, the theory would predict that the instigator would be disliked but the dislike would not be expressed.

One can readily see that Kiesler's theory rivals reciprocity-reactance theory in its ability to explain our data. However, it is weaker insofar as it does not account for the obligations that many subjects felt to reciprocate the "abnormal" instigations. Furthermore, as Worchel has pointed out (1974), Kiesler's data can be accounted for by reactance

theory.

On balance, though a reciprocity-reactance theoretical explanation of this set of data seems warranted, the question of whether the reciprocity-reactance approach, or Kiesler's approach, best explained the behavior of people in situations such as those in our study cannot be established conclusively in post hoc analysis, but rather, needs to be established through future research.

Implications of these theories for the norm of reciprocity.

The question of which theory better explains our data has important implications for determining whether the people in such situations are at all motivated by a norm of reciprocity. Reactance-reciprocity theory assumes the existence of such a norm, despite the fact that people do not consciously acknowledge and accept such obligations. Kiesler's theory makes no such assumption and as such, leaves us with self report data which is devoid of any acknowledgement of the existence of such a norm.

Neutral-Moderate Harmful Behavior

It will be recalled that subjects receiving these instigations responded, for the most part, in a neutral to helpful manner that became less helpful as the level of aggressive instigation increased. Only a few subjects responded in an outwardly aggressive manner. Overall then, this behavioral data did not seem consistent with reciprocity theory's contention that a negative norm of reciprocity exists and causes people to reciprocate aggression with aggression.

On the surface, the self report data also seemed inconsistent with what one would expect to find if a norm of reciprocity had been directly indicating to subjects how they should behave. Contrary to our reciprocity theory, based on expectation that subjects' self reports would indicate that they consciously felt obligated to retaliate, very few subjects reported

such conscious feeling of obligation. Instead, the self report data showed that subjects receiving these instigations for the most part did not feel obligated to retaliate, but rather, felt angry, annoyed, anxious and uncomfortable (and typically avoided letting this show in their overt behavior).

Typically, their self reports described an unwillingness to allow the "other student" to agitate them which they felt he was trying to do, coupled with a derogation of the "other student's" behavior as selfish, crazy or the actions of a confederate. Typically, they also expressed a feeling that it would not be worth it to retaliate although the "other student" would deserve it if they did.

Explanatory framework for this data.

The self report data of these subjects provided very little direct evidence that a norm of reciprocity made subjects feel obligated to reciprocate. A few subjects did make strong reference to the fact that they felt their pride, masculinity and self-esteem depended on making an aggressive response and in this sense, they would have felt obligated to make such responses. However, for the most part, most subjects merely felt justified in possibly retaliating, not obligated to do so.

The failure to find overt self-reports of feeling obligated to retaliate does not however, as we have already seen, automatically mean that most of our subjects did not internalize, or take into account, a negative norm of reciprocity that Gouldner postulated to exist. It is theoretically possible that subjects did indeed have such an internalized norm, but kept it out of their conscious awareness and as such, did not play a direct role in guiding their behavior.

One theoretical reason why subjects may not have consciously acknowledged such an internalized norm occurred to us while examining the self

report statements of subjects receiving these instigations. A number of them expressed a feeling which could best be described by the cliché expression: they didn't want to let the other student "get their goat". In such remarks, as in the case of the strong to extreme constructive instigations, was expressed the idea that they felt externally obligated to respond and as a consequence refused to do so. Presumably, as before, such a view of the situation would be the outcome of an attempt to externalize an internalized acknowledgement of the negative norm of reciprocity so that it could be dealt with on this basis. However, the question remains - what motivated the externalization, and what motivated the anger, annoyance and derogation of the confederate that the subjects also expressed?

A strict rendering of reactance-reciprocity theory offers one theoretical framework, consistent with reciprocity theory, which would account for this data. From this theoretical perspective, the primary consideration in the subjects' minds would have been the loss of freedom to act normally and/or in a constructive manner, in this situation. This loss would have been the result of the aggressive instigation, with the focus on the cause of the loss being attributed to the confederate. Once such a loss was experienced, subjects would have then tried to fight their desires to retaliate and would try to stubbornly resist the confederate's instigation. Blaming the confederate for the problem, derogating him by covertly considering him to be "sick" or "not really a bother", would have been a way some subjects found to restore their lost freedom to behave normally and constructively, according to this view.

As we have pointed out, there was some evidence for the above theory. However, in looking at the self report data, we were more impressed by subjects' anxious concern about retaliating than we were with any feelings of "reactance" they expressed. In view of the fact that they seemed more

occupied with fears of retaliating than with reactance to retaliating, we would propose a modification of reactance theory to explain this data. According to this view, if a norm of reciprocity was indeed operating, subjects did not acknowledge its existence more because they were afraid of the anxiety-provoking implications in doing so, and less, because they stubbornly refused to do so. Our opinion then is that any "reactance" of this latter sort was probably a secondary derivative of their fear of being drawn into an anxiety-laden confrontation.

However, regardless which theory best explains the data, the important implication of these theories is that it is possible that, even given the lack of overt evidence for the negative norm of reciprocity in subjects' self reports, such a norm could have been operating and could have theoretically produced the self report data we did obtain.

On the other hand, it is also possible that there was no negative norm of reciprocity existing in the population studied. Perhaps the population we studied, e.g. middle-class college students, consists of people who do not make decisions about whether to aggress, based on such a norm of reciprocity.

In point of fact, in contrast to the statements of a few of our subjects, most subjects did not feel their "pride, masculinity and self-esteem" were on the line if they did not reciprocate. In fact, a few subjects stated they had been taught not to reciprocate. Moreover, we also had one or two subjects who had such low self-esteem, pride and feelings of masculinity that they readily accepted the confederate's desires as legitimate and theirs as illegitimate to avoid lowering their self-esteem further. If more of these people had been our sample, even less evidence for a negative norm of reciprocity would have been obtained. Studies have indeed shown that name calling can even lead to compliance with middle

class population (Steele, 1975), presumably due to their shaky self-esteem. Thus, it is theoretically possible that in populations similar to the middle class college student population we sampled, there is no negative norm of reciprocity, while in others, as yet undefined populations, there is.

Implications of the above explanatory frameworks for the negative norm of reciprocity.

As discussed, there are two main ways that the lack of overtly expressed feelings of obligation to retaliate in subjects' self reports could be theoretically explained. Each of these theories has different implications for the attempt to demonstrate the existence of the negative norm of reciprocity.

If the anxiety-reactance-reciprocity theoretical formulations proved to be the best explanation, the implication would be that while subjects were influenced by the negative norm of reciprocity, they were not conscious of its influence and indeed, tried to resist becoming conscious of it.

If the theory that middle class college students have not internalized a norm of reciprocity best explains our data, the implication is that the norm of reciprocity may be totally irrelevant to understanding the dynamics of aggression in a significant segment of our population.

The question of which theoretical orientation is the most fruitful cannot be answered at present. However, in either case, it seems obvious that the negative norm of reciprocity, to the extent it exists, is much less potent or direct controller of behavior than previous research had led us to believe it was. Further, since previous research utilized middle class college students as well, the following question needs to be answered in future research: If college students' aggressive responses to provocations are not strongly controlled by a negative norm of reciprocity, as our data indicated they are not, what other set of processes can account for the empirical reciprocity of aggression obtained in previous lab research?

The answer to such a question should advance our knowledge of the dynamics of aggressive retaliation considerably.

Strong-Extreme Aggression

We will now conclude this section by saying a little about the self report data of subjects receiving strong to extreme aggressive stimuli.

It will be recalled that most subjects receiving these instigations responded in a neutral to slightly helpful manner, e.g. as before, most tried to keep the peace and comply with the confederate's obnoxious demands. However, despite the mild-mannered behavior of most subjects, subjects typically reported having extreme aggressive fantasies, along with derogations of the confederate, calling him "sick," "an idiot," "nervous", etc. A few subjects severely distorted their perception of the instigation, thereby rendering it less aggressive than it was. (This distortion and derogation, as mentioned earlier, probably served to lower their urge to respond.)

Typically, too, subjects felt that if they did act out what they were feeling, the other person would have gotten what he deserved and the subject would feel "good" for having done this. However, most subjects said they did not express their hostility, either because they were afraid of great retaliation or because as one subject put it, "you would only have hurt yourself and not have changed his mind" or, as another put it, "there'd be too many consequences to suffer even though I'd break his nose". A few subjects reported being especially frightened by the abnormality of the behavior. A few cited "moral objections" to "hitting", "starting a fight" as fighting inside an office-waiting room - or implied that it would have been inconsistent with their self-image and preferred their own overt behavior. In contrast to the above, a few subjects did become extremely behaviorally agitated to the point that they acted very aggressively. One of them pulled a cigarette out of the confederate's mouth and stomped it

on the floor, threatening to "beat the shit" out of him. Another subject seemed like he was about to reach for a weapon in his back pocket. Several subjects were so enraged that they could not concentrate during the self report procedure and they needed a great deal of support to continue.

As can be seen, the behavioral data and the self reports of subjects receiving the strong-extreme aggressive instigations were qualitatively similar to those subjects receiving neutral-moderately aggressive instigations but quantitatively, the feeling and desires reported were much more intense.

On a theoretical level, this data would be subject to the same comments we have already made about the less extreme aggressive instigations. However, such behavior was largely atypical in the population studied.

Explanatory framework for the above data.

To avoid being repetitive, we will merely state that this data is consistent with the various theoretical explanations proposed earlier and the reader is referred to them.

Summary of Perspective Gained From Above Observations Concerning the Self Report Data

To recapitulate, through looking at the self report data from our subjects, we have gleaned some new ideas about the norm of reciprocity which needs to be tested in future research.

Our main observation is that in contrast to what a simple statement of reciprocity theory would predict in a realistic social context, subjects do not consciously feel obligated to reciprocate aggression or constructiveness that is initiated by strangers. As such, it seems that not only cannot a simple statement of reciprocity theory explain the overall behavior of people in such situations, but moreover, it cannot readily account for the lack of conscious feelings of obligation in subjects' self reports.

We have also discussed several alternative explanations for this lack of felt obligation and have discussed their implications for the existence of the positive and negative norms of reciprocity. We have tentatively theorized, based on the self report data, that while subjects did not consciously feel the obligation to reciprocate, they were nonetheless influenced by such norms in a rather complex manner. We have seen that with some modification, reactance theory serves as a good explanatory framework for this self report data.

At the same time, we could not entirely rule out the possibility that the lack of overt evidence for existence of the norms of reciprocity in the self report data may have been due to our selection of middle class college students as the focus of our study. To the extent that this was the case, it is likely that had we sampled other subject populations, stronger evidence for the direct influence of such norms would have been obtained and as such, norms of reciprocity would be more culturally limited in scope than had been previously thought.

Impressions From Post Hoc Inspection of
Statistical Patterns in the Data - How
Did the Contexts Affect Our Subjects?

While the previous inspection of subjects' stated rationales for their conduct helped us to propose several hypotheses for understanding why people behave the way they do in such situations as the subjects found themselves in, inspection of the self reports did not yield precise enough information to explain why the two different contexts affected subjects differentially the way they did. As we have pointed out elsewhere in this report (see Appendix L p. 318), the effect of the contexted manipulation was quite subtle in its psychological effect on these subjects, in that very few reported being consciously aware of its influence on them and even fewer seemed to have been consciously guided by it in determining their behavioral responses. Nonetheless as our results have shown, the context did exert its effect on the degree of reciprocity shown by the subjects.

In attempting to understand how the context differentially affected our subjects, we decided that in addition to inspecting the subjects' stated rationales for their conduct, a second way to proceed would be to conduct a post hoc inspection of whatever patterns emerged in the data. We hoped that by such an inspection, it might be possible to infer personality and/or other variables mediating these patterns.

Inspection of these patterns revealed some interesting consistencies. These consistencies in turn, proved to be consistent with the previous theoretical formulations we made, of what was taking place in the subjects' minds that accounted for their general behavior. These theoretical frameworks thus helped us understand to some degree how and why our contextual manipulation affected the reciprocity behavior of the subjects.

In coming to the above understanding, several different aspects of the pattern of data were considered. The first aspect deemed important concerned the differential comparison of the direct response data obtained under the two different restraint conditions. The subjects in the normal restraint condition vs. the low restraint condition, showed lowered reciprocal behavioral change (lower r), and showed more caution in the amount reciprocated (lower b). Moreover, they were less likely to reciprocate even a "rough equivalence" to that received. Instead, their behavior seemed more generally cautious, and more toward the neutral point in magnitude. All of this would support the idea that psychological restraints induced by the context were what affected the behavior of these subjects. This of course was in line with our theoretically based predictions that normal restraints inhibit reciprocity.

The second aspect of the data pattern shed more light on the nature of these restraints. This involved our finding of substantially equal magnitudes of reciprocity obtained, under both contexts, in analysis of the spontaneous data and our finding that the magnitude of these obtained r s (.7) fell midway between the magnitudes of the r s obtained in low normal restraint for the direct data (.9) and that obtained for the direct data under normal restraint (.5). We had not expected to find equal magnitudes for both spontaneous correlations, but instead, had expected to find that the normal restraint spontaneous data would show a lower reciprocity correlation than the low restraint spontaneous data and in fact would be the lowest of all the correlational analyses done.

We based this expectation on our initial theoretical view that there were two factors which would inhibit reciprocity. One of these factors was of course the "normal restraints" of real life which we postulated would exert a purely inhibiting effect. The second factor was the lack of cues

in the situation that would make the norm salient, e.g. if you don't feel the situation "calls for" your reciprocation, you won't. We had theorized that these factors would act in an additive fashion. However, our post hoc analyses suggested that an interaction between these variables was at work in producing our findings. Specifically, the pattern of correlations suggested that when the situation only minimally "calls for" or demands reciprocity, normal and low restraint contexts have little effect on the strength of the reciprocity correlations, as is evidenced by the similar correlations shown in the "spontaneous" data obtained under both contexts (.7 in each case). However, when the situation maximally "calls for" a response, as was the case in the "direct data", the context will play an important role since now subjects presumably feel they ought to make some response but the question in their minds is, should they.

In the normal restraint conditions, subjects apparently were inhibited in reciprocating as we predicted they would be. Contrary to our initial predictions however, under these conditions they were more inhibited in reciprocating when the cues calling for reciprocity were strong than when they were weak. It was as if subjects not only were inhibited, but went out of their way to resist the influence of cues calling for reciprocation.

This result is similar to a recently discovered "boomerang attitude change" phenomenon. (Sensenig 1968), where a communicator who tries to change someone else's mind may in fact lead him to adopt the opposite attitude, when the subject feels he does not have the freedom to "make up his own mind". And just as those results are best accounted for by reactance theory, this pattern seems best accounted for by one of the variations of reciprocity-reactance theory mentioned earlier, though which variant does the best job we cannot say conclusively.

In short, based on the pattern of correlations, and after carefully

considering and ruling out other possible explanations of the pattern, we believed that reactance-reciprocity theory is the best explanation of why the contexts produced the effects they did.

If we are right in the opinion that subjects actively were resisting under normal restraint conditions, the next question then, is why did they feel the need to resist, e.g. why did they develop "reactance" in the broad sense of the term? According to previous researchers, reactance is experienced when one feels important freedoms are threatened. In view of the overall relative neutrality of the subjects' responses regardless of instigations received, it is probable that it was important to the normal-restraint subjects to behave in normal and acceptable ways and in ways that would keep one out of trouble. If so, any factors which they might feel would induce in them behavior patterns that deviated from these goals might be met with "reactance of some sort.

As we have seen, the data indicates that as the magnitudes of instigation increased, the subjects reported increased urges to behave in ways which deviated from the behavior pattern. As such, it is likely that "reactance" of one sort or another was the end result of an attempt to not let these desires affect their behavior.

In the low-restraint subjects, a trend to less overall neutrality was observed in their responses. This would suggest that for this group, due to our experimental manipulation, the goal to maintain their usual behavior became less important and as a result, temptation to violate it were more easily accepted as "reactance" was minimized.

However, the above suggests that under normal restraint conditions, subjects had strong desires to avoid letting their behavior be influenced by the instigations they received and as a result, actively resisted the temptation to be otherwise. Another source of the resistance to the instigation, under normal restraint condition, may be the goal we hypothesized that subjects have to

avoid fearful dealings with someone who behaved abnormally. While there is not much evidence showing that subjects did indeed have this as a goal, the fact that the normal-restraint subjects showed minimal reciprocity to the abnormally intense instigations---and maximal reciprocity to the neutral-moderately constructive normal ones---is significant. While we could account for this by pointing out that those instigations that were rated most abnormal, were also the most extremely intense, and hence most apt to threaten someone's attempts to avoid responding to them in normal restraint conditions, it is our opinion, based on some of the self report comments of the subjects, that the abnormality of the confederate's behavior created secondary additional reactance in these subjects against the idea of having to interact with a "disturbed individual". Our opinion is that under low restraint conditions, this source of reactance was minimized by the subtle experimental manipulation of the legitimacy of such behavior, thus helping to explain why the low restraint subjects showed universally higher levels of reciprocity to both normal and abnormal instigations.

In sum, our explanatory framework for the effect of context on reciprocity is best considered a tentative but compelling one. Under normal restraint conditions subjects wished to keep their behavior normal, acceptable, and neutral to moderately constructive. It was also probably important to them that the other student behaved likewise. When he did not, subjects felt a loss of freedom in proportion to how strongly they were forced to reciprocate, which in turn was a function of both the magnitude of instigation and whether or not they felt they were called upon by the confederate to make some response. This reactance inhibited their "direct data" behavior and made the subjects refuse to reciprocate. However, when the subjects no longer felt impelled to reciprocate (when confederate terminated his instigation) subjects now became more likely

to reciprocate, as evidenced by the increased reciprocity correlation in the spontaneous data.

Under low restraint conditions "reactance" was probably not a problem. Subjects were given the set that they and the other student were free to do anything they wanted. Therefore, they were more prepared to reciprocate and were more likely not to feel that the other subject was denying their freedom to act normally since the importance of this freedom had been downgraded by the procedures establishing this context.

In terms of the factors we speculated about at the outset as being important in determining whether or not reciprocity would occur, it appears that the abnormal intensity of the instigations coupled with the recipients' reactance against reciprocity based on the circumstances of normal restraint, played the most important roles in inhibiting the reciprocity of behavior in these subjects.

Inferring from the behavioral patterns in our data, it appears that in every day life, semi-public encounters with strangers, people such as those in the population studied, for a variety of reasons, have a great aversion to behaving in unacceptable, abnormally intense aggressive and constructive ways and have an aversion to responding to those that do. When people such as the ones we've studied, meet others who violated the norms governing the magnitudes of aggression and constructiveness allowable between strangers, if those others directly attempt to engage them in interaction "calling for" reciprocity, "reactance" of some sort will be aroused. In the recipients and minimized reciprocity will occur. On the other hand, if the instigator does not directly confront the recipient and the recipient feels he has more options open to him including total avoidance of the situation, we can suppose that "reactance" would be lessened and somewhat more reciprocity would be observed. It is only

when the restraints lifted however, as we theorized was the case to previous late studies, in our low restraint group or in a variety of real life situations, that maximal reciprocity behavior will be displayed and then again, only when the situation seems to "call for" it.

Why People Don't Reciprocate - The
Need for a Two Factor Approach

While we have already discussed our findings that our subjects did not significantly reciprocate in our simulated naturalistic normal restraint setting, we thought it might be useful to theorize why they did not reciprocate. As will be demonstrated, though perhaps not readily apparent to the reader, this question turns out to be somewhat independent of the previously discussed issue of whether or not subjects felt obligated by a norm of reciprocity to do so.

We will first present my theory as to why constructive instigations were not reciprocated and will then discuss the level of reciprocity of aggressive instigation.

With regard to the lack of reciprocity of constructive instigations, we have already discussed the evidence showing that many subjects did not feel desirous or obligated to reciprocate and seemed to resist any inclination in this direction when the instigations they received were abnormally intense.

However, while such a lack of desire to reciprocate these abnormally intense instigations seems to explain the lack of reciprocity shown to them, it is not necessarily a complete explanation. For example, even if a given subject did have the "will" to reciprocate, the "way" may have not been apparent to him, or some other factor may have stopped him or prevented him from doing so. While our subjects did not overly attribute their lack of reciprocity to such factors, it is likely that they were so engrossed in

their resistance to reciprocity that these other factors did not have a chance to play an important role in the decision process. Thus we would theorize that in general, whether or not reciprocity behavior occurs is not only dependent on the factor of whether people feel obligated to reciprocate, but moreover, is dependent on the additional factor of whether it would be too costly, or not worth it, to reciprocate.

With regard to such a two-factor theory of reciprocity of constructiveness, other researchers have found such an approach to be a useful one (Pormarol & Jaccard, 1976), in predicting altruistic behavior. There is little reason to believe such an approach would not be equally applicable to the prediction of whether people will reciprocate constructive instigations.

With regard to subjects' lack of reciprocity of aggressive instigations, our evidence strongly supported the need for a two-factor theoretical explanation of why subjects did not reciprocate. For while subjects did not feel obligated to retaliate, they did report in many cases having hostile retaliatory desires which were then resisted.

With regard to the question of why subjects did not act out their anger or in some cases even display it, most subjects receiving these instigations sensed that "it was not worth" the realistic consequences of getting into a fight with the other subject (such as physical injury), and/or did not feel that the potential responses they could have made would have effectively altered the other person's behavior. A few subjects cited philosophical reasons for their lack of response.

The fact that in many cases subjects gave different reasons as to why they felt the way they did-that were qualitatively different than the reasons they gave as to why they acted the way they did - suggested strongly that a two-factor model may be necessary to understand the circum-

stances under which aggressive behavior takes place. The model that again emerges is quite similar to that recently proposed by Pomarol & Jaccard (1976) that we mentioned previously. As we have said, this model was used to predict a given altruistic behavior, e.g., whether or not someone would donate blood during a blood drive, one week later. These authors, being familiar with the wealth of studies showing that numerous variables could affect the expression of altruistic behavior, decided to treat these variables as potential information that a person perceives, weighs and assigns values of importance to, in deciding whether or not to perform the altruistic behavior. Similarly, we have seen that a rather complex process may be involved in deciding whether to behave aggressively. These authors went on to point out however, that the decision to help only correlated .59 with their actual blood donating behavior. Their evidence suggested that other factors such as the person's perception of his ability to help, his actual ability to help and other such situational factors mediate between the intention and the actual behavior. They also suggested that where these factors are minimal, then the factors affecting the intention to help will in turn have their strongest influence on behavior. Our impressions were formed similarly, with reference to the aggressive instigations. We saw that even where a person developed the intention to retaliate, for whatever reason, he did not usually do so due to his perception of its "not being worth it". Clearly, a determination of variables that determine in a person's mind, whether or not it's "worth it" to aggress, may yield an especially fruitful understanding of aggressive behavior and lend to increased predictive power. It is likely further, that the variables that determine if it's "worth it" to aggress are apt to be rather different than those which determine the intention to aggress; hence the need for a two-factor approach in future attempts at predicting aggression following a

provocation and the need for assessing the subjects' perception of and valuation of the costs involved in reciprocating either with aggression or constructiveness.

Some implications of the obtained findings for reciprocity theory.

Reciprocity theory makes several basic predictions about people's reactions to aggressive or constructive instigations. First, it predicts that the more aggressive an instigator is the more aggressive the recipient will be and the more constructive an instigator is, the more constructive the recipient will be. Second, it predicts that aggression will be reciprocated with its rough equivalent and constructive acts will be similarly reciprocated. Lastly, it postulated the existence of a norm of reciprocity which forces people to respond in the above fashion by making them feel obligated to do so.

Previous laboratory studies done to test these predictions found support for the first two predictions and implied support for the existence of the norm of reciprocity as well. In contrast to these studies, our own research found only very limited support for these predictions, thus indicating the possible need for revision in the theory on which these predictions were made.

In our study, the predictions generated from reciprocity theory were experimentally tested in a naturalistic setting. In contrast to the first two predictions, we found that the empirical reciprocity of behavior is not as characteristic of people's responses to unprovoked constructive and aggressive acts—as simple statements of reciprocity theory and previous research implied it was. As such, the theory is too simplistic and must be reformulated in a more complex and specific manner if it is to continue to be useful to and not mislead the social scientist.

One step in the reformulation must be to pin down the limits of reciprocity theory. Our data indicates it either does not apply in many important respects in everyday life interactions and therefore, or there is a need to delineate the situations where it may or may not apply and/or to delineate the effect of contextual restraints and other possible moderating variables. As Gouldner originally suggested, its applicability must be established on a case by case basis. Through such studies, we may in turn increase our understanding of why it is useful in some circumstances but not in other.

A second step in this reformulation must be to better define the term "reciprocity". We have seen that the term "reciprocity" has a number of important connotations that must be considered in operationalizing this term in future research.

Thirdly, with regard to what we consider to be the most essential postulate of reciprocity theory, e.g. the assumption of the existence of a powerful norm of reciprocity that directly controls people's cognition and behavior, our evidence likewise suggests that such a postulate is much too simplistic and must be reformulated.

Our evidence suggests one of several possible lines along which such a reformulation might need to take place. In one such reformulation, such norms would be postulated to be characteristic of only certain segments of the population; in another, reformulation would be postulated to be situationally specific; and in a third, they would be postulated to interact completely with other intrapsychic dynamics, the outcome being that the norm is consciously allowed to guide respondents' behavior in some instances and resisted in other instances.

Perhaps, a reformulation that takes into account all these possibilities would prove to be the best explanatory framework for understanding the reciprocation of aggressive and constructive behavior. In any case, it appears necessary to reformulate the theory concerning the nature and limits of such norms if they are to continue to prove to be useful theoretical concepts.

Some Implications Obtained Findings Have
for the Methodology of Past and Future
Research on the Reciprocity of Aggressive
and Constructive Behavior.

At the outset of this study, we pointed out that as a result of certain methodological procedures employed in previous research, we believed that the results of this research on reciprocity would not be easily generalizable to real life settings. One feature of previous research that made us question the generalizability was the "low restraint" context induced in most cases. A second feature was the fact that previous researchers had used statistical procedures that we felt might maximize evidence supporting reciprocity by artificially wiping out individual differences from the analysis. A third feature of previous research that we believed limited its generalizability was its artificial restriction of the behavioral options allowed the subjects in responding to the stimuli. A final feature was due to the general lack of self report procedure used, it was hard to tell exactly what

motivated the subjects to behave as they did. Based on these and other observations, we postulated the need to replicate the findings in a real life context.

Let us now look at each of these points mentioned above and review the conclusions we have reached, and some of the recent research which support them.

Need for Consideration of Context in Laboratory Research

As will be recalled, previous laboratory research on aggressive or constructive behavior, as analyzed by Harshbarger (1974), showed a set of consistent relationships between aggressive and constructive stimuli and mean responses to them, which taken together suggested to Harshbarger that such behavior might be a highly predictable linear function of antecedent aggressive and constructive acts. Harshbarger, while struck by the consistently high .9 correlations and consistently stable slopes of the regression line relating such antecedent acts to subsequent behavior, suggested the need to replicate these findings in a more naturalistic context. The present research was the outcome.

Our hypothesis was that the laboratory context of the aforementioned studies was an important variable overlooked by previous researchers, and was probably in part responsible for the obtained results. Based on the theory that this context was different than that found in naturalistic settings, we predicted that in such a naturalistic setting, we would

obtain results demonstrating far less consistency, linear predictability and reciprocity than those previously cited. The results basically support the hypothesis originally made.

As such, they demonstrate that except under certain limited conditions outlined previously, these particular obtained laboratory findings may not be applicable to real life contexts. They further demonstrate the importance of taking such contexts into account in any new research in this area.

As such, our findings also add to the body of very recent research which suggests a need to consider the powerful influence that context may exert on aggressive and, by inference, constructive behavior. For example, Diener et al, have shown that the experimental manipulation of "altered responsibility" could significantly affect the amount of aggression displayed toward a passive role played under conditions where subjects were given styrofoam guns, balls, etc. and rubber bands and were told they could "do various things to him." These same authors also showed that "cognitive set", e.g. the experimental manipulation of the subjects' set that this was just a game or was a test of aggression, again significantly affected the amount of aggressive behavior displayed.

Other studies have produced similar results showing the effect of context. Geen, Stonnor & Shope (1975) produced results consistent with their hypothesis that circumstances which increased or decreased guilt following aggression in laboratory studies might be responsible for conflicting reports as to whether aggression leads to catharsis and thus reduced aggression, or leads to further, increased aggression.

Turner & Simons (1974) demonstrated that induced subject sophistication about the rationale of an aggression experiment, (where they received shock and then had to shock back, with a gun placed right near the shock key) led to decreased aggression, while also noting that Page et al (1971) found that subjects' sophistication led to increased aggression. They concluded, as we have, that the circumstances under which the sophistication occurred played a crucial role in determining how the knowledge of the reason for the manipulation would affect the subjects. They also found that when subjects were induced to experience more evaluative apprehension, they became more inhibited in their behavior and their behavior under these circumstances was not correlated with how angry they reported feeling following the shocks by the confederate.

Turner & Simons pointed out that such inhibitions as described above would obscure the more subtle effects of various independent variables on aggressive behavior. While Turner & Simons did replicate some of the findings of the laboratory in a naturalistic context, using horn honking at a stopped driver as the independent variable (1975), they noted that:

"in non lab settings, strong inhibitions might be experienced by individuals concerning the appropriateness or consequences of engaging in aggressive behavior. If a person is angry at someone, if he experiences low levels of inhibitions and if he is exposed to aggressive stimuli, then he might be likely to engage in aggressive behavior...however, angry individuals who are exposed to aggressive stimuli (such as weapons) under certain circumstances may not be aggressive since they might experience inhibitions...additional research about variables such as subject roles and evaluative apprehension which influence inhibitions in the laboratory may indicate variables which modify aggressive behavior in naturalistic settings" (p. 348)

Turner also demonstrated that there were wide individual differences in subjects' responses to a driver blocking subjects' paths in a naturalistic setting. Consistent with our data, some subjects were more inhibited than others and in general subjects tended to be inhibited in some situations. These authors suggested as we have, that there are limits to the generalizability of data on aggression collected under any given situational context. We believe our data shows the same limits should be applied to constructive behavior as well.

Need for Agreement on Statistical Procedures in Lab Research

Our study also provided evidence showing that the way the data is statistically analyzed can importantly affect the conclusions drawn. I believe that as much space should be devoted in future research to discussions of the variability of the data, as is presently given to discussions of changes in centrality measures. As our results indicate, aggressive and constructive behavior is far less predictable a function of antecedent aggression or constructiveness than previous authors' data suggest it was - even when their laboratory data is looked at.

Need for Allowing Subjects Maximal Freedom of Response

Our results support the idea that future laboratory research should leave subjects free to respond as they choose - aggressively or constructively. Workers may then be surprised to discover, as we have, the extent to which subjects may choose to not reciprocate aggression with aggression and constructiveness with constructiveness, especially when subjects' inhibitions against one or the other types of behavior, or just unacceptable behavior in general, is high.

Need to Incorporate Self Report Measures in Future Research

Our results suggest that as Krebs has pointed out, just because a particular behavioral event such as reciprocity, occurs and is given a certain label, does not mean that a similarly labelled or norm motive is behind it. Lab studies should be done with appropriate self report measures to see if the norm of reciprocity has any relevance at all, in explaining any resulting reciprocity relationships that occur.

Need to Study Aggressive and Constructive Reciprocity in Naturalistic Settings

Above all, our data had shown the need for a case by case examination of the determinants of people's actual responses to such stimuli in a naturalistic setting. More work needs to be done in developing predictive models for such behavior and especially in examining why some people feel it is "not worth it" to reciprocate while others do.

General Question of Limitations of Generalizations of the Obtained Findings

The purpose of this section is not to give a comprehensive list of all possible limitations but rather to alert the reader to important factors that might possibly produce different outcomes if manipulated in future research. As we have suggested, such outcomes should be established on a case by case basis.

Population Used

We sampled male college students who had volunteered for the study in order to satisfy their introductory psychology course requirements. All students lived in the greater New York City area, and the majority came from white, middle-class backgrounds and were still living at home.

They were under the impression that the "other student" whom they did not know, was also a college student from a similar background who was there for similar reasons.

These students thus represent a comparable population to that studied in previous lab research. It is likely however, in view of our findings concerning the role of context, etc., that had the population we sampled not been from the one described above, or had they thought that the "other students" they interacted with were different than they were, in one or more respects, then a different pattern of results would be obtained in certain aspects. This pattern might differ both on a behavioral and self report level.

General Aspects of the Experimental Context

Several aspects of the general context may limit the generalization used on this data. First, the interaction took place in a small waiting room-office which was presumably being used under the auspices of a Brooklyn College faculty member. The office was located right off a public corridor. Farther, I was not in the room at the time the interaction took place but was due to return in five minutes to lead them into a "lab" for a "study". There was only one small couch on which to sit.

It is possible that had the interaction taken place outside of school, in a remote and desolate area, or in a more formal or informal context than ours, or within easy view of an authority or in the complete absence of one, different results would be obtained in some respects.

A second aspect of the general context which may limit generalization based on this data was the deception employed in conjunction with the fact that the subjects were psychology students participating in a psychology study and as such were apt to be somewhat apprehensive of evaluation.

As we have already discussed, recent studies have shown that subjects, who are apprehensive about evaluation, may respond in a study in a way different than they might respond if they were not apprehensive, though the exact nature of their responses will depend on numerous other factors.

In our study, several factors, such as the MAACL, the disguised mirrors, my leaving the room prior to the instigations, etc. could potentially be thought to be deceptive by the subjects provided the subjects negated the strong compelling rationales offered them for what was taking place. To some extent, some of the subjects did just this. While typically, subjects seemed to harbor minimal feelings of being deceived prior to the confederate's behavior, subjects reported that following the confederate's behavior, they experienced, on average, mild to moderate suspicion of being deceived. The suspicion of being deceived was only characteristic of some subjects and was quite variable from subject to subject. It seemed motivated in large part by individual subjects' subjective fears of evaluation as well as by their need to cognitively deal with the behavior of the confederate by derogating him in this manner.

Whether these intervening variables affected the subjects' behavior or self reports in any way different than they would have affected his behavior in a totally naturalistic setting is hard to say, but the subjects' opinion was that such a differential effect was rather minimal. 70% of the subjects felt they would have behaved similarly if they had encountered the same instigations in a real life setting. Further, of the minority of subjects who were unsure (18.8%) or who felt they would have behaved differently (11.3%), very few felt that the experimental setting was responsible for the possible differences but instead, cited factors such as additional opportunities present for avoidance, presence

of bystanders, etc.

Overall, our opinion is that subjects did behave in a way that is typical for people in the population studied, in real life settings similar to ours. Our opinion is that the confederate's behavior by catching the subjects off-guard, elicited behavior that was typical of their usual responses and, as subjects suggested, subjects were minimally contaminated by evaluative apprehension since they had not been thinking along these lines prior to the instigation. Once the instigations occurred however, the level of apprehension and suspicion of our subjects increased and in turn may have affected their behavior. However, if such an effect did occur, we do not believe (as they did not believe) that their responses would have been different. We would base this on the proposition that people who let their behavior be guided by fearful fantasies of evaluative apprehension, in a simulated normal restraint context such as ours, are apt to do so in a real normal restraint context.

It will be noted that we limited the above statements to the population studied. It is quite possible that Psych 2 students are more apprehensive about the possible costs of making certain responses than are non-Psych 2 students or people in general for that matter and as such, any interpopulation generalizations need to be made with caution.

Regardless of the validity of our conclusions, we do feel that the effects of deception and evaluative apprehension need to be given more attention and/or rules in future research. Most deceptive research has used a similar type of population. Appallingly, as Stricker (1967) reported, in his review of 88 published deception studies, only 21 reported data regarding subjects' suspicions and most of that was very superficial. In our study, we attempted to assess and determine the influence of such factors on our data. Now that the pitfalls of the laboratory and simu-

lated contexts and particularly the use of Psych 2 volunteers has been increasingly explored, more consideration needs to be given to the justifiability of carrying out similar studies with naive non-student subjects in totally naturalistic environments.

Stimulus Variables

Nine different stimuli were used which fell along a restricted range of stimulus intensity. Some stimuli were predominantly non verbal, others predominantly verbal. The stimuli were the first significant interaction initiated by a person who prior to this had taken little interest in the subject. The stimulus itself lasted for a short period of time and was designed not to escalate or deescalate as a function of the subjects' behavior. It was further designed to appear non-provoked. Finally it did not involve physical violence.

Again, it is likely that had these parameters been substantially altered, quantitatively and qualitatively different results might have been obtained. Particularly with regard to the fact that the stimuli did not involve direct bodily harm, it is important to note that where such bodily harm occurred, many subjects may have reciprocated in order to "defend themselves", thus altering our results.

Response Restrictions

While we took steps to maximize the possibilities of responses subjects had at their disposal, there are a number of factors that if altered might have had an impact on the results. One is that subjects only had a limited amount of time to respond. Secondly, subjects were not provided with "convenient opportunities" to reciprocate but had to actively create them. Finally, the subjects could not just leave the situation permanently, since they still needed to get their credit slip. Moreover, since the room was small and there was no other place to sit, the subjects

could be said to have been constrained in this respect too. On the other hand, they could conveniently ignore the confederate by pretending to work on the biography form, or by looking at the posters, etc.

Possible Generalizations of Obtained Findings to Real Life Naturalistic Situations

As we have pointed out, implicit in the results of previous laboratory research into aggressive and constructive behavior was the suggestion that people in every day life situations reciprocate aggression with aggression and helpfulness with helpfulness. This research also suggested that the behavior of people, when either attacked or offered help, is a uniform, highly predictable linear function of the magnitude of harm or help received.

Subject to the limitations discussed in the preceding section, results from the present research support the theory that the above generalizations cannot be automatically applied to all real life situations. Rather they seem most likely to be applicable to real life situations where, for any one of a number of reasons, the normal social restraints inhibiting constructive and aggressive behavior have been completely eliminated or counteracted and/or where the individuals involved, presumably for intrapsychic reasons, are not brought under the influence of these restraints.

With regard to the more typical every day life encounters we have daily with strangers, our results suggest that the generalizations based on previous laboratory research only apply to the responses of the average person to a restricted range of instigations. These would be instigations which are judged to constitute, both in content and magnitude, normal and acceptable behavior for a stranger to initiate, in an average every day life situation. Our results suggest that in such an average every day life situation, there are consensually defined standards of how normal and acceptable different magnitudes of aggressive or constructive behavior

are. Our results further suggest that in such every day life situations, neutral to moderately constructive behavior is deemed to be the only behavior that falls in the normal and acceptable range and is therefore, on average, likely to be reciprocated whenever direct confrontation is instituted by a stranger. If a stranger initiates confrontation that falls outside this range, our data suggests that little reciprocity behavior will occur.

At the constructive end of the continuum, our data suggests that "abnormally" constructive confrontation by strangers will be responded to with behavior, on the average, that becomes increasingly neutral in nature as the level of constructiveness increases. While some people will tend to show a leveling off of the amount of helpfulness they will return, others will actually become increasingly less constructive as the more constructive a given stranger tries to be. Very few people will show increased constructiveness in response to these "abnormally" high levels of instigation than they would show to lower but acceptable levels of constructiveness, although a few will.

At the aggressiveness end of the continuum, our data suggests that any unprovoked aggression initiated by a stranger is judged to be abnormal and unacceptable. It will be responded to with behavior that on the average is normal and neutral to mildly constructive in nature, regardless of the level of unprovoked aggression people are exposed to. However, while most people will respond to aggressive provocations as described above, our data also suggests that a few individuals will reciprocate aggression with aggression and moreover, may reciprocate with an amount of aggression that is at least a "rough equivalence" to that received. Thus, while most of the people we studied when confronted did not reciprocate aggression with aggression, we would expect to find re-

reciprocity of aggression if we sampled from the minority of the population who do display this sort of behavior, even where normal restraints are operative.

It is important to note that our data suggests that the above observations are apt to be most characteristic of interactions where the person is actively being directly confronted by a stranger. When people have "more time to think", they are apt to demonstrate somewhat increased tendencies to reciprocate behavioral changes but again generally, within the narrow limits of acceptable neutral and moderately constructive behavior.

In terms of the determinants of aggressive or constructive behavior, we have seen that people typically do not consciously allow themselves to feel morally obligated to respond aggressively or constructively to strangers. Rather, fears of being obligated, antagonism over feeling obligated, good or bad affect, a desire to behave in a normal fashion consistent with their personalities, and consideration and fear over the possible costs involved dominate their consciousness. The outcome usually is that people are afraid or unwilling to carry out their real desires toward strangers.

Contrary to what a simplistic statement of reciprocity theory would imply, regardless of instigation level or its aggressiveness or constructiveness, most people will limit their behavior to being neutral to moderately helpful or constructive, and there will be little positive linear correlation associated with increased or decreased levels of abnormally intense aggressiveness or constructiveness received. In addition, consistent with Moriarity's findings (1975) our results suggest that most people even if they feel obligated to respond, and/or desire to do so, will not do so, if the reciprocity costs involved seem too high. At the same time, there are a few people who will not only reciprocate what has been done to them but are willing to pay back the initiator in spades. However, in general, people will not reciprocate an unexpectedly strong good or bad deed.

APPENDIX A

Graphed Data from Pilot Work

Scatterplot Relating Students' Mean Judgments of the Aggressiveness-Constructiveness of Various Actions to Other Students' Judgments of the Normality-Abnormality of these Same Actions (in an Office Waiting-Room Context).

SPECIFIC SITUATIONAL INSTRUCTIONS

Preliminary Instructions

Form II

This is a test of social judgment, one form of intelligence. The following page(s) contain a list of a variety of actions that you may have witnessed one person doing with another person, or that people may have done to you from time to time. You will be asked to rate each of the actions along the scale dimension provided. You should assume that in each case, the action takes place in the following setting.

You or someone else has been asked to report to a waiting room at the college to fill out some forms. Another person, one whom you have never met before, has also been asked to come to the waiting room for the same purpose. One of you arrives first, takes the form from a table, sits down on the only couch in the room and begins work on the form. A note written on the blackboard informs you that the person in charge had to step out for about 20 minutes, but that you should begin work on the form, and have it completed by the time the person in charge returns. While one of you is busily engaged in doing this, the second person, either you or the other student arrives, reads the instructions, takes the form, sits down on the same couch and likewise begins work on the form. At some point during these events, the other person initiates one and only one of the actions which you will be asked to rate. Prior to this, his behavior has been limited to that described above. You have had no other significant interaction with him besides that described above and the new behavior he now initiates.

Remember: Assume that each of the actions on the List of Actions is the first significant change in the behavior of the other person as described above. Before going on to read the Rating Instructions, please read through the List of Actions. Then, turn to the Rating Instructions and proceed as directed.

GENERAL ABSTRACT INSTRUCTIONS

Preliminary Instructions

This is a test of social judgment, one form of intelligence. The following page(s) contain a list of a variety of actions that you may have witnessed one person doing to or with another person or that people may have done to you from time to time. You will be asked to rate each of the actions along the scale dimensions provided. In making your judgments you should give your general impression of the action as it manifests itself in a lot of different situations, rather than judging the action solely on the basis of one specific incident that may have happened to you. You should bear in mind when making your judgments that the same action may occur in a variety of situations, and the people involved may or may not know each other and may be of any sex or height, etc. Before reading the Rating Instructions, please read through the List of Actions. Then, turn to the Rating Instructions and proceed as directed.

LIST OF ACTIONS TO BE RATED ON TWO DIMENSIONS

1. A person offers you a newspaper he has finished reading.
2. A person makes eye contact with you briefly for less than a second, having turned his/her head in your direction to do this.
3. A person stares at your face continually for up to five minutes with a neutral facial expression on his face. If you make eye contact with him, however, he diverts his gaze for five seconds and then continues to make it again, etc.
4. A person is taking a pack of gum out of his pocket, turns to you and offers you a piece.
5. A person acts as if he smells something peculiar, checks the soles of his feet and then looks at you or in your direction while sniffing the air. Even if you look at your soles and find nothing, he continues to sniff 3 more times and then moves a few inches away from you.
6. A person says to you "boy, you have the worst looking _____ (article of clothing, part of face) I ever saw", or "you have the worst..."
7. A person stands or sits extremely close to you even though more room is available.
8. A person concentrating on something else, absent-mindedly blows cigarette smoke in your face and drops ashes at your feet, not paying attention to fact this is happening.
9. Same action as Action 8. However, when you call it to his attention, he continues to do it as before and tells you to mind your own business.
10. Same as Action 8 except that when you call it to his attention he stops doing it but also tells you to mind your own business.
11. A person asks you for directions to a particular building, smiling as he does so, he then heads in that direction.
12. A person is concentrating on something, chewing bubble gum very loudly at the same time, while you are concentrating on something.
13. Same as action 12 except that you ask him to stop and he tells you to mind your own business, and continues as before.
14. A person asks you to move over when he has plenty of room.
15. A person says to you "boy that's the best looking (article of clothing, part of face) I ever saw or "you have the best..."
16. A person's pen runs out of ink. He takes a pen you are using out of your hand and affirmatively says "you won't mind if I use this - I'll give it back in five or ten minutes."
17. A person looks up from his newspaper, turn to you and says "boy I see those lousy (your ethnic or religious group) are starting trouble (over or about)..."

(continued)

18. A person smiles at you warmly the first time he makes eye contact.
19. A person blows Dristan into "his nose", blows his nose, and while concentrating on something else, repeatedly coughs all over you, not paying attention to his doing this.
20. Same as 19 except that when you ask him to stop he says "mind your own business" but doesn't stop.
21. Same as 19 except that when you ask him to stop he says "mind your own business" and doesn't stop but continues coughing after a minute intermittently as before.
22. A person does not move over when you indicate you want to sit down on the only nearby seat. Instead he says "I like where I'm sitting, why don't you find another seat".
23. A person smiles at you warmly, each of the first two times he makes eye contact with you.
24. A person borrows a pen from you and doesn't thank you. He uses it one minute, looks at his watch says "shit I forgot to put a dime in the meter" gets up to leave and it looks like he'll "forget" to give it back to you as he leaves.
25. A person smiles at you warmly and initiates a conversation on a typical topic such as school.
26. A person reads a newspaper and while concentrating on it, allows the pages he turns to fall in your lap as you are trying to do some work.
27. Same as 26 except that when you tell him to stop he says "mind your own business" and continues as before.
28. Same as 26, except the person says "I'm terribly sorry" and stops but continues reading for 2 minutes, following which the same thing happens again.
29. A person is concentrating on something else, has his legs crossed and is absent-mindedly kicking you while concentrating on something else.
30. A person offers you a newspaper he has been reading and has finished, saying that he thinks there are some interesting articles in it you might like.
31. A person starts scratching his back, turns to you and asks if you would do it for him.

*Please note: the you in each action may be replaced by another person as in the instructions

SAMPLE - AGGRESSIVENESS-CONSTRUCTIVENESS SCALEInstructions

You are being asked to judge each of the actions on the List of Actions on how Constructive or Aggressive they are. An action is considered to be Constructive to the extent that it helps or potentially helps people or contributes to their overall well being, physical or psychological. Constructive actions help people maintain valued states of mind, valued actions, valued assets, valued feelings, valued objects, etc. Aggressive actions are those that destroy, harm, or threaten to harm something physical or psychological, tangible or intangible, that is valued by other people. Aggressive actions harm people's valued states of mind, valued actions, valued assets, valued feelings, valued objects, etc. For example, if an action resulted in someone's loss of pride, self respect, security, etc., or resulted in lost happiness, peace of mind, a lost job, lost values, etc, it would be considered to be an aggressive act. On the other hand, if a person's actions tended to make other people feel happy, valued, secure, proud, wealthy, beautiful, popular, etc., they are defined as constructive actions.

You should consider Constructiveness and Aggressiveness as antonyms or opposites; the more Constructive an action is the less Aggressive it is and vice versa.

Please note that these definitions omit aspects of the common usage of these terms:

1. Do not consider the intenseness of an action as aggressiveness per se (as in the expression - "an aggressive salesman"). This is because an action can be an intensely constructive tool.
2. You should focus on the effects of the actions or their potential effects, and not on the intentions or the motivations of the person who is doing them. This is because harmful actions can be performed without any harmful intent. On the other hand, do not hesitate to rate an action as aggressive if a harmful intent is obviously also present. The important thing is to evaluate the actions, and to concentrate on them.

Make your judgments of each action on the scale provided, and mark your choice by circling your chosen response on the answer sheet. Look at the Answer Sheet. The left hand side is defined by the synonyms given at the top left of the page. The right hand side is defined by the adjectives and synonyms at the top right of the page. Mark any actions that you see as Aggressive on the left side; the more Aggressive the action is, the more extreme rating it should receive. Similarly, you should use the right side of the scale for actions you see as Constructive. The more Constructive an action is, the more extreme rating it should receive.

Below is an example of the ratings of six items. You should use these items as reference points in making your decisions.

- Item 1. a person pushes you very forcefully or pushes someone else.
2. a person cruelly insults you or someone else, picking on the person's most obvious areas of vulnerability.

3. a person walks near you or someone else to get an ash tray.
4. a person opens a book.
5. a person lavishly compliments you or someone else and the compliments appear sincere (it's not important if they are).
6. A person offers you or someone else a ride home in the rain - having known that the individual in question desperately needs one.

Item	<u>Agg</u>										<u>Conat</u>		
	Ext	Strong	Strong	Mod	Mild	Neut	Mild	Mod	Strong	Ext	Strong		
1		X											
2		X											
3						X							
4						X							
5												X	
6												X	

Please note that if an action seems much stronger than the strongest examples you may indicate this by putting a + next to the rating. Now turn to the List of Actions and rate each one on the Answer Sheet. There is a numbered row on the Answer Sheet for each numbered item. Please remember too, to follow the Preliminary Instructions carefully as well as the instructions above.

Each class of students rated the List of Actions on one of the two scales under one of the two contexts.

Table 1: N, Sex and Educational Background of Subject Groups
Used in Pretesting (and indicated on following graphs)

I. Group 1-(Students in Undergraduate Abnormal - Brooklyn College)

- a) Situational Appropriateness (S. App) of 30 pretest items obtained.
- b) N = 15
 - 1 - 7 males
 - 2 - 8 females

II. Group 2-(Students in Undergraduate Abnormal - Brooklyn College)

- a) General Appropriateness (G.App) of 30 pretest items obtained.
- b) N = 31
 - 1 - 15 males
 - 2 - 14 females
 - 3 - 2 no sex given

III. Group 3-(Students in Undergraduate Abnormal - Brooklyn College)

- a) Situational Appropriateness (S.App) of 30 pretest items obtained.
- b) N = 12
 - 1 - 6 males
 - 2 - 6 females

IV. Group 4-(Students in M. Ed. program taking Abnormal - Brooklyn College)

- a) Situational Aggress (S.Agg) of 30 pretest items obtained.
- b) N = 17
 - 1 - 3 males
 - 2 - 14 females

V. Group 5-(Students in Advanced Abnormal - Brooklyn College)

- a) General Aggression (G. Agg) rating of 30 pretest items obtained.
- b) N = 37
 - 1 - 16 males
 - 2 - other

NOTE: "Situational Groups" - given verbal description of waiting room setting before doing rating.

"General Groups" - asked to give impressions based on wide variety of situations, people, etc.

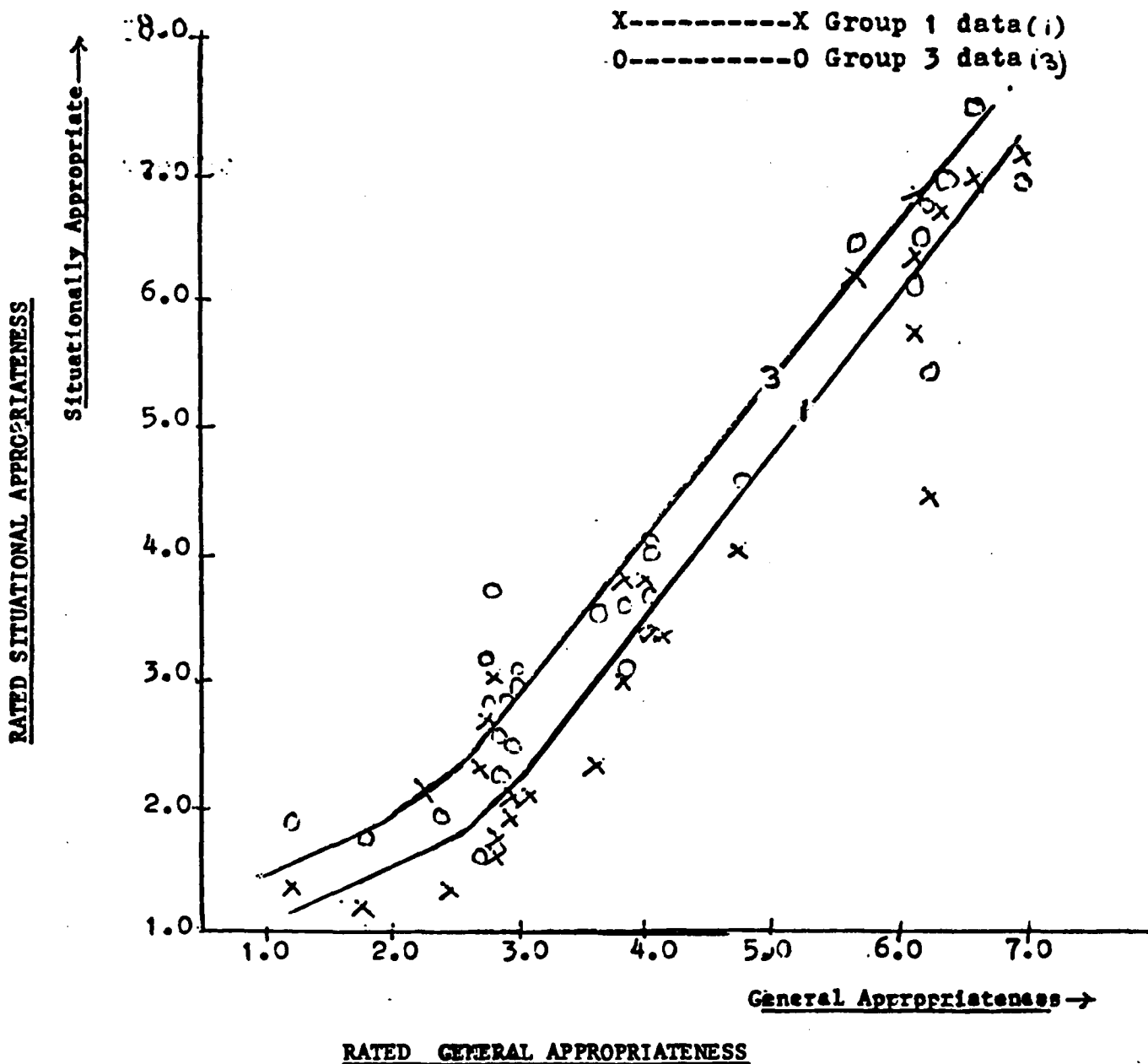


Figure 13: The relationship between the mean ratings of 30 pretest items on general appropriateness by group 2 and the ratings of the same items on specific appropriateness by groups 1 and 3. Two best fitting lines are also shown for each set of data.

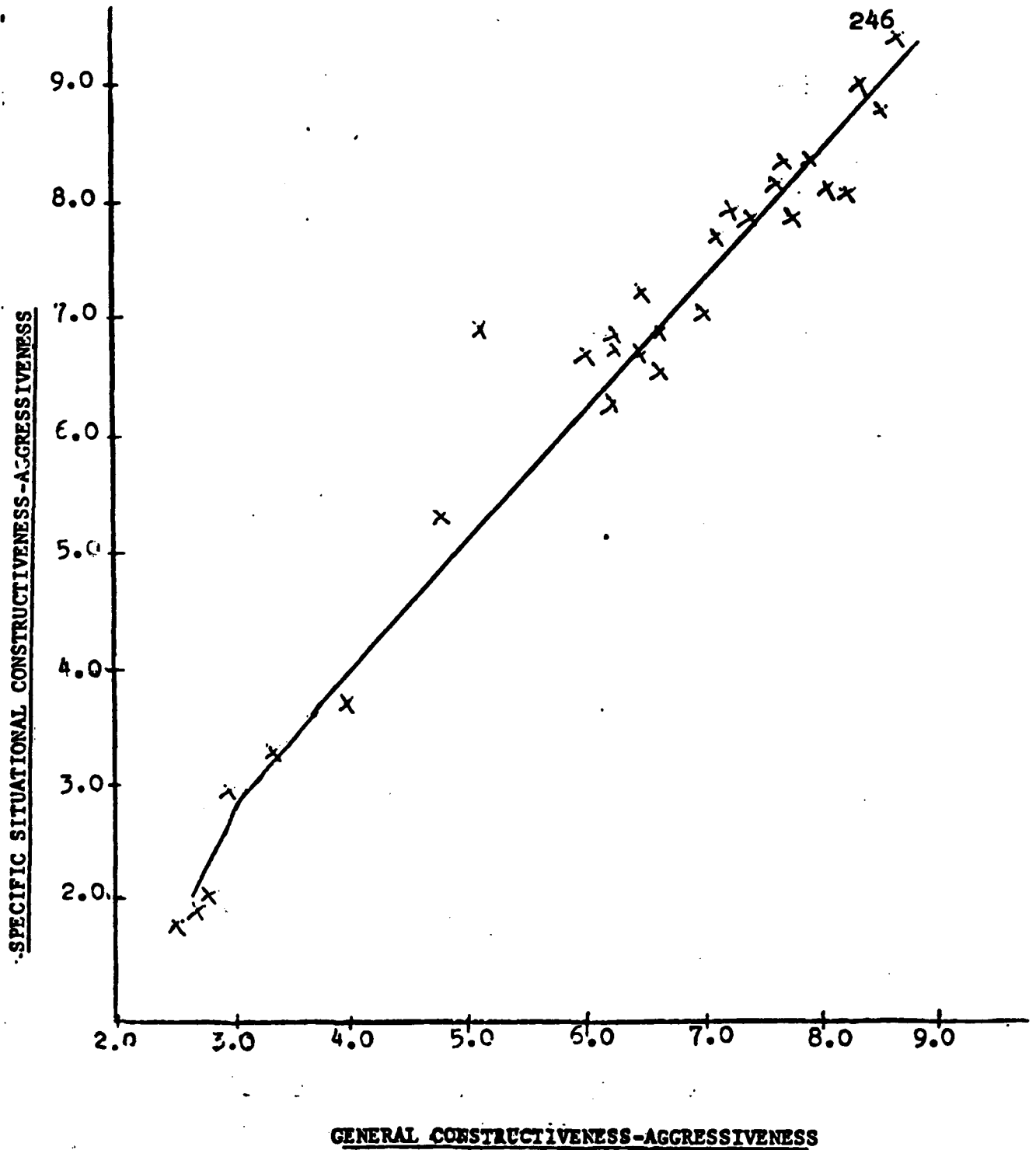


Figure 14: The relationship between the mean ratings of thirty pretest items on general aggression by group 5, and the ratings of the same items on specific appropriateness by group 4. A best fitting line is also shown for this data

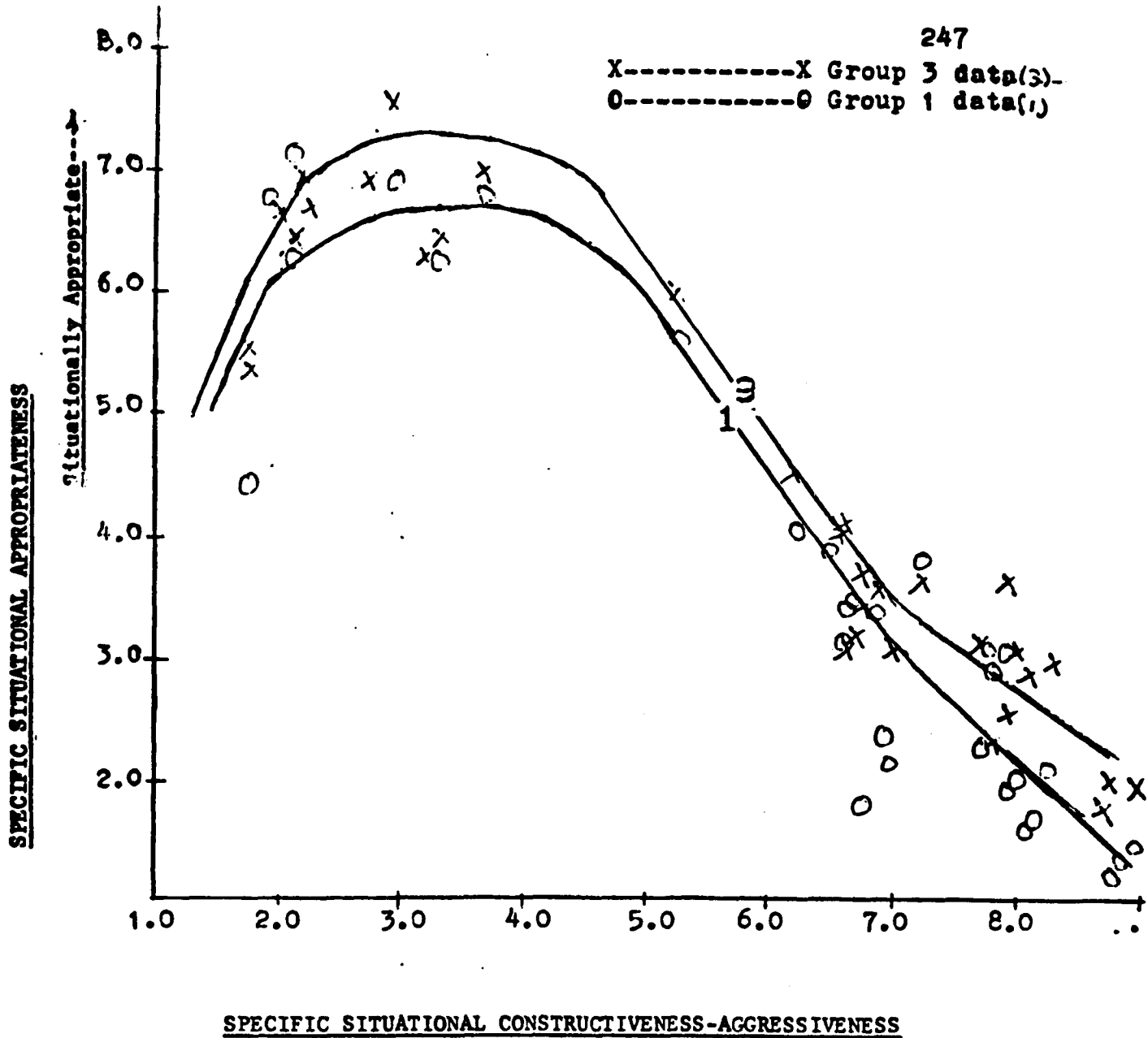


Figure 15: The relationship between mean ratings for 30 pretest items on specific situational aggression by group 4 and the mean ratings of these items on specific situational appropriateness by groups 1 and 3. Best fitting lines have been drawn in each case

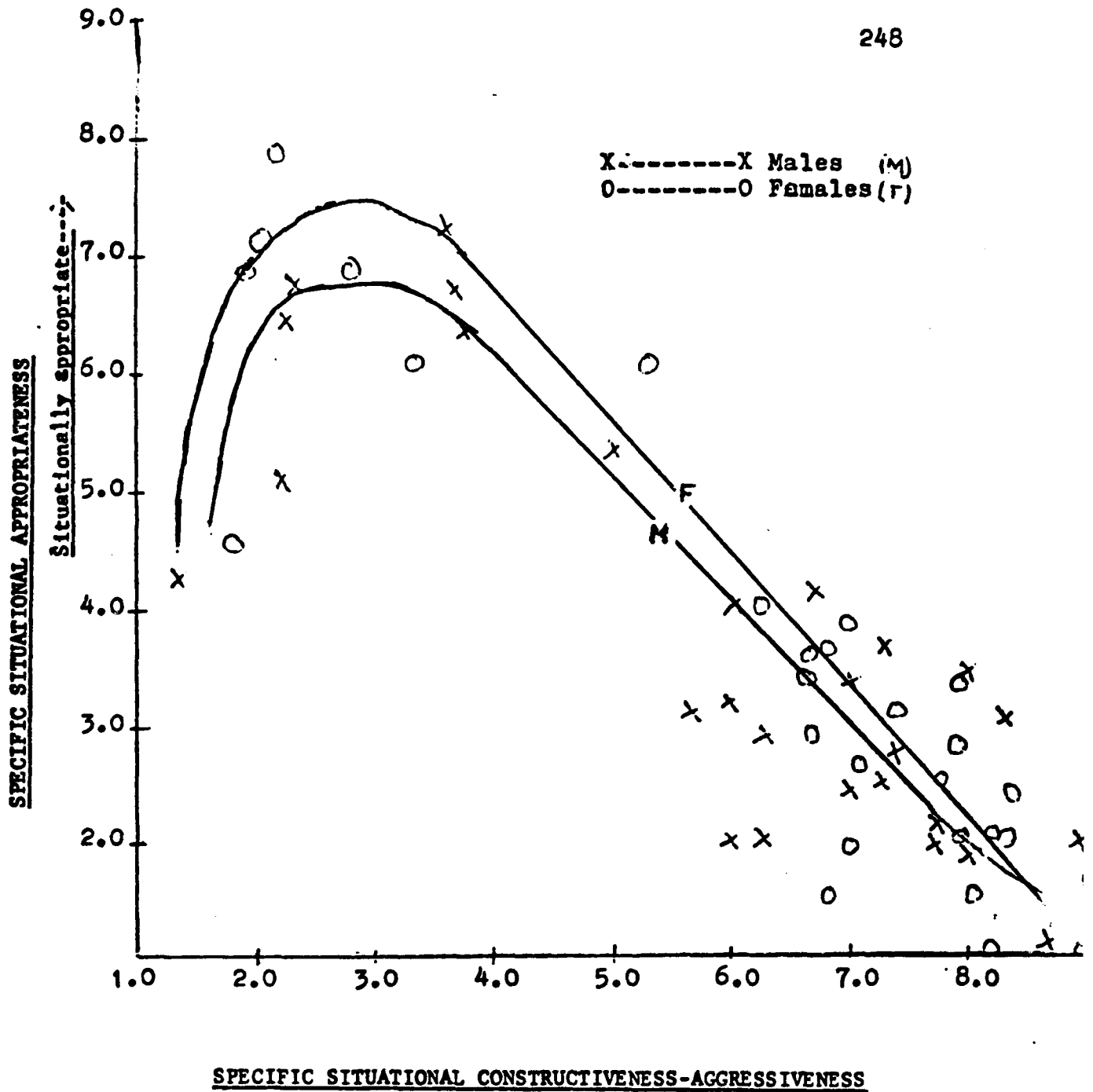


Figure 16: The relationship between mean ratings for 30 pretest items on specific situational aggression by group 4 (ratings of male and female raters plotted separately) and the mean ratings of these same items by group 1 on specific situational appropriateness. The hypothetical best fitting lines are also shown.

APPENDIX B

Notes on ethical considerations involved in the design of this study.

ELEMENTS OF THE DESIGN INCORPORATING ETHICAL CONSIDERATIONSSetting

1. The setting of this study is designed to be as realistic as possible consistent with the desire to secure from Ss' their voluntary participation in this experiment.
2. Further, each S is free to leave the experimental setting at any point in the experiment, and will receive debriefing in this case.

Stimuli

1. Care has been taken in the selection of the stimuli consistent with the hypothesis under test. While the stimuli may be either helpful or aggressive in nature, they are realistic, and are potentially no more uncomfortable than those that an individual might experience in a similar naturalistic setting. To the degree that they are uncomfortable, it is felt that this discomfort is an essential part of the experiment, to achieve the stated goals. Finally, it is noted that relatively few S's will be exposed to the most aggressive stimuli.

Methodology

1. Deception is an essential part of this study, to obtain candid and spontaneous responses, and to obtain accurate recording of such responses for judging. The deception consists of:
 - a. creating the illusion that the subject is seated in a waiting room with another student who is really a confederate. The experiment, on a somewhat similar topic, is supposedly to take place following completion of the forms in the waiting room. This deception is needed to provide for a candid spontaneous response to the confederate, and to provide for the situation where the Ss have volunteered for psychological research, at the same time.
 - b. having the video-tape equipment hidden behind a one way mirror disguised as modern art. Again, this is needed to insure spontaneous responses.

ADDENDUM TO ETHICAL CONSIDERATIONS

- I. Stimuli - Any stimuli which may be added to the list specified in the original proposal will be added as follows.
- A. Before any stimulus is considered for use:
 - a. the experimenter and his research assistant will consider such stimuli from an ethical viewpoint, that is, its potential for upsetting and causing discomfort to the Ss and the necessity of its use in the study.
 - b. In addition, at least one member of the sponsoring faculty will be reviewing all stimuli.
 - B. As a result of A, many potential stimuli will not be used. The data which will be considered in making this judgment will be:
 - a. our own impressions of the potential discomfort of these stimuli.
 - b. data obtained from initially presenting these stimuli to classes of judges, in questionnaire form, and having these judges rate the stimuli on their potential aggressiveness and inappropriateness.
 - c. the results of pilot trials with the old stimuli which indicate the inadequacy of the old stimuli and which suggest the need for new ones.
 - d. the results of pilot trials with the new stimuli.
 - C. All stimuli that are finally selected for use will be subject to the review of the Ethics Committee.
- II. Ratings - Since the video-tapes will be rated by judges and the possibility exists that they may know the Ss, the following precautions will be taken to ensure that the raters and the Ss do not know each other, and to minimize the problems associated with such possible foreknowledge.
- A. Ss will be briefed as to the fact that the video-tapes will be judged by other students as indicated below. They will be advised of the possibility that a judge may recognize them but will be told that the experimenter will do his best to prevent this, and will be told how this is to be done, that is:
 - B. All video-tapes will be judged by students at other colleges.
 - C. Since these are mostly freshmen, students in advanced elective courses will do the judging, where possible.
 - D. The experimenter, in presenting the tapes to classes, will try to show a still frame of the tape before it is run in its entirety for judging. If any judges indicate that they recognize an S, they will be asked to leave the room for the judging of that sequence of responses.
 - E. The judges will be debriefed as to
 - a. the true purpose of the study.
 - b. the normality of the behaviors exhibited and the experimenter's intentional eliciting of them.
 - c. the altruistic motives of the student subjects who have volunteered their videotapes for this research.

F. Any judge who still recognizes an S will be personally debriefed after the judging is over.

- III. Release - Ss will be asked to sign a release form which will read "I understand that video-tapes have been made of my participation in a social science research project. I also understand that these tapes will be shown to students at another college to be judged for research purposes. Precautions against the possibility that someone might recognize me have been explained to my satisfaction. This letter signifies my willingness to contribute the use of the video-tapes for research purposes."
- IV. Debriefing - The debriefing will consist of steps A and B, as indicated below. Step A will be delayed or omitted where Ss discomfort may have achieved very strong levels. At this point, step B will automatically be instituted by E and his confederate.
- A. Following the video-tape procedure, some open ended questions will be asked of the Ss by the experimenter, ostensibly to determine which students will be needed to participate in the "real" experiment. While such questions will be used for data collection, they will also allow for Ss expression of feelings about the experimental situation, the E, the C, etc., and the response may be valuable for the debriefing proper. A sample question is: "What thoughts, comments, or criticisms do you have about the screening procedure thus far, including the use of the questionnaire?" Following these open-ended questions
- B. C will introduce himself to S, with E present, and will explain that he is another student volunteer. He will apologize for his behavior, if necessary indicate he understands S's response was appropriate to the situation, and will reassure S that his own behavior toward S was not to be taken seriously but was part of the research.
- a. E will then also introduce himself, and will thank S for his cooperation. He will explore and listen to any negative feelings that S has and will stress to S that:
1. he understands why S may be upset, if he is, by C or E;
 2. that many people probably would feel the same way in his shoes;
 3. that the procedure was necessary to explore an area of human behavior where much exploration is needed;
 4. that S has potentially made a great contribution toward furthering our knowledge of the field;
 5. that Ss responses have been quite normal and appropriate given the provocation;
- b. E will then show S the experimental apparatus and answer his questions if any;
- c. E will then indicate that S can have his experimental data destroyed. However, he will stress Ss potential contribution to the field and explain that:
1. no data will be associated with his name;
 2. that all video-tapes will be judged at another college;
 3. efforts will be made to ensure that the judges do not recognize him;
- d. he will be invited to volunteer to be a confederate in future research;
- e. he will be told he can see the results when they are complete;
- f. he will be asked to sign the release and enlisted in maintaining the confidentiality of the study.



School of Social Science

Department of Psychology

PROPOSAL

1. Candid videotape segments were collected at Brooklyn College of different students' responses to Aggressive or Helpful behavior initiated towards them.
2. These students have given their written consent to allowing these videotapes to be shown at other colleges and they are to be shown anonymously.
3. Each class is to view several of these segments and is to rate them along one of four scales. They will be told these are tapes of potential subjects versus actual subjects but will be debriefed as to this later. These scales are
 - a. Aggressiveness - Constructiveness
 - b. Avoidance of Genuine Emotional Candor
 - c. Appropriate Normal - Inappropriate Abnormal
 - d. First Impression of how "helpful" or "harmful" each videotaped individual seems at first glance.
4. Before they do the ratings the students will be told
 - a. they are not to put their names on the IBM sheet - their ratings are anonymous;
 - b. they need not complete and submit the ratings if they do not wish to do so.
5. After they complete their ratings
 - a. Age, sex, height and weight will be obtained from each student.
 - b. They will be debriefed as to how their ratings fit into the larger research project and time will be spent with each class answering their questions and discussing some preliminary issues and findings.
 - c. My phone number at Brooklyn College will be given to them should they wish to obtain the results of the research or should they have any questions, comments, etc

PERMISSION FOR USE OF VIDEO-TAPE AND OTHER DATA FOR RESEARCH PURPOSES

I understand that video-tapes and other records have been made of my participation in a social science research project, and that my name will not be associated with this data. I also understand that these tapes will be shown to psych students at another college to be judged for research purposes of the present study. Precautions against the possibility that someone might recognize me have been explained to my satisfaction. My signature signifies my willingness to contribute the use of the data h... gained for research purposes.

APPENDIX C

Training Manual for Confederates

Includes description of the behavior called for in enacting each stimulus, during each phase.

MANUAL FOR CONFEDERATESGeneral Orientation

The confederates' function is to, at an appropriate time, present the naive subject with a stimulus situation that is either harmful to the subjects' valued states of mind or valued assets, or helpful to them. Your participation in the present research can be broken down to a series of stages as follows:

- A. Initial stage-this stage covers the period that starts from when you first come into the waiting room, and which ends when the experimenter leaves to get the forms he forgot. Your major aims during this period should be:
1. to convince the S that you are a naive subject as he is.
 2. to present the S with essentially neutral behavior on your part, that is, behavior which is neither helpful to the S nor harmful to the S. Since we are avoiding the use of physical harm in this experiment, and physical helpfulness, your harming or helping S's valued states of mind (security, anxiety, friendship needs, needs for information, etc.) become crucial.
- B. Experimental stage - this covers the period of time that starts when the experimenter leaves the room, and ends when the experimenter returns. There are three distinct substages involved as follows:
- a. pre-experimental waiting period. Your aim during this period of time (60) seconds following the leaving of the room by the experimenter) should be to maintain the neutrality of yourself as a stimulus. The purpose of this brief interval is to disassociate your production of the stimulus situation with the leaving of the room by the experimenter.
 - * b. experimental production of the stimulus situation. This period begins following the pre-experimental waiting period outlined above, and ends with the full production and elaboration of the stimulus situation. Your aim here is to present the S with a standard stimulus situation, which is somewhere along the Aggressive-Constructive dimension. Again, since we are using a minimal amount of physical activity, it is crucial that your emotions, posture, verbalizations, etc. be such that S is presented with a stimulus situation that either makes him feel "bad" or feel "good" depending on the case, that is, either harms his psyche or helps it to some degree. It is also essential that your actions be such that they occur independently of whatever S does and are minimally influenced by his behavior. You must not respond to his actions, except where called for in the design since if you do, this means each S is setting up the stimulus situation his way, and not ours as we intended. Judges should be able to agree that you have behaved in a standard way from one subject to the next, and that your behavior actually falls at the desired point on the Aggression scale regardless of the response that S makes to you.
 - c. post-experimental waiting period. This period begins following the production of the experimental stimulus and ends when the experimenter returns to the room (he will return after 2½ minutes have elapsed). This period is included in the design to allow the S to freely behave toward you once you have actively terminated your stimulus behavior toward him, particularly your verbal behavior. During this period you must not initiate any further interaction with S, and you must not respond to any initiated action on S's part with a response which further harms or helps S, that is, you must not respond with a response which annoys, angers, frustrates, relaxes, soothes, makes S feel good, etc. However, you must make some appropriate response, that is, one

direct
response
phase
(d.r.p.)

spontane-
ous
response
phase
(s.r.p.)

which is realistic to the situation and which allows S to continue to express, whatever he already is feeling inside as a result of your past actions. Your aim during this period should be to let S act on you and set up the situation his way, and not visa versa.

(This phase not included for stimuli 5, 6, and 8)

- C. Debriefing Stage - This begins when S returns and ends when S is dismissed.

Detailed Outline of Each Stage and Contingency Plans to Follow

A. Initial Stage *

1. to convince S you are naive:
 - look and act naive in your manner
 - ask S four questions including
 - * is this the right room for the Psych 2 experiment?
 - * when will the experiment be starting?
 - * do you care if we use pencil or ink in filling this out?
 - * which form did you say you wanted us to do first?
2. to maintain your neutrality as a stimulus:
 - do not initiate any interaction with S
 - if S initiates any interaction toward you, you must maintain your neutrality by
 - * using neutral gestures to answer S's questions such as shrugging your shoulders, shaking your head, grunting, etc.
 - * if it would be harmful to the average S not to answer a particular request for interaction with words, you may do so but you should keep it brief (Yes, No, Maybe, Don't know) and your affect must be neutral
 - * avoid eye contact with S as much as possible during this period
 - * if S asks you for gum, cigarettes, time, etc. tell him your watch is not set right, you just ran out of gum, you don't have any matches, etc.
- a. Outline of this stage - During this stage the "rationale" for the study is to be explained by E to both C and S at the same time. There are two different rationales that will be used. The aim of one of them (Opinion-Normal Restraint) is to deceive the S into thinking we are studying opinions and their formation, and to make S feel that during the time he is in the waiting room with C, the normal rules of conduct and social behavior apply and the normal consequences will result from their flouting. The aim of the second condition (Emotional-Low Social Restraint) is to make S feel it is safe to freely express and act out whatever he feels in the waiting room and that no external consequences will ensue or they will be minimized. At the same time, S. is to be deceived as to the true purpose of waiting in the waiting room.
 1. Note that C should leave the camera room and be one hundred feet down the corridor, eight minutes before the start of an experimental run. He should be in clear view of the experimental corridor. E will scratch his head as a signal for C to enter the room 30 seconds following the signal. S will arrive before this signal is given.
 2. During the Opinion conditions, C should present whatever opinions he is called on to deliver in an animated, involved firm way. During the Emotional Behavior Condition, the same is true. In addition, during this condition only (Emotional)
 - * C should respond to E's provocations, if any, with appropriate hostile or friendly affect and behavior. (See the instructions for this

condition for a fuller discussion of this and specification of C's responses)

* C must come across as a person who enjoys doing his own thing, even if that involves being inconsiderate of others in his life. He likes to do both nice things that others don't appreciate (see instructions for this condition for specification of these). Again, please note he is not to comment on S's behavior during this Initial Period nor is he to initiate any behavior during this period that is directed toward S.

* E will be supportive of all this and indicate it is OK to do whatever you please during the time spent in the waiting room and thereafter.

Experimental Stage - We will take up each stimulus situation and will discuss the three substages as they apply to each stimulus. Please memorize the lines on these sheets and the various contingencies in each case. Play your training tape of the particular stimulus, and read the instructions pertaining to it, at home. We will have regular practice sessions where you will review the video training tape and will rehearse the stimuli.

Stimulus 1 - Extremely Friendly, Helpful, Complimentary, Offer Tickets and/or Date.

Stimulus 2 - Strongly Helpful and Constructive - Engage S in discussion of his personal interests - flatter him somewhat indirectly.

Stimulus 3 - Moderately Helpful and Constructive - Offer S gum in a friendly manner, indicating you seek to establish a helpful interaction through your manner.

Stimulus 4 - Slightly Helpful and Constructive - Ask S for change in a Neutral-Slightly Friendly manner. Is slightly helpful as it breaks the ice.

Stimulus 5 - Neutral Stimulus - Initiate no interaction with S and do nothing that would inherently tend toward interaction with him.

Stimulus 6 - Slightly Aggressive and Harmful - Bump S slightly and chew gum loudly. Indicate in a Neutral-Slightly annoyed manner that you have no remorse for doing this.

Stimulus 7 - Tell S to move over on the couch in an irritated manner. He should wind up sitting on or near the edge of the couch. Moderately Aggressive. You have made him physically uncomfortable, have implied you don't care about his feelings, don't like him, etc.

Stimulus 8 - Strongly Aggressive - You smoke a cigarette blowing the smoke near S's face and dropping ashes near S's feet and legs. You indicate anger at S when he notices this and/or questions you. Involves more physical discomfort than 7 and more implied lack of concern for his feelings, etc.

Stimulus 9 - You tell S to move over and/or get out of the room. You become severely insulting, using standard insults and extreme anger.

DETAILS OF STIMULI AND OPTIONSStimulus 1 - (6 options outlined) - Extremely HelpfulPre-experimental time 60 seconds

I. A. Behavior

- a. get up and throw gum wrapper in basket; sit down very close to S
- b. no active initiation of action
- c. handle subjects' initiations with
 1. shrugging of shoulders
 2. paying attention to your forms
 3. indicating you don't have cigarettes, gum, time, etc. if subject requests;
 4. no use of words except where necessary and then restricted to yes; no; maybe; don't know; grunts; noises.

If S persists in questioning you, etc. more than 3 x he will be forcing you to either be friendly or unfriendly. You should say "I think we better finish these forms and discuss this later", if you feel you are being forced into one of these positions.

B. Affect neutral

C. Facial expression - neutral; avoid eye contact or turning in S's direction.

1. Aim here is to present S with a pre-experimental situation that is neutral - your desire is to fill out the questionnaire;
2. you must establish this; not the S.

II. Experimental (drp)

Turn toward S; smile broadly; extend hand - say "Hi, I'm...who is your psych teacher? (S answers) Say that's really far out...mine's... You got a girlfriend (S responds Yes) Broad smile...Say, farrrr. out, terrific.. what's her name? (S responds) that's outrageous, mine's named...(same name) too;(broad smile - turn toward S and say:the reason I asked was that you seem like a real together guy, I mean you really seem cool..and I just won these tickets to Steak and Brew, four of them, you know, and they're only good for this weekend and half way into next weekend...I was planning to ask someone else to go with me but I'm not sure if I want to go with them and you seem like you're a really together guy (broad smiles) and I'd really enjoy having you and your girl get together with me and my girl and use them...Are you doing anything this weekend..? (answers yes, maybe, no) If no, say Great!, that's really good, why don't we make it Friday night then, give me your number and address and we'll pick you up at 8:00 (Gives) OK, far out! (If he says maybe) say, OK far out; how about I'll give you a tentative call, call your girlfriend, and we'll try to arrange a convenient time, place, etc. I'd really like you and your girl to keep us company (smile);(if yes, say) Far out; well listen, I'd really like for us to get together with you and your girl, so why don't you give me your number and I'll give you a call next week and we'll make plans to party it up or something..

(If S has no girlfriend say)...You're kidding,that's really terrific.. that's really outrageous...the reason why I asked is you seem like a really together guy (smile as above) really cool,you know, and me and my

girl were going to do some partying this weekend, and her cousin's coming in from Wisconsin for a while...I met her before..she's really fun and is really a knockout...and I figured we'd all come back to my house and hang out all night...It might turn out pretty good (laughs)...I was planning to ask someone I know but I'm not sure if he's cool enough for my girl's cousin and you seem like a really together guy, I mean you really seem cool...and we'd really enjoy having you over this weekend, can you make it? (Yes, say as above) (Maybe, say as above with appropriate modifications) (No, say as above)

1. Behavior - smile, gesture, extreme use of adjectives, turn toward S, shake hands, emphasize compliments
2. Affect - happy, elated at points
3. Facial expression - solid eye contact, happy, friendly, look in S's direction, stop work

III. Post experimental - follows resolution of above. Spontaneous response data (srp)

1. Do not directly initiate anything further.
2. Respond to S's initiations with appropriate facilitating behavior.
3. Avoid presenting S with any new helpfulness during this period. If S behaves in an extremely constructive way to you, you can seem friendly, happy, make eye contact, etc. but avoid initiating new constructiveness (or aggressiveness)

Stimulus 5 - Neutral stimulus

- a. Pre-experimental (time 35 seconds) same as Stimulus 1 Pre-experimental
- b. Experimental (to end of this stage) same as a except no new gum wrapper is used (d.r.p. direct response phase).
- c. Post experimental - None (s.r.p., spontaneous response phase).

Stimulus 3 Moderately Helpful Stimulus - you are offering gum and trying to establish a friendly interaction - more than just polite

- a. Pre-experimental (time 60 seconds) same as Stimulus 1 Pre-experimental
- (d.r.p.) b. Experimental - Wait 20 seconds and say "What's your name? I'm... wait 20 seconds more and say... You want a piece of gum? (If no, wait 20 seconds and say)... You're sure you don't want a piece?"

1. Behavior - warm smile...gesture of offer...look at S when talking but don't turn body...moderate friendliness
2. Affect - moderate friendliness
3. Facial expression - slight to moderate friendliness

- (s.r.p.) c. Post experimental - after second offer

1. C does not initiate
2. C responds to S with appropriate affect and behavior to any initiations by S. Again, avoid adding anything to the stimulus but do allow S to do his thing.

Stimulus 4 - Mildly Helpful - Initiate Contact via asking for change

- a. Pre-experimental - same as Stimulus 1
- (d.r.p.) b. Experimental - Wait 20 seconds, fish through pockets for 30, then in a loud enough voice so S hears you say "do you have change of a quarter.. thanks or thanks any way."

1. Behavior - hold out hand..questioning tone of voice..polite... do not look at S...do not smile...very, very, slight friendly tone of voice is permissible
2. Affect - Slight, slight, friendliness
3. Facial Expression - neutral

- (s.r.p.) c. Post experimental - following thank you

1. Do not initiate anything-as in all post experimental situations, do not bury your head in you forms..look around the room a little and give S an opportunity to do the initiating
2. Respond to S's initiating in a facilitating manner. Do not behave more aggressively or constructively than the S.

Stimulus 7 - Moderately Aggressive - Move over twice.

- (d.r.p.) a. Pre-experimental - same as Stimulus 1 - be sure to get as close to S as possible (60 seconds)
- (d.r.p.) b. Experimental - Say, Hey man, move over, huh, in an annoyed tone... (wait twenty seconds) shifting around in an annoyed way...turn to S and say..Come on man, move over some more, huh, extending your hand swiftly in that direction.
1. Behavior - loud annoyed irritated tone...make eye contact..gesture.. if S protests say Goddam and/or look annoyed;if S moves, take up the excess room before repeating.
 2. Affect - annoyed irritated.
 3. Facial expression - same.
- (s.r.p.) c. Post-experimental - following second request:
1. do not initiate - leave yourself somewhat available as before.
 2. respond with annoyance or irritation if S initiates aggression, but again, do not add more harm. Say, "Goddam, come on, etc." if needed.
 3. OK to make eye contact here.

Stimulus 9 - Extreme Aggressive - Five demands to move overand/or get out of room with insults.

- (d.r.p.) a. Pre-experimental - same as above.
- (d.r.p.) b. Experimental - Say, You want to move over man. (give dirty look.. take up excess room - wait five seconds and say): Come on man, you wanta fucking move over, geez...(wait 15 seconds whether or not S has moved off the couch and say): You know you remind me of a fucking cow, not letting anyone have any room, you must be a dumb mother, I didn't know they let pricks like you go to school here...(5 seconds) I would like you to just move your ass over to the end (or out of this room right now...look, just move over prick (or get the fuck out)..come on you mother,,move it, you're such a prick you don't even know what I'm talking about.
1. Behavior - Look at S, glare at him, start out being very annoyed and wind up being very angry..make no threats...escalate the loudness of your voice...gesture...drag out your curses...this is a monologue in its entirety..extra loud peaks
 2. Affect - escalates from irritation to extreme anger.
 3. Facial Expression - angry look, eye contact, face S.
- (s.r.p.) c. Post-Experimental - following the last move over or out:
1. Do not initiate; Do not insult or aggress further
 2. Allow S to initiate - do not insult S or curse further; if appropriate, you can look angry and respond with "God Damn" "Jeez"
 3. Can eye contact, look angry, if S initiates behavior, etc. calling for it.

Stimulus 6 - Mildly Aggressive-Bumping and Gum Chewing Loud-Annoying

- a. Pre-experimental - same as Stimulus 1 except throw something besides a gum wrapper in basket.
 - b. Experimental - Shift around looking in your pockets for some gum and slightly and accidentally disturbing S twice (very,very slight) chew three pieces of bubble gum as loudly as you can
1. Behavior..chewing shifting..slight, slight bumping..effect is to annoy S to some degree through distraction, etc.
 2. Affect - Slightly annoyed; very slightly raised voice.
 3. Facial - Do not look at S or make eye contact.

Note: Do not initiate; If S does, respond in an annoyed manner and say.. sorry, that's how I chew.(Will you cut it out?) No, that's how I chew.

- c. Post-experimental - None

Stimulus 2 - Strongly Helpful - Friendly Conversation

- (d.r.p.) a. Pre-experimental - same as Stimulus 1
- (d.r.p.) b. Experimental - Say Hi, (look at S and smile (pause) Who is your psych teacher? Really...Mine's....I noticed on this form it asks for interests..what are you into? S tells. Pick the most personal and workable and say: Really, I always (smile) wanted to do that..read a lot about it..it takes a lot of...never got into it because it must take a lot of...it's really nice...you're into it...are you into (name some specific aspect of the interest)..yeah..that's (smile) nice..I always figured it would take a lot of...that's why I never got into it. I always wanted to though.
- (s.r.p.) c. Post experimental - following end of above
1. Don't initiate - leave yourself open
 2. Respond to S with less affect and constructiveness or aggressiveness than that received. Do not try to compliment further. If appropriate it's OK to smile, make direct eye contact, etc.
- Note: If S is not ego-involved with the specific aspect you selected in the experimental phase, carry out the rest of the monologue with reference to the more general interest.
1. Stimulus Behavior - happy, eye contact, very friendly, monologue... no physical contact with S or future plans made..do not turn your body toward S..do not directly compliment S and do not overdo your "it must take a lot of's." Remember this stimulus falls between Stimulus 1 and Stimulus 3 (offering gun) Remember that extremely sincere direct compliments constitute one of the anchor points.
 2. Affect - very happy and friendly, but not extremely so.
 3. Facial Expression - Smiles, Happy

Stimulus 8 - Strong Aggression - Cigarette Smoking and Anger

- a. Pre-experimental - Same as Stimulus 1 except don't throw gum wrapper out...light up during this period..drop match to get close
- b. Experimental Situation - blow smoke in S's face and drop ashes around his feet
1. Behavior-do it so that it's obviously not being done accidentally but don't do it while he's staring at you if possible. If S objects say: "It's a free country, man and I'll smoke wherever I want". Start out with this...and go up to an angry: "Cut the shit man and quit hassling me, I'll smoke wherever I damn please".
 2. Affect-Anger, eye contact, strongly raised voice
 3. Facial Expression - Anger, shaking head in disgust
- c. Post Experimental Situation - None
- Note: If S says anything else to you that's aggressive (besides "cut it out"), respond with anger saying "Goddamn it..Geez..For God sakes. Come on ," etc. Do not insult.

General Note 1: Stimulus 1 is the most helpful; 9 the least. Each of the other stimuli you produce should fall along discernible points on the scale. If you have trouble discriminating them in your acting, see me. Reviewing them in numerical order should be done to make sure you are doing them properly.

General Note 2: If at any time, you feel you wish to terminate a subject because of clear and present danger to you or him, please do so. Bear in mind that any deviations from the script will probably result in the data not being used.

General Note 3: The last stimuli on the training tape are anchors falling at the Extreme ends of the scale.

General Note 4: Once the Experimental Situation has been produced do not bury your head in your forms. Look up from them pretty often, etc. so S has an opportunity to initiate. .don't spur S to initiate, though, by looking directly at him for example.

General Note 5: You will be enacting each stimulus twice, once under each of the Initial Conditions, in random order. This means that to complete one block, you will probably need to do 20 6 minute enactments. Since each subject will take about 1½ hours to run, plan on filling in this time with studying, lunch, etc.

General Note 6: It is extremely important that you do not do anything on camera toward S that S does not sense at some level. Since your behavior will be rated by judges for the amount of aggression displayed, it is important that your aggression or the lack of it be sensed by S, if we are to meaningfully correlate S's behavior with yours. In other words, an angry face is of little use if S does not see it.

MANUAL FOR CAMERA CREW

1. Since we will be splitting the screen up for ratings, it is essential that S be kept on one side of the screen and C on the other, throughout each filming.
2. It is essential that actions that affect S, particularly those he notices, be captured on the tape (such as ash dropping.) It may be necessary to swing the camera down for this.
3. Filming should start from the time E leaves the room to the time all three exit for Debriefing.
4. A code should be entered at the start of each segment. The code will be LUMBERJACK. Each letter will indicate one specific stimulus situation as follows:
L=1; U=2; M=3; B=4; E=5; R=6; J=7; A=8; C=9; K=terminated prematurely.
In addition, you are to indicate which of the two conditions the stimulus falls under (Initial Conditions by using O for the Opinion Condition, E for the Emotional Condition. You should append one of these to one of the letters in LUMBERJACK. To indicate a Strong Aggressive Stimulus in the Emotional Condition, it would be AE.
5. Register the Code by pulling out the mike jack from the video machine, putting it into record, and speaking the code into the camera. Be sure to reinsert the mike jack.
6. At the completion of each sequence, check off the appropriate box on the master chart.

APPENDIX D

Procedure for Inducing One of Two Situational Contexts.

NORMAL SOCIAL RESTRAINT

Grp 1 (Opinion Formation Group)

E is present in the room when S walks in. E greets S saying "hello, I'm _____ and I guess you're here for the study." "Another student should also be showing up soon so let's wait a few minutes until he does - I've asked you to both come at the same time so I can explain the preliminary instructions to both of you at the same time - this way, I won't have to repeat myself and the study will go more efficiently. Until he shows up, please complete the following preliminary form; its purpose will be explained to you shortly." * E gives S the first bio sheet and casually leaves room to signal C. Approximately two minutes later, C walks in and says "Are you _____? I'm supposed to be in a study for Psych 2. E says, "Yes I am, we were just waiting for you to arrive before we began.

Now that you're both here, let me explain to both of you that I've asked you both to come down here so that I can explain the details of the study, to both of you at the same time. As I may have mentioned to you (S) just before you (C) came, the research grant for this study will be expiring soon and we're hoping to save time by having subjects come in pairs for the remainder of the study so we can brief both of you on what's involved and also have you fill out some routine biographical forms and so on; hopefully this will advance the study's completion date. Anyway, I guess by now you're probably wondering what you are here for. Well, we are conducting a study of written opinions, what they are, how opinions form and change, how they're related to other inner states such as moods. Before we go on, I'd like you to fill out this form (E hands S and C the MAACL, gives rationale*)

Past research shows that studying opinions is a very difficult task since they are inner states, are difficult to quantify, and are affected by other aspects of the person's experiences. People often are very flip about their opinions and this can confuse us as to how they really feel. The only way we will be able to study people's opinions is by having subjects who cooperate with us by giving their carefully considered opinions on a variety of topics.

Now do either of you have any idea about how we psychologists go about measuring opinions? C-(Pause)

C-Well, we learned about something yesterday in class called the, I think the Guttman Scale..is that what it's called?

E-Yes, that's one of the things we use. How about you, (S) did your teacher talk about opinions yet? (S response)

E-Well the type of scale we're using here is called the Likert Scale. Did either of you ever hear of it?

C-No, not me.

S-

E-Well, my personal feeling is that since all Psych 2 students are supposed to fulfill the experimental requirement, their teachers should teach this early in the term.

*E reads these Mood Form Instructions: We will need an initial estimate of your mood stability and changeability, before we go into the lab for our experiment, since the mood you are in may affect your opinions. On this sheet you will find words which describe different kinds of moods and feelings. Please put an X in the box beside those words which describe how you feel now during the last few minutes. Some of the words may sound alike, but we want you to check all the words which describe your feelings. Work rapidly.

Well, anyway, as I was saying before, what we want to do in the experimental phase of the study is to be able to accurately describe and quantify your opinions so they can be studied in depth. It therefore is of prime importance, that you be able to think out your opinions in a carefully reasoned way, and that you are then able to indicate your opinion through proper use of the Likert Scale.

C-When will the experiment start?

E-As soon as we complete some preliminary forms.

Now let me inform you that before we go on, all of your ratings on these opinion scales and all written information you provide is considered confidential. In fact we will not use any of your ratings on any scales without your permission. Further, your ratings on these scales will not be shown to each other, or to anybody else for that matter, without your written permission. Finally, as per the students' Bill of Rights, let me point out that I am required to inform you that you can leave at any point during the study, or are free to not answer any particular question, if by some chance you find it offensive. We are pointing this out to you in advance because it is very important that you provide us with accurate considered information and opinions. If you feel you want to leave at this point and feel you would not be a good subject, please tell us at any time and I will give you your slip and you can go.

Essentially, what we will be doing with your written statements and ratings of your opinions on various issues is to look at them from a variety of angles, and to correlate all this information with other information you will provide us with, to attempt to answer the question posed at the beginning regarding the nature of opinions, opinion formation, etc.

Now as we have seen before, it seems that most Psych 2 students have not, through no fault of their own, been adequately exposed to the Likert Scale. For this reason, I think that it would be a good idea for me to explain the usage of the scale to you both, at this point, even before we go into the labs for the experiment, so that we can clear up any questions about its use. Now essentially, the Likert Scale that we will use consists of a scale such as this. (E shows) One side is labelled Strongly Agree, one side is labelled Strongly Disagree, and in the middle we have No Opinion or Not Sure. There are numbers going from Strongly Agree #1 to Strongly Disagree #9. You may circle any number, from 1-9 corresponding to the strength of your opinion one way or the other and this will allow us to quantify them, and correlate them with other data. Remember, too, you need not answer any question, and all written data you provide us with will not be used without your permission.

Now to make sure in our own mind that subjects know how to use this scale, before we send you off to the experimental lab, and also to get an initial estimate of what your current opinions are on certain topics of current interest, I would like to ask you some questions. I would like you to first write out your answer to each very briefly, in the box provided and then would like you to use the Likert Scale also provided. This way we can make sure your scale responses reflect your written ones and we will also have an initial index of your opinion on certain topics. This will be one way of getting an objective estimate as to whether each of you is properly using the Likert Scale and it will also give me an initial baseline estimate of your opinions in certain areas.

In this practice session, be sure you understand how to use the Likert Scale. Also be sure you understand which boxes to use and which scales to use for each item. Finally, be sure your handwriting is legible. If you have any questions let's clear them up now, during this practice session, before the experiment begins.

(E now asks) the following questions and C and S each write down their opinions privately in the spaces provided and use the Likert Scale.

E collects the opinion sheets and places them face down-he does not look at them. Good, did you have any questions about how to use the scale?

C-Not really.

S-Responds

Anyway, prior to the experiment I would like to get an idea of what your mood is like, since we will be trying to understand how your opinions and their formation is related to your mood stability or changeability when the experiment begins. To get an idea of what your mood is like please fill out this Mood form (E hands one to each and reads instructions below*)

OK now before we go into the experimental room for the start of the experiment, as I mentioned before, it's necessary for me to get some biographical information from you. So why don't you both finish the biographical form at least up to here (points) (As they begin working E announces) Listen, while you're going this, I'm going to go to the john - haven't had much chance before.

C-Do you care if we use pencil or ink?

E-Either

C-Which should we do first?

E-The biographical stuff

Please try to complete them. I'll be back in 5-10 minutes, so just work on those forms, and do whatever else you happen to feel like doing, until I return.

60 seconds pass - C aggresses - S responds

Following E's leaving the room, the experimental phase begins. E returns and asks both parties if they have had any trouble with the bio form. A few seconds later he announces:

Well listen, why don't you stop work now on the biographical data and if it's not finished we'll return to it in a few minutes. At this point, we are ready to proceed to the experimental room for the experimental part of the study. (C and S are split up) However, just before we do, I'd like to get one more of these mood forms from each of you, since as mentioned, it's necessary to have them filled out more than once. (Both fill out the mood form for the third time and leave the waiting room)

*MOOD FORM INSTRUCTIONS - "On this sheet you will find words which describe different kinds of moods and feelings. Put an X in the boxes besides the words which describe how you feel now, and during the last few minutes (5). Some of the words may sound alike, but we want you to check all the words that describe your feelings. Work rapidly (when finished, E collects)

LOW SOCIAL RESTRAINT GROUP

Grp 2 (Freedom of Emotions Expression Group)

E is present in the room when S walks in. E greets S saying "hello, I'm _____ and I guess you're here for the study." "Another student should also be showing up soon so let's wait a few minutes until he does - I've asked you to both come at the same time so I can explain the preliminary instructions to both of you at the same time - this way, I won't have to repeat myself and the study will go more efficiently. Until he shows up, please complete the following preliminary form; its purpose will be explained to you shortly." E give S bio sheet and walks out of the room briefly to signal C. Two minutes later C walks in and says "Are you _____, I'm supposed to be in a study for Psych 2." E says, "yes I am, we were just waiting for you to arrive before we began."

Now that you're both here, let me explain to both of you that I've asked you both to come down here so that I can explain the details of the study to both of you at the same time. As I may have mentioned to you (S) just before you (C) came, the research grant for this study will be expiring soon and we were hoping to save some time by having subjects come in pairs for the remainder of the study so we can brief both of you on what's involved and also have you fill out some routine biographical forms and so on; hopefully this will advance the study's completion date. Later on when we're ready to go into the lab for the experiment, I'll split you up. Anyway, I guess by now you're probably wondering what you are here for. Well, we are considering a study of needs, desires and emotions, what they are, what affects them, how we recognize them in ourselves and others, and their relation to behavior. Before we go on I'd like you to fill out this form (E hands S and C the IAACL, gives rationale*) Past research shows that studying emotions and desires is a very difficult task since emotions are inner states and psychologists can only observe behavior. Thus, the only way we will be able to study people's emotions and desires in this study is by having subjects who can feel relaxed enough in our experimental lab to freely express what they are feeling through their behavior or to freely communicate and act on their desires through their actions, verbal or non-verbal.

Now do any of you have any idea about why people don't always express what they feel or act on their desires, in such a way as to gratify themselves.

C-Well I think sometimes people think that if they did anything they wanted or even told others what they felt, they would be regarded as weird, abnormal, unreasonable, immature, etc.

E-What do you (S) think?

E-(adding to above remarks when necessary) Well, I think in addition, people don't do what they feel or desire to do in any given instance because

1. they're afraid of the consequences
2. they would feel embarrassed or ashamed if others really knew what was on their minds
3. they feel guilty about being themselves

Now my personal belief is that we all have such feelings and desires at times, but since we are generally too afraid to express them we never learn that other people may have similar needs, feelings and desires.

*We will need an initial estimate of your mood stability and changeability before we go into the lab for our experiment. On this sheet, you will find words which describe different kinds of moods and feelings. Please put an X in the boxes besides those words which describe how you feel now and during the last few (>) minutes. Some of the words may sound alike but we want you to check all the words that describe your feelings. Work rapidly.

What we hope will happen during the experimental phase of this study when you go into the lab is that you will be able to express and act on what you really feel so that we will be able to understand and describe people's gut level feelings in more detail, and so that we can get a better idea of what people's spontaneous emotional reactions would be like if there were no real consequences to their behavior or emotional expressions. (pause) C-When will the experiment start? E-After I've split you up and you've finished the preliminary forms ...in about ½ hour. Anyway..let me repeat that our purpose here is to try to enable you to express what you feel, honestly, whatever it is you feel, when you feel it, no matter how immature or mature, reasonable or unreasonable, appropriate or inappropriate, normal or abnormal, such feelings may be. We are interested here in trying to observe, understand your feelings, not judge them. In this special setting, no harm will come to you by being yourself, in contrast to the outside world where sometimes, an honest expression of feelings can have important consequences. In this study, no such consequences can ensue. For one thing, the data we collect will not have your name attached to them so you need not feel anything that happens will be used to your detriment. For another, no data will be used without your expressed permission at the study's conclusion. Finally, you will have no further contact with the experimenter or with each other once the study is over, unless you so desire. So you need not be concerned about our approval since we are not important people in your respective lives. In short, whatever you do or say, will not have the kind of consequences that people sometimes worry about in real life. In fact, the reality of the situation is that the more you feel free to be yourself and to spontaneously translate your feelings into behavior, the purer the results of this study will be. In sum, you absolutely need not be afraid to do or say anything during the entire conduct of the study, with the proviso that none of us break the law, of course. Your participation in the study will probably represent one of the few times in your life when you can realistically do or say whatever you choose without fear of consequences.

Now my feelings is that each of ^{you} may find it hard at first to adjust to the freedom we will be offering you during the experimental phase of this study, since it is so new and unusual. I therefore think that even before we begin the experiment itself you should feel free to be yourself and to obey your spontaneous feelings, no matter how unusual you may now think your behavior might seem to each of us. Again, let me assure you that whatever you desire to do in the lab or here is fine with me. The only proviso is that no law is to be broken since I cannot control those consequences. If you want to leave at any time, that's fine too. I'll give you your slip and you can go. I hope you will feel free at any given moment from this time on to be yourselves and again let me assure you that no matter what you choose to express or how you choose to express it, there will be no long range consequences extending from this nor will I be making any judgments about your behavior. I think it's important that we allow ourselves to be as free as possible even before the experimental phase begins so we can loosen up a bit, so whatever, and I mean whatever, you want to do or say is OK with me, since it will help us understand people's emotions better, and will help you loosen up for the experiment itself. I think that by testing out your freedom before the experiment begins, you will be better able to use that freedom during the experiment and will be thus able to maximize your contribution to our research.

Now in order to loosen us all up a bit, let's go through some verbal items on this scale I've prepared and we will each give our spontaneous responses. While these are just verbal statements and don't directly involve actual actions these statements will hopefully enable us to loosen up somewhat.

However, in addition, feel free to do anything you want. I'll also try to give my spontaneous answer to each one.

1. I am here now because:
 - a. E-I need to complete this study as part of my job
 - b. C-because my teacher told me to do this
 - c. S-
2. I like psychology because :

(At this point E puts his foot on the couch. C gives him a dirty look)

 - a. E-it helps me understand myself better.
 - b. C-Well, I'm not very interested in psychology and I'm taking it because it's required. I also think this is kind of a dumb question. I mean, assuming everyone is interested in psychology..By the way, I normally wouldn't talk back to a professor but since you said we are free to do what we feel, would you mind moving your foot off the couch before my pants get dirty
E-OK, I'm sorry, I don't want to offend you. I'm glad you told me that though, because as I said, I want you to do what you both feel
 - c. S
3. I like privacy because :
 - a. C-other people often object to your being yourself, like you can't even do something simple like, you know, belch, when you're full and other people are around, because they'd look at you kind of like you were peculiar and maybe even reject you
 - b. E-I work with people all day and I like to be alone at times, and contrary to popular belief, not all psychologists like to bring their work home with them
 - c. S
4. My worst habit is that:
 - a. S
 - b. E-I always forget to take my keys when I leave my house
 - c. C-Well, I don't usually like to talk about it but I guess like you said, you people don't know me and it's not really important what you think-anyway some people, too many for comfort have said that I'm, well, inconsiderate-I don't really think I am though, I just think people can't accept me the way I am
 - d. OK, the last one here, I think it's too bad that :
 - a. C-people always put you down when you do something that's just a little different. Like, if I was passing a girl in my car at a bus stop, and it was snowing, and I offered to give her a lift, she'd probably give me a dirty look for my efforts, even though I was just trying to do her a favor
 - b. E-people are not always open and honest about their feelings toward each other. Incidentally that example you just brought up seems very relevant because I think it illustrates what I was saying before, namely that people are too afraid to be themselves at times, but that while there are consequences to this at times, if they are not themselves it can be self-defeating. In this case, she would get all wet rather than accepting a ride which would have been most uneventful.

Note: For each question, E will respond to C's comments and S's where appropriate by indicating he is glad that they have been spontaneous in their emotional responses. In addition, for certain of C's responses E will

1. be supportive as to their content (3,4,5)
2. indicate that people are different and while he might not like C's motives, he respects C's disclosures (1,4) and C has nothing to fear from all this

- b. E will also try to shape up S's spontaneity by
1. asking open-ended questions if S's responses are constricted
 2. the methods mentioned in a.

E now will smile and say, "OK, I think at this point, we're as loosened up by this exercise as we can be. I think it's important that as I said before, that you both try to adjust to your freedom during this time before the experiment itself begins, and please don't hesitate at any time to do or say anything you want, to me or to each other, I hope by now we all understand the need for this."

Anyway, prior to the experiment I would like to get an idea of what your mood is like, since we will be trying to understand your mood stability or changeability when the experiment begins. To get an idea of your mood, please fill out this scale, and let me know when you're finished. E hands scale and reads instructions below*

OK now before we go into the experimental room for the start of the experiment, as I mentioned before, it's necessary for me to get some biographical information from you. So why don't you both finish the biographical form at least up to here (points). (As they begin working, E announces, "Listen, while you're doing this, I'm going to go to the john - haven't had much chance to go before")

C-do you care if we use pencil or ink

E-either

C-which should we do first

E-the biographical stuff

Please try to complete them. I'll be back in 5-10 minutes so just work on these forms, and get used to relaxing and whatever else you happen to feel like doing, until I return.

5 seconds pass - C aggresses - S responds

Following E's leaving the room, the experimental phase begins. Later, E returns and asks both parties if they have had any trouble with the bio form. A few seconds later he announces: Well, listen, why don't you stop work now on the biographical data and if it's not finished we'll return to it in a few minutes. At this point, we are ready to proceed to the experimental room for the experimental part of the study. (C and S are split up) However, just before we do, I'd like to get one more of these mood forms from each of you, since as mentioned, it's necessary to have them filled out more than once. Both fill out the mood form for the third time and leave the waiting room.

*MOOD INSTRUCTIONS: "On this sheet you will find words which describe different kinds of moods and feelings. Put an X in the boxes besides the words which describe how you feel now, & during the last few (5) minutes. Some of these words may sound alike, but we want you to check all the words that describe your feelings. Work rapidly. (E collects the paper)

APPENDIX E

Sample MAACL Form

Code _____

- 1 active
 2 adventurous
 3 affectionate
 4 afraid
 5 agitated
 6 agreeable
 7 aggressive
 8 alive
 9 alone
 10 amiable
 11 amused
 12 angry
 13 annoyed
 14 awful
 15 bashful
 16 bitter
 17 blue
 18 bored
 19 calm
 20 cautious
 21 cheerful
 22 clean
 23 complaining
 24 contented
 25 contrary
 26 cool
 27 cooperative
 28 practical
 29 prose
 30 cruel
 31 daring
 32 desperate
 33 destroyed
 34 devoted
 35 disagreeable
 36 discontented
 37 discouraged
 38 disgusted
 39 displeased
 40 energetic
 41 enraged
 42 enthusiastic
 43 fearful
 44 fierce
- 45 fit
 46 forlorn
 47 frank
 48 free
 49 friendly
 50 frightened
 51 furious
 52 gay
 53 gentle
 54 glad
 55 gloomy
 56 good
 57 good natured
 58 grim
 59 happy
 60 healthy
 61 hopeless
 62 hostile
 63 impatient
 64 incensed
 65 indignant
 66 inspired
 67 interested
 68 irritated
 69 jealous
 70 joyful
 71 kindly
 72 lonely
 73 lost
 74 loving
 75 low
 76 lusty
 77 mad
 78 mean
 79 weak
 80 merry
 81 wild
 82 miserable
 83 sorrowful
 84 obliging
 85 offended
 86 outraged
 87 penitency
 88 patient
- 89 peaceful
 90 pleased
 91 pleasant
 92 polite
 93 powerful
 94 quiet
 95 reckless
 96 rejected
 97 rough
 98 sad
 99 safe
 100 satisfied
 101 secure
 102 shaky
 103 shy
 104 soothed
 105 steady
 106 stubborn
 107 story
 108 strong
 109 suffering
 110 sulken
 111 sunk
 112 sympathetic
 113 tame
 114 tender
 115 tense
 116 terrible
 117 terrified
 118 thoughtful
 119 timid
 120 tormented
 121 understanding
 122 unhappy
 123 unobtainable
 124 upset
 125 vexed
 126 warm
 127 whole
 128 wild
 129 willful
 130 wilted
 131 worrying
 132 young

APPENDIX F

Mood Scale Calibration Packet - Part 1

MOOD SCALE CALIBRATION INFORMATION

Please answer the questions below as thoroughly, accurately and honestly as you can. Your comments are anonymous and will not be shown to anyone outside the research staff nor will they be used for my data analysis without your expressed permission. Remember, please be honest and candid. If you need more room for a particular answer, you may use the back of the sheet.

Time Period I

I. The following question is with reference to the time period which began when you signed up for the study and ended five minutes after you met the experimenter in person.

1. What fantasies, thoughts, ideas or feelings did you have about the experiment during the time period in question?

Time Period II

II. The following questions refer to the period of time beginning when you met the experimenter for the first period of time and ending right before he asked you for your opinion on a number of topics.

1. We all form initial impressions of others and these can affect our mood. Describe any initial impressions you had of the experimenter when you met him for the first time during the time period in question.
2. Describe what he did, and said, during the time period in question. Also, describe his general manner, demeanor, and conduct during this period. Include any and all aspects of his conduct, in particular, that may have affected your mood during this period.
3. What were your mood and feelings during this period?
4. Are there any other environmental influences or thoughts about them, that you may have had, that significantly affected your mood, in your opinion, besides those covered above. Please describe.

GO TO THE NEXT PAGE *****

Calibration Quotient_____

5. Sometimes in the course of running this study, we ask more than one subject to show up for the preliminary procedure to save time where possible. Was there another subject in the waiting room with you during the preliminary explanation of the study you are to be in?

Circle one: Yes No More than One
If Yes, see below; If No, please go to question 3

If there was another subject in the room with you, please answer the questions below so we can calibrate our scale based on this information. These questions still refer to the period of time beginning when you met the experimenter for the first time and ending right before he asked you for your opinion on a variety of topics.

6. We all form impressions of others and these can affect our mood. Describe any initial impressions you had of the subject(s) during the time period in question.

7. a. Describe what the subject(s) did, and said, during the time period in question. b. Also, describe his general conduct, demeanor, and manner during this period. c. Include any and all aspects of his conduct or demeanor, in particular that may have affected your mood during this period.

a.

b.

c.

8. Describe your mood and feelings during the time period in question.

9. Additional Comments.

GO TO THE NEXT PAGE *****

Calibration Quotient _____

MOOD SCALE CALIBRATION SHEETTime Period III

The following questions pertain to the period of time following the conveying of your opinions to the experimenter, and ending following your filling out of the mood form, for the 2nd time.

1. Were the impressions that you had about the study you signed up for changed from before?
 Circle One: Yes Maybe No
 If you answered Yes or Maybe, describe what your new impressions were during this time period. If you answered No, go to question 2.

2. Were your impressions of the experimenter any different than before?
 Circle One: Yes Maybe No
 If you answered Yes or Maybe, describe what your new impressions were during this time period. If you answered No, go to question 3.

3. Describe what the experimenter did and said during this new time period. Also, describe his general manner, demeanor, and conduct during this period. Include any and all aspects of his conduct that you particularly feel may have affected your mood or caused it to change.

4. Were there any other environmental influences that may have affected your mood during this time period in question or alternately, were there any new thoughts that you had about aspects of the environment that may have affected your mood?
 Circle One: Yes Maybe No
 If Yes or Maybe describe below; if No, go to next page

GO TO THE NEXT PAGE *****

Calibration Quotient _____

5. Was there another subject(s) in the waiting room with you during the time period in question?

Circle One: Yes No More than One

If Yes or More than One see below; if No, please go to question 8.

If there was another subject(s) in the room with you, please answer the questions below so we can calibrate the Mood Form you took during this time interval based on this information. Remember that these questions still refer to the period of time beginning with the discussion of your opinions with the experimenter, and ending following your filling out of the mood form for the 2nd time.

6. Were your impressions of the other subject(s) any different than before?

Circle One: Yes Maybe No

If you answered Yes or Maybe, describe what your impressions of the other subject(s) were during this new time period.

7. a. Describe what the other subject(s) did and said during this new time period. b. Also describe his general conduct, demeanor, and manner during this period. c. Include any and all aspects of his conduct, behavior or demeanor, that you particularly feel may have affected your mood during this new time period.

a.

b.

c.

8. Give a description of your mood and feelings during the time period in question.

9. Additional comments.

GO TO THE NEXT PAGE ****

Calibration Quotient _____

MOOD SCALE CALIBRATION SHEET

Time Period IV- The following questions pertain to the period of time, beginning with your filling out the rest of the Biographical Data Sheet, and ending with your filling out the Mood Form for the 3rd time.

1. Were the impressions that you had about the study you signed up for changed from before?

Circle One: Yes Maybe No

If you answered Yes, or Maybe, describe what your new impressions were during this new time period. If you answered No, go to question 2.

2. Were your impressions of the experimenter any different than before?

Circle One: Yes Maybe No

If you answered Yes or Maybe, please describe what your new impressions were during this new time period. If you answered No, go to question 3.

3. Describe what the experimenter did and said during this new time period. Also, describe his general manner, demeanor, and conduct during this period. Include any and all aspects of his conduct that you particularly feel may have affected your mood or caused it to change, during this new time period.

4. Were there any other environmental influences that may have affected your mood during the time period in question, or alternately, were there any new thoughts that you had about aspects of the environment that may have affected your mood, during this new time period?

Circle One; Yes Maybe No

If Yes or Maybe, describe below; if No, go to next page

GO TO THE NEXT PAGE *****

Calibration Quotient _____

Code _____

5. Was there another subject(s) in the waiting room with you during the new time period in question?
 Circle One: Yes No More than One
 If Yes or More than One see below; if No, please go to question 8. If there was another subject(s) in the room with you, please answer the questions below so we can calibrate the 3rd mood form that you took, based on this information. Remember that these questions refer to the period of time which began with the filling out of the rest of the Biographical Information Sheet and ending with your filling out the Mood Form for the 3rd time.
6. Were your impressions of the other subject(s) any different than before?
 Circle One: Yes Maybe No
 If you answered Yes or Maybe, describe what your impressions of the other subject(s) were during this new time period.
7. Describe what the other subject(s) did and said during this new time period. b. Also describe his general conduct, demeanor, and manner during this period. c. Include any and all aspects of his conduct, behavior or demeanor, that you particularly feel may have affected your mood during this new time period.
- a.
- b.
- c.
8. Give a description of your mood and feelings during this new time period.
9. Additional Comments.

GO TO THE NEXT PAGE*****

Calibration Quotient _____

APPENDIX G

Mood Scale Calibration Packet - Part 2

Self Report Scales

EXPERIMENTAL RATING SCALE FOR MOOD SCALE CALIBRATION

We have devised the following rating scales for you to fill out. In future studies we may use the following rating in place of the written questions in calibrating our Mood Scale, depending on the results of the preliminary use of them. To help us evaluate them, would you please fill out these scales, with reference to the same time period already covered in your previous comments, that is, the period or time starting when you finished the rest of the Biographical Data Sheet and ending with the 3rd Mood Form.

General Instructions: For each scale a set of instructions appears in the instruction sheets following the scales themselves. Before you use a scale, look at it, then read the instructions pertaining to the scale, then complete the scale itself by circling the mark on the scale corresponding to your choice. If you are unsure about how to proceed at any point, please ask the assistant (or me) for help.

INSTRUCTIONS - SCALE 1

One important dimension along which actions differ is Aggressiveness-Constructiveness (Harshbarger). On the first scale I would like you to indicate how Constructive or Aggressive you think the behavior of the party(s) in question was to you. An action is considered to be Constructive to the extent that it helps or potentially helps people or contributes to their overall well being, physical or psychological. Constructive actions help people maintain or secure valued states of mind, valued actions, valued assets, valued feelings, valued objects, etc. Aggressive actions are those that destroy, harm or threaten to harm something physical or psychological, tangible or intangible, that is valued by other people. Aggressive actions harm people's valued states of mind, valued actions, valued assets, valued feelings, valued objects etc. and make them feel unpleasant or unhappy. For example, if an action resulted in someone's loss of pride, self respect, security, etc., or resulted in lost happiness, peace of mind, a lost job, lost values, etc. it would be considered to be an aggressive act. On the other hand, if a person's actions tended to make other people feel happy, valued, secure, proud, wealthy, beautiful, popular, etc. they are defined as constructive actions. You should consider Constructiveness and Aggressiveness as antonyms or opposites; the more Constructive an action is the less Aggressive it is and vice versa.

Please note the following:

1. Do not consider the intensity of an action as aggressiveness per se (as in the expression-"an aggressive salesman"). This is because an action can be intensely constructive too.
2. You should focus on the effects of the actions on you, and not on the intentions or the motivations of the person who is doing them. This is because harmful actions can be performed without any harmful intent. On the other hand, do not hesitate to rate an action as Aggressive if a harmful intent is obviously also present. The important thing is to evaluate the action's impact on you and to concentrate on them.
3. Do not hesitate to rate a harmful or helpful intent as aggressive or helpful to you in its consequences if you felt it was.

Make your ratings on the scale provided and mark your choice by circling your chosen response on the answer sheet. Look at the Scale. The left hand side is defined by the synonyms given at the top left of the page. The right hand side is defined by the adjectives and synonyms at the top right of the page. Mark any actions that you think are Aggressive on the left side; the more Aggressive the action is, the more extreme rating it should receive. Similarly, you should use the right side of the scale for items you think are Constructive. The more Constructive an action was the more extreme rating it should receive.

Below is an example of the typical ratings of six items. You should use these items as anchor points in making your decisions.

- Item 1. a person pushes you intentionally & forcefully or pushes someone else.
2. a person cruelly insults you or someone else.
3. a person walks near you or someone else to get an ash tray.
4. a person opens a book.
5. a person lavishly compliments you or someone else and the compliments appear sincere (it's not important if they are)
6. a person offers you or someone else a ride home in the rain-having known that the individual in question desperately needs one.

Item	Agg.	9	8	7	6	5	4	3	2	1	Const.
		Ext. Strong	Strong	Mod	Mild	Neut	Mild	Mod	Strong	Ext	Strong
1		X									
2		X									
3						X					
4						X					
5											X
6											X

Please note that the extreme actions listed above don't involve a great deal of physical harm or help. Most of the harm or help is psychological and intangible. Therefore, since the anchors are not too extreme to begin with in an absolute sense, be very careful in the distinctions you make in making your rating. Also, please note the adjectives on the scale are relative to the anchor items and not to some other abstract standard, and are equally spaced.

Note too that if an action seems much stronger than the extreme examples you may indicate this by putting +s next to the rating. Each + you use represents one numerical extension of the ratings, that is, a 9++ is scored at an 11, a 1++ as two scale units below one.

Now you complete your ratings on the rating sheet for Scale 1

SCALE 2 - INSTRUCTIONS

On the second scale I would like you to indicate how "Appropriate" or "Inappropriate" you think the behavior of the other party(s) was. An action is considered Appropriate if it is the kind of action that you expect of a normal person in our society, in the present circumstances. These are actions that could be described as normal, rational, reasonable, justifiable, understandable, natural, typical, everyday, etc., and are carried out by the normal people in our society. An action is called inappropriate if it is the kind of action that is not expected of a normal person in our society, by the other members of society. These actions could be described as abnormal, irrational, unreasonable, not justifiable, weird, crazy, deviant, sick, and possibly even bizarre in their extreme. The person carrying out such actions might be thought to be irrational, unreasonable, abnormal, weird or crazy. You should consider Appropriate and Inappropriate as antonyms, that is, the more Appropriate an action is, the less Inappropriate it is and visa versa. Please note the following:

1. Don't base your decisions of how Appropriate or Inappropriate an action is on how polite or impolite it is, per se. Actions may be rather impolite and yet be considered Appropriate to some degree (for example not saying thank you). On the other hand, actions may be both impolite and inappropriate (saying go to hell when someone offers you two tickets to a rock concert). Similarly, some actions that are polite may be Appropriate while other polite actions may be Inappropriate (saying thank you when no one has done anything for you). In short, you should concentrate on the Appropriateness of an action or its Inappropriateness and not on its politeness, per se.
2. For similar reasons, do not consider an action Inappropriate just because it is immoral or illegal or bad, per se, since many of these actions are considered normal and Appropriate. Again, concentrate on the Appropriateness or Inappropriateness of the action, and not its immorality or illegality.
3. Do consider the circumstances under which the actions occurred, their context, & the relationship between you and the other party(s) in making your ratings. This is because an action might be considered appropriate in one context, but not another. Make your judgments on each action on the scale provided. Be sure you also follow the preliminary instructions as you proceed. Mark your choice of response by circling the chosen response on the Answer Sheet. The left hand side is defined by the synonyms given at the top left of the page. The right hand side is defined by the adjectives and synonyms at the top right of the page. Mark any actions that you see as Appropriate on the left side; and the more Appropriate an action is, the more extreme the rating it should receive. Similarly, you should use the right side of the Answer Sheet for the items you see as Inappropriate. The more Inappropriate an action is, the more extreme the rating it should receive.

Below is an example of the rating of three items given the circumstances existing in the last time period.

It.	<u>App.</u>	9	8	7	6	5	4	3	2	1	<u>Inapp.</u>
		Extreme	Strong	Moderate	Mild	Neut	Mild	Moderate	Strong	Extreme	
1										X	
2						X					
3			X								

Please note that if an action seems much stronger than Extremely Strong, you may indicate this by putting ++ next to your rating. Remember each + you use represents one numerical extension of the scale ratings, that is, a 9++ will be scored as 11.

Now complete Scale 2.

SCALE 3 - INSTRUCTIONS

People sometimes wish to or desire to do helpful, Constructive things for other people, but for a variety of reasons don't always do them. As defined before, a Constructive act is one that helps a person achieve or maintain valued states which may be either tangible (ex. money) or intangible (pride, self-esteem, security, peace of mind.)

You may have had a specific desire to behave in a given Constructive way, toward the other subject(s) during the last time interval. Alternatively, you may not have had a specific action in mind but may have just felt a desire to be helpful and constructive. In either case I would like you to indicate just how helpful or Constructive you desired to be to the other subject(s) during the last time interval.

Please note:

1. We are interested in knowing how helpful or Constructive, you really felt like or really desired to behave like during the last time interval, and not what you actually may or may not have done.
2. We are interested in knowing how you really felt like acting, not necessarily in your feeling of liking for the other person.

Look at the Scale. Scale point 5 is labelled "Neutral"-Did not desire to be helpful or Constructive. Scale point 9 is labelled "Desired to be Extremely Helpful and Constructive"- desired to flatter greatly or do a great favor (giving a lift home for example. All other adjectives take this point as their reference and are equally spaced.

If what you really wanted to do was less Constructive than #9, circle the line corresponding to the appropriate degree of Constructiveness. If what you really wanted to do was more Extreme than #9, circle some point beyond 9. For example, if you felt you would have really liked to have helped the other person with his homework for the rest of the semester, and you felt this to be helpful and Constructive, you would probably circle some point greater than 9. Now complete your rating on Scale 3.

Scale 4. People sometimes wish to do harmful or Aggressive things to other people, but for a variety of reasons don't always do them. A harmful or Aggressive act is one which destroys, or even threatens to destroy valued states which may be tangible (one's car) or intangible (one's pride, self-esteem, security, peace of mind, etc.)

You may have had a specific desire to behave aggressively during the last time period or you may have just had a general desire to behave in an aggressive or harmful manner.*

Please note:

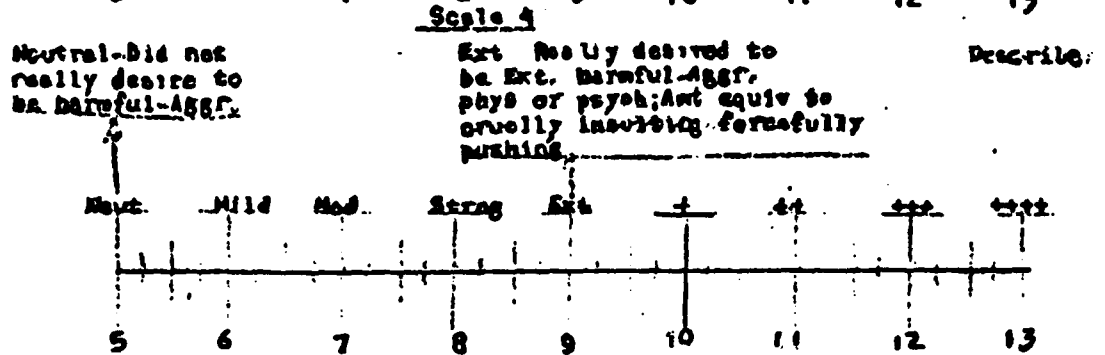
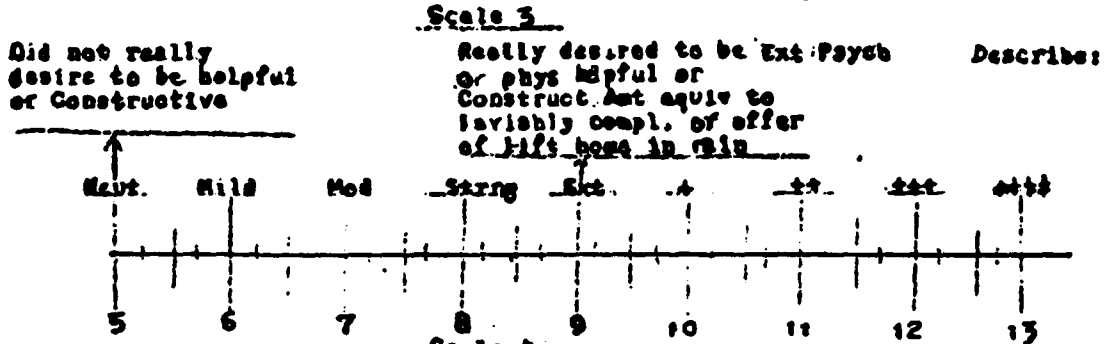
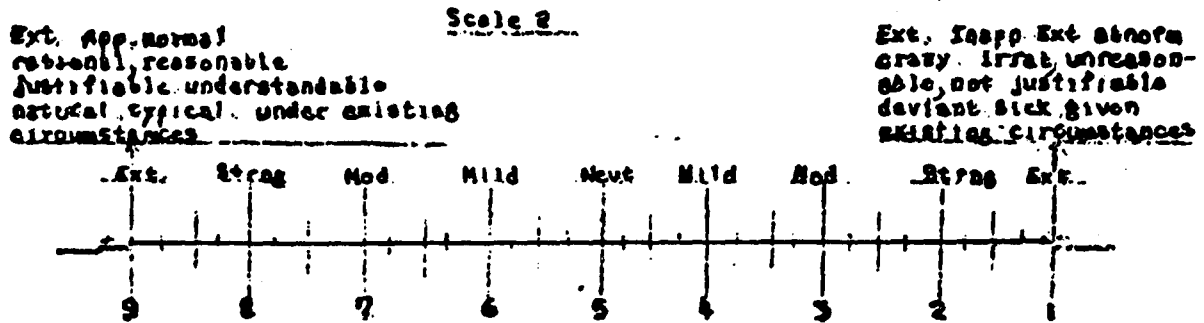
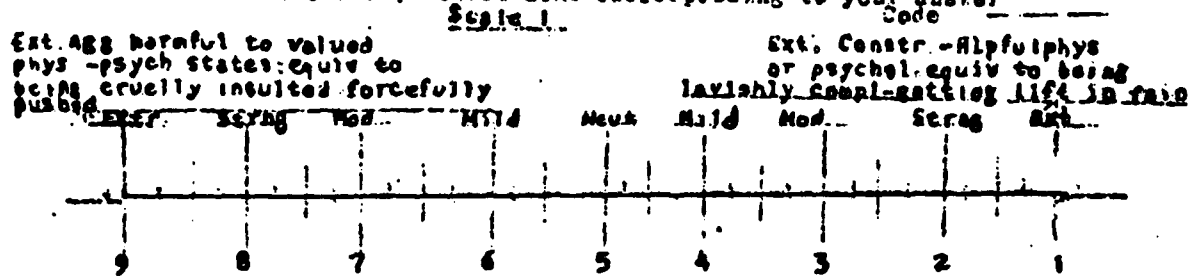
1. We want to know how aggressive or harmful you really felt like or desired to behave like during the last time interval, and not what you actually may or may not have behaved like. Also, focus on what you really felt like doing, and not necessarily on your feelings of dislike for the other person(s) if any.

Look at Scale 4. Scale point 5 is labelled Neutral-Did not wish to Aggress or be harmful. Scale point 9 is labelled "Desired to be Extremely Harmful or Aggressive"-desired to cruelly insult him or push him intentionally or forcefully.

If what you desired to do was less harmful or aggressive than #9, circle the line corresponding to the appropriate magnitude. Note that the other adjectives use the Extreme anchors as their reference point. If what you really felt like doing was more Extreme than 9, circle some point beyond 9. For example, if you had really wanted to fracture his jaw, some rating beyond 9 would be appropriate. Now complete Scale 4.

*Note: as a direct result of his actions during that period.

Instructions Circle the vertical line corresponding to your answer



Scale 5 - Instructions: Rate the degree to which you felt either Angry, or Happy as a direct result of the other party(s) behavior. The most extreme end of this scale is Extreme Anger. Lesser values might be termed annoyance, irritation, animosity, dislike etc. The other end of this scale is Happy and Gleeeful. Less extreme values are pleased, good, comforted, receptive, warm, etc. Please note that we are interested in your feelings and not necessarily in your actions, or your desires to take action. Focus on your feelings at the time, in making these ratings. Now you rate your feelings on Scale 5.

Scale 6 - Instructions: People often are reluctant to respond the way they really feel, or desire to, in a particular situation, because they anticipate unwanted external consequences of an Aggressive or harmful nature if they do (ostracism, a dirty look, a punch in the nose, a misinterpretation of their behavior, etc.). Rate the amount of Aggression or harm that you anticipate the other subject(s) would respond with, should you do what you really would have liked to do or felt like doing during the last time period.* Remember, we have defined Aggression as anything that is harmful physically or psychologically. Reread the Scale 1 definition if you have questions about this or ask the interviewer.

Look at Scale 6. Scale Point 5 is labelled Neutral-no future Aggressive consequences were anticipated. Scale Point 9 is labelled Extreme-Aggressive. Consequences were anticipated-anticipated being cruelly insulted or forcefully pushed.

If what you anticipated was less harmful or Aggressive than #9, circle the line corresponding to the appropriate magnitude. Note that the other adjectives use the Extreme anchor as their reference point and are equally spaced

If what you anticipated was more harmful or Aggressive than #9, circle the line corresponding to the magnitude beyond point 9 that you anticipated. If you anticipated a fractured jaw for example, some point beyond point 9 would be appropriate. Now rate your anticipation on this scale.

Scale 7 - Instructions: Rate the amount of trouble you anticipated you would be likely to get into with the experimenter and/or the school authorities if you did what you really wanted to during the last time interval. Trouble is defined as physical or psychological harm you anticipated as forthcoming.

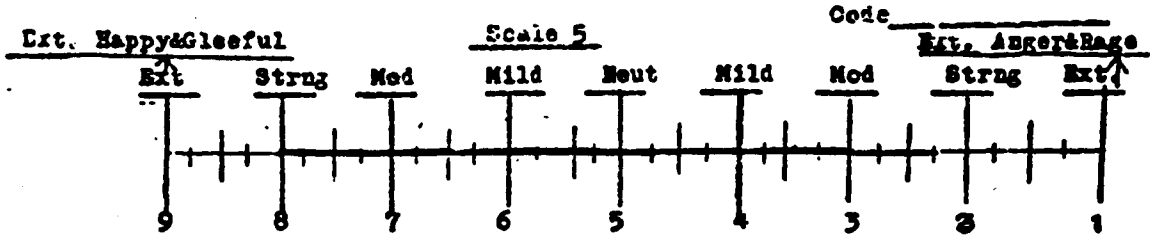
1. Please base your rating on your actual anticipation or what you think your anticipations may have been if any, of such harm during the last time period.
2. Do not rate the amount of harm you potentially could suffer at their hands. Rate the amount of harm, you felt likely to suffer, given all the circumstances and aspects of the situation in which you found yourself during the last time period, if you did what you really wanted to do to or with the other subject(s).*

Look at Scale 7. Scale point 5 is labelled Neutral-no harm or trouble anticipated. Scale point 9 is labelled Extreme Harm or Trouble anticipated-likely I would be cruelly insulted or pushed by the experimenter. If what you anticipated was less harmful than #9 circle the proper scale point corresponding to the magnitude of the harm involved in the trouble you would be in. Note that the other adjectives use the Extreme anchor as their reference point and are equally spaced. If what you anticipated was more harmful than #9 circle the appropriate point beyond #9. If you anticipated a likely suspension from school if you did what you really wanted, you would probably pick a point beyond #9. Now you complete Scale 7.

* as previously indicated

Scale 8 - Instructions: Rate the magnitude of internal Agression or internally produced harm (such as guilt, fear, sorrow, shame, anxiety, embarrassment, etc.) if any, that you would have likely experienced, if you ****** did what you really desired to do during the last time period, even if neither the subject nor the authorities cared if you did it. Look at Scale 8. Scale point 9 is labelled Extreme Internal harm-as harmful as being cruelly insulted or forcefully pushed by someone else. All adjectives are with reference to #9, and are equally spaced. If you would experience internal harm that you feel is less harmful than the amount of harm in #9 circle a point between 5 and 9. If you feel for example, you would hate yourself or be antious for a month, a scale rating more than #9 might be appropriate. Now rate Scale 8.

* As previously indicated

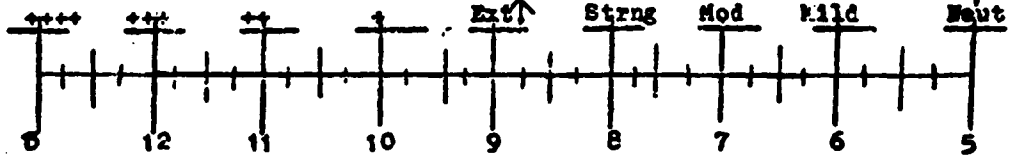


Describe:

Scale 6

Extreme Agg consequences anticipated from S
Ext ant of harm equiv to being cruelly insulted or forcefully pushed

Neutral- No future Aggressive or Harmful consequences anticipated

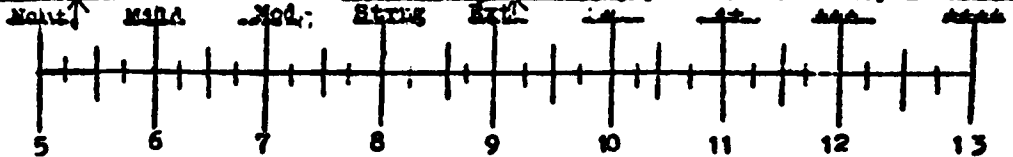


Scale 7

Describe:

Neutral-anticip. no harm, Aggr., or trouble, to be forthcoming from authorities if I did what I really wanted, given the circumstances

Extr. anticip. ext harm, Aggr., or trouble likely to be forthcoming from authorit. if I did what I wanted, given the circumstances. equiv to bng pushed or cruelly insulted

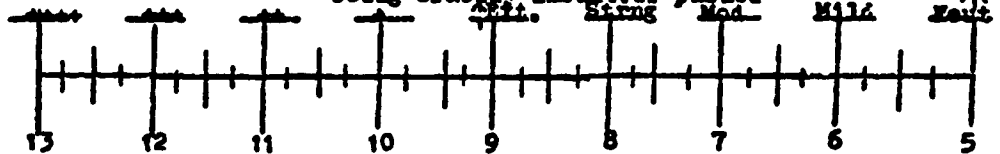


Scale 8

Describe:

Ext I would have likly exp. Ext. internal harm if I did what I really wanted given the circum, even if no one else cared-equiv to being cruelly insult. or pushed

Neut I would have likely experienced no internal harm if I did what I really wants under these circum.



Comments:

APPENDIX H

Mood Scale Calibration Packet - Part 3

Self Reported Suspicion Data, etc.

Code _____

At this point, let me inform you that more than one subject was asked to report to the waiting room for the preliminary procedure. Part of the reason this was done was to expedite matters. However, we also wanted you to rate each other's behavior to get mutual measures of each other's moods, to see if this is an effective way to calibrate the mood scale, and to see if your mood was expressed in any way through your behavior so that another person might recognize it. Please answer the questions below with reference to the last time period, that is, the one beginning with your completion of the Biographical Data Sheet and ending with the 3rd Mood form.

1. Did your behavior, during this time period, reflect the full intensity of your feeling?
 Circle One: Yes No Not Sure
 If No or Not Sure, how intense would your behavior have been if it had mirrored the intenseness of your feeling?

2. Did your behavior reflect the actual content of your feelings, that is, did you carry out the ideal activity you would have like to carry out in the situation?
 Circle One: Yes No Not Sure
 If No or Not Sure, what would you have like to do and why? Also, why did you not do it? (Be specific, detailed)

3. Would you have behaved the same way if you had not received the preliminary instructions for the experiment you will be in?
 Circle One: Yes No Maybe
 If No or Maybe, how might your behavior have been different during this time period?

4. Would you have behaved the same way if you were not waiting to participate in a psychology study?
 Circle One: Yes No Maybe
 If No or Maybe, how might your behavior have been different during this time period?

5. Would you have behaved differently if you were with a different experimenter or subject during this time period?

Circle One: Yes No Maybe

If No or Maybe, how might your behavior have been different?

6. Did you feel that the other subject(s) (Circle One)
1. did not behave in a way that affected your mood, one way or the other
 2. did behave in a way that affected your mood, but probably did not realize he had performed the behavior in question, as he was not concentrating on it.
 3. did behave in a way that affected your mood, and was aware of his behavior, but probably felt it would not have harmful consequences to you and/or could not help himself.
 4. behaved in a way that affected your mood, was consciously aware of this behavior, but did not care if it harmed you or not.
 5. behaved in a way that affected your mood, when consciously aware of this behavior, and hoped it would harm you.
 6. None of the above; please specify _____
-

STOP: DO NOT TURN PAGE UNTIL REQUESTED TO DO SO.

Calibration Quotient _____

We have found through preliminary testing that some people have suspicions and fears which can affect their mood. To help us deal with this source of mood variability please complete the following information, honestly.

1. Do you feel you have been and will be open and honest in the expression of your desires, opinions and feelings in answering the questions?
 Circle One: Yes No Maybe
 If No or Maybe indicate why not.

2. Have you ever been down to the speech and education clinic or have you taken any advanced education courses? If Yes, describe.

Answer the following with reference to your previous suspicions if any.

3. Were you, during the last time period we questioned you about, suspicious that the mirrors in this clinic are special 1-way mirrors?
 Circle One: Yes No Maybe

4. Were you at that time suspicious that you were being observed?
 Circle One: Yes No Maybe
 If Yes or Maybe, answer the following, if No go to question 8.

5. Rate how suspicious you were at that time.
 1 2 3 4 5
 Mild Moderate Strong Extreme Convinced

6. On what were your suspicions based?

7. Why did you think I might want to observe you?

8. Were you during the time period in question suspicious that we really were studying the behavior of the other subject and yourself during that time period and that the experimenter left the room on purpose?
 Circle One: Yes No Maybe
 If Yes, answer questions below, if No, go to question 12.

Calibration Quotient _____

9. Rate your degree of suspicion at the time.
- | | | | | |
|------|----------|--------|---------|-----------|
| 1 | 2 | 3 | 4 | 5 |
| Mild | Moderate | Strong | Extreme | Convinced |
10. On what were these suspicions based?
11. At the time, why did you think we wanted to study subject(s)' behavior during this time period?
12. During this period were you suspicious the other subject was really an actor paid by the experimenter?
 Circle One: Yes No Maybe
 If Yes or Maybe, answer below. If No, go to 16.
13. Rate how suspicious you were at the time.
- | | | | | |
|------|----------|--------|---------|-----------|
| 1 | 2 | 3 | 4 | 5 |
| Mild | Moderate | Strong | Extreme | Convinced |
14. On what were these suspicions based?
15. Why did you suspect we hired an actor?
16. Misc. Comments

Please complete the first Biographical Data Sheet and stop. Wait for Instructions.

Calibration Quotient _____

APPENDIX I

Biographical Questionnaire

First two pages used in experimental procedure; last two given at end of debriefing.

Code _____

BIOGRAPHICAL QUESTIONNAIRE

Age _____

Sex _____

Race _____

Height _____

Weight _____

My three main hobbies, interests, or favorite activities _____

Do you have cold, allergies - if yes, please specify _____

ulcers, high blood pressure, heart disease of any kind - please

specify _____ eye condition - please specify _____.

cancer - please specify _____ asthma, diabetes, infections-

please specify _____, anemia respiratory ailment or lung

disease, epilepsy, ear impairment, dermatitis, kidney disease,

migrain headaches, brain damage, liver difficulties, fainting

spells or fits, taking medication - please specify _____,

genito-urinary condition

Class and Curriculum Number _____ Occupation if FT _____

Major (probable) _____ Career goal _____

Number of siblings _____

Mother's Height _____ Father's Height _____

Mother's approx. age _____

Father's approx. age _____

Mother's occupation _____

Father's occupation _____

My parents live together, are separated, divorced, only my father
is living, only my mother is living.

I was raised until my fifth year by my mother, my father, both parents,

other _____.

Father's political beliefs liberal, middle of the road, conservative.

Mother's political beliefs liberal, middle of the road, conservative.

Code _____

Father's Social Beliefs (attitudes towards sexual permissiveness, maintenance of sex roles, drug usage, permissive child rearing, alternate life styles, etc.)

() liberal () middle of the road () conservative

Mother's Social Beliefs

() liberal () middle of the road () conservative

Your Social Beliefs

() liberal () middle of the road () conservative

Father's voting preference in major U.S. elections (Senate, House and Presidential only). Votes for members of the given party in these elections at least 3/4 of the time.

() Republican () Democrat () Liberal () Conservative

() Other () No party preference

Mother's voting preference

() Republican () Democrat () Liberal () Conservative

() Other () No party preference

Your voting preference

() Republican () Democrat () Liberal () Conservative

() Other () No party preference

Father's voting preference for local office (City Council, Mayor, State Senate, Assembly, Judges, etc.)

() Republican () Democrat () Liberal () Conservative

() Other () No party preference

Mother's voting preference for local office

() Republican () Democrat () Liberal () Conservative

() Other () No party preference

Your voting preference for local office

() Republican () Democrat () Liberal () Conservative

() Other () No party preference

Religious Preference: For each question indicate:

1. the answer that applies to you by checking the "me" column.
2. the answer that applies to your mother by checking the "mother" column.
3. the answer that applies to your father by checking the "father" column.

MeMotherFatherPresent religion

Catholic (list type)

Protestant (list type)

Jewish (list type)

Agnostic-Atheist

Other (list type)

Frequency of worship (of each above)

1. Every day
2. Almost every day
3. Twice a week
4. Once a week
5. Only on holidays
6. Once a year
7. Never

Code _____

Religious Preference (continued) Me Mother Father

Compared to other members of the general faith (all Jews, Protestants, etc.) indicate the strength of religious conviction for each family member.

1. Devoutly religious - follows :
 almost all religious customs
 God plays a great role in
 life, second to none
2. Strongly religious
3. Moderately religious,
 believes in God, follows
 some religious customs,
 generally considered to
 be a good _____.
4. Slightly religious
5. Atheist or agnostic

Ethnic Background (check all that apply)

- 1) Puerto Rican 2) Italian 3) Jewish 4) Chinese 5) Irish 6) Polish 7) German
- 8) Russian 9) Israeli 10) Greek 11) American Indian 12) WASP 13) Other Oriental
 (please specify _____ 14) Arabic (please specify) _____ 15) African
 (please specify _____ 16) South American (please specify) _____ 17) West
 Indies (please specify) _____ 18) East European (please specify) _____
- 19) Other (please specify) _____ 20) None of the above (please specify)
 _____.

For each family member listed below,

1. Give the approximate percentage of each person's life that he has resided in the fifty states.
2. Give country of birth of each.
3. Give the name of the chief country or territory where the remainder of that individual's life was spent.

<u>Family Member</u>	<u>U.S.</u>	<u>Birthplace</u>	<u>Remainder</u>
You			
Mother			
Father			
Mother's mother			
Mother's father			
Father's mother			
Father's father			
Mother's grandparents (if known)			
Father's grandparents (if known)			

Combined Parents' Gross Income _____

Since my fifth year, I have 1) lived with my parents or guardian 2) lived with my parents until I was _____ years old and am not now living with them 3) lived with my parents or guardian except for all the time intervals greater than one year duration listed below (list your age when separation began and age when it ended if it did) - (also specify nature of separation)

1. _____
2. _____
3. _____
4. _____

Siblings Information: For each brother and sister, list name, age, age difference from you, sex, height, weight, present grade level (eighth, eleventh, sophomore, second year Master's, etc.) or last grade level achieved, if not in school, major in college, occupation if working.

	Name	Age	Age Diff.	Sex	Ht.	Wt.	Gr. Lev.	Maj.	Occup.	Comments
1.	_____	_____	_____	_____	_____	_____	_____	_____	_____	_____
2.	_____	_____	_____	_____	_____	_____	_____	_____	_____	_____
3.	_____	_____	_____	_____	_____	_____	_____	_____	_____	_____
4.	_____	_____	_____	_____	_____	_____	_____	_____	_____	_____
5.	_____	_____	_____	_____	_____	_____	_____	_____	_____	_____
6.	_____	_____	_____	_____	_____	_____	_____	_____	_____	_____
7.	_____	_____	_____	_____	_____	_____	_____	_____	_____	_____
8.	_____	_____	_____	_____	_____	_____	_____	_____	_____	_____
9.	_____	_____	_____	_____	_____	_____	_____	_____	_____	_____
10.	_____	_____	_____	_____	_____	_____	_____	_____	_____	_____

For each numbered sibling listed above, check which of the following periods of your life that sibling was not living with you.

Your age	Numbered Sibling									
	1	2	3	4	5	6	7	8	9	10
0-2										
3-5										
6-8										
9-11										
12-14										
15-17										
18-20										
21-23										
24-26										

Cigarette Smoking: Indicate your frequency of smoking cigarettes during the past year. a) did not smoke at all; b) one to five cigarettes a week; c) a pack a week to two packs; d) three to four packs; e) five to six packs; f) seven to eight packs; g) more than eight packs a week.

My three main sources of unhappiness in the last six months have been

1. _____
2. _____
3. _____

My three main sources of happiness in the last six months have been

1. _____
2. _____
3. _____

APPENDIX J

Materials used in videotape judging procedure.

PRELIMINARY INSTRUCTIONS

We are currently obtaining volunteer unpaid subjects for future participation in an ongoing lab research project which will require us to spend a great deal of effort and time on each subject. It is therefore highly important that we are able to choose the appropriate subjects for our work before we begin research procedures with them.

To obtain volunteers with the appropriate characteristics and background for our project, we asked many people to come to our facilities to fill out a biographical background questionnaire. Besides the biographical data on each prospective volunteer, we also wanted to get an idea of what each person's natural behavior was like under neutral, non-experimental conditions, when the researchers were not around. To do this, as part of our screening process we left the volunteers alone in pairs while they filled out the biographical form, and a video-tape was secretly made of their behavior through a hidden camera.

Now, in order to objectively measure each volunteer's behavior compared to all those available for our research, we'd like you to aid us by rating his video-taped behavior along the scale(s) you have been given. Since we had so many video-tapes, we can't show all of them to you; other classes will view the rest.

Please note that there was almost always more than one potential volunteer present during any given video-tape segment. Since we only want you to rate the behavior of one volunteer during any given segment, we have masked half the TV screen. To ensure the validity of your ratings, it is essential that you ignore the verbal and non-verbal behavior of the masked individual in making your ratings. Try your best to judge the unmasked individual's behavior, verbal and non-verbal, as if he were the only person you could see and hear. This way, we can get independent ratings for all participants. Other classes, will rate the masked individuals on the same scales. Ideally we would have edited the tapes so only one volunteer appeared but this was technically unfeasible. Instead we must rely on you to remain uninfluenced by the masked person's behavior or statements.

Please note too, that some words or actions on the video-tape may be hard to make out. If you feel this might affect your rating of a particular volunteer, ask us what was actually said or done and we'll clarify it for you.

If you have any questions, please ask. If not, read your scale instructions and look up when you're ready to begin viewing.

SCALE 1 - AGGRESSIVENESS - CONSTRUCTIVENESS

We would like you to indicate how Constructive or Aggressive you think the actions of each unmasked individual would be to an average person. We are not necessarily referring to how Aggressive-Harmful or Constructive-Helpful it is to the individual whose face is masked but rather how Aggressive-Constructive it would be to any person. Actions that are generally considered Constructive are actions that in general, help or potentially help other people or contribute to their overall well being, physical or psychological. A Constructive action might be one that in general, helps others maintain or secure something they value such as a valued state of mind, a valued asset, a valued object, or valued feelings such as happiness, security, etc. Actions that are considered Aggressive are those that, in general, destroy, harm, or threaten to harm something physical or psychological that is valued by others, such as a valued state of mind, a valued asset, a valued object, or valued feelings such as happiness, security, etc. For example, if an action is generally considered to result in another person's loss of pride, self respect, security, etc., or resulted in lost happiness, peace of mind, a lost job, lost values, etc., it would be considered an Aggressive act. On the other hand, if a person's actions tended to make other people feel happy, valued, secure, proud, wealthy, beautiful, etc., they are defined as Constructive.

Please note that the key issue is whether the action harms or helps people as described above. Other definitions that you may have learned for the words Aggressive or Constructive should be ignored. For example, some people use the phrase "he's an aggressive salesman". They are using the term in a different way from that meant above. Concentrate on the definitions listed above in making your ratings.

Also, please note that most of the behavior you will be seeing does not involve a great deal of physical harm or help. In fact most of the harm or help is psychological in nature. Therefore, in constructing the scale you will be using for your ratings, we have given you examples of actions that we feel are probably more extreme one way or the other, than those you will be encountering. Use these actions as your reference points in making your ratings. We are arbitrarily defining them as Extreme. Therefore, any "less than Extreme" rating should be made with reference to these actions and not to some other standard of Extreme (like murder) that you may have usually held.

Look at the scale below. One side is labelled Extremely Aggressive - equivalent in harm to someone cruelly insulting someone or forcefully pushing them. This point has been given a numerical value of 0. The other end is labelled Extremely Constructive - equivalent to someone lavishly and sincerely complimenting someone or offering someone a lift home in the rain when he really needs one. This point has been given a numerical value equal to 8. In between these two points there are lesser levels of Aggression or Constructiveness. The middle point (4) is for actions that are judged to be neither Aggressive nor Constructive. You may pick any point for your rating and enter the numerical value on your IBM sheet as directed. Remember though, that all ratings are with reference to the two opposite poles that we have defined through actions we are calling Extreme.

REPLICA OF ONE BOX TO BE MARKED ON THE IBM SHEET

0
1
2
3
4
5
6
7
8
9
1

Extremely Aggressive: This action involves the same amount of harm as cruelly insulting someone or forcefully pushing him.
Strongly Aggressive.

Moderately Aggressive.

Mildly Aggressive.

Neutral Midpoint.

Mildly Constructive.

Moderately Constructive.

Strongly Constructive.

Extremely Constructive: This action involves the same amount of warmth, and friendliness as someone lavishly or sincerely complimenting someone or offering him a lift home in the rain when he really needs one

DO NOT USE RATING 9: for experimenter's use only

This number represents the sequence number of box on IBM sheet. If you would like to indicate an extreme either more than 0 at one end or farther out than 8 at the other, mark in 0 or 8 and then add a + mark or +s (indicating number of more extreme units you would give it) next to the sequence number 1. For example: If "0" were marked in, this would mean two units farther out than 0 - Extremely Aggressive as described above.

Scale 2 - InstructionsAppropriate-Normal --- Inappropriate-Abnormal

We would like you to indicate how "Appropriate and Normal" or how Inappropriate and Abnormal you think the actions of each unmasked individual are. "Appropriate Normal" actions could be defined as actions that are normal, rational, reasonable, justifiable, understandable, natural, typical, and everyday, etc., which are carried out in general, by the "normal" people in our society. An action is called "Inappropriate and Abnormal" when it could be described as abnormal, irrational, unreasonable, not justifiable, weird, crazy, deviant, sick, and possibly even bizarre in the extreme. People who act this way are thought to be "irrational, unreasonable, abnormal, weird or crazy".

Please note that we are not talking about actions that are merely impolite, immoral or illegal, since sometimes some of them may be considered fairly appropriate, for example, not saying thank you if someone holds the door for you.

Look at the scale below. One side is labelled Extremely Appropriate and has been given the number 0. The other side is labelled Extremely Inappropriate and Abnormal and has been given the value 8. Actions may be rated anywhere along this scale and there are lesser equally spaced values and numbers for this purpose. Pick any point for your rating in each case and enter the numerical value of it on your IBM sheet as directed. You will make one rating for each segment.

Replica of One Box to be Marked on the IBM Sheet

. 0 .	→	Extremely Appropriate, Normal.
. 1 .	→	Strongly Appropriate, Normal.
. 2 .	→	Moderately Appropriate, Normal.
. 3 .	→	Mildly Appropriate, Normal.
. 4 .	→	Neutral Midpoint.
. 5 .	→	Mildly Inappropriate, Abnormal.
. 6 .	→	Moderately Inappropriate, Abnormal.
. 7 .	→	Strongly Inappropriate, Abnormal.
. 8 .	→	Extremely Inappropriate, Abnormal.
. 9 .	→	DO NOT USE RATING 9: For experimenter's use only.
1		This number represents the sequence number of box on IBM.

AVOIDANCE OF GENUINE EMOTIONAL HONESTY

We can define the Avoidance of Genuine Emotional Honesty as the avoidance of a forthright, honest, candid and frank expression and display of one's feelings about another individual, to that individual in any given situation. Some people are unable to tell other people the way they are feeling about them under most circumstances, while with others it varies with the situation. Please rate each volunteer (unmasked) as to how completely he avoids letting the other volunteer(s) present, know what he is feeling about them or their actions, that is, the extent to which he Avoids Showing Emotional Honesty.

In each segment you'll see, the volunteers sat together for a while and worked on the Biographical forms, which there under no compulsion to completely finish at that time. In each segment, only rate the unmasked individual. Use the scale below to make your ratings on the IBM sheet as instructed. Make one rating for each segment.

Replica of One Box to be Marked on the IBM Sheet

0	→	No Avoidance: . gives honest, candid, frank expression and display of his feelings to the other volunteer about the other volunteer or his actions.
1	→	
2	→	Mildly avoids giving honest, candid frank expression...
3	→	
4	→	Moderately avoids giving honest, candid, frank expression...
5	→	
6	→	Strongly avoids giving honest, candid, frank expression...
7	→	
8	→	Extreme Avoidance: extremely avoids letting the other volunteer know what his feelings are about the other volunteer or his actions.
9	→	DO NOT USE THIS RATING: for experimenter's use only
1		Represents sequence number of box on IBM sheet.

Note that the intermediate values of 1,3, 5 and 7 may be used, and all numbers are equally spaced from each other.

BROOKLYN COLLEGE COMPUTER CENTER
DATA CODING SHEET

0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
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31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	!

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9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9
61	62	63	64	65	66	67	68	69	70	71	72	73	74	75	76	77	78			

NAME _____ ACCT. # _____

**BROOKLYN COLLEGE COMPUTER CENTER
DATA CODING SHEET**

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9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	
9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30

0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0		
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7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7		
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9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9	9		
8	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60

0	0	0	0	0	0	0	0	0	0	
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5	5	5	5	5	5	5	5	5	5	
6	6	6	6	6	6	6	6	6	6	
7	7	7	7	7	7	7	7	7	7	
8	8	8	8	8	8	8	8	8	8	
9	9	9	9	9	9	9	9	9	9	
68	69	70	71	72	73	74	75	76	77	78

APPENDIX K

Formulas

Descriptions and derivations of formulas used
for analyzing results.

FORMULAS

t test for significance of sample correlation

$$t = \frac{r_{xy} \sqrt{N-2}}{\sqrt{1-r_{xy}^2}} \quad N-2 \text{ df.}$$

Fisher Z test formula for test of differences between sample correlations
(see Guilford p.589).

$$Z = \frac{Z_4 - Z_5 \text{ (Fisher)}}{\sqrt{\frac{1}{N_1-3} + \frac{1}{N_2-3}}}$$

Fisher based test of difference between sample correlation and theoretical population value (see Cohen p.51)

$$Z = \frac{Z_4 - Z_5 \text{ (Fisher)}}{\sqrt{\frac{1}{N-3}}}$$

Hotelling's test of the differences between two correlations drawn from same sample with one variable in common (see Cohen p.53)

$$t = \frac{(r_{xy} - r_{xv}) \sqrt{(N-3)(1+r_{xy})}}{\sqrt{2(1-r_{xy}^2 - r_{vy}^2 - r_{xv}^2 + 2r_{xy}r_{xv}r_{vy})}} \quad (N-3) \text{ df.}$$

Cohen (p.53) based formula for test of difference between independent slopes.

Cohen states:

$$t = \frac{B_E - B_F}{\sqrt{\frac{\sum (Y_E - Y'_E)^2 + \sum (Y_F - Y'_F)^2}{N_E + N_F - 4}}}$$

FORMULAS (contd.)

Since the unbiased standard error of the estimate

$$(SEE) = \sqrt{\frac{\sum (y - y')^2}{N - 2}}$$

(See Nie et al p.329)

$$\sum (y - y')^2 = N - 2 (SE)^2 \quad \text{therefore,}$$

substituting this value, the formula becomes

$$t = \frac{BE - BF}{\sqrt{\frac{N_E - 2 (SEE_E)^2 + N_F - 2 (SEE)^2}{N_E + N_F - 4}}} \quad (df = N_E + N_F - 4)$$

Cohen based formula for test of differences between sample slope value and population slope value .

$$\text{Since } t = \frac{BE - BF}{\sqrt{\frac{N_E - 2 (SEE_E)^2 + N_F - 2 (SEE_F)^2}{N_E + N_F - 4}}}$$

Then if BF = population slope

$$\begin{aligned} t &= \frac{BE - B_{pop.}}{\sqrt{\frac{N_E - 2 (SEE_E)^2}{N_E - 2}}} = \sqrt{\frac{BE - B_{pop.}}{SEE_E^2}} \\ &= \frac{BE - B_{pop.}}{SEE_E} \quad * \quad N - 2 \text{ df.} \end{aligned}$$

*

formula based on personal communication with Thad Harsnbarger

APPENDIX I

Independent Checks on Effectiveness of Experimental
Manipulations and Validity of Our Rating Procedures.

Independent Checks on Effectiveness of Experimental Manipulations and Validity of Our Rating Procedures

Independent Checks of Aggressiveness-Constructiveness Manipulation

Nine instigations, deemed to vary widely along the aggressiveness-constructiveness continuum were chosen for use in this study. They were chosen based on questionnaire data from a pilot study, which indicated they were rated as falling at widely varied points along the continuum (see Appendix A).

To see if the actual performance of the confederates likewise had a wide-ranged harmful-helpful impact on our subjects the following checks were made:

Check 1 - Distribution of Judges' Ratings of Stimuli

The first check was to inspect the frequency distribution of judges' ratings of the instigations along a 9 pt. scale (see Table 21).

Table 21

Frequency Distribution of Judgments by Rates of Aggressiveness-Constructiveness of Instigations Performed in This Study

Level of Judged Instigation	Number of Instigations Performed
Extremely Aggressive	6
Strongly Aggressive	11
Moderately Aggressive	9
Mildly Aggressive	11
Neutral	16
Mildly Constructive	9
Moderately Constructive	7
Strongly Constructive	8
Extremely Constructive	2

As can be seen, college student raters judged the actual instigations that were performed in the study to vary widely along the aggressiveness-constructiveness dimension. Moreover, they judged that indeed, we had effectively produced instigations that could be deemed extreme in nature.

The one possible exception to this is the low number of instigations judged to be extremely constructive. It may be that we had trouble producing these extremely constructive stimuli. However, an alternate interpretation is that people in general do not see acts that might logically be constructive as constructive when performed by strangers, for reasons discussed elsewhere. Overall, however, it appeared that the goal of producing stimuli that varied along a wide range of aggressiveness or constructiveness was effectively achieved.

Check 2 - Distribution of subjects' own self report data

While the ratings of our instigations by "objective judges" demonstrated that they did indeed range along the scale of aggressiveness-constructiveness, another check on the effectiveness of our manipulation of aggressiveness-constructiveness was to inspect the frequency distribution of ratings of the subjects themselves. This data is presented in Table 22.

Table 22Frequency Distribution of Subjects' Own Judgments of Aggressiveness-Constructiveness of Instigations Performed in This Study Based on Scale 1 Self Report Data

Subjects' Judgment of Instigation	Number of Instigations Judged Performed
Extremely Aggressive	10
Strongly Aggressive	10
Moderately Aggressive	7
Mildly Aggressive	10
Neutral	12
Mildly Constructive	15
Moderately Constructive	10
Strongly Constructive	4
Extremely Constructive	1

As can be seen, this data likewise indicated that by the subjects' own self report, they judged themselves to have been exposed to instigations varying widely along the scale dimension of Aggressiveness-Constructiveness. Again, probably for reasons previously discussed, there were not as many extremely constructive acts reported by the subjects as one would have liked and the reported scarcity was somewhat greater than that based on outside judgments of raters. Nonetheless, the data suggest that the nine instigations did have the expected wide ranging effect on the subjects that had been hoped for.

Check 3 - Summary, anecdotal and verbal self report evidence

A final check on the impact of our instigations on the subjects consisted of their written self report of their experiences. A review

of these self reports revealed that the subjects did indeed judge the confederates to have behaved in a harmful or helpful manner and as planned, the stronger instigations tended to have a more powerful effect. This review also showed however, as expected, a certain amount of variability ^{to} as /how subjects "took" each of the instigations. For example, some subjects tended to evaluate our constructive stimuli as annoying and hence harmful. Other subjects tended to be oblivious to the harm contained in some of the instigations. One subject became annoyed with our confederate, not because our confederate was smoking a cigarette in his face, but because he was afraid the confederate was trying to seduce him. Overall, however, subjects did verbally report perceiving the stimuli pretty much in the harmful or helpful manner that was intended.

Independent Checks on the Effectiveness of the Manipulation of Context

As will be recalled, prior to the instigations by the confederates, we had attempted to induce one of the two contexts in the minds of any given subject. Two questions that needed to be answered were first, did the subjects believe the contexts that were induced, and second, did the subjects relate the context in any way to their behavior.

Check 1 - Check on whether the contextual manipulation was accepted by the subjects

With regard to the first question, there was little doubt that subjects did believe and accepted the particular context that was induced. Up until the time the experimental instigation took place, the subjects were totally immersed in the procedures that I utilized to induce the context. Written self reports of what was going on at that time revealed that almost all subjects accepted the statements made by me as to what would happen and what their role in it would be. Self reports

of what was occurring at this point in the procedure, mirrored the instructions and procedures I adopted. This was in contrast to self reports describing events during and immediately following the instigation, which at times were at variance to what I had maintained was taking place. There is little doubt that the induced context was accepted and active in the subjects' minds when the instigation occurred.

Check 2 - Check for independent evidence of effect of context

With regard to the question of whether or not either of the contexts became the basis for an alteration of the subjects' behavior from their usual way of response, one check that was made was to ask subjects if they felt their behavior was affected by the context as well as to ask them why they responded as they did. Near the end of the administration of the "Mood Scale Calibration" packet, subjects were asked several relevant questions and were asked to indicate a Yes, Maybe, No, choice for each. These responses were tested to see if they were significantly related to the particular context each subject had received, using the χ^2 statistic.

Only the data of those subjects receiving an aggressive instigation or constructive instigation of at least moderate intensity were included in this analysis, since it was assumed that due to the nature of the instigations they received, they would have been the ones who were most likely emotionally aroused, while at the same time, for these reasons they were the ones most likely to be inhibited by the normal restraints of everyday life from making a response. I reasoned that if they felt the low restraint context was effective in loosening these restraints, their answers would be the most sensitive indicator of differential effect of manipulation of context.

The first question asked subjects was if they believed their behavior reflected the true intensity of their feelings. The two groups receiving either of the two contexts did not differ significantly in their answer. Across both groups approximately 55% said Yes, 7.1% said Not Sure and 37.5% said No.

The next question asked the subjects was if they thought they would have behaved the same, even if they had not received the instructions they had prior to the instigation. Again the computed X^2 showed no significant relationship between conditions of their answers and the context they had received. Approximately 76.8% felt they would have behaved the same way regardless. 7.1% were not sure and only 16.1% said they would not have behaved the same.

Analysis of a third question also revealed similar findings. When subjects were asked if they felt they would have behaved the way they had even if they had not been waiting to participate in a psychology study, once again there were no significant differences in the response of the groups receiving either of the two contexts. 69.6% felt they would have responded the same; 19.6% said they weren't sure or it would depend on the situation, and only 10.7% felt they would have responded differently had they not been waiting to be in a study.

Thus, on all three questions, the results indicated that most subjects did not attribute their behavior to the experimental situation and moreover, they did not see the context as having crucially affected their behavior one way or the other, regardless of the context they had received.

While we were thus unable to establish that subjects felt the context to be an important determinant of their behavior, this does not

mean that it wasn't. Rather, based on the results of my personal observations, and a review of the data I feel that the context was so persuasive and compelling that it affected the subjects in a very subtle manner. For example, while subjects receiving each of the two contexts did not differ significantly in suspecting that they were being observed, or in suspecting that the other "student" was really a confederate, they did show a significant difference in how suspicious they were that I left the room on purpose just prior to the instigation. Subjects in the low restraint group reported themselves significantly more suspicious that I left the room to give the "students" a chance to be alone together so I could see how they would interact. This increased suspiciousness was seen as an indicator that subjects receiving the low restraint contexts showed an increased tendency to think of their behavior in the room as something I might be interested in observing in order to see if the subjects were able to "be themselves" with me out of the room, prior to the beginning of the study itself and thus, as one subtle indicator that subjects had accepted the context and made it relevant to their behavior.

To sum up, while we feel confident that the subjects accepted the context, the influence of the contextual manipulation - to the extent it did indeed influence the way the subjects behaved - was much more subtle in its operation than we anticipated.

Assessment of the Concurrent Validity of Our Judging Procedure

To ensure that the video-tape judging procedure employed in this study is valid, several different checks of its validity could be made.

Check 1 - Concurrent Validity - Comparison of raters' judgments with experimenter's prior judgments.

Since each of the stimulus situations selected for use in this research had been given an approximate rating of aggressiveness and constructiveness by me, based on questionnaire ratings of such actions by student judges in prior research, one check was to see whether the judgments made by judges' viewing the video-tapes of each instigation concurred with the judgments made of the aggressiveness-constructiveness of each instigation by me. Table 23 tabulates my judgment of how aggressive or constructive each of the stimulus situations would be to the average person, against the median judgments of judges viewing each of the instigations on video-tape (with different confederates and subjects each time the instigation occurred.) It also shows the obtained correlation between the magnitude of the instigation I decided a given subject would receive a priori, and the raters' judgments of what that subject received across all 79 subjects.

Table 23

Table of Mean Raters' Judgment of Aggressiveness-Constructiveness for Each of the 9 Instigations Used in Study and Given a A Priori Value of Aggressiveness-Constructiveness by E

Stimulus	E's A Priori Judgment	Value	\bar{X} Rater Value	N	r
9	Extremely Aggressive	(9)	8.40	8	.83*, N = 79
8	Strongly Aggressive	(8)	7.19	11	
7	Moderately Aggressive	(7)	7.51	9	
6	Mildly Aggressive	(6)	6.44	6	
5	Neutral Constructive	(5)	4.71	7	
4	Mildly Constructive	(4)	4.97	13	
3	Moderately Constructive	(3)	3.90	8	
2	Strongly Constructive	(2)	2.80	9	
1	Extremely Constructive	(1)	3.12	8	

Note: All values are based on 9 point scale (where 1 = extremely constructive; 5 = neutral; 9 = extremely aggressive)

* $p < .00001$

As can be seen from Table 23, a high correlation was obtained between the ratings by college students of each instigation performed and the a priori magnitude of aggressiveness-constructiveness assigned to each type of instigation by the experimenter prior to its performance. Not only was a high correlation obtained ($r = .88$, $N = 79$, $p < .00001$), but the mean rater judgment for each of the nine stimulus situations was markedly similar to those made on a a priori basis by E using the pilot questionnaire data as a guide (see Appendix A), with the exception of stimulus 8 and stimulus 1.

With regard to those two stimuli, it is likely that rather than the judging procedure being at fault, I overestimated how aggressive or constructive each of these instigations would be, by not taking into account the wide variability in how these actions might be perceived.

Overall then, the data indicated that college student raters were making valid distinctions between the instigations produced by the confederates and hence by inference, between the responses made to them by the subjects as well.

Check 2 - Concurrent Validity - Comparison of Raters' Judgments with subjects' ratings of instigations

A second check of the concurrent validity of these judgments was to tabulate the means of the subjects' ratings of aggressiveness-constructiveness for any given level of judgment of aggressiveness-constructiveness as rated by the raters. This tabulation appears in Table 24. All numerical values are with reference to a 9 point scale where 1 = extremely constructive, 9 = extremely aggressive.

Also shown is the correlation obtained between the raters' judgment of the magnitude of aggressive-constructive instigation any given S received, and the given S's on rating of what he received, (using Scale; see Appendix G pp. 205-289) across all S's.

Table 24

Comparison of Means of Actual Subjects Judgments of Aggressiveness or Constructiveness for Each Level of Rater Judged Aggressive or Constructive Instigation

Judged Instigation	^a Value	Mean S's Judgment	^b N	Computed r
Ext - Strong Aggressive	(8.5)	8.72	11	.70*, N = 78
Strong - Mod. Aggressive	(7.5)	7.80	10	
Moderate - Mild Aggressive	(6.5)	6.24	12	
Mild - Neutral Aggressive	(5.5)	5.73	8	
Neutral - Mild Constructive	(4.5)	4.11	14	
Mild - Mod. Constructive	(3.5)	3.88	10	
Mod. - Strong Constructive	(2.5)	3.67	6	
Strong - Extreme Constructive	(1.5)	3.97	7	

a - Numerical values of instigation level have been recoded to allow ready comparison of numerical magnitudes.

b - Data obtained from Scale 1 (see Appendix G, pp.275-289).

* $p < .00001$

As can be seen, a high correlation was obtained between judgments by student raters of how aggressive or constructive the 79 instigations actually performed were, and the subjects' own judgments of how aggressive or constructive the instigations they received were ($r = .70$, $N = 78$, $p < .00001$). A look at the subjects' mean judgment of how aggressive or constructive each of the judged instigation levels was, shows that not only is the relation between the variables quite strong but both subjects and independent student raters showed marked similarity in their magnitudes of their judgments, with the exception of the extremely constructive

instigation levels. As discussed in other sections of this paper, the latter disagreement would be theoretically expected. Overall, then, the comparison again illustrates the concurrent validity of the judges' ratings of the instigations on the video-tapes and by inference, their ratings of the subjects' responses to these instigations.

Assessment of the Construct Validity of our Judging Procedure

Another set of checks on the validity of our judging procedure involved an independent check of its construct validity.

If our judges successfully evaluated the aggressiveness-constructiveness of our experimental instigations, then when these judgments were grouped into nine instigation levels, ranging from Strong-Extreme Aggressive to Strong-Extreme Constructive, subjects who received the more extreme aggressive level should show higher mean levels of MAACL hostility, MAACL depression, self reported hostile desires and fears of harm from the C's, the E or one-self, than subjects who were judged to have received lower instigation levels. Conversely, subjects judged to have received the Strong-Extreme Constructive should show higher mean levels of self reported desires to be helpful than subjects receiving less constructive instigation levels, but should not show high mean scores on the other self report measures.

Results of these checks (Checks 1 - 2) are shown below. Also shown are the correlations obtained between the magnitude of aggressive or constructive instigation each S received and his actual self report score on each scale, across all subjects.

Check 1 - Construct validity - correlation between rater judgments and subjects' MAACL depression and hostility scores following the instigations

The first check on the construct validity of our judging procedure was to see if the judgments would correlate with the subjects' MAACL

self reported affect obtained immediately following the instigations that were actually received. (see Appendix D, p. 264) (see Appendix E, p. 274) If the judgments were accurate, a good correlation would be obtained. MAACL 3 was administered just after the instigation occurred and standard MAACL hostility and depression scores were obtained. The mean self rating of MAACL hostility and depression for subjects receiving each of the nine judged instigation levels appears in Table 25, as does the correlation obtained between the magnitudes of instigation each S. was judged to have received and his MAACL hostility and depression scores, across all 79 subjects.

Table 25

Breakdown of Mean MAACL 3 Hostility (H) and Depression (D) Scores Obtained Following Each Different Level of Judged Aggressive or Constructive Instigation

Instigation Level	Value ^a	b		N	r
		\bar{X} H	\bar{X} D		
Ext-Strong Aggressive	(7.5)	16.18	21.18	11	r = .60*, N = 79
Strong-Mod. Aggressive	(6.5)	11.88	18.10	10	r = .46*, N = 79
Moderate-Mild Aggressive	(5.5)	12.67	19.90	12	H
Mild-Neutral Aggressive	(4.5)	10.63	16.13	8	D
Neutral-Mild Constructive	(3.5)	9.67	17.27	15	
Mild-Moderate Constructive	(2.5)	7.30	14.20	10	
Mod.-Strong Constructive	(1.5)	7.67	13.83	6	
Strong-Extreme Constructive	(.5)	6.71	14.57	7	

^a Numerical values of instigation level have been recorded to allow ready comparison of numerical magnitudes.

^b Based on standard MAACL scoring system (see Appendix E, p. 274 for MAACL)

*p < .00001

As can be seen, both the pattern of the data and the obtained correlations support the construct validity of the video rated judgments.

As can be seen, the relation between each judged level of instigation and the mean MAACL hostility and depression scores found for all these subjects receiving that level, across all levels, was quite high and the pattern was as expected.

Further, a strong correlation was obtained between the magnitude of the instigation each person received, as judged by independent raters, and the magnitude of MAACL hostility and depression he showed, across all 70+ subjects. This data thus also supported the construct validity of the raters' judgments of the instigations.

Check 2 - Construct validity - correlation between Rater Judgments of instigations and Subjects' Self Report of desires to Reciprocate and fears of retaliation for reciprocity (see Appendix I, p. 312)

Other data also supported the construct validity of our manipulation. Tables 6, 7, 28, 29 and 30 show for any given level of judged instigation - the mean level of subjects' self reported desires to be helpful in return (\bar{X} 's, Scale 3); desires to be harmful (\bar{X} 's, Scale 4); degree of harm anticipated from subject if S reciprocated the instigation (\bar{X} 's, Scale 6); degree to which S would have feared retribution by E if he reciprocated (\bar{X} 's, Scale 7); and degree to which S would have harmed himself but felt bad if he reciprocated (\bar{X} 's, Scale 8). The values for levels of instigation have been recorded along a 9 point scale where 1 = extremely constructive; 9 = extremely aggressive and 5 = neutral.

The correlation obtained between the magnitudes of instigation each S was judged to have received and his self report score on each of the above scales, across all subjects, is also shown on a table by table basis.

Table 26

Means of Subjects' Reported Magnitudes of Desires to be Helpful to Confederate Following Each Different Level of Judged Aggressive or Constructive Instigation

Instigation Level	^a	\bar{X} Magnitude of Desire	^b	Computed r
	Value		N	
Ext-Strong Aggressive	(1.5)	5.50	11	r = .26*, N = 78
Strong-Mod. Aggressive	(2.5)	5.76	10	
Moderate-Mild Aggressive	(3.5)	5.36	12	
Mild-Neutral Aggressive	(4.5)	5.38	8	
Neutral-Mild Constructive	(5.5)	5.82	14	
Mild-Moderate Constructive	(6.5)	6.00	10	
Mod.-Strong Constructive	(7.5)	6.27	6	
Strong-Extreme Constructive	(8.5)	6.43	7	

^a
Numerical values of instigation level have been recoded to allow ready comparisons of numerical magnitudes.

^b
Data obtained from Scale 3 (see Appendix G, p. 288) where 5 = no helpful desires; 9 = desire to be extremely helpful and constructive as defined by anchor points.

*p < .01

Table 27

Means of Subjects' Self Rated Magnitudes of Desires to be Harmful to Confederate Following Each Different Level of Judged Aggressive or Constructive Instigation

Instigation Level	a Value	b		Computed r
		\bar{X} Magnitude of Desire	N	
Ext-Strong Aggressive	(8.5)	8.86	11	r = .47*, N = 78
Strong-Mod. Aggressive	(7.5)	6.50	10	
Moderate-Mild Aggressive	(6.5)	5.94	12	
Mild-Neutral Aggressive	(5.5)	5.25	8	
Neutral-Mild Constructive	(4.5)	5.07	14	
Mild-Moderate Constructive	(3.5)	5.40	10	
Mod.-Strong Constructive	(2.5)	5.00	6	
Strong-Extreme Constructive	(1.5)	5.00	7	

a
Numerical values of instigation level have been recoded to allow ready comparison of magnitudes.

b
Based on Scale 4 (see Appendix G, p. 288) where 5 = no desires to be harmful; 8 = desires to be extremely harmful, as defined by anchor point.

*p < .000001

Table 28

Means of Subjects' Self Rated Magnitudes of Retaliation Feared From Confederate Following Different Levels of Judged Aggressive or Constructive Instigation if Subjects Acted Out Their Real Desires

Instigation Level	a Value	b		Computed r
		\bar{X} Anticipated Retaliation	N	
Ext-Strong Aggressive	(8.5)	8.80	11	r = .57*, N = 76
Strong-Mod. Aggressive	(7.5)	7.25	10	
Moderate-Mild Aggressive	(6.5)	6.59	11	
Mild-Neutral Aggressive	(5.5)	5.43	7	
Neutral-Mild Constructive	(4.5)	5.19	14	
Mild-Moderate Constructive	(3.5)	5.36	10	
Mod.-Strong Constructive	(2.5)	5.00	6	
Strong-Extreme Constructive	(1.5)	5.14	7	

a
Numerical values of instigated level recoded to allow ready comparison of magnitudes.

b
Based on Scale 6 data (see Appendix pp. 290) where 5 = no fear of retaliation; 9 = fear of extreme retaliation, as defined by anchor point.

*p < .00001

Table 29

Means of Subjects' Self Rated Magnitude of Retribution Feared From Authorities Following Different Levels of Judged Aggressive or Constructive Instigation if Subjects Acted Out Their Real Desires

Instigation Level	a		b	
	Value	\bar{X} Anticipated Retaliation	N	Computed r
Ext-Strong Aggressive	(8.5)	6.77	11	r = .35*, N = 77
Strong-Mod. Aggressive	(7.5)	5.50	10	
Moderate-Mild Aggressive	(6.5)	5.03	12	
Mild-Neutral Aggressive	(5.5)	5.00	7	
Neutral-Mild Constructive	(4.5)	5.00	14	
Mild-Moderate Constructive	(3.5)	5.00	10	
Mod.-Strong Constructive	(2.5)	5.00	6	
Strong-Extreme Constructive	(1.5)	5.00	7	

a
Numerical values of instigation level have been recoded to allow ready comparison of magnitudes.

b
Based on Scale 7 data (see Appendix G, p. 290) where 5 = no fear of retribution; 9 = fear of extreme retribution; as defined by anchor point.

* $p < .00079$

Table 30

Means of Subjects' Self Rated Anticipated Intropunitiveness
Following Each Different Level of Judged Aggressive or
Constructive Instigation if They Acted Out Their Real Desires

Instigation Level	a		b	
	Value	\bar{X} Anticipated Intropunitive	N	Computed r
Ext-Strong Aggressive	(8.5)	6.44	11	r = .33*, N = 77, p < .002
Strong-Mod. Aggressive	(7.5)	5.35	10	
Moderate-Mild Aggressive	(6.5)	5.39	12	
Mild-Neutral Aggressive	(5.5)	5.00	7	
Neutral-Mild Constructive	(4.5)	5.09	14	
Mild-Moderate Constructive	(3.5)	5.11	10	
Mod.-Strong Constructive	(2.5)	5.00	6	
Strong-Extreme Constructive	(1.5)	5.14	7	

a

Numerical values of instigation level have been recoded to allow ready comparison of magnitudes.

b

Based on Scale 8 data (see Appendix G, p. 290) where 5 = no fear of intropunitiveness; 9 = fear of extreme intropunitiveness; as defined by anchor point.

* p < .00079

As can be seen, for each scale the relation between each judged level of instigation and the mean self reported response to it across levels, was quite high and the pattern was as expected. Further, inspection of the correlations obtained between the actual magnitudes of the instigations college student raters judged each of the 70+ subjects to have received in each case, and each subject's self reported motivational, emotional and cognitive reactions following each instigation, showed strong significant positive correlations in each case. In sum, the consistency of this data provided further evidence for the construct validity of the 80 college students' rating of the instigations shown on the video-tape and by inference, supported the validity of rating of subjects' responses to these instigations.

Assessment of Rater Bias

While we have found strong evidence in support of the validity of the video rating procedure, we have not so far eliminated the possibility that rater bias could have produced some of this evidence. There are two types of rater bias in question here.

The first type would consist of judges rating the instigator's behavior based on the subject's response. If this occurred, we would then obtain the strong "construct validity" correlations not because the instigator's behavior could predict the subject's affect and motivational arousal, but because the judgments of the instigators' behaviors were contaminated and thus correlated highly with subjects' behaviors which in turn correlated highly with the self report data, e.g. given this type of bias, raters would have rated the subjects' behavior in effect but would have been unable to obtain a clear uncontaminated picture of the confederate's behavior.

Inspection of the previously reported data however, suggested that this error was not being made excessively (if at all) because if it were, the raters' judgments would not have correlated as highly with my a priori judgments of those instigations. Further, if such bias was a potent factor we would not have been able to demonstrate the effect of context on reciprocity as we were able to do. This is because if rater bias was the factor producing the correlations obtained between instigation judgments and response judgments, no such context effect would have been found, since the raters and the experimenter were double blinded in the judging procedure as to which subject was used under which context. In sum then it appears that the elaborate procedures we used to minimize rater bias were rather effective in eliminating this type of rater bias.

The second type of rater bias that might have occurred would have been for ratings of the subjects' responses to be contaminated by the behavior of the confederate, e.g. raters would inadvertently be assigning ratings for the subjects' responses based on confederates' behavior. This would produce spuriously high reciprocity data.

Once more, the differential results as obtained under different contexts negated this possibility as playing a strong role in our data ratings, as did the low correlations found between instigation and response ratings under certain theoretically delineated conditions, of which the rater had no awareness.

As a further step in showing the minimal influence of rater bias in producing the stronger correlations we obtained, we can look at the spontaneous response data, where for reasons mentioned earlier in our discussion of methodology, rater bias could not logically have played a role since raters of the responses had no knowledge of the instigations (which were rated by other raters).

The reader will recall that the spontaneous response data consisted of correlations between raters' ratings of instigation with other raters' ratings of the "spontaneous" responses. For the verbal instigations at least, the raters' rating the responses had no access to the instigations since the responses they were rating were filmed following the termination of the instigation by the C's, and since the response raters were not allowed access to viewing or hearing this initial filmed instigation.

Analysis of this verbal response data showed a strong significant correlation ($r = .9$, $N = 7$, $p < .005$), under low restraint conditions, between the ratings by those raters who saw the instigations and those raters who saw only the "spontaneous" responses of the subjects following the ceasing of the instigations. Rater bias would not have produced those results, as in this case; the two sets of ratings were made totally independently. Thus, despite being unable to rule out rater bias in this instance, we were able to obtain a strong correlation between instigation and response ratings rivaling those obtained elsewhere. This fact suggests that our other results were valid as well, despite the fact that some correlations were quite high, and were not merely a figment of the judges' imagination.

Overall then, in view of the suggestions from the above findings and our procedures to safeguard against rater bias, we felt fairly confident in ruling out rater bias as an important factor in producing our results.

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