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The Influence Of The Pretesting Phase On The Group Shift Effect

Duncan Archibald Ferguson

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THE INFLUENCE OF THE PRETESTING
PHASE ON THE GROUP SHIFT EFFECT

by

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Submitted in partial fulfillment
of the requirements for the degree of
Doctor of Philosophy

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London, Ontario

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ABSTRACT

Research on the group shift phenomenon has, for the most part, employed a within subjects repeated measures design. That is, subjects have typically completed an individual-pretest prior to being formed into groups to discuss the tasks and reach group decisions on them. Recently, however, much concern has been expressed about the influence which pretesting may exert on the phenomenon.

This thesis reports three experiments which used risk-evoking Choice Dilemma Questionnaire (CDQ) items to examine the ramifications of the pretesting phase for the group shift effect. The effects of expectation of discussion, of pretest completion, and of anonymity on subjects' CDQ item preference selections were examined. In addition, the reliability of the shift effect was considered, as was the extent to which subjects pay attention to (or are aware of) certain experimental instructions.

Statistically significant group shifts to risk were obtained in all three experiments with a number of different pretest manipulations.

Although the influence of exposure to expectation-inducing manipulations was not consistent across studies, anticipation of forthcoming discussion was related to relatively cautious initial preference selection. There was, however, no evidence to suggest that subjects' expectations are causally implicated in shift occurrence. Moreover, the third experiment suggested that if expectations about future item-related activities, such as group discussion, are strongly aroused, they may suppress the shift effect. Finally, a postexperimental

questionnaire indicated that powerful manipulations may be necessary in order to convince subjects that they will be discussing the CDQ items with others.

Comparison of conditions with pretests to those without pretests offered some support for the notion that pretest completion is not causally implicated in the group shift effect. That is, in two of the three experiments, group decision scores of subjects who were not pretested were significantly more risky than the initial preferences of pretested subjects. It was also noted that in the absence of a pretest, group decision and posttest preferences tended, across studies, to be more variable than when pretesting had been employed. Thus, while it is unlikely that the pretest is causally implicated in the shift effect, it may exert a stabilizing influence on the magnitude of shift.

The participants' degree of anonymity was examined in the third experiment. Subjects were either asked to put their names on the questionnaires or were told that no personal identifiers were necessary. This manipulation had little or no influence on any of the results.

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GENERAL INTRODUCTION

(1) Purpose of the Research

The phenomenon that has been referred to as the risky shift has been studied for more than 15 years and in that time has accumulated a literature of well over 300 articles. A shift to risk is said to occur if postdiscussion group decisions on certain issues are more risky or extreme than the average of the prediscussion opinions of individual group members. Much of the research dealing with this effect has used a single instrument, the Choice Dilemmas Questionnaire (CDQ), and a single repeated measures research design. Despite the more than 300 publications about this phenomenon, consequences for the external validity of the effect which might arise from the extensive use of the CDQ items and the repeated measures design have not been clearly delineated. The task and methodological generalizability of the phenomenon are issues of considerable concern and importance (Cartwright, 1971, 1973; Dion, Baron and Miller, 1970; Myers and Lamm, 1976; Pruitt, 1971b).

The present research consisted of three experiments which attempted to examine the generalizability question by clarifying the impact of pretest completion on group decisions and of anonymity and anticipation of discussion on initial pretest preference selection. The exact nature of the shift and the variables which influence it must be understood. As Cartwright (1971) has pointed out, generalization to natural groups "requires careful consideration of the conditions under which the results have been obtained" (p. 378).

(2) Background

In its original form the CDQ contained 12 hypothetical life situations (see Pruitt, 1971a). Although a large number of studies have used this original questionnaire developed by Wallach and Kogan (1959), many others have employed abbreviated versions or have incorporated new items constructed on the same format. Both the original 12 items and more recently developed ones using the same format will be referred to as CDQ items. Since a shift has also been found on non-CDQ items often having no explicit risk content, it has been suggested that this phenomenon might more aptly be described as a "polarization" shift, an "extremity" shift, or a "group" shift (Doise, 1969; Moscovici and Zavalloni, 1969; Pruitt, 1971a; and Vidmar, 1974). The present author will refer to the phenomenon as a group shift.

The basic repeated measures paradigm used in much of the group shift research involved having subjects make decisions three times for each item. The first of these decisions (I_1) is a pretest and is made individually by each subject. The subjects are then usually assigned to a group (most often ranging between three and six members), and are asked to discuss the items as a group and to arrive at a unanimous group decision (G). Following G the subjects are asked to reread the items as individuals and to indicate their final risk preference for each item (I_2). They are usually informed that this last decision is to be made in case they felt dissatisfaction with the group decision and want to change their answers. Group shift is commonly considered to be the amount of change from the averaged member I_1 decisions to the G decision.

• Researchers have experimentally approached the generalizability question by attempting to replicate the shift with non-CDQ materials and by altering the popular repeated measures design. Although studies incorporating either or both of these strategies are fairly well represented in the literature, it is not yet possible to draw firm conclusions about the extent of the group shift's generalizability or about the factors which may enhance or reduce it.

Attitudes towards rival schools (Doise, 1969), risk taking in consumer product selection (Johnson and Andrews, 1971), racial attitudes (Myers and Bishop, 1970), hypothetical traffic cases (Myers and Kaplan, 1976), ethical-legal dilemmas (Myers, Schreiber and Viels, 1973), attitudes of French high school students toward DeGaulle and toward Americans (Moscovici and Zavalloni, 1969), altruism items (Schroeder, 1973), Pettigrew's Category Width Scale (Vidmar, 1974), a Blackjack Task (Blascovich, Ginsburg, and Howe, 1975, Blascovich, Ginsburg, and Veach, 1975), attitudes toward relaxation of air pollution controls (Madsen, 1975), and items pertaining to values of freedom and equality (Pruitt and Cosentino, 1975) have been used as vehicles for the empirical generalization of the group shift effect. However, the above studies utilized the repeated measures design so common to the group shift work. Thus one might offer the somewhat limited conclusion that the group shift effect can be generalized to many non-CDQ materials (frequently having no explicit risk content) when the traditional design is retained. Although these studies do help clarify the phenomenon's external validity with regard to tasks, they do not address the crucial problem of external validity being affected by the pretest paradigm.

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The effects of the pretest paradigm have also been examined by studies using both CDQ items and different types of tasks in conjunction with some form of between groups design. While most of these studies have been concerned with the occurrence of a pretest by treatment interaction, a few have been addressed to the effects of the pretesting environment on selection. Unfortunately the results have been equivocal. Taken as a whole, the studies leave many unanswered questions and do not lead to firm conclusions about the influence of pretesting on the group shift effect. The present research was undertaken in order to further examine this issue.

There are a number of ways in which the pretesting phase may influence the group shift. Some possible influences (such as pretest by treatment interaction) which pretest completion may exert on group decision scores will now be considered. Following this discussion, considerable attention will be paid to the influence which the pretesting environment (i.e. testing room, individual vs. group pretesting, etc.) may exert on pretest score selection.

(3) Effects of Pretest Completion

Consider the possible influence of actually completing the pretest. Initial exposure to (and preference selection on) the items may influence subjects' reactions to group discussion and thus affect their group decisions. The completion of a pretest preference on a particular item may sensitize subjects to certain aspects of that item, may lead to greater commitment to a particular alternative, or may give subjects practice in dealing with, or "solving", items of a general type. A

number of authors have already attempted to examine the nature of pretest completion.

At least five studies have been completed which employed both non-CDQ items and a between groups design. Heimbach (1970) using a jury task, Blank (1968) using a gambling task, Myers and Bach (1974) using militarism-pacifism items, and Yinon, Jaffee, and Feshback (1975) using an electric shock as punishment manipulation, obtained evidence that neither the CDQ nor pretesting seem essential for the occurrence of a group shift. Only one study (Davis, Kerr, Sussman, and Rissman, 1974, using duplex bets) concluded that there was some degree of justification for concern about the effects of pretesting. They found that pretested groups and subjects, on subsequent testing, tended to perceive bets of zero or positive expected value as more attractive than did non-pretested groups of subjects. On the whole, the evidence from these five studies tends to favour the conclusion that pretesting is not essential for a group shift. Additional studies using CDQ items and a between group design (Baron, Baron, and Roper, 1974; Carlson and Davis, 1971; Castore, 1972; Gaskell, Thomas, and Farr, 1973; McCauley, Teger, and Kogan, 1971; Myers, Bach and Schreiber, 1974; and Vidmar, Ferguson, and Heapy, 1973) were also concerned with the group decision consequences of having completed a pretest. These latter studies, taken as a whole, obtained more equivocal results. Consideration must be given at least three ways in which initial preference selection may affect group decisions.

First, subjects selecting an alternative on a specific item may become sensitized to various aspects of that item. This sensitization to the specific item may then interact in some fashion with group

discussion on that item and thus influence the nature of the group decision scores. Most studies have been addressed to this possibility and have tended to assume that if an interaction occurred it would result in subjects selecting greater risk at G than at I₁. Castore (1972) concluded that without a pretest there is no increased group preference for risk and that the usual group shift to risk could most aptly be attributed to an interaction between the prediscussion assessment process and the effects of group discussion (p. 165). On the other hand, Gaskell et al. (1973), and Myers et al. (1973) obtained significant group discussion effects in the absence of a pretest. McCauley et al. (1971), Carlson and Davis (1971), Vidmar et al. (1973), and Baron et al. (1974) obtained inconclusive results. The group decision scores in their no-pretest conditions were not significantly riskier than the I₁ selection of subjects in standard conditions. All four studies did, however, point out that the results in the group conditions tended to be more risky (at a statistically nonsignificant level) than the pretest scores in the standard conditions.

Second, completion of a pretest may influence group responses if subjects become committed to their I₁ selections. Commitment to a particular I₁ selection might make subjects less inclined to change their opinion during discussion and result in a decreased treatment effect. Although numerous studies in the attitude literature (e.g., Deutsch, Krauss, and Rosenau, 1962; Gerard, Blevans, and Malcolm, 1964) vouch for the importance of this factor, there is little empirical evidence available in the group shift literature itself to either support or deny this possibility.

Third, pretest experience with items of the CDQ type may make subjects "familiar" with item characteristics or give them practice solving items of the CDQ type. This general familiarity might then interact with later discussion exposure to the same items or to items of a similar type. Gaskell et al. (1973) found no evidence to support this notion and concluded that pretest effects do not generalize to items which were not included in the pretest.

Some of the above studies indicate that a significant shift occurs on CDQ items in the absence of a pretest whereas others are inconclusive and one concludes that without pretesting no shift occurs. Thus the consequences of pretest completion, especially with respect to CDQ items, are not at all clear. Because the issue is so critical for the generalizability of the shift, it deserves concentrated and systematic examination.

(4) Effects of the Pretesting Environment

In addition to the possibility that I_1 selection during the pretesting phase influences later decisions, there is also a possibility that the selection of I_1 itself may be unintentionally influenced. This section will consider the possibility that alternatives selected during the pretesting phase may themselves be influenced by the environment in which they are chosen. For example, the commonly used group pretesting situation, the laboratory setting, or some other aspect of the situation might supply subjects with cues that discussion is imminent. If subjects expect discussion they may select more cautious (or perhaps less extreme) I_1 preferences than they would in the absence of such expectations.

Anticipation of having to discuss, and perhaps defend, a pretest opinion may lead to initial selections that are easy to defend rather than ones which reflect true feelings.

The possibility that I_1 may be viewed as a dependent variable influenced by pretesting conditions has received little explicit consideration in past studies. Most studies have assumed that the I_1 scores obtained using the standard repeated measures design are subjects' true initial preferences, uninfluenced by artifact. However, Gaskell et al. (1973) and McCauley (1970) both suggested that I_1 scores so obtained may not represent subjects' true preferences.

Gaskell and his associates felt that, "in the pretest phase subjects probably anticipate group activity and this anticipation or expectancy can affect pretest scores (p. 193)." When attempting to test this notion they did not obtain a significant difference between the mean pretest scores of subjects in the traditional paradigm and those in an "expectancy control" condition who worked alone on what they were told would be their only task. On the other hand, they found that the pretest score variance of subjects who worked alone was greater than that of the other subjects. To the present author's knowledge this is the only publication which simultaneously examined expectation and suggested that commonly obtained I_1 preferences might not be true preferences. Other studies concerned with pretesting effects have focused largely on one or the other of these possibilities. Heapy (1974) and Vidmar et al. (1973) both explored the effects of discussion expectation on I_1 selection, whereas McCauley (1970), while not explicitly concerned with expectancy,

effects, attempted to see if I_1 selections in the traditional design were indeed true preferences.

McCauley suggested that co-working (being in a group during I_1 completion) may in some fashion be restraining subjects from exercising the full extremity of their opinions. He found that more extreme I_1 responses were selected when subjects were alone than when they were co-working. Vidmar et al. (1973) attempted to determine the influence of explicit discussion expectation on subjects' responses at I_1 , at G and at I_2 . In one condition they employed the traditional design with group pretesting. Another condition differed in that subjects were told, prior to I_1 solicitation, that they would be broken into small groups to discuss and to come to unanimous group decisions on the items. There were no between condition differences on any of the dependent measures. The results of this study do not appear to support the notion that expectancy may influence group shift responses.

On the other hand, there are studies which indicate that subjects who have reason to expect further item-related activities will select different initial preferences on those items than subjects who do not. A study by Heapy (1974) using CDQ items obtained evidence that subjects who are told to expect a group discussion tend to take a significantly more cautious prediscussion stance than subjects who do not receive such information. Additional support was furnished by a study (Bem, Wallach and Kogan, 1965) which lead subjects to believe that personally aversive physical consequences might result from their decisions. Subjects were told, after pretest solicitation, that they would be participating in a group discussion in order to unanimously agree upon an alternative to

expose themselves to as a group. With the forthcoming discussion in mind they were asked to reconsider their initial preferences and to decide on an alternative to recommend to the group. The second preferences selected were significantly more cautious than the initial preferences.

The occurrence of expectancy-induced, prediscussion preference changes receives further support from an examination of the attitude change literature. A number of studies have shown that under certain conditions individuals who anticipate receiving a persuasive communication or participating in a face-to-face discussion about some issue will alter their opinions on that issue prior to exposure to the communication or discussion. Studies by Cooper and Jones (1970), Dinner, Lewcowicz and Cooper (1972), McGuire and Millman (1965), McGuire and Papageorgis (1962) and Papageorgis (1967) suggest that if subjects are forewarned of a potential persuasive message about some issue they will, prior to receiving the message, shift their issue opinion in the direction of the perceived point of view of the expected message.

Cialdini, Levy, Herman and Evenbeck (1973), referring to such change as an accommodation effect, proposed that forewarning might actually be causing subjects to select more neutral initial opinions (a moderation effect) than they normally would. They demonstrated that subjects, when they anticipated a monitored face-to-face interaction with peers in which they had to give and defend their opinions, tended (regardless of the perceived issue position of the other discussant) to endorse more neutral statements about that issue than subjects who simply expected to hear a peer present his views on the issue. Recently

other research (Haas, 1975) (employing expectation of persuasive communications from experts rather than expectation of discussion with peers) was reported which indicated that both moderation and accommodation effects may occur. Moreover, Haas and Mann (1976) present additional evidence that subjects who are forewarned of exposure to a counter attitudinal message show anticipatory belief change.

It seems that pretesting environments have the potential to influence the nature of subjects' initial responses. The evidence indicates that fairly explicit information about forthcoming discussion (or an imminent message) results in selection of more cautious (or more moderate) initial preferences. It does not indicate that mean I_1 responses in standard group shift paradigms have reflected anticipation of discussion. Although subjects in such paradigms are not explicitly informed that they will be participating in group discussion of the CDQ items, it is fairly certain that if they do come to expect discussion their I_1 selection may be influenced. Cues which would lead subjects to expect discussion may well be present in common pretesting environments.

Participants in group shift experiments have often been recruited for studies of "group decision making" (e.g., Ferguson and Vidmar, 1971; Baron, Dion, Baron, and Miller, 1971). Many studies have had individuals meet in groups and fill out their pretest in the group setting before being requested to discuss the items. Some studies Baron et al., 1971; Ferguson and Vidmar, 1971; Myers and Bishop, 1970; Vidmar and Burdeny, 1971) have pretested quite large groups before dividing them into smaller groups for discussion. In other studies (Carlson and Davis, 1971; Clark, Crockett and Archer, 1971; Fraser, Gouge and Billig, 1970; McCauley et al., 1971; Myers and Bishop, 1971; Schroeder, 1973) subjects have

been recruited one discussion group at a time and have been seated together at a table (frequently the table at which the ensuing discussion occurs) or in the same room while they fill out their I_1 preferences. The group pretesting situation may supply subtle cues which could tip subjects off about a forthcoming discussion. Subjects would appear to be very sensitive to any such information (e.g., Orne, 1962; Keisler, Collins, and Miller, 1969). For example, in some cases they may have wondered why they were sitting around a table, or why they were doing a ten minute task after having signed up for a one or two hour experiment. Furthermore, the experimenters, not concerning themselves with the possible effects of subjects being aware of their immediate futures, may have inadvertently made cues available which would suggest that they would be discussing the items in groups. It is important to realize that explicit attempts to control for the possible effects of expectation were not undertaken, or at least were not reported, in many group shift studies. It appears plausible that to a greater or lesser extent, depending on the particular study, subjects may have had the opportunity to anticipate group discussion or at least to anticipate some forthcoming activity. While plausible, however, the argument that the nature of the standard pretesting situation may lead subjects to expect discussion is not supported by empirical evidence. For this reason the author conducted a pilot study in order to get some feeling for the link between pretesting conditions, expectations, and I_1 selections.

(5) A Pilot Study on Expectancy Effects

It was thought that if traditional pretesting situations influence I_1 selection through the creation of expectancy, then subjects who have

received different initial cues about the nature of an experiment should tend to endorse different I_1 choices. In addition, they should admit to different expectations about the nature of forthcoming events. The pilot was undertaken in order to get some idea about whether subjects tend to respond in this fashion. It employed eight CDQ risk items.

The pilot study incorporated three conditions manipulating three levels of cues which might lead to anticipation of discussion. In the Maximum Cues Condition 17 subjects were recruited for a study in "group decision making," worked on their pretest sitting around the same table in groups of no more than five, were told that they would be discussing the items as a group, and were told that they would have to defend the I_1 preference they selected. The Medium Cues Condition differed in that 13 subjects were told nothing by the experimenter about an impending group discussion and were not given any indication that they would be committed to their I_1 preference selections. In the Minimal Cues Condition 17 subjects were recruited for a study in "individual decision making," were seated in individual rooms to complete their I_1 selections, were told nothing by the experimenter about an impending group discussion, and were not given any indication that they would be committed to their I_1 preferences.

Subjects in all conditions completed I_1 preferences for the eight CDQ risk items prepared for this study. They were then told that the experiment was essentially over but that the experimenter was conducting a pilot study and wanted to know what thoughts or feelings they had during the experiment. Subjects were given an opinion questionnaire dealing with the task they had just completed. In addition the

Experimenter discussed the experiment with them and attempted to form an impression of their thoughts about it.

The previously cited evidence indicated that expectation of discussion results in I_1 selections which are cautious. Therefore the greater the availability of cues about an impending discussion the greater should be the likelihood that cautious I_1 preferences will be chosen. As expected, the mean I_1 scores of the Maximum Cues Condition (5.68) tended to be more cautious than those of the Medium Cues Condition (4.96), $t(6) = 1.86$, $.10 < p < .20$. However, contrary to expectation, the mean I_1 scores of the Minimum Cues Condition (5.88) also tended to be more cautious than those of the Medium Cues Condition, $t(6) = 2.31$, $.05 < p < .10$.

The opinion questionnaire and subsequent discussion indicated that subjects receiving Maximum Cues felt an impending discussion was more likely than those receiving Medium Cues or Minimum Cues. In addition an experimental interest in opinion change was seen to be more likely by the Maximum Cues subjects than by the Minimum Cues subjects. This information is consistent with hypotheses about the subjects' expectations and with the nature of the I_1 scores obtained in the Maximum and Medium Cue Conditions.

On the other hand, subjects receiving Minimum Cues were least likely to expect discussion or to feel that opinion change was of interest to the experimenter but were just as cautious as those receiving Maximum Cues. These data appear to contradict the notion that subjects should select more risky I_1 preferences when they do not expect discussion. Expectation of group discussion is not, however, the only reason why

subjects alter their initial opinions. Anticipation of a persuasive message also seems to result in premanipulation opinion change (Cooper and Jones, 1970). Perhaps any situation which led subjects to believe that they would have to defend or justify their initial choices could result in their being concerned about their preference selection. If subjects feel that they will have to justify their selection, it is reasonable to suppose that they will be rather careful to choose a position which they perceive as relatively easy to defend or justify.

When asked what they expected to occur next, subjects receiving Minimum Cues were most likely to indicate that they expected to have to explain why they picked a particular probability and to justify or 'rationalize' their choice. This justification theme was not so prevalent in the other two conditions. Subjects receiving Minimum Cues, although not expecting discussion per se, were clearly expecting further activities and were concerned about justifying their I_1 choices. In this light, the similar rather cautious I_1 responses in the Maximum and Minimum Cues conditions are not surprising.

Subjects' expectations appeared to be related to their I_1 selections. Moreover, these differences in expectation and I_1 selection seemed to be related to the pretesting situations in which subjects found themselves. Thus the pilot study was consistent with the suggestion that the pretesting phase may influence group shift results by affecting subjects' expectations and I_1 selections.

(6) Synopsis

Although previous studies have yielded somewhat ambiguous results,

they do suggest that the generalizability of the group shift may be limited by the pretest paradigm. The evidence reviewed indicates that pretesting phase influence may operate through either of two mechanisms. First, expectation of further activities (such as group discussion) may exert some influence on subjects' initial preference selection. Second, the process of choosing the I_1 preferences or the preferences actually chosen may in turn affect later G and I_2 decisions. Furthermore it should be emphasized that consequences for the group shift effect may arise from the simultaneous operation of these factors. In addition, it is possible that failure to control for expectancy effects may have contributed to the ambiguity apparent in the results of previous studies. Experiment I was designed to examine these issues.

EXPERIMENT I

When explicitly informed about a forthcoming group discussion (or further item-related activities), the evidence from previous studies and from the pilot study suggests that subjects tend to select more cautious (or more moderate) initial preferences. That is, they tend to be less likely to endorse alternatives recommending a risky course of action than subjects in standard conditions who have not received explicit information. Although such explicit information has not usually been made available in past group shift studies, it seems plausible that subjects in standard conditions may have been exposed to sufficient cues to discover that group discussion or some other item-related activity was imminent. If anticipation of discussion is generated in the somewhat ambiguous standard condition, then one would expect that I_1 preferences solicited in that condition would be more cautious than those solicited in a condition in which subjects were relatively less inclined to expect discussion. If, on the other hand, the standard condition does not lead to expectation, then I_1 preferences solicited in that condition ought to be the same as those solicited in a condition with minimal expectancy-inducing cues (i.e., an Expect-Nothing condition).

Assessment of the likelihood of expectancy effects has already been attempted by Gaskell et al. (1973). These authors included an expectancy control condition in which subjects were tested alone and were told that a single completion of the items would be their only task. Presumably such a condition should have tapped subjects' true initial

preferences, uninfluenced by possible anticipation. However, these subjects, although told that I_1 completion was their only task, were alone with the experimenter and might have expected further activities. Thus it is possible that the expectancy control condition used by Gaskell et al. was not actually expectancy free. The scores obtained in the Minimum Cues condition of the pilot study, which failed to make subjects think that I_1 completion was their only task, are consistent with this possibility. The present author attempted to devise a more suitable means of retesting this notion. The scores of subjects in a Standard condition were compared to the I_1 scores of subjects in a newly designed Expect-Nothing condition.

A replication of the expectancy control group employed by Gaskell et al. was also undertaken (an Alone-Replication condition). It has been suggested that their expectancy control group, while conceptually similar to the currently proposed Expect-Nothing condition, may have failed to accomplish its intended purpose. While the proposed Expect-Nothing condition could obtain relatively risky I_1 scores because it was indeed expectancy free relative to Gaskell et al.'s manipulation (and to the Standard condition), a conclusive demonstration required a replication. Successful replication of the Gaskell et al. results, in conjunction with the results of the altered Expect-Nothing condition, would increase our confidence that any differences in the Expect-Nothing condition were due to intentional changes in the experimental procedure rather than to other extraneous factors such as a different subject population, experimenter, laboratory set up, etc. Without a replication, any differences between Gaskell et al. (1973) and the present

Expect-Nothing condition could be attributed to such differences rather than to intentionally introduced ones (Aronson and Carlsmith, 1968).

A fourth (Expectancy) condition in which subjects were clearly informed that discussion of the items would follow I_1 completion was included to demonstrate (and thus replicate the previous findings) that explicit warning would lead to cautious I_1 preference selection. It was hoped that this condition would help to determine the limits of expectation effects and serve as an end point or anchor against which to weigh the responses obtained in the other conditions.

Although expectancy is one mechanism which may influence the group shift results, a pretest by treatment interaction is another. However, earlier studies which have examined this aspect of the pre-testing phase have, especially when using CDQ items, been plagued by inconsistent findings. For this reason it was pointed out in the introduction that there is enough uncertainty in the findings to warrant the conclusion that the effects of pretest completion are as yet not clear. Whether or not expectancy influences I_1 selection, there remain at least three possible outcomes at G which might result from an interaction of pretest and group discussion.

First, sensitization, occurring because of I_1 completion, may interact with the treatment in such a way as to enhance or cause the shift. Second, there may be no pretest-treatment interaction and thus no effect on the nature of the shift. Third, sensitization, occurring because of I_1 completion, may interact with treatment in such a way as to suppress or decrease the magnitude of the shift. Most studies have tended to assume that if the pretest is reactive then it must

serve to enhance or cause the shift. The alternative outcome which might result because of a reactive pretest has not been considered. To distinguish between these possibilities a No-Pretest condition was incorporated into the study. Relative to the Standard condition, the results obtained by the No-Pretest condition should facilitate the understanding of pretest-treatment interaction.

One further possibility should be mentioned. Independent examination of expectancy and pretest-treatment interaction may result in an inadequate reflection of the processes occurring in many experiments. Although pretest phase effects may indeed occur either because of expectancy or, because of I_1 completion, it is also possible that both factors operate simultaneously. For example, subjects expecting discussion might well select a somewhat cautious I_1 which in turn might interact with the treatment to enhance or suppress the ensuing shift. An I_1 which interacts (or does not interact) with a treatment may itself have or have not been affected by expectancy.

Thus there were five conditions in the study which employed CDQ items.¹ In the Expect-Nothing and the Alone-Replication conditions subjects completed the pretest and were allowed to leave (The Alone-Replication condition was intended to replicate Gaskell et al.'s expectancy control condition). In the Standard and Expectancy conditions subjects completed I_1 , G and I_2 selections. Subjects in the No Pretest condition immediately started group discussion and thus completed only the G and I_2 preferences.

The uncertain and contradictory results obtained by previous authors and by the pilot study make this examination

of expectancy and pretesting exploratory in nature. This being the case, general statements of an 'if...then' nature were deemed to be more appropriate than specific predictions. For example, one might say that if expectancy occurs then I_1 scores will be less risky in the Standard condition than in the Expect-Nothing condition, but if expectancy does not occur then there should be no between-condition differences in I_1 score selection. The statements deal with three areas of interest:

(1) relationships of mean I_1 scores across conditions, (2) relationships of mean I_1 scores to mean G and I_2 scores both across and within conditions, and (3) relationships of mean G and I_2 scores across conditions.

(1) If the availability of expectancy-inducing cues influences I_1 selection then the more expectation-inducing cues present the less risky will be the mean I_1 scores across conditions (i.e., Expect-Nothing < Alone-Replication < Standard < Expectancy). If expectancy has no influence on I_1 selection then the mean I_1 scores in all conditions should be similar.

(2) The G and I_2 scores of the Standard and Expectancy conditions should be more risky than the I_1 scores of those conditions. That is, a usual group shift should occur. The relationship of the G and I_2 scores of the No Pretest condition to the I_1 scores of other conditions should be dependent on expectancy and on interaction effects. Possible expectancy effects on I_1 scores have been discussed. Therefore it will suffice to describe the relationship of No Pretest G and I_2 scores with respect to the Standard condition I_1 scores. If pretest-treatment interaction neither causes nor suppresses the shift effect then the G and I_2 scores of the No Pretest condition should be more risky than the

I_1 score of the Standard condition. If interaction suppresses the shift then the G and I_2 scores of the No Pretest condition should also be more risky than the I_1 score of the Standard condition (and more risky than the G and I_2 scores of the Standard condition). If interaction causes the shift then the G and I_2 scores of the No Pretest condition should be similar to the I_1 score of the Standard condition and less risky than its G and I_2 scores.

(3) If a pretest-treatment interaction causes the shift then the G and I_2 scores of the No Pretest condition should be less risky than those of the Standard condition. If a pretest-treatment interaction suppresses the shift then the G and I_2 scores of the No Pretest condition should be more risky than those of the Standard condition. If a pretest-treatment interaction does not influence the outcomes then the G and I_2 scores of the No Pretest and the Standard conditions should not differ. If expectancy suppresses the mean I_1 preferences of the Expectancy condition then the G and I_2 scores of that condition should be less risky than those of the Standard condition. If not then they should be similar to those of the Standard condition.

As these statements clearly indicate, failure to obtain significant differences must, in some cases, be considered an acceptable outcome. For this reason care must be taken when interpreting the results. If, for example, there were no differences between I_1 or between G and I_2 preferences, it might be because expectancy had no influence or it might have reflected a manipulation failure. While both significant and nonsignificant results can reflect inadequate manipulations, this

possibility is of greater concern in the face of nonsignificant results (Greenwäld, 1975).

Method

Subjects and Assignment to Groups

The 160 male subjects were recruited for an experiment in 'Decision-Making' from the introductory psychology classes at the University of Western Ontario. Seventy-three subjects were recruited as part of a course requirement from the introductory psychology subject pool. The remaining 87 subjects also came from introductory psychology classes but were recruited for a fee of \$3.00.² There were 32 subjects in each of the five experimental conditions. In the Alone-Replication condition subjects were recruited and appeared at the laboratory singly, whereas in the other four conditions they were recruited and appeared in groups (Sessions) of two to eight subjects. The condition run in a particular session was randomly determined.

Tasks and Dependent Variables

Five CDQ risk items (see Appendix A), selected from among those employed in other studies, were presented to subjects in each of the five conditions. The three dependent measures were solicited on a probability preference scale which allowed endorsement of chances of one to nine in ten. A tenth alternative, scored as ten chances in ten, allowed subjects to choose not to endorse the risky alternative no matter what the chances of success.

The three dependent measures were the subjects' responses to the items during the pretest (I_1), the group decision (G), and the individual posttest (I_2). Pretest preferences were solicited in separate booklets, whereas the group decisions and the I_2 scores were both obtained in the same booklet.

Procedure

Upon arrival at the laboratory, subjects were given participation receipts (paid subjects did not get these) and, except for those in the No Pretest conditions, had their I_1 preferences solicited. With the exception of those in the Alone-Replication condition, subjects were initially seated in the same room and where appropriate were pretested.

Subjects in the No Pretest condition were given their task booklets (see Appendix B) and then read the following instructions:

Please put your name on the upper right hand corner of the booklets. Our main interest in this experiment is concerned with how groups make decisions. I am now going to assign you to rooms in small groups in order to discuss the items in the Questionnaire which you have in front of you. Before I do so I'll read through the cover instructions with you. Please follow along with me as I do so.

The experimenter then read through the cover instructions and assigned the subjects to separate rooms in groups of four to discuss the items.

When the subjects completed the group task they were asked to make postdiscussion individual decisions on the items. The experimenter read them the following instructions (these instructions were used to elicit I_2 preferences in all conditions):

Now that you have finished the group task I would like you to make some final individual decisions. I want you to go back over each of these situations and indicate your own personal decision with the letter "P". That is, in some cases you may feel that the group decision was the best one which could have been made, in which case you would place the "P" on the same line as the check mark. In other cases you may tend to disagree with the group decision, in which case the "P" would be placed on a different line from the one with the check. In each case I want you to indicate what you personally, now, at this moment, feel would be the best decision.

Upon completion of this task, the questionnaires were collected and the subjects debriefed.

In the four conditions employing pretesting the initial instructions used to solicit I₁ preferences were varied according to the level of expectancy-inducing cues desired.

In the Expect-Nothing condition the experimenter handed out the booklets and then said "This should only take a few minutes. I'll read through the cover instructions with you. Please follow along with me as I do so." Most subjects had by this time put their names on the booklets. If any had not they were asked to do so. The cover instructions were modified for this condition through inclusion of the following statement:

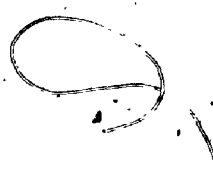
As soon as you have completed the questionnaire you may go. When you have finished leave the questionnaire on the desk in front of you and pick up a copy of the information sheet at the door on your way out. It explains the nature and purpose of the experiment. Please leave quietly so that you won't disturb those who are still working. (Note: Paid subjects were told to "please pick up your money on the way out.")

When the cover instructions had been read the subjects were told to begin. It was reiterated that they should leave quietly and not forget the information sheet (their money).

In the Alone-Replication condition the subject arrived alone and was seated alone in a small room. The experimenter gave them the booklets and read through the cover instructions with them. He then told them that, "I just want your personal decisions for each of these items. When you have completed the items, the experiment will be over and you will be free to go." If subjects had not put their name on the booklet by this time they were asked to do so. When the subject completed his I_1 selections the questionnaire was collected and he was debriefed.

Subjects in the Standard condition were given their task booklets and asked to put their names on them. The cover instructions were then read to them (see Appendix B). When subjects completed the pretest, the questionnaires were immediately collected. As soon as all subjects had completed the pretest the second booklet was passed out and the new cover instructions read to them (see Appendix B). They were then assigned to groups of four and taken to smaller rooms to discuss the items. Upon completion of the group task, I_2 preferences were solicited and the subjects debriefed and allowed to go.

With the exception of some additional expectancy-inducing instructions the Expectancy condition was the same as the Standard condition. When given their pretest questionnaires subjects were asked to put their names on them so that the experimenter "will be able to match your work on this questionnaire with later responses you will be making." The experimenter then said, "please listen," and read:



Our main interest in this experiment is with how groups make decisions. In a little while you are going to be assigned to smaller groups, taken to separate rooms, and asked to discuss and reach unanimous group decisions on the items in the questionnaire which you have in front of you. Before I assign you to groups, however, I want you to first answer the problems as individuals. Select your own preferences carefully since I will want you to represent, or to argue for them during the group discussion. I'll read through the cover instructions with you. Please follow along with me as I do so.

When everyone was finished and the questionnaires collected, the group discussion booklets were passed out and the subjects asked to put their names on them. The cover instructions were read to them, and they were assigned to groups of four to complete the discussion task. Posttest preference solicitation and debriefing followed completion of the group task.

Results and Discussion

The first area of interest dealt with the relationship of pretest scores across the Expect-Nothing, Alone-Replication, Standard and Expectancy conditions. It was suggested that the more expectancy-inducing cues available in a condition the less risky would be the I_1 scores in that condition. This was tested by a 4 by 2 (Condition by Payment) unweighted means analysis of variance performed on the subjects' mean I_1 scores of the four conditions employing pretests (see Table 3; condition by decision means for Experiment I are presented in Table 1).³ No significant results were obtained for the Condition or Payment main effects, suggesting that neither expectancy manipulation nor payment of

subjects consistently influenced pretest preference selection. Although payment *per se* did not seem to have an influence on I_1 selection in every condition, the significant Condition by Payment interaction (see Table 2), indicated that its influence was felt in conjunction with specific expectancy manipulations. Since this interaction was not directly relevant to the issue currently being examined, a discussion of its implications will be postponed until later.

The second area of interest was concerned with the traditional group shift effect; the differences obtained between the I_1 and the G and I_2 scores. It was predicted that the G and I_2 scores of the Standard and Expectancy conditions would be more risky than the I_1 scores of those conditions. The first portion of this prediction (I_1 to G differences) was tested by a 2 by 2 by 2 (Condition by Decision by Payment) unweighted means analysis of variance on group mean I_1 and G scores with repeated measures on the Decision factor (see Table 4). As indicated by the significant Decision main effect, an overall group shift to risk was obtained. Moreover, *t* tests using the error term in the analysis of variance showed that the I_1 to G shift for the Standard condition, was marginally significant, $t(12) = 1.854$, $.05 < p < .10$ whereas that for the Expectancy condition, was statistically significant $t(12) = 3.220$, $p < .01$.

The failure of the Condition and Payment main effects to attain acceptable levels of statistical significance suggests that neither information about forthcoming discussion nor the fee for participation exerted an influence on subjects' responses. These findings vouch for the robustness of the shift effect.

TABLE 1

EXPERIMENT I: Condition by Decision Means for all Conditions

Condition	All Subjects ^a			Subset of Subjects ^b	
	I ₁	G	I ₂	I ₁	I ₂
Expect-Nothing	5.156				
Alone-Replication	5.556				
Standard	5.338	4.925	4.969	5.492	4.967
Expectancy	5.025	4.225	4.513	4.883	4.350
No Pretest		4.225	4.406		

^a The means in the first three columns were based on the total sample and were employed in the analyses using group means as data units.

^b The means in the fourth and fifth columns were based on a randomly selected subset of the subjects and were used to examine the I₁ to I₂ shift (see Footnote 4).

TABLE 2

EXPERIMENT I: Condition by Payment Means for the I_1 scores, illustrating the significant Condition by Payment Interaction

Payment of Subjects	Condition			
	Expect- Nothing	Alone- Replication	Standard	Expectancy
Non-Paid	5.671	5.978	5.075	4.850
Paid	4.573	5.014	5.600	5.130

TABLE 3

EXPERIMENT I: Analysis of Variance Summary Table for the
 Condition by Payment (4 x 2) Unweighted Means Analysis
 on the I_1 scores: Subject Means as Data Points^a

Source	df	MS	F
Condition (C)	3	1.581	0.814
Payment (P)	1	3.087	1.588
C X P	3	5.461	2.810*
Subjects	120	1.944	

* $p < .05$

^a This analysis was performed on all subjects who completed the pretest.

TABLE 4

EXPERIMENT I: Analysis of Variance Summary Table for the
 Condition by Decision by Payment (2 x 2 x 2) Unweighted Means
 Analysis on I₁ and G Scores: Groups Means as Data Points

Source	df	MS	F
Condition (C)	1	1.932	1.356
Payment (P)	1	0.906	< 1
C X P	1	1.210	< 1
Groups (G)	12	1.425	---
Decision (D)	1	2.468	12.458*
C X D	1	0.179	< 1
D X P	1	0.028	< 1
C X D X P	1	0.577	2.911
D X G	12	0.198	---

* p < .005

An additional analysis of variance, using subjects' mean scores as data units, was undertaken to determine whether or not the I_2 scores were more risky than the I_1 preferences.⁴ This additional test was undertaken using a $2 \times 2 \times 2 \times 3$ (Condition by Decision by Payment by Groups) analysis of variance with repeated measures on the Decision factor (see Table 5).⁵ The significant Decision main effect indicated that an overall I_1 to I_2 shift to risk was obtained. t tests using the error term for the analysis of variance showed that the I_1 to I_2 shifts for both the Standard, $t(8) = 3.472$, $p < .01$, and the Expectancy, $t(8) = 3.520$, $p < .01$, conditions were statistically significant. Neither the main effects for Condition and Payment nor any of the interactions attained statistical significance. The statistically significant Group main effect reflects the considerable variability from one discussion group to the next.

Next, consider the relationship of the G score of the No Pretest condition to the I_1 score of the Standard condition. If the pretest did not interact with the treatment to cause the shift, then the G score of the No Pretest condition should be more risky than the I_1 score of the Standard condition. If the completion of a pretest is instrumental in causing the shift, then the G score should not be more risky than the I_1 of the Standard condition. A planned independent t test used to compare these scores attained statistical significance, $t(14) = 2.413$, $p < .05$, and thus furnished support for the notion that the shift is not caused by pretest completion in interaction with the treatment.

Such a finding is, however, not commonplace in the group shift literature. A number of authors (Baron et al., 1974; Carlson and Davis,

1971; McCauley et al., 1971) have found that although a significant within group shift occurs very consistently, the "shift" is much less likely to reach traditional levels of statistical significance if an independent groups design is employed. Baron et al. (1974) suggested that perhaps the shift, while reliable, may account for a small enough portion of the variance that it will not reach significance unless intersubject variability is controlled by a within groups repeated measures design. The current study did not support that conclusion.

A 3 by 2 by 2 (Condition by Decision by Payment) analysis of variance with repeated measures on the Decision factor was performed to examine G and I_2 score differences across conditions (see Table 6). None of the main effects and none of the interactions were significant. Thus payment or the lack of it did not influence subjects' responses, and, as expected, there was no significant difference between the G and I_2 scores.

The failure to find a Condition main effect is consistent with the results of the preceding two analyses and supports the findings about expectancy and pretest completion. First, explicit information that group discussion was imminent did not seem to influence subjects' group decisions or posttest preferences. There was no significant difference between the G and I_2 scores of the Expectancy and Standard conditions. Second, pretest completion did not appear to be causally implicated in the group shift. The G and I_2 scores of the No Pretest condition were not significantly less risky than those of the Standard condition. More to the point, they were not significantly more risky than the G and I_2 scores of the Standard condition. Thus there is no support for the notion that pretesting suppresses the extent of the shift effect.

TABLE 5

EXPERIMENT I: Analysis of Variance Summary Table for the
 Condition by Decision by Payment by Groups (2 x 2 x 2 x 3) Analysis
 on I_1 and I_2 Scores: Subject Means as Data Points^a

Source	df	MS	F
Condition (C)	1	9.004	1.189
Payment (P)	1	3.300	< 1
C X P	1	3.920	< 1
Decision (D)	1	6.720	18.245**
D X C	1	0.000	< 1
D X P	1	0.094	< 1
D X C X P	1	0.634	1.721
Group (G)	8	7.575	2.347*
Subjects (S)	36	3.277	---
D X G	8	0.368	≤ 1
D X S	36	0.455	---

* $p < .05$

** $p < .005$

^a This analysis was performed on a subset of the subjects who participated in the experiment (see Footnote 4).

TABLE 6

EXPERIMENT I: Analysis of Variance Summary Table for the
 Condition by Decision by Payment (3 x 2 x 2) Unweighted
 Means Analysis on G and I₂ Scores: Group Means as Data Points

Source	df	MS	F
Condition (C)	2	1.906	< 1
Payment (P)	1	1.175	< 1
C X P	2	1.139	< 1
Groups (G)	18	2.113	---
Decision (D)	1	0.285	1.712
C X D	2	0.037	< 1
D X P	1	0.026	< 1
C X D X P	2	0.150	< 1
D X G	18	0.166	---

At this point a few observations may be offered. First, there was no support for the notion that a pretest-treatment interaction is causally implicated in group decision selection. Second, expectancy, as manipulated across the experimental conditions, did not differentially influence selection of I_1 , G, and I_2 preferences. Finally, the usual reliable group shift effect was obtained. It did not seem to be unduly influenced by the conditional variations in the pretesting situations.

Reconsider now the significant Condition by Payment interaction obtained in examining the I_1 score differences (see Table 2). This interaction offered clues about the effects of the experimental manipulations and necessitated some modification of the preceding conclusions about expectancy. It suggested that expectancy was not without some influence in the study. Subjects responded differently to the various treatments depending on whether or not they were paid for participation in the experiment. Across levels of payment there were greater differences in I_1 selection in the two conditions where subjects were alone and/or not likely to expect further exposure to the items than in the Standard and the Expectancy conditions. Within levels of payment it can be seen that among subjects who were not paid those in the Expect-Nothing and Alone-Replication conditions selected somewhat more cautious preferences than those in the other two conditions. The import of this interaction for the present study is not clear. It implied that under certain circumstances expectations, as manipulated, influenced I_1 selection. However, there is ample opportunity to develop any number of plausible, or at least possible, specific interpretations.

The manipulations may have resulted, as intended, in subjects having certain expectations, but these expectations may have influenced responses only in specific situations. Second, the manipulations may have lead to expectations, but the expectations may not have influenced responses; some other aspect of the manipulation may be responsible. Third, the manipulations may not have influenced subjects' expectations in the least; the differences may be due to some other aspect of the manipulations. Fourth, contrary to prediction, aroused expectations may lead to the selection of still more risky initial preferences. Moreover, it is possible that payment of subjects may have confounded the issue by serving as an additional expectancy-inducing cue. Subjects may perceive that \$3.00 for a few minutes work is too much and thus may be inclined to expect further work for their money.

Two points are apparent. First, the expectancy manipulations in conjunction with payment did have some effect on initial preference selection. If anything this influence would appear, at least for the non-paid subjects, to be in a direction opposite to that predicted. Second, it is impossible to determine if this occurred because of subjects' expectations or because the manipulation had no effect or for some other reason. In retrospect it is clear that a manipulation check should have been employed to assess the nature of pretesting phase expectations.

The goal of this thesis was to enhance the generalizability of the group shift phenomenon by clarifying the effects of the pretesting phase. Experiment I, which left a number of unanswered questions, was obviously not too successful in this regard. First,

the influence of subjects' expectations is quite unclear. The present study failed to replicate the findings of past studies which suggested that subjects who have explicit information about forthcoming discussion select more cautious or moderate initial preferences than those in Standard conditions. Why did this failure to replicate occur? In light of the interaction which indicated that expectation, as manipulated, is not without influence on subjects' initial preference selection, further examination of expectancy effects seemed crucial. Second, what is the relationship of the expectancy manipulation to actual expectations during the pretesting phase? Answers to this question, made salient by the current results, would involve the employment of some form of manipulation check. Third, what is the influence of pretest completion on G and I_2 selection? The results of the present No Pretest condition, suggesting that pretest completion is not implicated, are at variance to the findings of many previous studies. The above questions are sufficient to indicate that the stated goal has not been reached. The need for further examination of these questions was apparent.

There are, however, other related issues which may be raised and which, given the need to undertake further experimentation, may also be examined. First, in the context of an Expect-Nothing manipulation, an attempt will be made to further assess the reliability and robustness of the shift. Second, a procedural issue (to what extent do subjects assimilate the oral expectancy-inducing instructions, and what influence does this have on their preference selection) will be scrutinized. Third, subjects' degree of anonymity during the

experiment should be considered. Personal identification with one's opinion should have an important bearing on statements of that opinion. These three issues were examined in the next two studies.

A reassessment of the influence of pretest completion, of the replicability of the expectancy condition results and of subjects' assimilation of verbal expectancy instructions was undertaken in Experiment II. Experiment III examined the anonymity question and the issue of whether or not a group shift could be obtained in an Expect-Nothing condition. Post-experimental questionnaires designed to elicit subjects' expectations at the time they were working on the pretest were incorporated into both experiments.

EXPERIMENT II

Consider the results of the Expectancy condition in the previous experiment. Contrary to prediction, subjects who should have expected discussion were not more cautious than subjects who should have been less likely to expect discussion. They were, if anything, more risky. One might attribute this result to a number of things. Perhaps subjects' expectations were not what might be expected according to the manipulations. Perhaps the subject population (subjects were run in the later part of the second term in a period when tests and essays tend to abound and when final examinations are fast approaching) was unusual, or the subjects, concerned about their own problems, were simply not paying attention to the experimenter. Whatever the reason it is important to attempt to replicate the Standard and Expectancy conditions. If the Expectancy manipulation does result in more cautious I_1 selection than the Standard manipulation, then it may be concluded that the results of the first experiment were due to chance or at least that some uncontrolled factor was operating. If not then some reconsideration of the effects of expectation should be undertaken.

A second issue involved the question of whether or not subjects actually heard and understood the explicit information telling them that group discussion was forthcoming. The expectancy instructions in the first experiment were presented orally; they were not written on the questionnaire. Perhaps the participants may not have heard or assimilated them. The instructions were read at the beginning of the session, and it is possible that many participants were still occupied putting their

names on the questionnaires, glancing at the written instructions, getting out pens or pencils, or otherwise not paying full attention to the experimenter. If this was the case, it may have had implications for the preceding results. In order to guard against this possibility, a condition (Explicit Expectancy) was included in which the expectation-inducing instructions were presented both orally and in written form. The manipulation was intended to ensure that subjects expect discussion, or at least that they will be quite aware that it was mentioned.

In Experiment I the I_1 score of the Standard condition was significantly less risky than the G score of the No Pretest condition and tended to be ($p < .10$) less risky than the I_2 score of the No Pretest condition. This finding is not inconsistent with the outcomes of some previous research on pretesting effects (Gaskell et al., 1973; Myers et al., 1974), however, it is not the most common finding. A majority of studies concerned with this problem have found that scores in No Pretest conditions are not significantly more risky than the I_1 scores in Standard conditions. It is not yet possible to reach consensus on the influence of pretesting. Perhaps such consensus will eventually be obtained only through a large number of replications comparing Standard and No Pretest conditions. Since Experiment II was necessary in order to examine the crucial expectancy question, and since it was of considerable interest to do so, a No Pretest condition was incorporated into this study. Even if not leading to significant results, it will at least provide additional data about the responses of subjects who have not completed a pretest.

Finally, a post-experimental questionnaire was included in order to assess subjects' expectations while they were filling out the items during the pretesting phase. This allowed an estimate to be made about the relationship between the experimental manipulations and subjects' stated expectations.

Experiment II thus incorporated four conditions: an Expectancy condition, a Standard condition, a No Pretest condition and an Explicit Expectancy condition. The first three conditions were essentially replications of those used in the first study; the last was new. In essence this experiment was attempting to reexamine the findings in the first experiment and to examine an additional factor (instructional clarity) which might have a bearing on the results.

Method

Subjects and Assignment to Groups

One hundred and twenty males from the introductory psychology subject pool participated in the experiment as part of a course requirement. The subjects were tested in sessions ranging in size from three to nine individuals. Within each session subjects were randomly assigned to three-man groups to discuss the items. The condition run in a particular session was randomly determined. Subjects who completed a pretest were assigned to groups immediately after its completion, whereas subjects in the No-Pretest condition were assigned to groups as soon as they had received the initial instructions. Thirty subjects were tested in each of the four conditions.

Tasks and Dependent Variables

Six CDQ items chosen from among those employed by previous authors were employed (five of the items were also used in Experiment I; see Appendix A for the additional item). As before, subjects responded three times to each of the items (twice in the No Pretest condition). The responses were made on the traditional CDQ item scale of one, three, ..., nine chances in ten with an additional alternative which was scored as ten chances in ten and which allowed subjects to reject the riskier alternative no matter what the chances of success.

In addition, a post-experimental questionnaire designed to elicit subjects' relevant expectations during the pretesting phase was incorporated into the experiment (see Appendix C). Subjects were asked to indicate whether or not they expected discussion and to what extent they anticipated having to justify their preferences to the experimenter or to their fellow subjects. Subjects in the No Pretest condition did not fill out this questionnaire; the first contact they had with the items was during group discussion.

Procedure

The Standard, Expectancy and No-Pretest conditions in this study were (with the exception of the post-experimental questionnaire presented after I_2 completion) conducted in a manner identical to that of the first experiment. The Explicit Expectancy condition differed from the Expectancy condition in that the expectancy-inducing instructions were included on the coversheet of the pretest booklet (see Appendix B) and were read aloud to the subjects as they followed along.

Results and Discussion

The overall condition by decision means are presented in Table 7. The CDQ item results were analysed in a manner analagous to that used in the first experiment.

A one way analysis of variance (3 levels of Condition) employing subjects' mean scores as the units of analysis was used to examine differences between I_1 scores (see footnote 3 and appendix F). The marginally significant Condition effect, $F(2,87) = 2.863$, $p < .10$, indicates that subjects exposed to expectancy-inducing, initial instructions tended to select more cautious initial preferences than those in the Standard condition. A t test performed using the analysis of variance error term indicated that the I_1 scores of subjects in the Expectancy condition were significantly less risky than those of subjects in the Standard condition, $t(87) = 2.335$, $p < .05$. The I_1 scores selected in the Explicit Expectancy condition, while less risky than those in the Standard, were not significantly so, $t(87) = 1.623$, N. S.). These findings are consistent with the original suggestion that expectation influences I_1 selection and with results obtained in previous studies concerned with the effects of expectancy. They are not consistent with the findings of Experiment I and hint that those findings should be considered with scepticism. The weight of evidence previously cited would suggest that expectancy should have some moderating influence. On the other hand, the magnitudes of the present differences were small and did not inspire a great deal of confidence in the conclusion that expectation of discussion results in cautious I_1 selection.

The occurrence of a shift to risk was examined by an analysis of variance using I_1 and G group means as data units, and by t tests completed using the analysis of variance error term. The only significant result obtained by the 3 by 2 (Condition by Decision) analysis with repeated measures on the Decision factor was for the Decision factor (see Table 8). This finding mirrors those of the first experiment; an overall group shift to risk did occur. The I_1 to G shift in the Explicit Expectancy condition was statistically significant, $t(27) = 2.427$, $p < .05$. The corresponding shift in the Standard condition was marginally significant, $t(27) = 2.045$, $p < .10$, whereas that in the Expectancy condition was not significant, $t(27) = 1.258$, N.S. The Condition main effect in this analysis was not significant, indicating that the expectancy manipulations did not evoke overall response differences to the items.

A 3 x 2 x 10 (Condition by Decision by Groups) analysis of variance with repeated measures on the Decision factor was used to determine whether or not the I_2 scores were more risky than the I_1 preferences (see Table 9). The subjects' mean scores were employed as units for the analysis. The significant Decision main effect indicated that an overall I_1 to I_2 shift to risk occurred. t tests using the error term for the analysis of variance obtained results similar to those found in the I_1 to G analysis. The shift in the Explicit Expectancy condition was statistically significant, $t(27) = 2.848$, $p < .01$. In the Expectancy, $t(27) = 1.709$, $p < .10$, and the Standard, $t(27) = 1.957$, $p < .10$, conditions the shifts were marginally significant. The non-significant Condition main effect indicated that the manipulations did not influence

subjects' overall I_1 and I_2 selections. The significant Groups main effect reflects overall decision variability from one group to the next, while the Decision by Groups interaction suggests that the groups varied in their responses from the I_1 to the I_2 decisions.

Comparison of the Standard and the No Pretest conditions once again leave the influence of pretest completion uncertain. As can be seen in Table 7, the mean I_1 score of the Standard condition is quite similar to the G, $t(18) = 0.381$, N.S., and the I_2 , $t(18) = 0.184$, N.S., scores of the No Pretest condition. Based on this examination of the means, pretest completion would appear to be implicated in the group shift effect (such a conclusion is not without precedent in the group shift literature; Castore, 1972). On the other hand, the G and I_2 scores of the No Pretest condition were not less risky than the corresponding scores of the Standard condition. The 4 by 2 (Condition by Decision) analysis of variance (with repeated measures on the Decision factor) using the group mean G and I_2 scores as data units did not reveal a significant Condition main effect (see Table 10). Thus, neither expectations, as manipulated, nor pretesting seems to affect subjects' group decisions or postdiscussion preferences.

On the surface this finding is similar to that in the first experiment (the G and I_2 scores of the No Pretest condition were not significantly less risky than those of the Standard condition). However, it should be pointed out that the relationship of the Standard and the No Pretest conditions is quite different when the first and the second experiments are compared. In the previous experiment the scores of the No Pretest condition were somewhat more risky (although not significantly

TABLE 7

EXPERIMENT II: Condition by Decision Means for all Conditions

Condition	I_1	G	I_2
Standard	5.156	4.650	4.711
Expectancy	5.939	5.633	5.556
Explicit Expectancy	5.700	5.100	5.061
No Pretest		5.033	5.212

TABLE 8

EXPERIMENT II: Analysis of Variance Summary Table for the
 Condition by Decision (3 x 2) Analysis on I₁ and G Scores:
 Group Means as Data Points

Source	df	MS	F
Condition (C)	2	3.946	2.403
Group (G)	27	1.642	---
Decision (D)	1	3.345	10.946*
C X D	2	0.108	< 1
D X G	27	0.306	---

* $p_e < .005$

TABLE 9

EXPERIMENT II: Analysis of Variance Summary Table for the
 Condition by Decision by Groups (3 x 2 x 10) Analysis on
 I_1 and I_2 Scores: Subject Means as Data Points

Source	df	MS	F
Condition (C)	2	9.899	1.885
Group (G)	27	5.250	3.117**
Subjects (S)	60	1.684	
Decision (D)	1	10.675	14.141**
C X D	2	0.271	
D X G	27	0.755	1.723*
D X P	60	0.438	

* $p < .05$

** $p < .001$

TABLE 10

EXPERIMENT II: Analysis of Variance Summary Table for
 the Condition by Decision (4 x 2) Analysis on the
 G and I₁ Scores: Group Means as Data Points

Source	df	MS	F
Condition (C)	3	2.782	1.355
Group (G)	36	2.054	---
Decision (D)	1	0.022	1
C x D	3	0.066	1.170
D x G	36	0.057	---

so) than the corresponding ones of the Standard condition, whereas in the present experiment they are somewhat less risky. Rather than contributing to its solution, the additional data about the effects of pretest completion would seem to have complicated the issue.⁶

Finally, consider the results obtained in the post-experimental questionnaire designed to elicit subjects' expectations during the pretesting phase of the experiment. Responses were solicited to three questions.⁷ A yes or no response to the statement "When you were working on the items did you think that you would be discussing them with the others." was requested. In addition, subjects were asked to indicate to what extent they expected to have to justify their responses to the experimenter or to the other subjects present during the pretesting phase. These two questions were answered on seven-point scales ranging from "very unlikely" to "very likely". These results are presented in Table 11.

As can be seen, subjects responding to the first question in the Standard condition were less prone to say they expected discussion than those in the Expectancy, $\chi^2(1) = 6.239$, $p = .02$, or in the Explicit Expectancy, $\chi^2(1) = 11.915$, $p < .001$, conditions. Thus the expectancy manipulations did work. However, they did not seem to work well. Only 14 of 30 subjects in the Expectancy condition admitted to thinking that discussion would follow. This suggests that at least half the subjects did not assimilate the information contained in the initial verbal instructions or perhaps that they did not believe the experimenter.

The results obtained in the Explicit Expectancy condition are still more perplexing. A few more subjects in this condition (18 of 30)

admitted to anticipating group discussion than in the Expectancy condition. The fact remains, however, that 12 of 30 subjects in the Explicit Expectancy condition, after having the opportunity of reading the instructions and having had the instructions read to them, did not indicate that they felt group discussion was imminent. While the instructions in the Expectancy condition might not have been heard or assimilated, this explanation seems less applicable to the Explicit Expectancy condition. Perhaps the instructions were heard and understood but were simply not believed.

In addition to obtaining subjects' responses on the first Impressions Questionnaire, the experimenter also solicited oral responses from a number of the subjects who checked the 'no' response in the Explicit Expectancy condition. Although the experimenter assumed that disbelief of the instructions would be the main reason for the 'no' response, this was not the case for any of the six subjects questioned. They seemed somewhat perplexed by the inquiry and tended to respond with hesitancy. When shown the printed instructions and asked if they perhaps had not believed them, three of the subjects openly admitted that they hadn't really paid much attention to the instructions. The other three said essentially the same thing but with much less candor. It seemed that the Explicit Expectancy condition was not nearly as explicit as desired.

It was suggested (Experiment I) that people who expected group discussion might be more likely to select cautious initial preferences. The purpose of the present manipulations was to make people expect discussion in order to see what effect it would have on their I_1 selection. Since two fifths of the subjects said they did not expect discussion

TABLE 11

EXPERIMENT II: Mean Responses to Questions 1 and 2 of the First Impressions Questionnaire by Condition and (for Question 2) by Question 1 Responses

Condition	Response	Number of Subjects	(1) Did you expect discussion?		(2) Justify to Experimenters		(2) Justify to Others	
			No	Yes	No	Yes	No	Yes
Standard	No	(25)	2.60	2.50	2.76	2.80		
	Yes	(5)	2.00		3.00			
Expectancy	No	(16)	2.50	3.30	3.44	4.27		
	Yes	(14)	4.21		5.21			
Explicit Expectancy	No	(12)	2.83	2.90	3.42	4.40		
	Yes	(18)	3.00		5.06			
Collapsed Across Conditions	No	(53)	2.62	2.91	3.17	3.82		
	Yes	(37)	3.32		4.84			

TABLE 12

EXPERIMENT 11: Analysis of Variance Summary Tables for the Condition by No-Yes Responses (3 x 2) Unweighted Means Analyses performed on Responses to Question 2 of the First Impression Questionnaire

Source	df	MS	F
Justification to the Experimenter			
Condition (C)	2	6.598	1.835
No-Yes (N-Y)	1	3.200	1
C X N-Y	2	8.130	2.261
Subjects	84	3.596	---
Justification to the Others			
Condition (C)	2	13.636	3.067*
No-Yes (N-Y)	1	30.706	6.887**
C X N-Y	2	5.440	1.220
Subjects	84	4.459	---

* p < .10

** p < .025

(and the questioned subjects indicated that they did not hear or were not aware of the expectancy instructions), it would appear that the manipulation was only partly successful. Thus, expectancy influence in the conditions as a whole would have been attenuated, and perhaps for some subjects non-existent. A post-hoc examination of the initial preferences was consistent with this notion. The mean F_1 scores across the Standard, Expectancy, and Explicit Expectancy conditions of subjects who said they expected discussion was 5.905, while that of subjects who said they did not expect discussion was 5.384. This difference was marginally significant, $t(88) = 1.861$, $p < .10$.

The two parts of the second postexperimental question solicited expectations about the likelihood of initial preference justification either to the experimenter or to the other subjects. Mean responses to these questions are summarized in Table 11. The perception of having to justify initial responses to the experimenter did not differ as a result of experimental condition (see unweighted means analysis of variance summary, Table 12). Moreover, this possibility was not perceived as very likely in any of the three conditions. On the other hand, subjects' perception of whether or not they would have to justify their initial preferences to the others did vary somewhat across conditions. Subjects in the Expectancy and the Explicit Expectancy conditions were slightly more likely to assume that the experiment would involve justifying their initial preferences to others than were those in the Standard condition (see Table 12). This reinforced the notion that the expectancy manipulations worked, since in addition to discussion of the items, a justification to others of initial position on those items was seen as likely.

The perceived amount of justification to others as a function of No-Yes responses to the first question were also consistent with this notion. As indicated by the unweighted means analysis of variance (see Table 12) and as evident in Table 11, amount of perceived justification to others differed as a function of No-Yes responses while perceived justification to the experimenter did not. That is, across conditions subjects who said that they expected discussion were more likely to anticipate justification of initial responses to others than were subjects who said that they did not expect a forthcoming discussion. Examination of Table 11 indicates that this was especially true of subjects in the Expectancy and the Explicit Expectancy conditions. The likelihood of impending justification as perceived by subjects who said 'no' in the expectancy conditions was below the midpoint of the response scale. This is consistent with and supports the anecdotal evidence obtained by asking some subjects in the Explicit Expectancy conditions why they said 'no'. These subjects said no, told the experimenter they were not aware of the instructions, and were little inclined to expect justification to others.

The results of this experiment suggest that expectation of discussion is related to more cautious initial preference selection. Subjects in the expectancy conditions tended to select more cautious initial preferences on the CDQ items than subjects in the Standard condition. They also tend to be more inclined to say that they expect to have to discuss the items and to justify their responses to others. Moreover, subjects who said that they expected group discussion were more likely to expect to have to justify their preferences to others than subjects who said

they did not expect discussion. As yet, however, there is no evidence that expectations influence the later selection of G and I₂ preferences. Unfortunately, the influence of expectation may have been attenuated as a result of many subjects' failure to be aware of the expectancy instructions. Since every subject should at least know what the instructions said (whether or not they believe them), the need for a very strong expectancy manipulation was evident. The Expectancy condition incorporated in Experiment III was designed in an attempt to meet this requirement.

The No Pretest condition lived up to its reputation of providing ambiguous results across studies. The cautious trend in this condition in the present study hinted that pretesting completion may be implicated in the group shift. Since this finding was contrary to that of Experiment I, it was decided that a further replication of the No Pretest condition would be useful.

EXPERIMENT III

Intuitively, personal identification with one's opinion would seem to have an important bearing on statements of that opinion. Previous research supports this notion. For example, nonanonymous subjects tend to conform more to a group norm (Deutsch and Gerard, 1955), and when anticipating future interaction tend to choose preferences more in line with a perceived group norm (Raven, 1959). There is also some evidence that perceived anonymity may influence preference selection on the CDQ items. Baron et al. (1974) found that anonymous subjects select I_2 preferences which are significantly more cautious than their G scores, whereas subjects who affixed their names to the questionnaires selected I_2 preferences which were not different from the group decisions. In some degree, anonymity may influence I_1 selection in a very similar fashion to that suggested for expectation. Presumably in an Expectancy condition subjects should expect discussion involving, or justification of, their initial preferences and should select opinions which are easy to defend or justify. If identifiability makes subjects more inclined to feel personally responsible for or associated with their opinions, they also may be more inclined to select initial alternatives which are easy to defend or justify.

Anonymity should be examined. It may be implicated in subjects' initial preference selections and may modify the influence of the expectation manipulations. Half the subjects in this experiment were asked to put their names on the questionnaires whereas the remaining

half put no personal identifiers on any of the experimental materials.

An overall group shift to risk was obtained in both Experiments I and II and for the most part seems to be quite reliable. The notion that the shift is reliable would be further enhanced if it was found that subjects in Expect-Nothing and in Alone-Replication conditions who actually discussed the items in groups shifted to risk. Evidence that a shift would occur in such conditions would be important. It would hardly be worthwhile to be greatly concerned about the influence of a paradigm on, or the generalizability of, a weak and unreliable phenomenon. Since the reliability of the group shift has already been questioned (e.g., Clark and Willemis, 1969), and since it is an important issue, its limits should be clearly delineated.

Experiment III incorporated a condition in which discussion groups were formed after pretesting using an Expect-Nothing manipulation. The manipulation was somewhat different from, and perhaps not as powerful as, that used in the first experiment. The difficulty with using a manipulation identical to that in Experiment I lies in the instruction that subjects leave the room quietly when they complete the questionnaire. Obviously the present subjects could not be told to leave. If such instructions were given and then rescinded at the last moment, anger or some form of reactance (e.g., Brehm, 1965) might be the dominant factor in the remainder of the experiment. Thus the manipulation, while similar to that in the first experiment, did not include explicit instructions telling

subjects that they could leave after I_1 completion. Instead, an attempt was made to word the instructions such that the possibility of leaving would be strongly implied rather than explicitly stated.

Experiment II found expectation effects which were more consistent with current predictions and previous evidence than did Experiment I. However, these results were not strong and the manipulation check suggested that many subjects in the two expectancy conditions did not actually expect discussion. Post hoc examination of pretest responses based on whether or not subjects admitted to expecting discussion revealed differences consistent with the hypothesis. Perhaps if discussion had been almost universally anticipated in the high expectancy conditions, the results would have more strongly supported the predictions. In order to achieve or at least to closely approach this end, it would appear that a very strong expectancy manipulation must be used. Such a manipulation was designed and incorporated into the study in order to obtain further data about the influence of subjects' expectations.

Finally, the No Pretest condition in the previous experiment did not really help to clarify the influence of pretest completion on the selection of G and I_2 preferences. Given the rather different conclusions spawned by examining the findings across past studies, the present results were perhaps not surprising. They did suggest, however, that further replication was important. Perhaps a consensus of findings or even an "averaging" of results across a number of studies, rather than obtaining consistent significant results, may be the only way in which the influence of pretest completion can

be determined. With this in mind, and in view of the considerable variability both of previous studies and of the first and second experiments, a No Pretest condition was employed in the third.

Experiment III consisted of four conditions (Standard, Expect-Nothing, Expectancy, and No Pretest) each of which tested half of the subjects anonymously (i.e., no names or personal identifiers were used) and the other half nonanonymously (i.e., subjects signed their names to the questionnaires). In addition, a First Impressions Questionnaire was filled out in the first three conditions after the I_2 preferences had been solicited. This questionnaire was quite similar to that which was employed in Experiment II.

Method

Subjects and Assignment to Groups

Thirty male subjects from the introductory psychology subject pool, participating in the experiment as part of a course requirement, were assigned to each of the four conditions. Subjects were run in sessions ranging from three to nine in size; each session was randomly assigned to a particular condition. Within each session subjects were in turn assigned to three person groups to discuss the items.

Tasks and Dependent Variables

Subjects completed I_1 , G and I_2 preferences for four CDQ items (see Appendix A) selected from among those already employed. Responses were made on the traditional CDQ item probability scale. Following I_2

completion, a First Impressions Questionnaire (see Appendix C) similar to that employed in Experiment II was given out in all but the No Pretest condition.

Procedure

The anonymity manipulation consisted of whether or not subjects signed their names to the questionnaires. For the 60 subjects who signed the questionnaires the basic procedure did not differ from previous studies. No space was provided on the cover sheets for the other 60 subjects to sign their names. In addition the experimenter, while passing out the pretest booklets, said, "Please don't put your names on the questionnaires, I don't need those." After I_1 completion the subjects were assigned to groups and the group discussion questionnaire passed out. While passing out the questionnaire the experimenter told subjects to write their group number on both questionnaires. He then collected the I_1 questionnaire, read the group discussion instructions and took subjects to their rooms to discuss and reach decisions on the items. Upon completion of all dependent measures the subjects were given paper clips and asked to clip their own pretest, group decision, and first impressions questionnaires together. The subjects were then debriefed and allowed to leave. The Standard and No Pretest conditions were, with the exception of the anonymity manipulation, identical to those in the preceding experiment.

In the Expect-Nothing condition the experimenter, while passing out the I_1 questionnaire, said "OK, this should only take a few

minutes. I'll read through the cover instructions with you. Please follow along as I do so." The last sentence of the cover instructions constituted the remainder of the Expect-Nothing manipulation. It said:

When you have completed the questionnaire please remain seated so that you will not disturb others who are still working on the items.

It was hoped this statement would imply, in conjunction with the initial comment, that the reason people were not allowed to leave immediately was because they would disturb others. The remainder of the condition was conducted exactly like the Standard condition.

In the Expectancy condition the paragraph which constituted the manipulation was placed on a separate cover sheet (see Appendix B) which included the instruction that subjects not turn the page until asked to do so. After the questionnaires were passed out the experimenter asked the subjects to follow along and read through the expectation instructions with them. The experimenter then told subjects to turn the page, read through the cover instructions with them, told them to go ahead, and said "when you finish the items I'll assign you to your groups to discuss them." From this point the procedure mirrored that of the Standard condition.

Results and Discussion

Differences between the I_1 preferences of the Standard, Expect-Nothing and Expectancy conditions were examined by a 3 by 2 (Condition

by Anonymity) analysis of variance (see Footnote 3 and Appendix F). The nonsignificant Condition main effect, $F(2,84) = 0.855$, N.S., indicated that although the I_1 scores were in a direction consistent with the hypotheses the trend was not significant (condition by decision means are presented in Table 13). This failure to find a significant expectancy effect is not too surprising. Pretest differences in the preceding study were not robust and would not of necessity suggest similar results in a second study. On the other hand, the stronger expectancy manipulation in the present experiment would lead one to think that if differences were going to be obtained, then they should have been found here.

The Expect-Nothing manipulation did not result in different I_1 selection from that in the Standard or the Expectancy conditions. Lack of significant differences also characterized the Anonymity factor, $F(1,84) = 1.103$, N.S., and the Condition by Anonymity interaction, $F(2,84) = 0.164$, N.S. Thus, neither degree of anonymity nor subjects' expectations, at least as manipulated in this experiment, affected the selection of pretest preferences.

A 3 by 2 by 2 (Condition by Decision by Anonymity) analysis of variance with repeated measures on the Decision factor was used to examine differences between the group mean I_1 and G scores in the Standard, Expect-Nothing and Expectancy conditions (see Table 14). The absence of an Anonymity main effect and of any interaction with anonymity argue for the lack of influence of this factor. The marginally significant Condition main effect suggested that the expectancy

manipulation exerted some influence. The significant Decision main effect indicated that an overall shift to risk was obtained. The significant Condition by Decision interaction showed that preference selections were differently influenced depending upon the condition in which they were made.

The experimental manipulations exerted some degree of influence on the selection of I_1 and G scores. Examination of Table 13 shows that the Standard condition scores are more risky than those in the Expect-Nothing and the Expectancy conditions. However, the I_1 scores of these three conditions were not significantly different from one another. This suggests that the present marginally significant difference between conditions is largely due to the influence of the manipulation on G score selection. This possibility is clearly reflected in the Condition by Decision interaction. (This interaction is likely a function of the failure of the Expectancy condition to shift to risk.)

The significant Decision main effect vouched for the occurrence of an overall group shift to risk. However, as pointed out in the preceding paragraph, it did not occur in all three conditions. Using the analysis of variance error term, t tests were performed on the I_1 to G score difference, collapsed over the nonsignificant anonymity factor.⁸ These tests indicated that the group shifts were statistically significant in the Standard, $t(24) = 6.287$, $p < .001$, and in the Expect-Nothing, $t(24) = 2.874$, $p < .01$, conditions but not in the Expectancy condition, $t(24) = -0.270$, N.S. The occurrence of a group shift in

the Expect-Nothing condition argues for the reliability, if not for the robustness, of the group shift effect. On the other hand, the G score in the Expectancy condition was not more risky than the I_1 score of that condition (indeed it was slightly more cautious). It would appear that the manipulation, while not appreciably influencing pretest preferences, did exert an effect on later responses.

The overall differences between I_1 and I_2 scores were examined by a $3 \times 2 \times 2 \times 5$ (Condition by Decision by Anonymity by Groups) analysis of variance with repeated measures on the Decision factor (see Table 15). This analysis employed subjects' mean scores as the data units. The significant Decision main effect, the significant Condition by Decision interaction, and the marginally significant Condition main effect mirror the results obtained in the preceding I_1 to G analysis. In addition, t tests using the analysis of variance error term obtained similar results to those in the preceding analysis. The group shift in the Expect-Nothing, $t(24) = 2.735$, $p < .02$, and in the Standard, $t(24) = 4.328$, $p < .001$, conditions were statistically significant. There was no group shift to risk in the Expectancy condition, $t(24) = -0.519$, N.S. The significant Group main effect reflects overall decision differences from one group to the next.

Table 16 depicts the results of the $4 \times 2 \times 2$ (Condition by Decision by Anonymity) analysis of variance with repeated measures on the Decision factor which was performed on the G and I_2 scores.

TABLE 13

EXPERIMENT III: Condition by Decision Means (Collapsed over Anonymity) for all Conditions

Condition	I_1	G	I_2
Standard	5.567	4.400	4.525
Expect-Nothing	6.058	5.525	5.400
Expectancy	5.950	6.000	6.075
No Pretest		4.625	4.483

TABLE 14

EXPERIMENT III: Analysis of Variance Summary Table for the Condition
by Decision by Anonymity (3 x 2 x 2) Analysis on I_1 and
G Scores: Group Means as Data Points

Source	df	MS	F
Condition (C)	2	5.568	3.193*
Anonymity (A)	1	0.389	.1
C X A	2	0.035	.1
Groups (G)	24	1.744	---
Decision (D)	1	4.537	26.355**
C X D	2	1.851	10.754**
D X A	1	0.474	2.754
C X D X A	2	0.195	1.132
D X G	24	0.172	---

* p .10

** p .001

TABLE 15

EXPERIMENT III: Analysis of Variance Summary Table for the Condition
by Decision by Anonymity by Groups. (3 x 2 x 2 x 5) Analysis on the
 I_1 and I_2 Scores: Subject Means as Data Points

Source	df	MS	F
Condition (C)	2	14.817	2.980*
Anonymity (A)	1	1.850	1
C X A	2	0.401	1
Group (G)	24	4.973	2.139**
Subjects (S)	60	2.325	---
Decision (D)	1	12.403	14.271***
D X C	2	5.304	6.103**
D X A	1	0.834	1
D X C X A	2	2.185	2.514
D X G	24	0.869	1.073
D X P	60	0.810	---

* p < .10

** p < .01

*** p < .001

TABLE 16

EXPERIMENT III: Analysis of Variance Summary Table for the Condition
by Decision, by Anonymity (4 x 2 x 2) Analysis on the G and I₂
Scores: Group Means as Data Points

Source	df	MS	F
Condition (C)	3	11.408	5.087*
Anonymity (A)	1	0.050	1
C X A	3	0.447	1
Groups (G)	32	2.242	---
Decision (D)	1	0.006	1
C X D	3	0.093	1
D X A	1	0.000	1
C X D X A	3	0.158	1.021
D X G	32	0.155	---

* p < .01

of the Standard, Expect-Nothing, Expectancy and No Pretest conditions. The main effect for the Decision factor indicated that there were no significant differences between the G and the I_2 scores, and the non-significant main effect for Anonymity indicated that once again this factor had little influence.

The strong between-condition differences suggested that the experimental manipulations had considerable impact on the G and I_2 selections in this experiment. Examination of Table 12 shows that this effect was due to the more cautious outcomes in the Expect-Nothing and (especially) the Expectancy conditions relative to those of the standard and No Pretest conditions. A t test collapsing over anonymity and using the analysis of variance error term indicated that the G score of the Standard condition was significantly more risky than that of the Expectancy condition, $t(64) = 5.660$, $p < .001$. Thus, subjects who were given explicit information about a forthcoming discussion selected more cautious group decisions than subjects who were given no specific information about impending activities. Exposure to the Expect-Nothing manipulation also resulted in a relatively cautious selection of group decisions relative to the Standard condition, $t(64) = 3.980$, $p < .001$. Although a group shift to risk occurred in this condition, its group decision was still significantly less risky than that of the Standard condition. Perhaps the manipulation, while not acting to prevent a shift (as would appear to have been the case in the Expectancy condition), did exert some influence on the G and I_2 selection in this condition.

A further issue examined in the experiment was the effect of pretest completion. An independent t test was performed between the G score of the No Pretest condition and the I_1 score of the Standard condition, $t(18) = 2.839$, $p = .02$. The difference between the I_1 of the Standard condition and the I_2 of the No Pretest condition was also significant, $t(18) = 2.912$, $p = .01$. These differences supported the position that pretest completion is not causally implicated in the group shift effect. Group discussion by itself was sufficient to cause selection of alternatives which were more risky than those selected by individuals alone. This finding was consistent with the results in the first experiment but not with the opposing trend in the second. It is important to realize that the somewhat contradictory findings of this series of experiments were not inconsistent with the findings of previous research on the pretesting question. The trend in previous research tends to favour the conclusion that pretest-treatment interaction has little influence on the selection of G and I_2 scores. On the whole, the findings of these experiments supported that conclusion.⁹

Based on the results of the above analyses, a few conclusions may be drawn. First, anonymity, as manipulated, did not have an effect on the I_1 , G , and I_2 scores. Second, an overall group shift to risk occurred. However, there was no shift in the Expectancy condition, an occurrence in need of explanation. Third, the manipulation of expectancy-inducing cues did not significantly influence the selection of pretest preferences; it did have an effect on the selection of group decisions and I_2 preferences. Finally, the comparison of the No Pretest and the Standard conditions suggested that pretest completion was not causally implicated in the group shift effect.

A number of questions remain. Why, if the group shift is such a reliable phenomenon, was no shift to risk found in the Expectancy condition? Did all subjects tend to expect discussion in this condition and/or did they expect to have to justify their preferences to the experimenter or, to the others? Were subjects in the Expect-Nothing condition more inclined to think that they would be free to go following completion of the pretest than subjects in the other conditions? The post-experimental questionnaire was intended to help clarify the relationship between the manipulations, subjects' stated explanations, and the shift to risk; it should help provide answers to these questions.

The questionnaire probed four aspects of the pretesting phase:

- (1) Did the subject expect a group discussion?
- (2) Did the subject anticipate having to justify his I_1 preferences to the experimenter?
- (3) Did the subject anticipate having to justify his I_1 preferences to the others present?
- (4) Did the subject think he could leave upon completion of the pretest?

The first three questions were particularly relevant to the expectation-manipulation relationship while the fourth was best used to judge the success of the Expect-Nothing condition. A summary of those results is presented in Table 17. Examination of the Yes-No responses to the first of these questions indicated three things. First, subjects in the Standard and the Expect-Nothing conditions did not evidence differing degrees of expectation. Second, subjects in the Expectancy condition were significantly more likely to admit to anticipating discussion than those in the Standard condition, $\chi^2(1) = 12.894, p < .001$, or the Expect-Nothing condition, $\chi^2(1) = 10.335, p < .01$. Third, a surprising number of subjects receiving

TABLE 17

EXPERIMENT III: Mean Responses Collapsed Over Anonymity to Questions 1, 2, and 3 of the First Impressions Questionnaire by Condition and (for Questions 2 and 3) by Response to Question 1

Condition	Response	Number of Subjects	(1) Did you Expect Discussion		(2) Justify to Experimenter		(3) Justify to Others	
			No-Yes	\bar{X}	No-Yes	\bar{X}	No-Yes	\bar{X}
Standard	No	(26)	3.00	3.37	2.31	2.60		
	Yes	(4)	5.74		4.50			
Expect-Nothing	No	(25)	3.08	3.33	3.24	3.82		
	Yes	(5)	4.60		6.80			
Expectancy	No	(13)	4.15	3.77	4.69	5.63		
	Yes	(17)	3.47		6.35			
Collapsed Across Condition	No	(64)	3.26	3.49	3.16	4.02		
	Yes	(26)	4.04		6.15			

the strong Expectancy manipulation (13 of 30) said that they did not expect discussion. This was reminiscent of results in the previous experiment in which 16 subjects in the Expectancy condition and 12 subjects in the Explicit Expectancy condition indicated that they did not expect discussion. The present manipulation was intended to be even stronger than the Explicit Expectancy condition of Experiment II. Since the first page of the booklet was devoted to nothing else, it seemed to the experimenter that subjects could hardly have avoided noticing the explicit and conspicuous information about a forthcoming discussion. Presumably almost all of the subjects should have indicated that they expected group discussion; 13 did not.

Additional informal data was obtained orally. The experimenter asked subjects in the Expectancy condition why they felt there would be no discussion. Each of the nine subjects who were questioned indicated that they had been aware of the expectancy-inducing instructions but that they had not believed them. Although reasons offered for this disbelief ranged from a general distrust of psychology experiments to the perceived length of time available, the outcome was essentially the same. It would seem fairly safe to conclude that even if they did not believe it, subjects in the Expectancy condition were at least aware of the warning that group discussion was forthcoming. This fact may help to account for the failure to find a group shift to risk in the Expectancy condition. Perhaps subjects who were aware of the instructions may have been more inclined to think about having to justify their responses even if they did not believe that discussion was forthcoming.

10

As was the case in Experiment II, subjects who said they did not expect discussion selected more risky I_1 scores (5.750) than those who said that they did expect discussion (6.125).¹¹ While in the expected direction, this difference was not even marginally significant, $t(88) = 1.066$, N.S. This lack of difference may in part reflect the fact that subjects in the Expectancy condition in Experiment III were aware of the manipulation and were actually anticipating further activity. The I_1 selections in this condition are consistent with this notion. Subjects in the Expectancy condition who said they expected discussion selected a mean I_1 preference of 5.897, while those who said they did not selected a mean I_1 preference of 6.019.

Responses to the second question supplied little additional information (see Table 17). There were no between-condition differences in the perceived likelihood of having to justify initial responses to the experimenter (see Table 18).

Subjects' impressions about whether or not they might have had to justify their responses to others differed significantly as a function of the experimental conditions (see Table 17 and Table 18). They did not differ as a function of Anonymity, or of Condition in interaction with Anonymity. Subjects in the Expect-Nothing condition were more likely to indicate they expected to have to justify their initial preferences to others than those in the Standard condition. In turn, those in the Expectancy condition saw such justification as more likely than those in the Expect-Nothing condition. The salience of impending justification reflects the success of the Expectancy manipulation and supports the notion that subjects who were told about a potential discussion

TABLE 18

EXPERIMENT III: Analysis of Variance Summary Tables for the Condition
by Anonymity by No-Yes Response (3 x 2 x 2) Unweighted Means
Analysis on Questions 2 and 3 of the First Impressions Questionnaire

Source	df	MS	F
Justification to the Experimenter (Question 2)			
Condition (C)	2	0.244	1
Anonymity (A)	1	5.699	1.247
No-Yes (N-Y)	1	6.698	1.466
C X A	2	11.504	2.517*
C X N-Y	2	7.292	1.596
A X N-Y	1	16.118	3.527*
C X A X N-Y	2	6.596	1.443
Subjects	78	4.570	---
Justification to the Others (Question 3)			
Condition (C)	2	24.435	7.313**
Anonymity (A)	1	3.936	1.178
No-Yes (N-Y)	1	61.479	18.401***
C X A	2	1.849	1
C X N-Y	2	4.448	1.331
A X N-Y	1	0.879	1
C X A X N-Y	2	0.716	1
Subjects	78	3.341	---
* p < .10			
** p < .005			
*** p < .001			

would be likely to believe that the discussion would involve defending and/or justifying their own initial preferences. Across conditions, the more likely that justification is perceived to be, the less risky are the G and L responses.

Responses to question three were also examined as a function of whether or not subjects said they expected discussion during the pre-testing phase. The analysis of variance included Yes-No responses to the first question as two levels of another independent variable. This analysis (see table 18) demonstrated that subjects who said they expected group discussion were significantly more likely to anticipate having to justify their preferences to others than were subjects who said they did not expect discussion. Thus, when group discussion was expected a defense of one's opinion was also perceived as more likely. In addition, between-condition differences were apparent among those subjects who gave a No response to the first question. Subjects in the Expectancy condition who said they did not expect discussion were somewhat more inclined (mean of 4.69 on Question three) to anticipate justifying their responses to others than similar subjects in the Standard (2.31) or the Expect-Nothing (3.24) conditions. Thus, subjects in the Expectancy condition who said that they did not expect discussion seemed to have a fairly strong impression that they would have to justify their responses to others. It is possible that this anticipation of justification may have resulted from subjects' awareness of the expectancy inducing instructions and may have been implicated in the failure to obtain a group shift to risk in the Expectancy condition.

The fourth question in the post-experimental questionnaire solicited subjects' opinions about whether or not they would be free to leave after completing the pretest. Only about one-fifth to one-quarter of the subjects in each of the three conditions indicated that they expected to be able to go. Most felt there was more to the experiment than the initial completion of the CDQ items. This did not help to clarify the cautious results in the expectancy condition. It did supply information that the Expect-Nothing manipulation did not work. Subjects in that condition were no more inclined to see the experiment as terminating after pretest completion than subjects in the other two conditions.

In summary, a few observations may be offered. First, an overall group shift to risk (with two of three conditions shifting) was obtained. This supports the notion that group shift to risk is a reliable phenomenon. Second, the results of the No Pretest condition in comparison with the Standard condition supports the conclusion that pretest completion does not influence the group shift to risk. Third, anonymity, as manipulated, did not influence the results, either directly or in interaction with other variables. Fourth, manipulated expectancy, while not affecting I_1 choices, did exert considerable influence on group decisions and I_2 scores. Finally, examination of the First Impressions Questionnaire showed that the Expectancy manipulation worked, that the Expect-Nothing condition did not, and that subjects' expectations may well have been implicated in their failure to shift to risk in the Expectancy condition.

A Post-Hoc Analysis

One additional analysis was conducted to further examine expectation effects. In the second and third studies, subjects who said they expected discussion tended to select more cautious initial preferences than those who did not. Since in both experiments these findings were based only on responses to the first question, a more stringent criterion was employed to examine the notion that subjects who anticipate discussion or justification also make more cautious initial choices than ones who do not. The justification measures as well as the responses to the first question were used to split subjects into two groups: Anticipators and Non-anticipators. The Non-Anticipators were defined as those subjects who said they did not expect discussion and who selected a value of three or less on both of the justification questions. Anticipators were those who said they expected discussion and/or selected four or more on either or both of the two expected-justification scales. The results of this post hoc analysis are presented in Table 19.

Considered across all six conditions in Experiments II and III, a high degree of expectation tended to be associated with the selection of more cautious initial preferences than a lower one. A simple independent t test conducted on the overall mean differences between the Anticipators and the Non-Anticipators indicated that subjects who were prone, by their own admission, to expect further item-related activities also selected more cautious I_1 preferences than less prone subjects, $t(178) = 2.949, p < .01$. It is of equal interest to note that in each of the six conditions examined those who expected discussion or justification consistently selected more cautious initial preferences.

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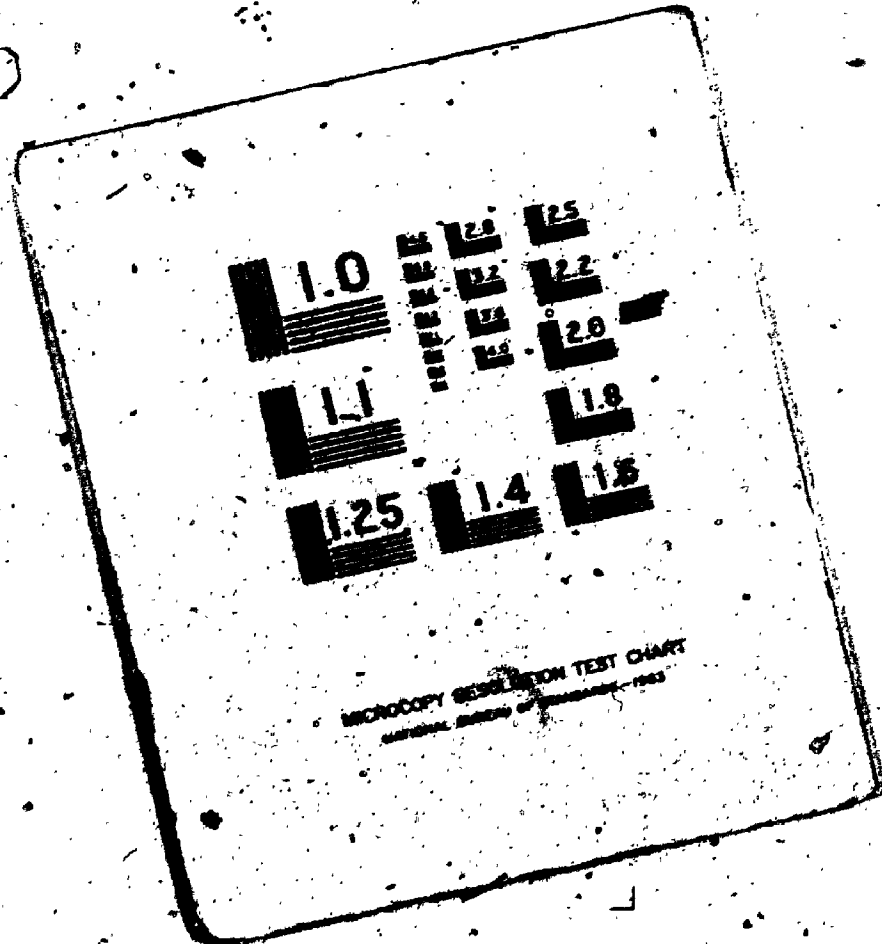


TABLE 19

EXPERIMENTS II AND III: Mean Pretest Responses and Number of Responses by Condition for Subjects Classified as Anticipators and Non-anticipators

Experiment	Condition	Non-anticipators		Anticipators	
		N	\bar{x}	N	\bar{x}
II	Standard	13	4.93	17	5.32
	Expectancy	6	5.44	24	6.06
	Explicit Expectancy	4	5.54	26	5.73
	Total	23	5.17	67	5.74
III	Standard	16	5.56	14	5.57
	Expect-Nothing	11	5.05	19	6.64
	Expectancy	1	3.75	29	6.03
	Total	28	5.29	62	6.11
Collapsed Total Both Experiments		51	5.24	129	5.92

GENERAL DISCUSSION

Experiment I failed to obtain definitive evidence about all of the issues which it set out to investigate. Although, as expected, a significant group shift to risk was obtained, no influence of manipulation of subjects' expectations materialized. On the other hand, the inclusion of the No Pretest condition helped clarify the effects of pretest completion: The findings were consistent with the notion that pretest completion is not instrumental for the group shift. Experiments II and III were conducted in order to further examine the expectancy question by attempting to tease out factors which might contribute to subjects' expectations and to obtain additional data about the influence of pretest completion.

The following discussion, in attempting to integrate the results of present and past research, will focus on three main issues. The first issue involves the influence of subjects' expectations on their selection of I_1 , G, and I_2 preferences and the nature of factors which may mediate this influence. Second, the effect of pretest completion on selection of G and I_2 preferences will be examined. Scrutiny of the present results in conjunction with previous outcomes should suggest some tentative conclusions about this problem. Finally, evidence pertaining to the reliability and robustness of the group shift phenomenon will be discussed.

Effects of Expectancy

In Experiment I there were no significant pretest preference

differences due to the manipulation of expectancy or to the Payment main effects. There was, however, a significant Condition by Payment interaction which suggested that expectancy manipulations did influence I_1 selection depending on whether or not subjects were paid for their participation in the experiment. Examined within levels of Payment, it appeared that non-paid subjects were prone to select more cautious initial preferences in the Expect-Nothing and Alone-Replication conditions than in the Standard and Expectancy conditions. Choices in the Standard condition were, however, very similar to those in the Expectancy condition. Subjects who were told to expect discussion did not choose more cautious initial preferences than subjects who were not told what to expect. Although this experiment hinted that subjects' expectations influence their pretest selections, it did not support the notion that expectation of future activity may lead to cautious I_1 selection. Similarly, the expectancy manipulation in Experiment III failed to provide support for this notion. The I_1 selections in this powerful expectancy condition were less risky than those in the Standard condition but this difference was not even marginally significant. These two experiments did not indicate that manipulating expectation of discussion would result in cautious I_1 preference selection.

Experiment II did offer some support for this notion but it was not strong support (the analysis of variance on pretest scores was only marginally significant, and t tests indicated that only one of the two expectancy conditions produced a mean I_1 score which was significantly less risky than that of the Standard condition), and in

the light of the other two studies must be considered with some scepticism. It would seem reasonable to suppose that since such a result was obtained in the second study, it also should have been obtained as a result of the stronger expectancy manipulation in the third. On the whole, examination of I_1 selection as a function of condition revealed only minimal support for an expectation-cautious I_1 selection relationship.

The postexperimental First Impressions Questionnaire employed in the second and third studies offered some additional evidence that there was a relationship between anticipation of future activity and I_1 selection. Subjects in Experiment II who said they did not expect discussion (based on responses to Question one) tended to select more risky initial preferences than subjects who said they did anticipate discussion. A similar, although weak, relationship was obtained in Experiment III. In addition, the more stringent examination reported in Experiment III offered further support for the existence of this relationship. Anticipators consistently selected more cautious initial preferences than did the Non-anticipators. It may be concluded that to some degree subjects who tend to select more cautious initial choices also think that they will have to discuss them with or justify them to others. This particular evidence is, of course, correlational in nature and does not allow one to conclude that anticipation causes cautious I_1 selection.

The third study in this series obtained fairly strong evidence that under certain circumstances expectation may influence the selection of G and I_2 preferences. The Condition main effect and the

Condition by Decision interaction found in the analyses using the I_1 and G and the I_1 and I_2 scores vouch for the influence of the manipulation on the group decision and I_2 scores. The G and I_2 scores of the Expectancy condition were less risky than those of the Standard condition. In addition, as reflected in the interaction, no shift to risk (and no trend to greater risk) was found in the Expectancy condition. The very explicit manipulation may have suppressed the shift in this condition. The results obtained in the Expectancy condition in Experiment II were not inconsistent with this finding. The failure to obtain a significant shift in that condition may have been suggestive of some degree of suppression of the group shift effect. The expectancy manipulation in that study was not as powerful as the one in the third experiment, and thus was perhaps less likely to influence G and I_2 preferences.

If shift suppression in Experiment III was attributable to increased expectation, it was not apparent in responses to the first question. There were no essential between-study differences in the proportion of subjects in expectancy conditions who admitted to expecting discussion. However, subjects in the Expectancy condition of Experiment III were distinguished from subjects in other expectancy conditions by their perception of the likelihood of having to justify their responses to others. As can be seen in Table 16, subjects who claimed to expect discussion and those who did not both thought that impending justification was a distinct possibility. Such uniformity of anticipation (especially for those who said they did not expect discussion) did not occur in the expectancy conditions in the second study. Perhaps when subjects anticipate the need to justify their initial responses they are less prone to shift to risk. 13

Two other points deserve comment. First, anonymity, as manipulated in this experiment, did not influence subjects' responses to the CDQ items either by itself or in interaction with any other variable. It did not appear to be a contributing factor in the experiment since it did not temper or mediate the effects (such as they were) of expectation.¹⁴ Second, the difficulty of obtaining a powerful manipulation was quite apparent. In order to have subjects believe what the experimenter wanted them to believe, considerable effort had to be expended. Even then many subjects failed to believe the experimenter. The manipulation was successful only to the extent that subjects thought impending justification was very likely. This suggests that expectations may not have been strongly aroused in the first experiment, and in addition that past studies may not have been seriously influenced by such an elusive effect.

In conclusion a number of points may be made. First, although the evidence is not strong, it would appear that expectation of group discussion (or justification of initial choice) did have some effects, albeit a weak one, on the selection of initial preferences. Second, subjects' expectations, provided those expectations were strongly aroused, may have made them more caution prone in their selection of G and I₂ preferences. That is, there was no shift in the Expectancy condition in Experiment III. Third, in the present context, anonymity did not influence the results. Finally, given the difficulty encountered in convincing subjects that they should expect further activities, and the minimal effect such expectation appears to have on I₁ preferences, excessive concern about this factor, provided reasonable care is taken in study preparation, would not seem to be appropriate.

Influence of Pretest Completion

The second major issue addressed concerns the influence of pretest completion on the selection of G and I_2 scores. The present findings will be discussed and then considered in conjunction with results of previous research.

The first experiment found evidence that pretest completion did not influence the shift effect; G scores in the No Pretest condition were significantly more risky than I_1 preferences in the Standard condition. In the second study, however, the scores in the No Pretest condition were not significantly riskier than the I_1 score of the Standard condition; indeed they were very similar to it. This finding would suggest that pretest completion may be implicated in the shift. Such a conclusion is prevented only by the fact that the G and I_2 No Pretest scores were not significantly less risky than those in the Standard condition.

In Experiment III the G score of the No Pretest condition was again significantly more risky than the I_1 score of the Standard condition and not less risky than the G score of that condition. This finding supported the conclusion that the pretest was not causally implicated in the occurrence of the group shift effect. Were Experiments I and III the only ones presented, the conclusion would be straightforward and unambiguous; however, there was the second study to consider. Taken as a whole, these three experiments mirror the outcome variability which has been found in previous research and which has made it so difficult to formulate definitive statements about the influence of pretest completion.

Suggestive but frequently non-significant findings have plagued group shift research in the past and may well continue to do so in the future. The present series of experiments suggest that obtaining differences with between groups designs may be difficult. This notion was consistent with the previously mentioned and similar suggestion made by Baron et al. (1974). Those authors thought that the group shift, although reliable, may be sufficiently weak that a repeated measures design (controlling for intersubject variation) is needed in order to obtain significant I_1 to G differences. Previous research which found significant I_1 of Standard to No-Pretest differences (Myers et al., 1974; Gaskell et al., 1973) obtained No Pretest scores which were much more risky than the I_1 scores of the Standard condition. The present Experiments I and III also found unusually large mean differences between the I_1 of the Standard and the G of the No Pretest conditions. Perhaps, large mean differences are necessary in order to overcome the variability inherent in the data.

Taking the insights from the data reported here, an attempt was made to compare the Standard and No Pretest results of the various studies which have examined the question of the effects of pretest completion. These studies have differed in several ways. Different subject populations, payment or the lack of it, different CDQ items and numbers of items, sex differences, and presumably a host of other factors have undoubtedly contributed to interstudy variation. Thus a direct comparison using the condition by decision means from each study was not attempted. Rather the mean I_1 score of the various Standard conditions was selected as a baseline or zero change point, and all

other scores were represented as differences (I_1 of Standard minus Other Score) from the I_1 selection. The results of this comparison, a comparison which must be viewed with some care, are presented in Figures 1 and 2.

A cursory inspection of these Figures makes two points immediately apparent. First, shift magnitude within the Standard conditions, whether I_1 to G or I_1 to I_2 was quite similar from one study to the next. The only real exception to this was the somewhat larger shift found in Experiment III of the present series. Second, the results of the No Pretest conditions were less homogeneous. This was especially true for the I_1 of Standard to the I_2 of No Pretest differences shown in Figure 2. Clearly the disparate outcomes in the current group of experiments were not unusual. When an experimenter undertakes a No Pretest condition he should perhaps be prepared to expect anything.

Awareness of the existence of these differences, while instructive, is not too helpful in suggesting what causes them. Perhaps they are simply a reflection of random variation in the data. Another possibility is that pretest completion exerts some type of stabilizing influence on the group discussion or the group decision making process such that group decisions are likely to be restricted in range relative to the I_1 selections. In addition there may be subtle mediating variables which may fluctuate in degree and type from situation to situation depending on the nature of some other factor (e.g., sex of subjects, size of group) or combination of factors. A number of possibilities (sex of subjects, payment, size of discussion groups, source of subjects, length of discussion time, and number of items employed) were considered but

Figure 1. A comparison of pretesting studies using CDQ items,
based on I_1 of Standard as zero point: I_1 of
Standard v.s. G of Standard and G of No Pretest.

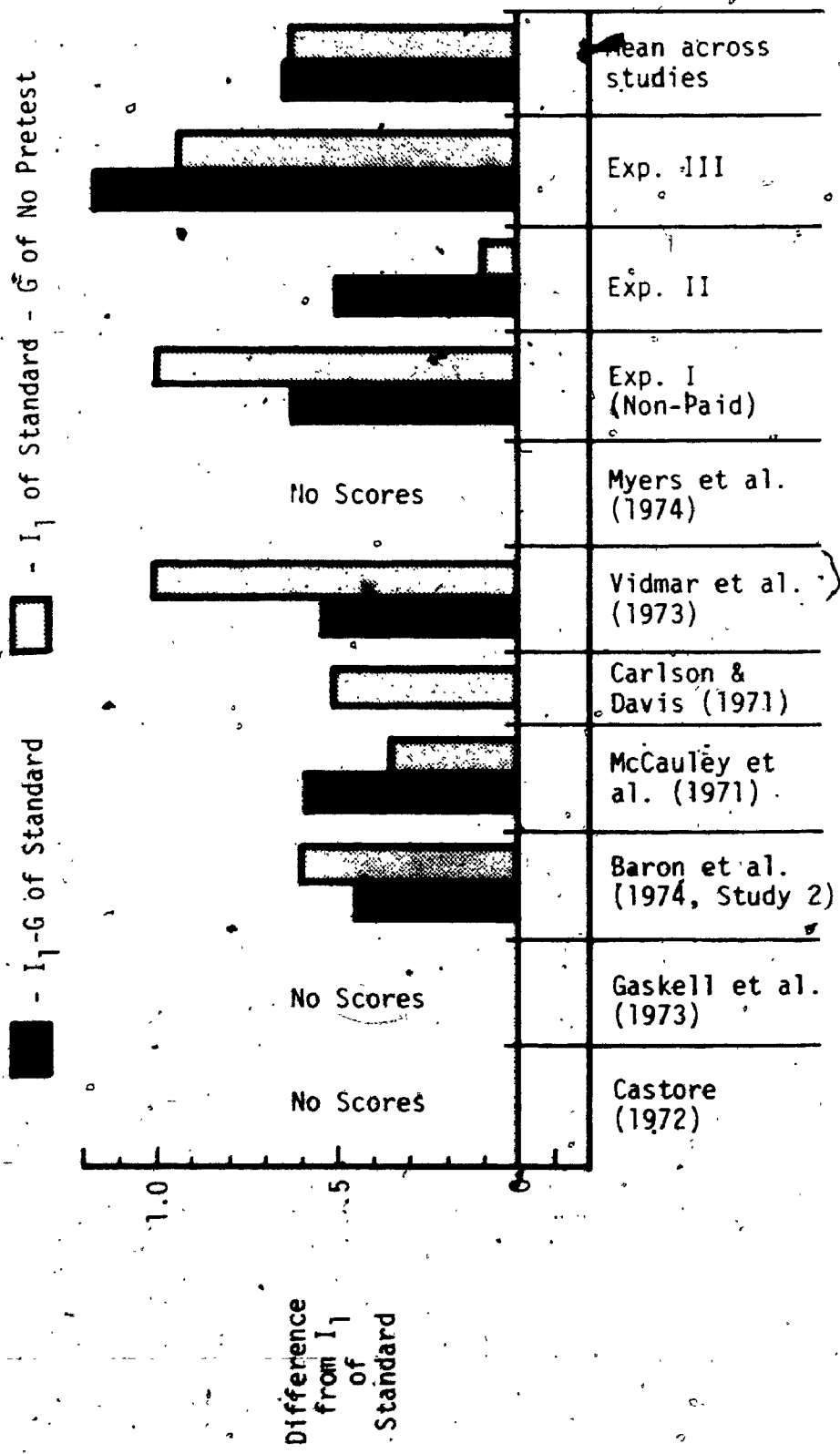
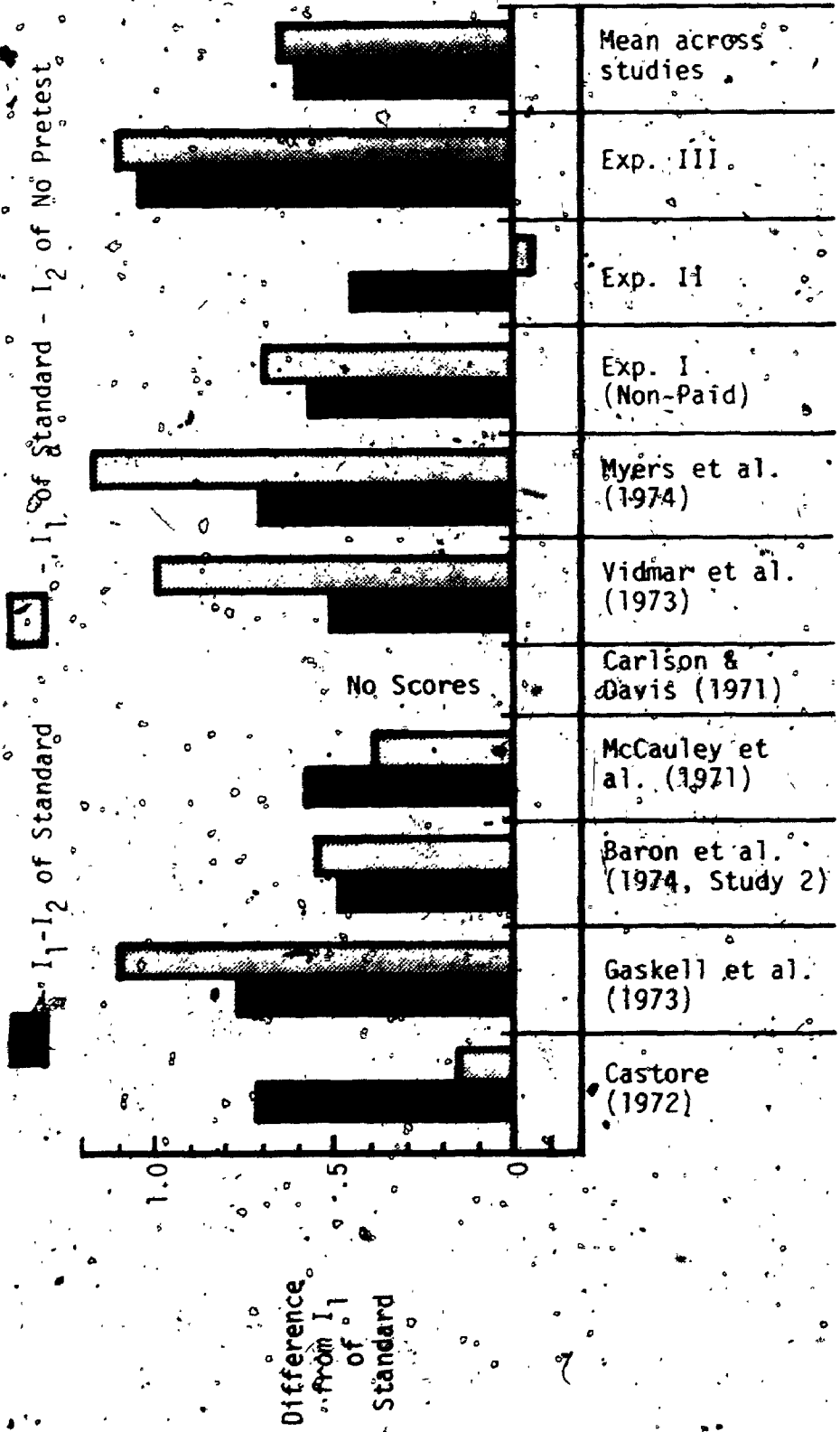


Figure 2. A comparison of pretesting studies using CDQ items,
based on I_1 of Standard as zero point: I_1 of
Standard v.s. I_2 of Standard and I_2 of No Pretest.



none were consistently associated with greater or lesser magnitude of No Pretest results. At present, it is not possible to suggest what factors, if any, are responsible for these results.

In summary, it appears that pretest completion, while not completely without influence, is a matter of secondary importance; robustness is of primary importance, as discussed in the following section.

First, it seems unlikely that I_1 selection (in interaction with the experimental treatment of group discussion) acts to cause the group shift effect. As can be seen in Figures 1 and 2, all except one of the No Pretest condition scores were more risky (some significantly so and some not) than the mean I_1 preferences of the Standard conditions.

Second, since in no case were the G and/or I_2 preferences of the No Pretest condition significantly more risky than the analogous scores in the Standard conditions, there is little reason to think that pretesting might be suppressing the extent of the group shift. The No Pretest results which were more risky than G and I_2 preferences of Standard conditions were probably symptomatic of variability rather than suppression.

Third, anonymity had no influence on the outcomes preferred by nonpretested subjects. Finally, it would appear to be important for future research to make some attempt to examine possible causes of the variability apparent in the preferences selected in different No Pretest conditions.

It should be noted that this last statement, while not negating the first conclusion, does temper or modify it to some degree.

While a pretest by treatment interaction does not appear to cause the shift, the pretest may be implicated to the extent that it stabilizes the magnitude of the shift.

Reliability and Robustness of the Shift

The third issue deals with both the reliability and the robustness of the phenomenon. The reliability question essentially asks to what extent the effect is obtained when the basic pretest, group discussion (and decision) and posttest paradigm is employed. It is also concerned with the extent to which various changes in presentation may influence the shift even though the basic design remains the same. The second issue, that of robustness, deals with the effects of group discussion when a within groups design is not employed and asks whether or not group discussion by itself results in increased risk.

Given the traditional paradigm there is ample evidence attesting to the group shift's reliability (see Cartwright, 1971). Although some authors (e.g., Clark and Willems, 1969) have questioned shift reliability and have suggested that instructional changes (apparently minor ones) may prevent its occurrence, most research does vouch for its replicability. The present series of studies, obtaining overall shifts in all three cases, are indicative of a fairly reliable phenomenon. The Expect-Nothing condition in Experiment III did not, as originally intended, demonstrate that subjects who do not expect further activity will, if given the opportunity, shift (most subjects, 22 of 30, in this condition did not think that the experiment would terminate following pretest completion). Rather, reliability was demonstrated to the extent that a shift occurred with another and different set of pretest instructions. The third study in the present series also suggested that anonymity did not influence the nature of the shift. Subjects who were personally identified responded no differently from those who were not. 15

The shift did not occur in two of the four expectancy conditions. In one of these, the very strong manipulation in Experiment III, no shift was evident whereas in the other a non-significant trend in the right direction was apparent. Thus, subjects' perception of certain forthcoming events, provided they are quite salient, seem to reduce or prevent the occurrence of the shift. In the absence of such strong manipulations, however, the effect seems to be reliable.

The second issue, robustness of the shift, has essentially been dealt with in the preceding section on pretesting influence. Past research demonstrated that without a pretest, group discussion did not, for the most part, result in G - or I_2 preferences which were significantly more risky than pretest scores of Standard conditions. A phenomenon which readily occurs only when a within groups design is used to control for intersubject variation cannot be considered robust. Although two of three experiments in the present series found significant No Pretest differences, the third did not. The present evidence for the shift's robustness is not unqualified. The experiments reported here must lead to the conclusion (similar to that of Baron et al., 1974) that the group shift effect using CDQ items is a reliable but not an overly robust phenomenon.

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FOOTNOTES

1. Three additional conditions which were run conjointly with Experiment I but which were tangential to the main thrust of this thesis are discussed in Appendix D.
2. It was necessary to offer a fee to obtain sufficient subjects. Experiment I was carried out late in the academic year and a large majority of people in the subject pool had already obtained their required participation credits.
3. Differences between subjects' initial preferences were also tested using the group mean I_1 scores across items rather than the subject mean scores across items. The results for these analyses of variance for both the current and the succeeding two experiments are presented in Appendix F.
4. Separate analyses were employed for the I_1 to G and the I_1 to I_2 shifts. The former involved a single unanimous group decision by the participants in a particular group. Thus the appropriate unit of analysis was the group mean. The latter shifts, however, were from initial individual preferences to final individual preferences (which may be influenced by participation in group discussion). The appropriate unit of analysis here are the subjects means (within groups).

5. As pointed out in the Method section and in Footnote 2 it was necessary to offer money in order to obtain enough subjects. Thus another factor, Payment (non-paid versus paid subjects), had to be considered in the data analysis. Unfortunately it was impossible to obtain equal numbers of subjects in the two levels of the Payment factor. Although this made no difference in the analysis employing Group Means it did have to be considered when the testing involved subjects within groups in the I_1 to I_2 analysis. In this latter analysis use of all subjects would have resulted in unequal numbers in a factor other than the replication factor. Thus it was necessary to randomly drop or delete a number of subjects and/or groups from the various experimental conditions. This resulted in a total of 24 subjects (12 non-paid and 12 paid) in each of the conditions. The I_1 to I_2 analyses for Hypotheses 2 was performed on this subset of the subjects.
6. A further examination of this comparison between studies one and two was undertaken using the means of the five items which the second study had in common with the first. That is, the non-common item in Experiment II was dropped and means calculated using the same five items employed in Experiment I. Conclusions about the influence of pretest completion were unaffected. The mean I_1 score of Standard condition was 5.153 while the G and I_2 scores of the No Pretest condition were 5.020 and 5.160 respectively.

7. Subjects who indicated that they expected discussion were also asked to estimate how likely they thought discussion would be. This second question may have been perceived to be redundant since it was only answered sporadically. Thus responses to it were not considered.
8. Neither the anonymity main effect nor any of its interactions reached significance in this experiment. It was considered safe to collapse over this factor.
9. These between study comparisons were also examined using the items common to the studies being compared. Experiments I and III had three items in common, whereas Experiments II and III had four. Means calculated using the three common items did not alter conclusions about pretest influence. That is, the mean G and I_2 preferences of the No Pretest condition in Experiment I (3.458 and 3.813) were more risky than the I_1 score of the Standard condition (5.104). This was also the case when the means over the three common items were calculated for Experiment III (I_1 of Standard = 5.489; G and I_2 of No Pretest = 4.200 and 4.200 respectively).

The four items which study two had in common with study three were used to compute means for study two. The G and I_2 scores of the No Pretest condition (4.650 and 4.858 respectively) appeared somewhat more risky relative to the I_1 score of the Standard condition (5.400) than was the case when all six items were employed. This might be construed as offering tentative support

for the notion that pretest completion is not necessary for the shift. However, such a finding, the result of an internal analysis, must be viewed with extreme caution. It cannot be considered to substantially alter the conclusions arrived at in the body of the paper.

10. Rather than being suspicious and disbelieving, subjects may have been confused by the question about whether or not they expected discussion. A brief "pilot" study was run to examine this possibility. After participating in an Expectancy condition analagous to that in Experiment III, some subjects (from a summer session introductory social psychology class) were asked to indicate what the instructions told them would happen. All 18 of these subjects admitted hearing the expectancy instruction (although three indicated they did not believe discussion would really occur). Of the twenty subjects who completed the form used in the third study, four indicated that they did not expect discussion. This is consistent with the notion that subjects in the third study were aware of the instructions even if they did not believe them.
11. When the four items which Experiment II had in common with Experiment III were considered a similar relationship was obtained. On the four common items in Experiment II, subjects who said they expected discussion had a more cautious mean I_1 selection (5.552) than subjects who indicated they did not expect discussion (5.953).

As reported in the discussion of Experiment II, these values were 5.384 and 5.905 respectively when all six items were used.

12. This post hoc analysis occasioned a final comparison using common items. A second post hoc analysis was carried out for Anticipators and Non-Anticipators using the four common items for Experiment II (see Table 21, Appendix G). Once again the common items examination did not suggest that different conclusions were appropriate. When common items were used there was a significant difference between the I_1 preferences of Anticipators and Non-Anticipators $t(178) = 2.457, p < .02$.

13. Experiment III suggests that expectation of discussion results in cautious group decisions and no shift to risk. To further explore this notion, an additional examination was undertaken using the group decisions and mean group I_1 to G shift scores. Groups were defined either as Non-anticipating Groups or as Anticipating Groups. Since group mean scores were employed, the anticipation measure had to be one which would represent the group rather than the individuals comprising it. Non-anticipating Groups were considered to be those in which one member or no members expected discussion and in which the mean of each justification question was less than four. Anticipating Groups were those in which two or three subjects said they expected discussion and/or the mean of either justification question was four or greater.

The results, while very weak, were in a direction consistent with the results of the Expectancy condition in Experiment III. That is, across both experiments Anticipating Groups tended to select more cautious group decisions and to show smaller shifts to risk than Non-anticipating groups. The mean group-decision score for the 22 Non-anticipating Groups was 4.958, while that for the 38 Anticipating Groups was 5.368. The mean I_1 to G shift for the former was also somewhat larger, (+0.664) than that for the latter (+0.420).

14. It should be emphasized that anonymity in this experiment means only that subjects did not sign their names to any of the experimental materials. However, they could be publically identified with their opinions in the sense that they had face-to-face contact with two other group members who, during discussion, would find out what they had selected. Thus these subjects were in a situation which some authors (e.g., Deutsch and Gerard, 1955; Mouton, Blake and Olmstead, 1956; and Raven, 1959) might consider to be public with attendant strong conformity pressures. However, at I_2 the subjects were indeed anonymous both with regards to the experimenter and the other subjects. The subjects were nameless and completed the final preferences without interaction.
15. Anonymity was also manipulated in the study by Baron et al. (1974) as a means of assessing real personal opinion change as opposed to change induced by conformity to the group's opinion. It was

expected that anonymous subjects who were not personally identified would be more free to revert to their original preferences after the group decision if that decision did not represent their own opinion. Evidence of I_2 recidivism in their anonymous No Pretest condition lead those authors to conclude that the shift is, at least in part, attributable to conformity. The present experiment, while not designed with that issue in mind, offered no support for such a conclusion.

APPENDIX A

Choice Dilemma Items used in
experiments I, II, and III

Mr. A is captain of College X's football team. College X is playing its traditional rival, College Y, in the final game of the season. The game is in its final seconds, and Mr. A's team, College X, is behind in the score. College X has time to run one more play. Mr. A, the captain, must decide whether it would be best to settle for a tie score with a play which would be almost certain to work, or, on the other hand, should he try a more complicated and risky play which could bring victory if it succeeded, but defeat, if not.

Imagine that you are advising Mr. A. Listed below are several probabilities or odds that the risky play will work. Please check the lowest probability that you would consider acceptable for the risky play to be attempted.

(Wallach and Kogah)

(used in Experiments I, II, III)

Mr. C, an electrical engineer, who is married and has one child, has been working for a large electronics corporation since graduating from college five years ago. He is assured of a lifetime job with a modest, though adequate salary, and liberal pension benefits upon retirement. On the other hand, it is very unlikely that his salary will increase much before he retires. While attending a convention, Mr. C is offered a job with a small, newly founded company which has a highly uncertain future. The new job would pay more to start and would offer the possibility of a share in the ownership if the company survived the competition of the larger firms.

Imagine that you are advising Mr. C. Listed below are several probabilities or odds of the new company's proving financially sound. Please check the lowest probability that you would consider acceptable to make it worthwhile for Mr. C to take the new job.

(Wallach and Kogah)

(used in Experiments I, II, III)

Mr. E, a competent chess player, is participating in a national chess tournament. In an early match, he draws the top-favoured player in the tournament as his opponent. Mr. E has been given a relatively low ranking in view of his performance in previous tournaments. During the course of his play with the top-favoured man, Mr. E notes the possibility of a deceptive though risky maneuver which might bring him a quick victory. At the same time, if the attempted maneuver should fail, Mr. E would be left in an exposed position and defeat would almost certainly follow.

Imagine that you are advising Mr. E. Listed below are several probabilities or odds that Mr. E's deceptive play would succeed. Please check the lowest probability that you would consider acceptable for the risky play in question to be attempted.

(Wallach and Kogan)

(used in Experiments I, II, III)

Mr. B, a college senior, has studied the piano since childhood. He has won amateur prizes and given small recitals, suggesting that Mr. B has considerable musical talent. As graduation approaches, Mr. B has the choice of going to medical school to become a physician, a profession which would bring certain prestige and financial rewards, or entering a conservatory of music for advanced training with a well known pianist. Mr. B realizes that even upon completion of his piano studies, which would take many more years and a lot of money, success as a concert pianist would not be assured.

Imagine that you are advising Mr. B. Listed below are several probabilities or odds that Mr. B will become a successful concert pianist. Please check the lowest probability that you would consider acceptable for Mr. B to enter the conservatory.

(used in Experiments II, III)

Mr. F, a 30 year-old research physicist, has been given a five-year appointment by a major university laboratory. As he contemplates the next five years, he realizes that he might work on a difficult, long-term problem which, if a solution could be found, would resolve basic scientific issues in the field, and bring high scientific honours. If no solution were found, however, Mr. F would have little to show for his five years in the laboratory, and this would make it harder for him to get a good job afterwards. On the other hand, he could, as most of his professional associates are doing, work on a series of short-term problems where solutions would be easier to find, but where the problems are of lesser scientific importance.

Imagine that you are advising Mr. F. Listed below are several probabilities or odds that a solution would be found to the difficult long-term problem that Mr. F has in mind. Please check the lowest probability that you would consider acceptable to make it worthwhile for Mr. F to work on the more difficult long-term problem.

(Wallach and Kogan)

(used in Experiments I, II)

Mr. D is currently a college senior who is very eager to pursue graduate study in chemistry leading to the Doctor of Philosophy degree. He has been accepted by both University X and University Y. University X has a world-wide reputation for excellence in chemistry. While a degree from University X would signify outstanding training in this field, the standards are so very rigorous that only a fraction of the degree candidates actually receive the degree. University Y, on the other hand, has much less of a reputation in chemistry, but almost everyone admitted is awarded the Doctor of Philosophy degree, though the degree has much less prestige than the corresponding degree from University X.

Imagine that you are advising Mr. D. Listed below are several probabilities or odds that Mr. D would be awarded a degree at University X, the one with the greatest prestige. Please check the lowest probability that you would consider acceptable to make it worthwhile for Mr. D to enroll in University X rather than University Y.

(Wallach and Kogan)

(used in Experiments I, II)

APPENDIX B

I₁ and G Cover Instructions
for Experiments I, II and III^a

- a. Note: In the anonymous condition of Experiment III the place for subjects' names were not included on the cover sheets.

Name: _____

Life Dilemmas Questionnaire

On the following pages, you will find a series of situations that are likely to occur in everyday life. The central person in each situation is faced with a choice between two alternative courses of action, which we might call X and Y. Alternative X is more desirable and attractive than alternative Y, but the probability of attaining or achieving X is less than that of attaining or achieving Y.

For each situation on the following pages, you will be asked to indicate the minimum odds of success you would demand before recommending that the more attractive or desirable alternative, X, be chosen.

Read each situation carefully before giving your judgement. Try to place yourself in the position of the central person in each of the situations. There are five situations in all. Please do not omit any of them.

When making your decision, each time make a check mark in front of the statement which you have chosen as the alternative of your personal choice. Remember that each time the riskier alternative is always assumed to be more desirable than the safer course.

Similarly, the meaning of the odds you are being asked to mark are hypothetical, and thus it is not your task to decide what the odds might actually be in a real life situation. The odds you mark indicate the lowest odds you would be willing to take or still advise the central figure to give the risky alternative a try.

When considering the items, you may return to a previous one if you wish to change your answer after seeing some of the others.

You will have about 10 minutes to complete the questionnaire. Work rapidly, on your own without disturbing and/or consulting others about your decision.

As soon as you have completed the questionnaire you may go. When you have finished leave the questionnaire on the desk in front of you and pick up a copy of the information sheet at the door on your way out. It explains the nature and purpose of the experiment. Please leave quietly so that you won't disturb those who are still working.

If you have any questions ask them now by raising your arm.

(Experiment I: pretest cover-sheet for the Expect-Nothing Condition)

Name: _____

Life Dilemmas Questionnaire

On the following pages, you will find a series of situations that are likely to occur in everyday life. The central person in each situation is faced with a choice between two alternative courses of action, which we might call X and Y. Alternative X is more desirable and attractive than alternative Y, but the probability of attaining or achieving X is less than that of attaining or achieving Y.

For each situation on the following pages, you will be asked to indicate the minimum odds of success you would demand before recommending that the more attractive or desirable alternative, X, be chosen.

Read each situation carefully before giving your judgement. Try to place yourself in the position of the central person in each of the situations. There are five situations in all. Please do not omit any of them.

When making your decision, each time make a check mark in front of the statement which you have chosen as the alternative of your personal choice. Remember that each time the riskier alternative is always assumed to be more desirable than the safer course.

Similarly, the meaning of the odds you are being asked to mark are hypothetical, and thus it is not your task to decide what the odds might actually be in a real life situation. The odds you mark indicate the lowest odds you would be willing to take or still advise the central figure to give the risky alternative a try.

When considering the items, you may return to a previous one if you wish to change your answer after seeing some of the others.

You will have about 10 minutes to complete the questionnaire. If you have any questions ask them now by raising your arm.

(Experiment I: pretest cover sheet for the Alone-Replication, Standard, and Expectancy Conditions)

Name: _____

Life Dilemmas Questionnaires

The questionnaire you now have in front of you is the same one which you completed before. You have taken it in order to familiarize yourself with all the situations, and to give you some idea where you might stand on each one. What we are interested in now is having the group discuss each question in turn and arrive at a unanimous decision on each. You will recognize that a unanimous decision is different from a majority vote. That is, discussion should continue until everyone is in agreement. This time you should not return to a question; discuss each one until the group decision is reached and then go on to the next. When the group reaches its decision you are to mark it on your questionnaire so that you will have a record of the group's decisions.

I am not going to participate in any of the discussion, although I will be available to answer any procedural questions which may arise.

You will have 25 minutes to discuss and complete the five items. If you have any questions then open your door and I will help you out. Otherwise start immediately. Remember, in each case we are interested in only the group's final decision.

(Experiment I: group discussion cover sheet for the Standard Condition)

Life Dilemmas Questionnaire

The questionnaire you now have in front of you is the same one which you completed before. You have taken it in order to familiarize yourself with all the situations, and to give you some idea where you might stand on each one. What we are interested in now is having the group discuss each question in turn and arrive at a unanimous decision on each. You will recognize that a unanimous decision is different from a majority vote. That is, discussion should continue until everyone is in agreement. Although I want you to represent your initial choice in the group I do not want you to feel absolutely bound by it. After all you must reach unanimous group decisions on the items. This time you should not return to a question; discuss each one until the group decision is reached and then to go to the next. When the group reaches its decision you are to mark it on your questionnaire so that you will have a record of the group's decisions.

I am not going to participate in any of the discussion, although I will be available to answer any procedural questions which may arise.

You will have 25 minutes to discuss and complete the five items. If you have any questions then open your door and I will help you out. Otherwise start immediately. Remember, in each case we are interested in only the group's final decision.

(Experiment I: group discussion cover sheet for the Expectancy Condition)

Name: _____

Life Dilemmas Questionnaire

On the following pages you will find a series of situations that are likely to occur in everyday life. The central person in each situation is faced with a choice between two alternative courses of action, which we might call X and Y. Alternative X is more desirable and attractive than alternative Y, but the probability of attaining or achieving X is less than that of attaining or achieving Y.

For each situation on the following pages your group will be asked to decide on the minimum odds of success to demand before recommending that the more attractive or desirable alternative, X, be chosen.

Consider each situation carefully before reaching your decision. Try to place yourselves in the position of the central person in each of the situations. There are five situations in all. Please do not omit any of them.

When the group reaches its decision you are to mark it on your questionnaire so that you will have a record of the group's decisions. Each time make a check mark in front of the alternative chosen by the group. Remember that each time the riskier alternative is always assumed to be more desirable than the safer course.

Similarly, the meaning of the odds you are being asked to mark are hypothetical, and thus it is not your task to decide what the odds might actually be in a real life situation. The odds you mark indicate the lowest odds you would be willing to take or still advise the central figure to give the risky alternative a try.

What we are interested in is having the group discuss each question in turn and arrive at a unanimous decision on each. You will recognize that a unanimous decision is different from a majority vote. That is, discussion should continue until everyone is in agreement. Remember to mark the group's decision on your questionnaire.

I am not going to participate in any of the discussion, although I will be available to answer any procedural questions which may arise.

You will have about 30 minutes to discuss and complete the five items. If you have any questions then open your door and I will help you out. Otherwise start immediately. Remember, in each case we are interested in only the group's final decision.

(Experiment I: group discussion cover sheet for the No Pretest Condition)

Name: _____

Life Dilemmas Questionnaire

On the following pages, you will find a series of situations that are likely to occur in everyday life. The central person in each situation is faced with a choice between two alternative courses of action, which we might call X and Y. Alternative X is more desirable and attractive than alternative Y, but the probability of attaining or achieving X is less than that of attaining or achieving Y.

For each situation on the following pages, you will be asked to indicate the minimum odds of success you would demand before recommending that the more attractive or desirable alternative, X, be chosen.

Read each situation carefully before giving your judgement. Try to place yourself in the position of the central person in each of the situations. There are six situations in all. Please do not omit any of them.

When making your decision, each time make a check mark in front of the statement which you have chosen as the alternative of your personal choice. Remember that each time the riskier alternative is always assumed to be more desirable than the safer course.

Similarly, the meaning of the odds you are being asked to mark are hypothetical, and thus it is not your task to decide what the odds might actually be in a real life situation. The odds you mark indicate the lowest odds you would be willing to take or still advise the central figure to give the risky alternative a try.

When considering the items, you may return to a previous one if you wish to change your answer after seeing some of the others.

You will have about 10 minutes to complete the questionnaire. If you have any questions ask them now by raising your arm.

(Experiment II: pretest cover sheet for the Standard and Expectancy Conditions)

Name: _____

Life Dilemmas Questionnaire

Our main interest in this experiment is with how groups make decisions. In a little while you are going to be assigned to smaller groups, taken to separate rooms, and asked to discuss and reach unanimous group decisions on the items in the Questionnaire which you have in front of you. Before I assign you to groups, however, I want you to first answer the problems as individuals. Select your own preferences carefully since I will want you to represent, or to argue for them during the group discussion.

On the following pages, you will find a series of situations that are likely to occur in everyday life. The central person in each situation is faced with a choice between two alternative courses of action, which we might call X and Y. Alternative X is more desirable and attractive than alternative Y, but the probability of attaining or achieving X is less than that of attaining or achieving Y.

For each situation on the following pages, you will be asked to indicate the minimum odds of success you would demand before recommending that the more attractive or desirable alternative, X, be chosen.

Read each situation carefully before giving your judgement. Try to place yourself in the position of the central person in each of the situations. There are six situations in all. Please do not omit any of them.

When making your decision, each time make a check mark in front of the statement which you have chosen as the alternative of your personal choice. Remember that each time the riskier alternative is always assumed to be more desirable than the safer course.

Similarly, the meaning of the odds you are being asked to mark are hypothetical, and thus it is not your task to decide what the odds might actually be in a real life situation. The odds you mark indicate the lowest odds you would be willing to take or still advise the central figure to give the risky alternative a try.

When considering the items, you may return to a previous one if you wish to change your answer after seeing some of the others.

You will have about 10 minutes to complete the questionnaire. If you have any questions ask them now by raising your arm.

(Experiment II: pretest cover sheet for the Explicit Expectancy Condition)

Life Dilemmas Questionnaire

The questionnaire you now have in front of you is the same one which you completed before. You have taken it in order to familiarize yourself with all the situations, and to give you some idea where you might stand on each one. What we are interested in now is having the group discuss each question in turn and arrive at a unanimous decision on each. You will recognize that a unanimous decision is different from a majority vote. That is, discussion should continue until everyone is in agreement. This time you should not return to a question; discuss each one until the group decision is reached and then go on to the next. When the group reaches its decision you are to mark it on your questionnaire so that you will have a record of the group's decisions.

I am not going to participate in any of the discussion, although I will be available to answer any procedural questions which may arise.

You will have 25 minutes to discuss and complete the five items. If you have any questions then open your door and I will help you out. Otherwise start immediately. Remember, in each case we are interested in only the group's final decision.

(Experiment II: group discussion cover sheet for the Standard Condition)

Name: _____

Life Dilemmas-Questionnaire

The questionnaire you now have in front of you is the same one which you completed before. You have taken it in order to familiarize yourself with all the situations, and to give you some idea where you might stand on each one. What we are interested in now is having the group discuss each question in turn and arrive at a unanimous decision on each. You will recognize that a unanimous decision is different from a majority vote. That is, discussion should continue until everyone is in agreement. Although I want you to represent your initial choice in the group I do not want you to feel absolutely bound by it. After all you must reach unanimous group decisions on the items. This time you should not return to a question; discuss each one until the group decision is reached and then to go to the next. When the group reaches its decision you are to mark it on your questionnaire so that you will have a record of the group's decisions.

I am not going to participate in any of the discussion, although I will be available to answer any procedural questions which may arise.

You will have 25 minutes to discuss and complete the six items. If you have any questions then open your door and I will help you out. Otherwise start immediately. Remember, in each case we are interested in only the group's final decision.

(Experiment II: group discussion cover sheet for the Expectancy and Explicit Expectancy Conditions)

Name: _____

Life Dilemmas Questionnaire

On the following pages you will find a series of situations that are likely to occur in everyday life. The central person in each situation is faced with a choice between two alternative courses of action, which we might call X and Y. Alternative X is more desirable and attractive than alternative Y, but the probability of attaining or achieving X is less than that of attaining or achieving Y.

For each situation on the following pages your group will be asked to decide on the minimum odds of success to demand before recommending that the more attractive or desirable alternative, X, be chosen.

Consider each situation carefully before reaching your decision. Try to place yourselves in the position of the central person in each of the situations. There are six situations in all. Please do not omit any of them.

When the group reaches its decision you are to mark it on your questionnaire so that you will have a record of the group's decisions. Each time make a check mark in front of the alternative chosen by the group. Remember that each time the riskier alternative is always assumed to be more desirable than the safer course.

Similarly, the meaning of the odds you are being asked to mark are hypothetical, and thus it is not your task to decide what the odds might actually be in a real life situation. The odds you mark indicate the lowest odds you would be willing to take or still advise the central figure to give the risky alternative a try.

What we are interested in is having the group discuss each question in turn and arrive at a unanimous decision on each. You will recognize that a unanimous decision is different from a majority vote. That is, discussion should continue until everyone is in agreement. Remember to mark the group's decision on your questionnaire.

I am not going to participate in any of the discussion, although I will be available to answer any procedural questions which may arise.

You will have about 30 minutes to discuss and complete the six items. If you have any questions then open your door and I will help you out. Otherwise start immediately. Remember, in each case we are interested in only the group's final decision.

(Experiment II: group discussion cover sheet for the No Pretest Condition)

Life Dilemmas Questionnaire

On the following pages, you will find a series of situations that are likely to occur in everyday life. The central person in each situation is faced with a choice between two alternative courses of action, which we might call X and Y. Alternative X is more desirable and attractive than alternative Y, but the probability of attaining or achieving X is less than that of attaining or achieving Y.

For each situation on the following pages, you will be asked to indicate the minimum odds of success you would demand before recommending that the more attractive or desirable alternative, X, be chosen.

Read each situation carefully before giving your judgement. Try to place yourself in the position of the central person in each of the situations. There are four situations in all. Please do not omit any of them.

When making your decision, each time make a check mark in front of the statement which you have chosen as the alternative of your personal choice. Remember that each time the riskier alternative is always assumed to be more desirable than the safer course.

Similarly, the meaning of the odds you are being asked to mark are hypothetical; and thus it is not your task to decide what the odds might actually be in a real life situation. The odds you mark indicate the lowest odds you would be willing to take or still advise the central figure to give the risky alternative a try.

When considering the items, you may return to a previous one if you wish to change your answer after seeing some of the others.

You will have about 8 minutes to complete the questionnaire. Work rapidly, on your own, without disturbing and/or consulting others about your decisions. When you have completed the questionnaire please remain seated so that you will not disturb others who are still working on the items.

(Experiment III: pretest cover sheet for the Expect-Nothing Condition)

Name: _____

Life Dilemmas Questionnaire

On the following pages, you will find a series of situations that are likely to occur in everyday life. The central person in each situation is faced with a choice between two alternative courses of action, which we might call X and Y. Alternative X is more desirable and attractive than alternative Y, but the probability of attaining or achieving X is less than that of attaining or achieving Y.

For each situation on the following pages, you will be asked to indicate the minimum odds of success you would demand before recommending that the more attractive or desirable alternative, X, be chosen.

Read each situation carefully before giving your judgement. Try to place yourself in the position of the central person in each of the situations. There are four situations in all. Please do not omit any of them.

When making your decision, each time make a check mark in front of the statement which you have chosen as the alternative of your personal choice. Remember that each time the riskier alternative is always assumed to be more desirable than the safer course.

Similarly, the meaning of the odds you are being asked to mark are hypothetical, and thus it is not your task to decide what the odds might actually be in a real life situation. The odds you mark indicate the lowest odds you would be willing to take or still advise the central figure to give the risky alternative a try.

When considering the items, you may return to a previous one if you wish to change your answer after seeing some of the others.

You will have about 8 minutes to complete the questionnaire. If you have any questions ask them now by raising your arm.

(Experiment III: pretest cover sheet for the Standard Condition)

Name: _____

The purpose of this experiment is to examine how groups make decisions. In a few minutes you are going to be assigned to two or three man groups, taken to separate rooms, and asked to discuss the four items in the questionnaire which you have in front of you. I will want your group to discuss and to reach unanimous group decisions on each of the four items. Before I assign you to groups, however, I want you to first answer the items as individuals. Select your own preferences carefully since I will want you to represent, or to argue for them during the group discussion.

PLEASE DO NOT TURN THE PAGE UNTIL ASKED TO DO SO:

(Experiment III: first pretest cover sheet for Expectancy Condition)

Life Dilemmas Questionnaire

On the following pages, you will find a series of situations that are likely to occur in everyday life. The central person in each situation is faced with a choice between two alternative courses of action, which we might call X and Y. Alternative X is more desirable and attractive than alternative Y, but the probability of attaining or achieving X is less than that of attaining or achieving Y.

For each situation on the following pages, you will be asked to indicate the minimum odds of success you would demand before recommending that the more attractive or desirable alternative, X, be chosen.

Read each situation carefully before giving your judgement... Try to place yourself in the position of the central person in each of the situations. There are four situations in all. Please do not omit any of them.

When making your decision, each time make a check mark in front of the statement which you have chosen as the alternative of your personal choice. Remember that each time the riskier alternative is always assumed to be more desirable than the safer course.

Similarly, the meaning of the odds you are being asked to mark are hypothetical, and thus it is not your task to decide what the odds might actually be in a real life situation. The odds you mark indicate the lowest odds you would be willing to take or still advise the central figure to give the risky alternative a try.

When considering the items, you may return to a previous one if you wish to change your answer after seeing some of the others.

You will have about 8 minutes to complete the questionnaire. As soon as you are finished I will assign you to groups to discuss your preferences on the items.



(Experiment III: second pretest cover sheet for Expectancy Condition)

Life Dilemmas Questionnaire

The questionnaire you now have in front of you is the same one which you completed before. You have taken it in order to familiarize yourself with all the situations, and to give you some idea where you might stand on each one. What we are interested in now is having the group discuss each question in turn and arrive at a unanimous decision on each. You will recognize that a unanimous decision is different from a majority vote. That is, discussion should continue until everyone is in agreement. This time you should not return to a question; discuss each one until the group decision is reached and then go on to the next. When the group reaches its decision you are to mark it on your questionnaire so that you will have a record of the group's decisions.

I am not going to participate in any of the discussion, although I will be available to answer any procedural questions which may arise.

You will have 20 minutes to discuss and complete the four items. If you have any questions then open your door and I will help you out. Otherwise start immediately. Remember, in each case we are interested in only the group's final decision.

(Experiment III: group discussion cover sheet for the Standard and the Expect-Nothing Conditions)

Life Dilemmas Questionnaire

The questionnaire you now have in front of you is the same one which you completed before. You have taken it in order to familiarize yourself with all the situations, and to give you some idea where you might stand on each one. What we are interested in now is having the group discuss each question in turn and arrive at a unanimous decision on each. You will recognize that a unanimous decision is different from a majority vote. That is, discussion should continue until everyone is in agreement. Although I want you to represent your initial choice in the group I do not want you to feel absolutely bound by it. After all you must reach unanimous group decisions on the items. This time you should not return to a question; discuss each one until the group decision is reached and then go on to the next. When the group reaches its decision you are to mark it on your questionnaire so that you will have a record of the group's decisions.

I am not going to participate in any of the discussion, although I will be available to answer any procedural questions which may arise.

You will have 20 minutes to discuss and complete the four items. If you have any questions then open your door and I will help you out. Otherwise start immediately. Remember, in each case we are interested in only the group's final decision:

(Experiment III: group discussion cover sheet for the Expectancy Condition)

Name: _____

Life Dilemmas Questionnaire

On the following pages, you will find a series of situations that are likely to occur in everyday life. The central person in each situation is faced with a choice between two alternative courses of action, which we might call X and Y. Alternative X is more desirable and attractive than alternative Y, but the probability of attaining or achieving X is less than that of attaining or achieving Y.

For each situation on the following pages, you will be asked to indicate the minimum odds of success you would demand before recommending that the more attractive or desirable alternative, X, be chosen.

Read each situation carefully before giving your judgement. Try to place yourself in the position of the central person in each of the situations. There are four situations in all. Please do not omit any of them.

When making your decision, each time make a check mark in front of the statement which you have chosen as the alternative of your personal choice. Remember that each time the riskier alternative is always assumed to be more desirable than the safer course.

Similarly, the meaning of the odds you are being asked to mark are hypothetical, and thus it is not your task to decide what the odds might actually be in a real life situation. The odds you mark indicate the lowest odds you would be willing to take or still advise the central figure to give the risky alternative a try.

What we are interested in is having the group discuss each question in turn and arrive at a unanimous decision on each. You will recognize that a unanimous decision is different from a majority vote. That is, discussion should continue until everyone is in agreement. Remember to mark the group's decision on your questionnaire.

I am not going to participate in any of the discussion, although I will be available to answer any procedural questions which may arise.

You will have about 20 minutes to discuss and complete the four items. If you have any questions then open your door and I will help you out. Otherwise start immediately. Remember, in each case we are interested in only the group's final decision.

(Experiment III: group discussion cover sheet for the No Pretest Condition)

APPENDIX C

First Impressions Questionnaires
used in Experiments II and III^a

^aNote: In the anonymous condition of Experiment III. The place for subjects' names were not included on the Questionnaire.

First Impressions Questionnaire

The last thing I would like you to do is to think back to the first part of the experiment in the other room when you were filling out the items for the first time. I am interested in what your thoughts and feelings were at that time and would particularly like to know what you thought would happen next in the experiment. Now that you know what the experiment involves you may find it difficult to remember the way you felt at the beginning but I would like you to try and then to answer the following questions about those feelings.

- 1) When you were first working on the items did you think that you would be discussing them with others?

Please check one: _____ no
_____ yes

IF YES please estimate by checking one of the spaces below how confident you were that discussion would follow.

____:____:____:____:____:____:____:____:
Not at all con- extremely
fident; just a confident
slight suspicion

- 2) When you were first working on the items did you think it likely that I might want you to justify, or defend, your choices on the items EITHER:

To me personally: _____
very unlikely very likely
or did not occur
to me.

OR

To the others? _____
very unlikely very likely
or did not occur
to me.

- 3) Are there any other thoughts you had or comments you would like to make about your initial expectations?
Please comment on the back of the page.

APPENDIX D

A Discussion of three conditions
employing Universalism-Particularism
(U.P.) Orientation Items

Three conditions employing non-CDQ items were run concurrently with the five CDQ item conditions in order to examine the generalizability of the phenomenon. These three conditions employed Universalism-Particularism (U.P.) Orientation items selected from among those used by Horne and Long (1972). The results of previous studies using between-groups designs were less contradictory when non-CDQ items were employed than when CDQ items were used. It was thought that CDQ items may operate on a dimension of risk whereas other items (i.e. attitude items, altruism items, etc.) do not. Dealing with a dimension of risk may make subjects more concerned about others' preferences or about item-related arguments than dealing with some non-risk dimension. Thus CDQ items may be differently affected by the pretesting phase than non-CDQ items. If the group shift tends to be influenced more by expectancy and/or pretesting when CDQ items are used than when other items are used, it would suggest that the parent phenomenon (a group shift on CDQ items) might be plagued by artifact while its offspring (a group shift on other items) is not. Baron et al. (1974) have suggested that the CDQ item group shift may be a weak but reliable phenomenon accounting for little variance. They go on to point out, however, that this would not be calamitous for the group shift area unless shifts on other types of items were similarly weak. In the same vein, evidence of a strong artifactual influence on the CDQ group shift would not brand the phenomenon as trivial unless a group shift on other items was similarly influenced. The three U.P. item conditions were analagous to the Expect-Nothing, Standard, and No Pretest conditions employed with the CDQ items in Experiment I. The only difference was the use of these items. The

items and the relevant cover sheets are presented in Appendix E. The areas of interest discussed in Experiment I were also applicable to these three conditions; one need only replace "greater or lesser risk" with "greater or lesser universalism".

It was suggested that if expectancy influenced I_1 selection in the Standard condition then the scores in the Standard condition would be more particularistic than those in the Expect-Nothing conditions. A 2 by 2 (Condition by Payment) unweighted means analysis of variance completed on the I_1 scores showed that the Condition main effect and the Condition by Payment interaction were nonsignificant: manipulated expectancy did not influence I_1 selection. The statistically significant main effect for Payment, $F(1,60) = 5.722$, $p < .025$, indicated that paid subjects select more particularistic I_1 preferences than non-paid subjects regardless of which condition they find themselves in (the condition by decision by payment means for these three conditions are presented in Table 19).

Additional analyses of variance mirroring those in Experiment I were conducted on the I_1 to G, the I_1 to I_2 , and the G to I_2 scores. No significant main effects or interactions were obtained in any of these analyses. There was no evidence that expectation influenced I_1 , G, or I_2 selection, that pretesting influenced G or I_2 selection, or that there was a universalism shift. The failure to find an overall significant universalism shift on this Canadian sample (Horne and Long also employed a Canadian sample) is somewhat surprising. It suggests that the findings of Horne and Long (1972) must be viewed with some care. These items may not be an appropriate area for generalization of the group shift effect.

TABLE 20

Universalism-Particularism Items: Condition by Decision by Payment
 Mean Scores for all Conditions

Condition	Payment			Collapsed			
	I ₁	G	I ₂	I ₁	G	I ₂	
Expect-Nothing	(Non-paid)	3.85	--	--	4.34	--	--
	(Paid)	4.91	--	--			
Standard	(Non-paid)	3.35	3.20	3.20	3.94	3.63	3.68
	(Paid)	4.30	3.88	3.96			
No Pretest	(Non-paid)		5.0	4.80	--	4.33	4.26
	(Paid)		3.92	3.94			

APPENDIX E

Universalism-Particularism Items and
Cover Sheets for the Three U.P.
Item Conditions.

1. You have come from a secret meeting of a board of directors of a company. You have a close friend who will be ruined unless he can sell his stock in the company before the board's decision becomes known. You happen to be having dinner at his home this same evening.

How likely would you be to tell your friend about the board's decision? Place a check (✓) beside the statement which best indicates the probability that you would tell your friend about the decision.

- there is no (0%) chance you would tell your friend.
 there is a 10% chance you would tell your friend.
 there is a 20% chance you would tell your friend.
 there is a 30% chance you would tell your friend.
 there is a 40% chance you would tell your friend.
 there is a 50% chance you would tell your friend.
 there is a 60% chance you would tell your friend.
 there is a 70% chance you would tell your friend.
 there is a 80% chance you would tell your friend.
 there is a 90% chance you would tell your friend.
 you would tell your friend (100% chance).

2. You are a student casting for a new college play. An actor who is a close friend of yours is reading for an important role in competition with another actor who is not a friend. Your friend needs a break badly. But you think your friend is less qualified for the role than his rival.

How likely is it that you would give him the role? Place a check mark (✓) beside the statement which best indicates the probability that you would give your friend the role.

3. You are riding in a car driven by a close friend, who hits a pedestrian. You know your friend went through a stop sign. You are the only witness. Your friend's lawyer says that if you testify under oath that he did not go through a stop sign, it may save him from serious consequences.

How likely is it that you would testify that your friend did not go through the stop sign? Place a check mark (✓) beside the statement which best indicates the probability that you would testify he did not go through the stop sign.

4. You are a Reserved Book Room attendant at the library. A particular reserved book is in heavy demand. A close friend of yours is pressed for time and can take out the book only at a certain hour. He has suggested that you hide the book for awhile before his arrival so he will be sure to get it. He needs it badly.

How likely is it that you would hide the book? Place a check mark (✓) beside the statement which best indicates the probability that you would hide the book for your friend.

5. There is a vacancy in your firm for a junior executive's position, for which there is one capable eligible for promotion. Your son, who has left college, squandered money, and lost several jobs, asked you for the job. He needs the job badly.

How likely is it that you would give him the job? Place a check mark (✓) beside the statement which best indicates the probability that you would give your son the job.

Name: _____

Life Dilemmas Questionnaire

On the following pages you will find a series of situations that are likely to occur in everyday life. You as the central person in each situation are faced with a choice between two alternative courses of action. You are asked to indicate how likely you would be to choose a particular alternative.

For example, suppose you are employed by a professor to grade examination booklets in his course. A close friend of yours makes somewhat below a passing grade. If you give him a special break on the exam you can boost him over the passing line for the course. He needs the grade badly.

You might then be asked how likely is it that you would give your friend the break.

On a scale like the one below you would be asked to place a check mark (✓) beside the statement which best indicates the probability that you would give your friend the break. Thus if you felt there was about a 50% chance that you would give your friend the break you would check the 50% statement as indicated below.

- _____ there is no (0%) chance you would give your friend the break.
- _____ there is a 10% chance you would give your friend the break.
- _____ there is a 20% chance you would give your friend the break.
- _____ there is a 30% chance you would give your friend the break.
- _____ there is a 40% chance you would give your friend the break.
- _____ there is a 50% chance you would give your friend the break.
- _____ there is a 60% chance you would give your friend the break.
- _____ there is a 70% chance you would give your friend the break.
- _____ there is a 80% chance you would give your friend the break.
- _____ there is a 90% chance you would give your friend the break.
- _____ you would give your friend the break (100% chance).

Read each situation carefully before giving your judgement. Try to place yourself in the position of the central figure in each of the situations. There are five situations in all. Please do not omit any of them.

When considering the items, you may return to a previous one if you wish to change your answer after seeing some of the others.

You will have about 10 minutes to complete the questionnaire. Work rapidly, on your own without disturbing and/or consulting others about your decision.

As soon as you have completed the questionnaire you may go. When you have finished leave the questionnaire on the desk in front of you and pick up a copy of the information sheet at the door on your way out. It explains the nature and purpose of the experiment. Please leave quietly so that you won't disturb those who are still working.

If you have any questions ask them now by raising your arm.

(U.P. Items: pretest cover-sheet for the Expect-Nothing Condition)

Name: _____

Life Dilemmas Questionnaire

On the following pages you will find a series of situations that are likely to occur in everyday life. You as the central person in each situation are faced with a choice between two alternative courses of action. You are asked to indicate how likely you would be to choose a particular alternative.

For example, suppose you are employed by a professor to grade examination booklets in his course. A close friend of yours makes somewhat below a passing grade. If you give him a special break on the exam you can boost him over the passing line for the course. He needs the grade badly.

You might then be asked how likely is it that you would give your friend the break.

On a scale like the one below you would be asked to place a check mark (✓) beside the statement which best indicates the probability that you would give your friend the break. Thus if you felt there was about a 50% chance that you would give your friend the break you would check the 50% statement as indicated below.

- _____ there is no (0%) chance you would give your friend the break.
- _____ there is a 10% chance you would give your friend the break.
- _____ there is a 20% chance you would give your friend the break.
- _____ there is a 30% chance you would give your friend the break.
- _____ there is a 40% chance you would give your friend the break.
- _____ there is a 50% chance you would give your friend the break.
- _____ there is a 60% chance you would give your friend the break.
- _____ there is a 70% chance you would give your friend the break.
- _____ there is a 80% chance you would give your friend the break.
- _____ there is a 90% chance you would give your friend the break.
- _____ you would give your friend the break (100% chance).

Read each situation carefully before giving your judgement. Try to place yourself in the position of the central figure in each of the situations. There are five situations in all. Please do not omit any of them.

When considering the items, you may return to a previous one if you wish to change your answer after seeing some of the others.

You will have about 10 minutes to complete the questionnaire. If you have any questions ask them now by raising your arm.

(U.P. Items: pretest cover-sheet for the Standard Condition).

Name: _____

Life Dilemmas Questionnaire

The questionnaire you now have in front of you is the same one which you completed before. You have taken it in order to familiarize yourself with all the situations, and to give you some idea where you might stand on each one. What we are interested in now is having the group discuss each question in turn and arrive at a unanimous decision on each. You will recognize that a unanimous decision is different from a majority vote. That is, discussion should continue until everyone is in agreement. This time you should not return to a question; discuss each one until the group decision is reached and then go on to the next. When the group reaches its decision you are to mark it on your questionnaire so that you will have a record of the group's decisions.

I am not going to participate in any of the discussion, although I will be available to answer any procedural questions which may arise.

You will have 25 minutes to discuss and complete the five items. If you have any questions then open your door and I will help you out. Otherwise start immediately. Remember, in each case we are interested in only the group's final decision.

(U.P. Items: group discussion cover-sheet for the Standard Condition).

Name: _____

Life Dilemmas Questionnaire

On the following pages you will find a series of situations that are likely to occur in everyday life. You as the central person in each situation are faced with a choice between two alternative courses of action. You are asked to indicate how likely you would be to choose a particular alternative.

For example, suppose you are employed by a professor to grade examination booklets in his course. A close friend of yours makes somewhat below a passing grade. If you give him a special break on the exam you can boost him over the passing line for the course. He needs the grade badly.

You might then be asked how likely is it that you would give your friend the break.

On a scale like the one below you would be asked to place a check mark (✓) beside the statement which best indicates the probability that you would give your friend the break. Thus if you felt there was about a 50% chance that you would give your friend the break you would check the 50% statement as indicated below.

- _____ there is no (0%) chance you would give your friend the break.
- _____ there is a 10% chance you would give your friend the break.
- _____ there is a 20% chance you would give your friend the break.
- _____ there is a 30% chance you would give your friend the break.
- _____ there is a 40% chance you would give your friend the break.
- _____ there is a 50% chance you would give your friend the break.
- _____ there is a 60% chance you would give your friend the break.
- _____ there is a 70% chance you would give your friend the break.
- _____ there is a 80% chance you would give your friend the break.
- _____ there is a 90% chance you would give your friend the break.
- _____ you would give your friend the break (100% chance).

What we are interested in is having the group discuss each question in turn and arrive at a unanimous decision on each. You will recognize that a unanimous decision is different from a majority vote. That is, discussion should continue until everyone is in agreement. When the group reaches its decision you are to mark it on your questionnaire so that you will have a record of the group's decision.

Consider each situation carefully before reaching your decision. Try to place yourselves in the position of the central person in each of the situations. There are five situations in all. Please do not omit any of them.

You will have about 30 minutes to discuss and complete the five items. If you have any questions then open your door and I will help you out. Otherwise start immediately. Remember, in each case we are interested in only the group's final decision.

(U.P. Items: group discussion cover-sheet for the No Pretest Condition).

APPENDIX F

A brief report of some analyses of variance on the I_1 scores of Experiments I, II, and III.^a

^a These analyses, except for using group mean scores as data units, were analagous to those already performed and reported in the body of this thesis.

The 4×2 (Condition \times Payment) unweighted means analysis of variance performed on the group mean I_1 scores of Experiment I obtained results similar to those using subject mean scores. There were no significant main effects for Condition $F(3,24) = 1.541$, N.S., or for Payment, $F(1,24) > 1$, N.S. However, the Condition by Payment Interaction, which was significant in the previous analysis, did not reach significance when group means were employed, $F(3,24) = 1.812$, N.S. This is not surprising given that the degrees of freedom were sharply curtailed in the present analysis and that the interaction was not overly robust in the first instance.

The one-way analysis of variance (3 levels of Condition) performed on the second study using group means lead to conclusions similar to those of the subject's means analysis. The Condition main effect was marginally significant, $F(2,27) = 2.569$, $p < .10$, hinting, as before, that the manipulations may have exerted some influence on I_1 selection.

The 3×2 (Condition by Anonymity) analysis of variance used for the group mean I_1 scores of Experiment III obtained, as did its predecessor, neither significant main effects for Condition, $F(2,24) > 1$, N.S., or for Anonymity, $F(1,24) = 1.265$, N.S., nor a significant Condition by Anonymity interaction, $F(2,24) > 1$, N.S.

In sum, this examination of the I_1 data did not, on the whole, lead to conclusions different from those fostered by the first examination. As previously pointed out, the only difference involved the current, and not surprising, non-significance of the Condition by Payment Interaction in Experiment I. However, since a strong test of the I_1 differences is appropriate in these exploratory studies, the

use of subjects' mean scores as the units of analysis is to be preferred.

APPENDIX G

A re-examination of I_1 preferences of
Non-anticipators and Anticipators using
items common to both Experiments II and III

TABLE 21

EXPERIMENTS II and III: Mean Pretest Responses and Number of Responses by Condition for Subjects Classified as Anticipators and Non-anticipators: Mean Scores Calculated from Four Items Common to Both Experiments (Compare to Table 19)

Experiment	Condition	Non-anticipators		Anticipators	
		N	\bar{x}	N	\bar{x}
II	Standard	13	5.08	17	5.65
	Expectancy	6	6.25	24	6.01
	Explicit Expectancy	4	5.44	26	5.73
	Total	23	5.45	67	5.81
III	Standard	16	5.56	14	5.57
	Expect Nothing	11	5.05	19	6.64
	Expectancy	1	3.75	29	6.03
	Total	28	5.29	62	6.11
Collapsed Total Both Experiments		51	5.36	129	5.96